Debt, Drought, and Mobility: How Bankruptcy Institutions Shape Adjustment to Economic Shocks*

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Abstract

Understanding how economies adjust to negative shocks is a central question in economics. This paper provides causal evidence on how indebtedness affects occupational and geographic mobility after a shock. We focus on 19th-century U.S. agriculture and exploit repeated introductions and repeals of federal bankruptcy laws, granular variation in drought exposure, and rich county- and individual-level data. We find that bankruptcy protection, which reduces indebtedness, facilitates reallocation: farmers affected by droughts are more likely to exit agriculture and migrate to manufacturing jobs in urban counties. In contrast, in the absence of bankruptcy protection, farmers remain in agriculture and sell portions of their farms to other locals. These effects are reinforced by homestead exemptions, which protect local assets; bankruptcy law transforms such assets from illiquid to liquid ones. Our findings underscore the role of institutions in enabling individual mobility and structural change following adverse economic shocks.

Keywords: spatial economics, bankruptcy, geographic mobility, occupational mobility, structural change, droughts, agriculture
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1. INTRODUCTION

Negative economic shocks—such as droughts, job loss, or firm closures—are a recurrent feature of market economies. Individuals can recover from a negative shock by moving to a different location or to a different occupation (Nakamura, Sigurdsson, and Steinsson 2022, Hornbeck and Naidu 2014, Imbert, Seror, Zhang, and Zylberberg 2022). However, mobility frictions can hinder reallocation across space and across occupations, leaving individuals trapped in poor locations and low growth occupations.

An important variable that may affect spatial and occupational mobility is household "indebtedness". Indebtedness can reduce the incentives to exert effort (until debts are repaid, returns will go to creditors), may discourage risk-taking and reduce the opportunities to access further borrowing (Jackson 2001). Yet, causal evidence about how indebtedness affects occupational choices and spatial mobility decisions is scarce. On the one hand, indebtedness may force individuals to abandon their occupation or residence to avoid creditors. On the other hand, indebtedness may hinder the ability to move across occupations or locations if it prevents an individual from covering the required moving costs or destroys the incentives to exert effort. The literature has focused on studying moving costs as the most common source of mobility friction. However, the size and relevance of these moving costs differs across studies (Bryan and Morten 2019, Lagakos, Mobarak, and Waugh 2023, Imbert and Papp 2020) suggesting that other barriers, such as indebtedness, may be preventing frictionless mobility across jobs or locations.

In this paper, we study how indebtedness affects occupational and spatial mobility after a negative shock, and whether its role is moderated by the availability of liquid assets, such as cash, to cover moving costs. Estimating the role of indebtedness and liquidity when an individual faces a negative shock is empirically challenging, as these factors are likely correlated with the current and future economic outcomes of each individual.

We overcome this challenge by studying mobility and occupational decisions of farmers living in the United States between 1850 and 1920. First, by focusing on farmers, we can cleanly identify and observe exogenous negative shocks: droughts. Second, the United States introduced individual bankruptcy regulation during this period for the first time. The recurrent introduction and repeal of bankruptcy regulation, allowing individuals to get a full discharge from their debts, generates exogenous variation across time on the level of indebtedness. In addition, differences in state-level exemptions that protected part of an individual's property from creditors allow us to estimate the importance of liquidity once debt relief is available. Finally, this period allows us to combine aggregate-level and individual-level datasets to explore a wide range of outcomes, such as the county-level agricultural census and individual census records that are linked across census years.

Our empirical strategy follows the logic of a triple-differences design: we compare counties that experienced a drought in a given decade to those that did not, and examine how outcomes differ depending on whether a bankruptcy law was in place. Our setting offers several advantages over a standard triple-difference approach. First, we do not compare drought-prone areas to non-drought-prone areas. Instead, we compare counties within the same climate zone, defined by their long-run drought probability. Second, our setting resembles a repeated cross-sectional experiment: each decade, a different set of counties within the same climate zone experiences a drought, providing multiple draws of treatment assignment rather than a single comparison. Climate-zone-by-decade fixed effects absorb decade-specific differences across climate zones, and county fixed effects absorb any time-invariant differences across counties. Our identification relies on the assumption that within a given climate zone and decade, drought timing across decades is uncorrelated with decade-specific county-level determinants of agricultural outcomes. This assumption is particularly plausible in the 19th century, when systematic weather information was

scarce, climate mechanisms were poorly understood, and long-run drought probabilities were stable across centuries. Third, the repeated introduction, repeal, and reintroduction of the bankruptcy law allows us to exploit multiple quasi-experiments, rather than relying on a single policy change as in conventional event-study designs.

Our approach requires measuring droughts at a granular spatial level with consistent year-by-year coverage across the U.S. We leverage advances in the climatology literature (Cook, Seager, Cane, and Stahle, 2007) and use drought measures constructed based on tree-ring chronologies from the most recent *Living Blended Drought Atlas*. To measure outcomes, our analysis relies on two complementary data sources. First, we use decadal data from the agricultural census (1860–1920) on the number of farms, the size distribution of farms, acreage, farm values, and crop yields for ten major crops (Haines, Fishback, and Rhode, 2018). Second, we obtain information on farmers' future occupations and locations by linking individual census records from the restricted-access IPUMS dataset, using the record linkage method developed by the *Census Linking Project* (Abramitzky, Boustan, Eriksson, Pérez, and Rashid, 2020). Our sample consists of male individuals who appear as farmers in one census year and can be observed again in the subsequent census, covering the period 1850–1920.[1]

We begin by validating that our drought measure captures negative harvest shocks for farmers. To do so, we regress crop yields at the county-crop-year level on the county-level drought measure for the corresponding harvest year. The specification includes fixed effects to control for differential time trends across climate zones, crop types, and crop specialization at the county level. The results confirm that droughts have a significant negative effect on yields in the year of harvest.

In our main analysis, we find that the availability of a bankruptcy law reduces the number of farms. This reduction is driven by a reduction in the number of large or middle-sized farms, probably exceeding the amount of land protected by homestead exemptions, and accompanied by a reduction in the number of farmers and farm-related employment. The law also appears to facilitate access to credit for those who remain in farming: machinery and livestock per farm increase, suggesting greater capital investment. In contrast, in the absence of bankruptcy protection, the number of farms increases after a drought. Large farms are broken up into smaller units, resulting in an increase in the number of farms per county. Farmers use land sales to smooth out the negative shock, since land tends to be the only asset available for farmers to response to unexpected shocks (Arteaga, De Roux, Gafaro, Ibanez, and Pellegrina, 2025). In the absence of bankruptcy regulation, remaining farmers are unable to expand investment in machinery or livestock and instead rely more heavily on manual labor and effort, as indicated by a rising share of improved land—i.e., land cleared and used for grazing, grass, tillage, or lying fallow—in total acreage.

We find evidence that the differential responses to droughts are driven by bankruptcy law turning illiquid assets into liquid ones. To show this, we exploit state-level variation in homestead exemptions—a limited form of debtor protection available even in the absence of federal bankruptcy law. These exemptions prevented creditors from seizing the debtor's farm dwelling and a small surrounding area, but allowed them to claim all other assets. Importantly, any remaining debt still had to be repaid. The homestead, while protected, remained illiquid: if the owner sold it and converted it to cash, that cash became claimable by creditors. As a result, insolvent farmers had little incentive to sell and were more likely to remain on the land.

In contrast, under federal bankruptcy law, the homestead became a usable asset for restarting. Bankruptcy proceedings discharged all debts exceeding assets, and farmers were allowed to keep their homestead. In this case, selling the homestead and converting it to cash did not trigger new claims from creditors,

[1] Individual census data for 1890 are not available. We therefore link individuals between 1880 and 1900.

since debts were already cleared. This enabled farmers to use the proceeds to finance relocation or to start a new venture, facilitating productive reallocation after a shock.

The linked individual census data allow us to decompose changes in the number of farmers into continuing farmers, inflows into farming, and outflows from it. For the latter two groups, we can further distinguish whether these changes are driven by occupational transitions, geographic mobility across counties, or both. The results suggest that in the absence of a bankruptcy law, distressed farmers tend to remain in agriculture but sell parts of their land to other locals, especially young individuals entering the labor market. In contrast, when bankruptcy protection is available, farmers affected by droughts are less likely to remain in farming and more likely to migrate—particularly to urban counties where they take up manufacturing jobs. Homestead exemptions played a key role in shaping the occupational and geographic responses of distressed farmers: acting as a constraint when assets are illiquid, and as an enabler when they become liquid through bankruptcy protection. Overall, the evidence shows that financial frictions can impede mobility and delay structural transformation. By reducing indebtedness and unlocking illiquid assets, bankruptcy regulation facilitates adjustment to negative shocks and accelerates the reallocation of labor across sectors and space.

This paper is related to a large and growing literature that studies how individuals respond to negative shocks by moving across locations or across occupations.[2] Recent papers in this literature have studied widely varied negative shocks, from labor market shocks (Borusyak, Dix-Carneiro, and Kovak, 2022; Yagan, 2014) and trade shocks (Autor, Dorn, Hanson, and Song, 2014), to weather shocks (Gröger and Zylberberg, 2016; Albert, Bustos, and Ponticelli, 2023) and natural disasters (Nakamura et al., 2022). We contribute to this literature by exploring how the response of farmers to negative drought shocks depends on their level of indebtedness and their access to liquidity. Our findings suggest that indebtedness modulates the use of moving across locations and/or occupations in response to a negative shock.

Our study is also related to a new and growing literature that explores how the financial position of an individual affects their ability to move across occupations and locations. Fonseca and Liu (2024) show that interest rate movements can leave individuals locked-in their mortgages preventing them from changing houses and discouraging self-employment. Greaney (2023) and Head and Lloyd-Ellis (2012) develop quantitative models to study how home ownership status affects geographical mobility rates in the US. Giannone, Li, Paixao, and Pang (2023) take into account how differences in the credit score, the financial position and the home ownership status of individuals are important variables to understand differences in mobility responses in Canada. Relative to these papers, we explore the role of indebtedness and wealth liquidity and exploit the passing and repeal of the Bankruptcy Acts in the US as an exogenous source of variation in indebtedness levels. Similarly to this earlier work, we confirm that indebtedness is crucial to understanding mobility and occupational choices.

We also contribute to the literature studying the presence of barriers to structural transformation. This literature has highlighted the role of land frictions (Adamopoulos and Restuccia, 2014; Chen, 2017; Manysheva, 2022; Adamopoulos, Brandt, Chen, Restuccia, and Wei, 2024), credit market frictions (Rosenzweig and Wolpin, 1993; Arteaga et al., 2025), labor market frictions (Foster and Rosenzweig, 2022) and capital mobility frictions (Bustos, Garber, and Ponticelli, 2020) in slowing down structural transformation. Our results suggest that the presence of credit market institutions and access to liquidity can be important determinants of labor mobility and contributed to speeding up structural transformation

[2]Innovation and technology-adoption is another strategy to cope with negative shocks. Moscona (2025) explores how climate disasters can incentivize the adoption of new crop-varieties and technological change targeted at better climate adaptation, exploring the Dust Bowl climate catastrophe in the 1930s.

in the late 1800s in the United States.

Finally, this work is also part of the economic history literature that studies the effect of institutional changes around the mid-1800s. Goodman (1993) and Gropp, Scholz, and White (1997) study state-level homestead exemption regulations. Abramitzky, Frank, and Mahajan (2010) explore the effects of limited liability reforms in Brazil. Koudijs and Salisbury (2020) and Koudijs, Salisbury, and Sran (2021) exploit changes in limited liability from the passing of the Women Property Right Acts in the US, while Hazan, Weiss, and Zoabi (2019) study the impact of a change in women's inheritance rights in the United States on credit, farm outcomes and occupational mobility. To this literature we contribute by studying the historical bankruptcy acts of 1867 and 1898, and we investigate how it affected economic outcomes, mobility and occupational choices.

2. HISTORICAL SETTING

The US economy during the early and mid-nineteenth century was predominantly agrarian, yet highly dynamic and volatile. In 1850, 60 percent of male workers were employed in agriculture, reflecting the sector's centrality to production and employment. The century was also marked by rapid transformation. During the so-called *Age of Mass Migration*, the United States received more than twenty million European immigrants seeking economic opportunity and social mobility. Concurrently, the frontier expanded westward toward the Pacific, as settlers occupied newly annexed territories across the South and West. Between 1800 and 1900, the number of states in the Union grew from sixteen to forty-five; by 1915, every territory except Alaska and Hawaii had achieved statehood.

Economic activity in the nineteenth century was characterized by pronounced volatility and was particularly challenging for farmers. Shocks arising from weather fluctuations, swings in international commodity prices, and periodic banking panics have generated recurrent downturns. These episodes of financial distress rendered many individuals unable to meet their contractual obligations. Insolvent debtors frequently liquidated their property under duress to avoid imprisonment, or—whenever possible—absconded to evade their creditors.

The precariousness of the macroeconomic environment was particularly severe for farmers. Agricultural production operated largely on credit: farmers typically borrowed to finance seeds, tools, and labor, with repayment contingent upon post-harvest revenues months later. The sector remained technologically primitive; mechanization was minimal before the early twentieth century, and the use of chemical fertilizers was limited. Farmers' understanding of soil conditions and agronomic cycles was rudimentary. A common belief, summarized in the aphorism "rain follows the plow," held that continuous cultivation would induce favorable weather (Libecap and Hansen, 2000).

Under such conditions, even minor shocks—droughts, pest infestations, or decline in crop prices—precipitate widespread insolvency. As Zackin and Thurston (2024) note, the persistent exposure to financial fragility "taught farmers to recognize themselves as perennial debtors." The combination of limited technological progress, dependence on credit, and vulnerability to environmental and financial disturbances made rural producers emblematic of the broader instability that characterized the antebellum and postbellum American economy.

2.1 Farmer insolvency before federal bankruptcy laws

During the nineteenth century, farmers were commonly exposed to shocks that pushed them into insolvency.[3] These conditions led to the organization of farmers into associations to lobby for state and federal laws that would shield debtors against economic shocks (Zackin and Thurston, 2024). However, for most of the century, insolvency regulation in the US was creditor-friendly, a device to enable the state to take possession of the debtor's property and use it to repay its creditors.

In the event of insolvency, a farmer faced an extremely challenging situation. In many states, insolvent individuals could be sent to debtors' jail. In addition, their property would be repossessed by the state to be auctioned off to repay creditors. Any creditor could initiate the execution of a debtor's assets, resulting in a chaotic and uncertain first-come, first-served repayment process. If the debtor's assets were insufficient to cover all liabilities, the remaining debts remained outstanding and would be enforced against any future income, creating a potentially permanent debt overhang.

While at the beginning of the 19th century imprisonment for debt was common, this practice was abolished by most states by the mid-century. In 1821, Kentucky was the first state to pass legislation discharging debtors from prison, aiming to protect insolvents from the worst consequence of insolvency. Imprisonment was abolished at the federal level in 1833, and by the 1840s, most states had abolished it. Rhode Island was the last to do so, in 1870 (Tabb, 1995; Roehrkasse, 2023; Hansen, 2001).

In addition, some states passed insolvency laws that allowed for the discharge of debts, but these were often not enforceable against out-of-state creditors (Tabb, 1995). State regulation in matters of insolvency was also challenged to be unconstitutional since it went against the sacred protection of private contracts (Zackin and Thurston, 2024).

To provide a basic safety net for debtors and their families, some states enacted homestead exemption laws, which protected a part of a debtor's property—typically defined by a maximum acreage or dollar amount—from forced sale.[4] The first such regulation was adopted in Texas in 1839. By 1897, 45 states had implemented homestead exemption laws. Figure 1 illustrates the nationwide adoption of these laws over time. However, even under these protections, creditors retained the right to claim unpaid debts if the farmer eventually sold the exempted homestead (Goodman, 1993).

Institutional mechanisms to support farmers facing insolvency were virtually nonexistent in the 19th century. Crop insurance was not available at the time. A few private initiatives failed due to the high uncertainty and large losses from droughts. Only after 1941 did the federal government start a crop insurance program for farmers (Kramer, 1983). As an alternative, farmers used their social and family networks for assistance Emerick (1897), or moved to a different state with the aim of hiding before creditors.[5]

2.2 Federal bankruptcy acts of 1841, 1863 and 1898

In response to the lack of debtor protection and the harsh consequences of insolvency, various groups and associations lobbied for a more debtor-friendly legal framework. However, political support remained

[3] Notice that for the purpose of this paper we clearly distinguish between insolvency and bankruptcy: an individual is insolvent, when they are unable to pay their financial obligations; however, an individual is bankrupt only once they went through bankruptcy proceedings according to one of the federal bankruptcy laws.

[4] Between 1880-1920, approximately 70% of farmers owned their farm.

[5] In fact, Texas tried to incentive this behavior by offering land to new settlers and pledging to protect it from any previous out-of-state creditors. The strategy of running away to avoid creditors was known as GTT for "Gone To Texas". (Olmsted, F. Law. (1857))

intermittent and uneven across party lines, preventing the establishment of a lasting institutional framework. Northern Republicans were generally supportive of bankruptcy laws, while Democrats and Southern Republicans tended to oppose it (Hansen, 2001). Proponents argued that more progressive bankruptcy regulation would provide individuals with a fresh start after adverse shocks, ensure an equitable division of assets among creditors, and reduce the cost of debt collection (Jackson, 2001). Opponents, however, raised concerns about potential moral hazard and the administrative burden such legislation could entail.

This heated debate culminated in the adoption of two federal bankruptcy acts in 1841 and 1867, which were repealed in 1843 and 1878, respectively. Finally, with growing support for pro-debtor regulation among merchants and manufacturers, the federal government passed the 1898 Bankruptcy Act, which remained in place until 1978.[6]

The federal bankruptcy acts radically changed the legal treatment of insolvency. First, they allowed any debtor to voluntarily file for bankruptcy. Second, they introduced uniform rules for insolvency and debt relief across all US states, applicable to both debtors and creditors, thereby reducing legal uncertainty. Third, the acts established an orderly and equitable procedure for debt repayment, overseen by a bankruptcy court. Debtors' assets were liquidated, and creditors were repaid on a pro-rata basis, with no distinction between in-state and out-of-state creditors. As a result of the bankruptcy procedure, the debtor was discharged from any unpaid debts remaining after liquidation. This full discharge provided a fresh start, freeing the debtor from future obligations associated with past liabilities.

With the passage of this Act, the US had created the most protective debt relief regime in the world (Zackin and Thurston, 2024). Yet, the new bankruptcy regulation did not only benefit debtors. Thanks to the uniform federal system for handling insolvencies, the law also provided creditors with more equitable and predictable procedures. In its absence, the nation's economy, it was argued, would have been "held in check for fear of granting credit" (Frimet, 1991). Consistent with this view, farm mortgage credit did not appear to contract following the introduction of the 1898 Bankruptcy Act. As shown in Figure A.1 of the Appendix, the share of mortgaged farms remained stable at around 25% between 1890 and 1910 and increased to 28% by 1920, while average mortgage interest rates declined modestly from 7% to 6%.

When it came to homestead exemptions—that is, protecting parts of debtor's property from liquidation in the course of federal bankruptcy proceedings—the federal laws generally deferred to existing state-level homestead regulations, though some federal laws stipulated a minimum exemption amount. For example, the Bankruptcy Act of 1867 established a federal exemption amount of $500, but allowed for higher exemptions if permitted under state homestead laws in effect as of 1864. In 1872, this provision was extended to include state exemption laws in effect as of 1871 (Goodman, 1993). The Bankruptcy Act of 1898 did not establish a minimum amount, but instead fully referred to state exemptions (U.S. Congress, 1898).

Take-up of the federal bankruptcy laws was generally high. After merchants and wage earners, farmers were the third-largest group of individuals initiating voluntary bankruptcy proceedings, accounting for 9.7% of all cases in 1899. Under the 1898 Bankruptcy Act, the US recorded an average of 2.1 farmer bankruptcies per 1,000 farmers annually between 1899 and 1909 (Wickens, 1936). However, take-up was low in a few states, particularly in the West South Central and South Atlantic regions. As first noted by Wickens (1936), an agricultural economist at the Bureau of Agricultural Economics, this likely reflected limited awareness of the federal law: *"Familiarity with privileges under the law probably varies from one part*

[6]The familiar chapter-based bankruptcy system (e.g., Chapter 7 and Chapter 11) was only introduced in the 1978 Bankruptcy Reform Act.

of the country to another and so influences the relative use of the bankruptcy provisions by farmers." We exclude these states from our main analysis, and later exploit them in a placebo test.[7]

3. DATA

Our analysis combines several complementary datasets: drought occurrence measures derived from tree-ring records; decadal county-level data from the U.S. Agricultural Census; linked individual-level census records from the restricted-access IPUMS files; district-court level data on bankruptcies from the 1898 federal law; and historical information on state homestead exemption laws.

Drought occurrence. Our empirical strategy requires a geographically granular measure of drought exposure with consistent annual coverage across the United States. Because no systematic instrumental weather records are unavailable for much of the nineteenth century, we instead rely on dendrochronological climate reconstructions developed from tree-ring chronologies (Fritts, Blasing, Hayden, and Kutzbach 1971). These reconstructions infer historical drought intensity from annual tree growth patterns and are widely used in the study of long-run climate variation.

Tree-ring-based drought measures offer several advantages relative to other historical climate proxies. They are highly sensitive to changes in soil moisture and evaporation, provide broad spatial coverage, and achieve annual temporal resolution, allowing for the detection of single-year droughts. They are also precisely dated, facilitating consistent regional comparisons over long horizons. Importantly, these indices extend far back in time, enabling the construction of long-run climate series unavailable from instrumental records.

The drought indices used in this study have been rigorously validated against documented historical droughts and shown to capture temporal variation in moisture conditions with high accuracy (Cook et al., 2007; Cook, Woodhouse, Eakin, Meko, and Stahle, 2004). We therefore treat them as reliable, high-resolution indicators of drought incidence across space and time during the nineteenth century.

We use the most recent version of the *Living Blended Drought Atlas* (LBDA, version 2) , which provides reconstructions of the summer Palmer Modified Drought Index (PMDI). This LBDA recalibrates the original $0.5^\circ \times 0.5^\circ$ grid developed by Cook et al. (2007, 2004) to a 5-kilometer spatial resolution, yielding annual coverage for the continental United States from 0 CE to 2006 CE. The PMDI is a continuous measure of relative moisture conditions, with positive values indicating wet periods and negative values denoting dry conditions.[8] Palmer (1965) classifies PMDI values by drought severity, providing a standardized scale for interpreting magnitudes.

We link the LBDA grid to modern county boundaries using the 1990 county shapefiles provided by NHGIS. Each county's centroid is matched to the nearest grid cell, and we manually recover unmatched observations arising from historical county mergers or splits by consulting historical boundary shapefiles (also provided by NHGIS).

Our primary measure of drought exposure is an annual indicator variable equal to one when the PMDI is less than or equal to -2, capturing moderate, severe, and extreme drought years over the period

[7] Wickens (1936) grouped these states into the West South Central and South Atlantic Census regions, where farmer bankruptcy rates remained below 1 per 1,000 farms. West South Central: Louisiana, Arkansas, Texas, Oklahoma. South Atlantic: Delaware, Georgia, South Carolina, Virginia, Maryland, North Carolina, Florida, West Virginia, District of Columbia.

[8] The PMDI is a smoothed version of the original Palmer Drought Severity Index (PDSI) first developed by Palmer (1965). The U.S. Climate Prediction Center developed the PMDI as an operational refinement of the PDSI able to handle better transitions between dry and wet periods than the original index. In practice, the PMDI gives a smoother, more reliable real-time estimate of drought severity.

1830-1930.[9]

To facilitate comparison across areas with similar long-run exposure to climatic risk, we classify counties into climate zones according to the number of drought years experienced in the preceding century, 1730–1830. Figure 2 illustrates the spatial distribution of these climate zones. In Section A.3 of the Online Appendix we show that drought frequency is highly correlated across these two centuries, providing no indication of long-run climate change during this period.

Agricultural census. We combine county-level agricultural census data from 1860-1920 from the dataset compiled by Haines et al. (2018). To ensure consistent geographic units over time, we apply the EGLP crosswalk, which maps historical ICPSR counties to standardized 1990 county boundaries based on spatial overlap.

We construct two analytical datasets. The first is a crop-by-county panel reporting yields for ten major crops: wheat, barley, oats, rye, corn, hay, hops, tobacco, rice, and cotton. The second is a county-level panel containing agricultural structure and asset measures, including the number of farms; farm size distribution; improved acreage (cleared land used for grazing, cultivation, or fallow), total acreage (improved and unimproved land), and the value of farmland assets, including land and buildings, machinery, and livestock.

Linked individual census. We obtain information on farmers' subsequent occupations and locations by linking individual census records from the restricted-access IPUMS data using the record-linkage procedure developed by the Census Linking Project (Abramitzky et al., 2020). Our sample consists of male individuals who observed as farmers in one census year and successfully linked to the subsequent census over the period 1850–1920.[10]

We define farmers using three complementary measures. The first relies on the IPUMS occupation code identifying individuals as farmers. The second expands this definition to include farm-related occupations based on the same coding scheme. The third constructs an agricultural occupation classification using the original occupation and industry strings reported in the Population Census[11]. This approach improves classification precision beyond standardized occupation codes by implementing a text-based procedure that maps reported occupations to the agricultural sector.

The classification algorithm combines deterministic keyword rules with semantic similarity methods based on sentence embeddings (Sentence-Transformers , `all-MiniLM-L6-v2` architecture). Records containing clear sector-specific keywords (e.g., "cotton picker", "crop harvester", "farm hand") are assigned deterministically. For the remaining observations, we compute embeddings of the combined occupation–industry string (e.g., occupation as "general labor" and industry as "farm") and assign sectoral categories using cosine similarity relative to predefined keyword sets. Each assignment includes a confidence score that distinguishes high-certainty classifications from more ambiguous cases. Our baseline classification includes all deterministic matches and probabilistic assignments with confidence scores above the sample median (34 percent). Section A.4 of the Online Appendix shows that all three farmer definitions are highly correlated with county-level farm counts from the Agricultural Census.

To track occupational transitions, we extend the classification procedure to additional sectors—manufacturing,

[9] For sensitivity analysis w.r.t. the drought measure, see section 4.

[10] Individual census data for 1890 are not available. We therefore link individuals between 1880 and 1900.

[11] Based on the IPUMS occupation code `OCC1950`, the classification 'Farmers' includes code 100 (farmers, owners, and tenants). Farm-related occupations include codes 100 (farmers, owners, and tenants), 123 (farm managers), 810 (farm foremen), 820 (farm laborers, wage workers), 830 (farm laborers, unpaid family workers; only distinguished after 1900), and 840 (farm service laborers, self-employed).

services, and non-occupational responses—using the same methodology. These classifications are applied to individuals identified as farmers (under any definition) in the preceding census.

Tracking geographic mobility requires harmonizing historical county boundaries, which changed substantially during the study period due to state formation and administrative reorganization. To ensure consistent spatial units over time, we incorporate the EGLP crosswalk, which maps historical NHGIS counties to standardized 1990 county definitions based on spatial overlap.

We first collapse the crosswalk to a unique historical county–year mapping by selecting the 1990 county with the largest allocation weight. We then merge this mapping to the census panel using year and NHGIS geographic identifiers, assigning each record to a stable 1990 county and retaining allocation weights. This procedure resolves inconsistencies arising from historical boundary changes, splits, and mergers.[12]

In addition to individual outcomes, we classify counties as urban or non-urban using the IPUMS `URBAN` indicator for 1930.[13] For consistency across datasets, we harmonize 1930 counties to 1990 boundaries using the same crosswalk procedure.

Bankruptcies. We use data on the annual number of bankruptcies at the federal district court level from the *Reports of the General Attorney of the United States* for the period 1899–1920, digitized by Hansen, Davis, and Fasules (2016). Bankruptcies are reported separately for the following occupation groups: farmers, manufacturers, professionals, merchants, wage earners, and other.

Homestead exemptions. We construct an annual state-level database that indicates whether a state had a homestead exemption law in a given year, and, if so, the maximum exemption amount—expressed either in acreage or dollars—based on a range of historical sources documenting these laws (Goodman, 1993; Farnam and Day, 2000; Lalor, 1883; Palgrave, 1901, 1926). To enable comparison across states, we convert all acreage-based exemptions into dollar equivalents using average farm values per acre from the agricultural census, calculated as state-level averages across all counties.[14]

Figure 3 plots the value of homestead exemptions across states for the two years in which federal bankruptcy laws were introduced: 1867 and 1898. For 1867, we also incorporate the minimum federal exemption amount of $500. The figure reveals a positive correlation across the two years, suggesting that the relative generosity of homestead exemptions across states remained fairly stable over time.

4. VALIDATION: DROUGHTS AND AGRICULTURAL YIELDS

We begin by validating that our drought measure captures negative harvest shocks for farmers, using county-level yield data on ten different crops from the agricultural census. For each census year, we estimate the impact of experiencing a drought on crop yields by running the following regression:

$$\ln yield_{pct} = \beta Drought_{ct} + FE_{pc} + FE_{pt} + FE_{zt} + X_{c,t-10} + \epsilon_{pct}, \tag{1}$$

where $yield_{pct}$ is the quantity produced of crop p in county c and reported in census year t (decennial,

[12] In our baseline analysis, we restrict to counties with at least 99% area-weight consistency—i.e., those where the vast majority of the historical county maps cleanly into a single 1990 county. This ensures that migration flows are measured with minimal ambiguity due to boundary changes.

[13] We use 1930, i.e., the census after our sample period, to capture the long-run growth potential of a county.

[14] Utah allowed additional exemption amounts per family member. We use average family size from census to compute the total exemption. After 1900, Utah allowed for an additional exemption for wives; we estimate this using the average share of households with more than one family member.

1860 to 1920). Because the agricultural census reports the previous year's harvest, the drought indicator $Drought_{ct}$ equals 1 if the county experienced a drought year preceding the census year. We include crop-by-county fixed effects (FE_{pc}) to control for differences in crop specialization and measurement units across counties, and crop-by-decade fixed effects (FE_{pt}) to account for crop-specific technological change over time, such as the adoption of improved seeds or machinery.

Ideally, we would use a measure of productivity—such as yield per acre—as the outcome of this regression. However, the agricultural census only reports crop-specific acreage starting in 1880. To address this limitation and make yields more comparable across counties with different land inputs, we include as a control the total improved acreage in a county (i.e., land cleared and prepared for cultivation), measured in the previous census to mitigate endogeneity concerns ($X_{c,t-10}$). This allows us to interpret the outcome variable more closely as a proxy for productivity. In robustness checks, we restrict the sample to post-1880 census years and use crop-specific yields per acre directly as the dependent variable.

To isolate the causal effect of droughts on yields, we compare counties with the same long-run probability of experiencing a drought—referred to as climate zones—and then contrast those that happened to experience a drought in a given year with those that did not. Without accounting for this, agricultural production may be endogenous to the observed general climate of a county. Climate zones z are defined as in Section 3 based on the number of droughts a county experienced in the preceding century. To additionally allow for differential time trends within climate zones, we interact climate zones with decades (FE_{zt}), restricting the comparison to counties within the same climate zone and census decade.

Finally, our standard errors are two-way clustered at county-level and climate zone-decade-level to allow for arbitrary correlation over time and across crops within a county, and to allow for spatial correlation across counties in the same climate zones.

Column 1 of Table 1 estimates equation (1) and shows that our drought measure significantly reduces agricultural yields. The impact is sizable: an average drought reduces yields by roughly a quarter. This result is robust against several alternative specifications. For example, in column 2, we control for droughts in the preceding and following periods to show that yields respond specifically to the drought of the harvest year, but not to droughts in other years—as would happen if our estimation failed to account for differences in general climate conditions across counties. Column 3 includes only a lagged drought control to test whether droughts have persistent negative effects on yields, for example by reducing land quality or other inputs not fully captured by our control variables. However, the coefficient on lagged droughts is consistently small and statistically insignificant, suggesting that yield losses are primarily driven by contemporaneous drought shocks.

Column 4 redefines climate zones using deciles of the drought distribution rather than the raw number of droughts. Column 5 drops counties in the most extreme climate zones—the top and bottom deciles—which correspond to very arid or very humid areas with little drought variation across counties. Column 6 excludes extreme droughts (PMDI ≤ -4, based on the classification provided by Palmer 1965) to show that the negative effect on yields is not driven solely by specific, disastrous events (which are rare in our dataset). Since our results also hold for moderate and severe droughts, which are not as rare, this ensures that there is sufficient variation to study the effect of droughts in periods with and without bankruptcy law in later analyses.

In principle, floods can be as damaging to agricultural yields as droughts. However, extreme floods are rare in our data. In column 7, we separately control for severe floods and find a positive effect on yields. This suggests that the floods observed in our setting were generally beneficial, likely by providing additional water rather than causing destruction. Since our focus is on adverse shocks to farmers, we focus on droughts in the remainder of the analysis.

Columns 8 and 9 restrict the sample to years after 1880, when crop-specific acreage becomes available, allowing us to compute crop-specific productivity. Column 8 replicates the previous specification using total yields as the outcome. Column 9 instead uses yield per acre, confirming that both specifications consistently capture the negative effect of droughts.

5. FARMER BANKRUPTCIES AND DROUGHTS: DISTRICT COURT-LEVEL EVIDENCE

Did farmers view the federal bankruptcy law as a useful tool for coping with adverse shocks? Since we do not have data on individual- or county-level bankruptcies for this time period, we will conduct our main analysis in Section 6 in the spirit of an "intent-to-treat" research design, estimating the effect of droughts on economic outcomes depending on the bankruptcy regime.

However, we have data on farmer bankruptcies for a larger geographic aggregation, the level of district courts, following the federal Bankruptcy Act of 1898. We use this data to show that farmers did use bankruptcies as a way to cope with adverse shocks.

Figure 4(a) shows the coefficients from a regression of farmer bankruptcy rates on leads and lags of droughts, controlling for district-court fixed effects and climate-zone-by-year fixed effects to ensure comparison within climate zones.[15] We find that the number of farmer bankruptcies rises in the year of droughts and in the following year, but does not respond to earlier droughts (years $t-2$, $t-3$), nor anticipate future ones (years $t+1$, $t+2$ or $t+3$). The lack of a response to future droughts is reassuring, as it suggests that the analysis is not confounded by correlated time trends but instead reflects the precise timing of weather shocks. In contrast, Figure 4(b) shows that in our placebo analysis, the states with low take-up of bankruptcy proceedings due to unfamiliarity with the law, there is no response of bankruptcy rates to droughts. In terms of magnitude, our estimated coefficient for the main sample suggests that when a district court suffers a drought, the bankruptcy rate increases by 0.1 per 1,000 farms, a 50% increase relative to the mean farm bankruptcy rate (0.2).

This pattern suggests that droughts triggered insolvency and that farmers declared bankruptcy shortly after experiencing a drought shock—typically within one year—to take advantage of the ordered repayment procedure and the discharge of remaining debts.

We use the district court dataset to verify that the drought shock does not affect other sectors unrelated to farming. Reassuringly, the bankruptcy rate of manufacturers and professionals does not respond to droughts, as we can see in Figure 5(a). We can also study other professions that may be indirectly tied to farming in Figure 5(b). We find an effect for wage-earners, which includes wage earners in farming, and a positive but very small effect (relative to the mean) for merchants and other work classes, who could be linked to farmers as providers of credit.

[15] For details, see Section A.1.

6. INDEBTEDNESS AND THE ADJUSTMENT OF THE AGRICULTURAL SECTOR TO DROUGHT SHOCKS

6.1 Descriptive statistics

County-level descriptive statistics for our sample and period of interest are reported in Table 2. Panel A uses data from the agricultural census and shows that the average county in our sample includes 1,636 farms. The average farm covers 384 acres, roughly evenly split between improved and unimproved land. The value of farmland and buildings amounts to $34 per acre on average, or approximately $5,200 per farm. Each farm holds on average approximately $1,400 worth of machinery and livestock, giving a total average farm value of approximately $6,600. Counties cultivate on average 7 of the 10 major crops recorded consistently in the agricultural census.

Panel B aggregates the individual census to the county level. The average county has a population of 23,700, including 1,740 farmers. Despite drawing on a different data source, the number of farmers in the individual census is strikingly close to the number of farms in the agricultural census. For each farmer, there is approximately one additional person in a farm-related occupation, bringing the average county total to approximately 3,000.

Panel C shows the same variables for the linked individual census, which we use to study farmer mobility. The average county contains approximately 4,000 individuals who can be linked across consecutive census waves, roughly one-sixth of the total population. The match rate is higher among farmers: 636 of the 1,740 farmers can be linked over time in the average county, consistent with the linking method performing best for male individuals, who make up nearly all farmers.

Table 3 implements a balance check, examining whether counties that experience a drought in the following decade differ systematically from those that do not, based on observable characteristics measured at the beginning of the decade. Column (1) compares across all counties. While most differences are statistically insignificant, counties that will experience a drought tend to have more valuable land per acre, more machinery and livestock per farm, and fewer cultivated crops. These differences suggest that drought-prone areas have different farming practices, which we do not want to confound our estimates.

However, when we compare counties within the same climate zone, these differences disappear, as visible in column (2), which estimates the differences in means conditional on climate zone-by-decade fixed effects. These fixed effects play the role of strata fixed effects in a stratified randomized controlled trial, ensuring that all comparisons are made within groups where drought timing is as good as random. The contrast between columns (1) and (2) illustrates the importance of comparing within rather than across climate zones.

6.2 Number of farms

By reducing yields, droughts severely affect the livelihood of farmers by limiting both own consumption and market sales, making them the most costly natural disasters to strike North America (Cook et al., 2007). In this paper, we want to understand not only how individuals respond to such adverse shocks, but also how indebtedness (or the absence of debt) affects their ability to adjust. We begin by analyzing this question at the aggregate level, estimating how the number of farmers in a county responds to drought shocks.

Our empirical strategy follows the logic of a triple-differences design: we compare counties that experi-

enced a drought in a given decade with those that did not, and test whether outcomes differ depending on whether a bankruptcy law was in place. This captures the following idea: Droughts generate negative income shocks that raise insolvency risk and increase indebtedness. When a bankruptcy law is in place, farmers can declare bankruptcy and reduce indebtedness. The interactions between droughts and bankruptcy regulation thus captures the mechanism that negative income shocks increase indebtedness, but only if there is not a bankruptcy law in place. Therefore, bankruptcy can be seen like an exogenous reduction in indebtedness, which is relevant if individuals experience a negative shock.

We run the following regression at the county level:

$$\begin{aligned} \ln \#farms_{ct} &= \beta_1 PastDrought_{ct} \cdot BankruptcyLaw_t + \beta_2 PastDrought_{ct} \cdot NoBankruptcyLaw_t \\ &\quad + FE_c + FE_{zt} + FE_{st} + \epsilon_{ct}, \end{aligned} \tag{2}$$

where $PastDrought_{ct}$ is a dummy indicating whether a drought occurred any year during the decade preceding census year t, $BankruptcyLaw_t$ captures whether there was a federal bankruptcy law in place in census year t, and $NoBankruptcyLaw_t$ captures whether there was no federal bankruptcy law in place in census year t. Figure 6 shows the distribution of treated and untreated counties across climate zones. The majority of observations come from moderate climate zones, where the share of treated counties ranges between 50% and 75%. As in Section 4, standard errors are two-way clustered at the county level and climate zone-decade level to allow for arbitrary serial correlation within counties and spatial correlation across counties in the same climate zone.

Our setting offers several advantages over a standard triple-difference approach. First, we do not compare drought-prone areas to non-drought-prone areas. Instead, we compare counties within the same climate zone, defined by long-run drought probability over the century preceding our analysis as defined in Section 3. This ensures that any differences in farm behavior, sorting, or agricultural practices driven by long-run climate conditions are absorbed.

Second, our setting resembles a repeated cross-sectional experiment: each decade, a different set of counties within the same climate zone experiences a drought, providing multiple draws of treatment assignment rather than a single comparison. This is in contrast to a standard difference-in-differences design, where a fixed treatment and control group must be assumed to follow parallel trends. Because the treated and control groups change every decade, a confounder would need to be correlated with drought timing within a climate zone, not merely differ across fixed treatment groups.

Our identification relies on the assumption that within a given climate zone and decade, drought timing is uncorrelated with decade-specific county-level determinants of agricultural outcomes. Several features of our setting support this assumption. Droughts present an observable but unpredictable weather shock, making them an ideal quasi-experiment for identifying the causal effect of an adverse output shock on farmers (Feder, 1955). This was particularly true in the late 19th and early 20th century, when long-term precipitation records did not exist, and knowledge of the climate has been described as "primitive" (Libecap and Hansen, 2000). Even more problematic than the lack of data and knowledge were the folk theories of weather that emerged, which incorrectly argued that rainfall was endogenous to farmers' activities. The most influential were the "rain follows the plough" argument and the dryfarming doctrine. As a result, droughts were largely unanticipated. Moreover, long-run drought probabilities were stable across centuries: in Section A.3 we show that county-level drought frequency between 1730–1830 and 1830–1930 is highly correlated, with no indication of long-run climate change during this period. In Section 4, we further show that yields respond to contemporaneous droughts but not to leads or lags, confirming that our drought measure captures the precise timing of weather shocks rather than persistent differences across counties. Finally, the balance test in Table 3 shows that pre-period

observable characteristics do not systematically differ between drought and non-drought counties within climate-zone-decade cells.

Our fixed effects operationalize this assumption. County fixed effects (FE_c) absorb any time-invariant differences across counties, such as soil quality, crop specialization, farming techniques, or proximity to markets. Climate-zone-by-decade fixed effects (FE_{zt}) absorb decade-specific differences across climate zones, including differential adoption of irrigation, changing crop technologies, or shifts in agricultural policy that may have affected areas with different long-run drought exposure differently. State-by-decade fixed effects (FE_{st}) account for any state-level policies, including homestead exemption laws, that may vary over time (we will later exploit variation in the generosity of homestead exemption laws across states as it interacts with the bankruptcy law).

Third, the repeated introduction, repeal, and reintroduction of the bankruptcy law allows us to exploit multiple quasi-experiments, rather than relying on a single policy change as in conventional event-study designs. We focus on three major changes in federal law during our study period (1850–1920): the introduction of the Bankruptcy Act of 1867, its repeal in 1878, and the introduction of the Bankruptcy Act of 1898, which remained in effect beyond our sample period. As discussed in Section 2, political support for such laws was intermittent and highly contested across party lines, leading to repeated enactments and repeals. Because the bankruptcy law switches on and off at the national level, our setting does not face the staggered adoption concerns discussed in the recent econometrics literature on difference-in-differences with heterogeneous treatment effects.

We estimate regression (2) on the sample of states after they had joined the union, as US federal law was not applicable to territories; and exclude the states with the lowest bankruptcy take-up rates as explained in Section 5, as we do not expect the federal bankruptcy law to affect farmers if they were not using it. We exploit this variation in take-up in a placebo analysis.

Table 4 shows that the agricultural sector responds very differently to past droughts depending on whether bankruptcy regulation was in place. In periods with a bankruptcy law, droughts reduce the number of farms; in periods without such a law, droughts increase the number of farms. Column 1 presents the baseline specification, while the remaining columns report a series of robustness checks. More specifically, column 2 redefines climate zones using deciles of the historical drought distribution instead of the raw number of droughts in the previous century. Columns 3 and 4 demonstrate that the results are not sensitive to including either state-by-decade or climate zone-by-decade fixed effects. The addition of climate zone-by-decade fixed effects seem to be particularly helpful in reducing estimation noise, while not affecting the estimated coefficients.

Column 5 excludes extreme droughts (PMDI $\leq$ –4, based on the classification by Palmer 1965) to verify that the results are not driven by rare, severe droughts. Figure 7 conducts a related robustness check by estimating differential effects depending on whether a county experienced one drought or multiple droughts in the past decade. We would expect a series of droughts to have a larger adverse effect on farmers than a single one. Figure 7 shows that this is indeed the case: regardless of the bankruptcy regime, several droughts result in more farm exits compared to one drought. Finally, column 6 wants to ensure that results are not driven by long-run effects of previous droughts, and controls for one-decade-lagged droughts (i.e., capturing droughts that occurred 10 to 20 years before the census year), interacted with both indicators for having or not having a bankruptcy law in the census year.

Table 5 further demonstrates that the differential effect of bankruptcy law on the agricultural sector's adjustment to droughts is not driven by specific regions of the US. Column 2 drops all southern states, which historically had a distinct agricultural structure and underwent a radical transformation following the abolition of slavery after the Civil War. The results are not driven by these states; in fact, they become

slightly stronger when excluded. Column 3 drops the top decile of urban counties, which typically have only a small agricultural sector. Finally, as discussed in Section 2, in the absence of federal bankruptcy regulation, some states had implemented their own insolvency laws. These were far more limited than federal bankruptcy laws: they did not apply to out-of-state debt, were sometimes challenged as unconstitutional, and varied considerably in their procedures and coverage. To allow for a cleaner comparison, column 4 excludes the 17 states that had any form of insolvency law in place by 1880, based on data from Rand McNally and Company (1880).[16] Column 5 excludes the counties on the frontier of the settled states, since these states had high migration and land was probably being acquired and farmed for the first time with high frequency. Dropping these counties has no effects on the results confirming that our findings are not driving by the process of settlement. Finally, we drop the cotton-growing counties, and we show that the results are again unaltered.

Our estimated effect of the federal bankruptcy law relies on time variation by comparing periods without a bankruptcy law to periods with a law. To ensure that the observed differential effects are indeed driven by the possibility to declare bankruptcy rather than alternative explanations, we perform two additional exercises.

First, we should only observe an effect if farmers actually made use of the bankruptcy law. To test this, we exploit the state-level variation in farmer bankruptcy take-up rates introduced in Section 5. We divide states into high- and medium-take-up groups.[17] We also add the remaining states with the lowest take-up rates as a placebo group.[18]

Figure 8 shows that droughts only reduce the number of farms in states where farmers actually used the bankruptcy law. Moreover, the effect grows with take-up. In contrast, in states where farmers did not use bankruptcy due to unfamiliarity with the law (as discussed in Section 5), the effect of the law is positive but insignificant, similar to the drought effect in periods without bankruptcy regulation in Table 4.

Second, we estimate the effect of droughts on the number of farms separately for each census decade. Figure 9 shows that the pattern of estimated effects aligns strikingly well with the timing of the federal bankruptcy law's introduction and repeal rather than with other secular trends that may have influenced the agricultural sector over time, such as the gradual adoption of irrigation technologies or the rise of the manufacturing sector. In census years when a federal bankruptcy law was in place (1870, 1900, 1910, 1920), we consistently observe negative and mostly significant effects of droughts on the number of farms. In contrast, in years without a bankruptcy law (1860, 1880, and 1890), the estimated coefficients are consistently positive, though not statistically significant.

Our aggregate findings are also reflected in the sample of linked individuals from the census. As shown in Section A.4 of the Online Appendix, there is a strong correlation between various measures of farming in the individual census and the number of farms in the agricultural census. Table 7 estimates the effects of droughts and indebtedness using both data sources. To ensure comparability, we drop the year 1890 from the agricultural census sample in column 2, since individual census data for that year are not available; this adjustment has little effect on the results. Columns 3 to 5 use different definitions of farming occupations from the individual census and yield very similar estimates. Columns 6 to 8 estimates results for the full population and show that the decrease in farms is associated with a decrease

[16] These states are: Arizona, California, Connecticut, Idaho, Louisiana, Maine, Maryland, Massachusetts, Nevada, New York, North Carolina, North Dakota, South Carolina, South Dakota, Vermont, Washington, Wisconsin.

[17] High (medium) take-up rates are defined as ≥ 2 (1) farmer bankruptcies per 1,000 farms. The data refer to the period following the introduction of the 1898 Bankruptcy Act. We have not been able to locate similar data for the 1867 Bankruptcy Act.

[18] Low take-up: < 1 farmer bankruptcy per 1,000 farms.

in population, which is consistent each farm supporting not only the farmer but also his family and farm workers, and potentially also has demand effects on other sectors in the county. Effects are driven by the white population, and absent for blacks, who were less likely to be farmers and had less access to bankruptcy laws. Figure 12, which estimates effects separately by year, again shows the switch from positive to negative effects between years without and with bankruptcy law. The patterns mirror the findings from the agricultural census and align closely with the timing of the federal bankruptcy law's introduction.

Overall, our estimates provide strong evidence that indebtedness matters for how adverse shock affect entry and exit into farming. Without a bankruptcy law, droughts increase the number of farms. By contrast, with a bankruptcy law, droughts reduce the number of farms in a county. This suggests that the ability to get a discharge from debts seems to allow farmers to quit farming after a negative shock.

Bankruptcy proceedings entailed various costs, including filing fees, publication fees, and compensation for referees, trustees, and attorneys. As a result, bankruptcy may not have been a viable option for every insolvent farmer. Wickens (1936) notes that even by the early 1930s, the average farm entering bankruptcy proceedings was larger than the average farm overall. If bankruptcy was only accessible to farmers above a certain scale, this may have influenced the resulting size distribution of farms after droughts. To examine this systematically, we estimate the effects separately for small, medium, and large farms.[19] Figure 10 shows the results. Droughts have no discernible effect on the number of small farms, regardless of whether bankruptcy regulation is in place. For medium and large farms, however, the effects differ by bankruptcy regime. When a bankruptcy law is in place, the number of medium and large farms falls, consistent with insolvent farms exiting via formal bankruptcy proceedings. In contrast, in the absence of a bankruptcy law, the number of farms increases primarily due to a rise in medium-sized farms, while the number of large farms declines. This suggests that large farms are broken up and partially sold off to new farmers. Since medium-sized farms roughly correspond to the typical homestead exemption, this pattern is consistent with insolvent farmers either voluntarily selling land above the exemption amount or creditors seizing it, while retaining the homestead.

6.3 Farmer inflows and outflows: migration and occupational change

The linked individual census data allow us to decompose changes in the number of farms into continuing farmers, farmer inflows, and farmer outflows. For the latter two groups, we can further distinguish whether these changes are driven by occupational transitions, geographic mobility across counties, or both.

Our period of study is characterized by high geographical and occupational mobility of farmers, as Panels A and B in Table 8 show. Panel D reports inflows as a share of current farmers: only 28% of current farmers were farmers in the same county a decade earlier, while 23% entered farming from other occupations (including non-working) within the same county, and 49% arrived from other counties. New entrants from the same county were predominantly young: individuals under 16 in the previous census account for 15 percentage points of the 23% local inflow. Panel E reports outflows as a share of previous farmers: 41% remained as farmers in the same county, 12% switched to non-farming occupations locally, and 47% moved to a different county. Among movers, the large majority (34 out of 47 percentage points) moved to urban counties, with most taking up agricultural occupations in their destination. Only 3 percentage points moved to manufacturing occupations in urban counties, which is the margin most

[19] Small farms: fewer than 20 acres; medium farms: 20–499 acres; large farms: more than 500 acres. Medium farms approximately correspond to the range of homestead exemption amounts specified in acres across states, which ranges between 40 and 260 acres.

directly related to structural transformation.

Overall, farmers observed in a given census year consist of two main groups: continuing farmers and new entrants. New entrants can either come from other occupations within the same county or migrate in from other counties. Table 9 decomposes the farmer population into these categories, expressed as percentages of total farmers. Column 1 shows the share of continuing farmers: individuals who were already farmers in the previous census and remained in the same county. Column 2 captures occupational inflows: individuals who previously held non-farming occupations but lived in the same county. Column 3 shows inflows from other counties, which include all individuals who lived in a different county in the previous census, regardless of their prior occupation.

Given the earlier results showing that droughts increase the number of farms when no bankruptcy law is in place, we focus on the second row of the table to examine which group drives this increase. The results suggest that the rise is due to a larger share of continuing farmers and local occupational switchers, rather than migrants from other counties. This pattern is consistent with distressed farmers selling a part of their farm to other locals, while staying in their homestead. Columns 4 and 5 further decompose local farmer inflows (i.e., column 2) by age. Column 4 shows that these inflows are primarily driven by individuals who were under 16 years old in the previous census, suggesting that the additional farms were taken up by young people who might otherwise have entered non-agricultural occupations.

Table 10 presents the mirror image by focusing on farmer outflows. Farmers observed at the beginning of a decade may remain in the same location and continue working as farmers, but they may also leave farming either by switching occupations within the same county or by migrating to a different county. Column 1 captures continuing farmers: individuals who remain in the same county and continue farming. Column 2 shows occupational outflows: farmers who remain in the same county but switch to a non-farming occupation. Column 3 reports migratory outflows: farmers who moved to a different county, regardless of their subsequent occupation.

Given the earlier results showing that droughts reduce the number of farms when a bankruptcy law is in place, we now focus on the first row of the table to examine which group drives this decline. The results suggest that the decrease is driven by a reduction in continuing farmers, and that those exiting farming are more likely to migrate to other counties rather than switch occupations locally. This pattern is consistent with the interpretation that homestead exemptions provide liquid assets that help cover the cost of relocating to a different area.

The remaining columns of Table 10 explore the characteristics of destination counties and the occupations taken up after migration. Columns 4 and 5 show that migrating farmers are more likely to move to urban rather than non-urban counties. Columns 6 to 8 further decompose these moves by occupation in the destination urban county. Column 6 shows a significant increase in the share of outgoing farmers who transition into manufacturing occupations. While there is also an increase in agricultural occupations (column 7), the estimate is imprecise. No change is observed for service occupations (column 8).

Taken together, these findings suggest that bankruptcy regulation may have contributed to the structural transformation of the US economy by enabling distressed farmers to leave agriculture and move into manufacturing.

6.4 Farmer investment and behavior

We explore whether the level of indebtedness also affects farmer behavior. Debt levels and access to liquidity may not only affect entry into and exit from farming (the extensive margin), but also the level and type of investment within farms (the intensive margin), as well as risk-taking in terms of how many crops to plant. The agricultural census provides three indicators of "farm investment":

First, farmers could expand "improved" land—i.e., land usable for cultivation—through manual labor, converting previously "unimproved" land. Second, they could invest in capital inputs such as livestock and machinery. Third, we observe the number of different crops grown in a county. After a drought shock, one might expect farmers to increase investment to increase future returns and compensate for the current negative income shock. However, droughts also increase indebtedness risk, which may restrict access to credit (and cash), making capital investment more difficult than labor-based improvements, which primarily require increased time and effort by the farmer and their family. Table 6 supports this hypothesis: Row 1 shows that when a bankruptcy law is in place, farmers respond to droughts by increasing capital investment while reducing manual investment. In contrast, when no bankruptcy law is in place, farmers increase manual investment but reduce capital investment. This suggests that indebtedness shifts the composition of farm investment away from capital-intensive improvements and toward labor-intensive efforts. Finally, columns 3 and 4 studies the effect on the number of grown crops, to explore whether the presence of a bankruptcy law pushed farmers into crop diversification or specialization. As expected, in the absence of a bankruptcy law, we see in a diversification behavior, with an increase in the number of crops. By contrast, the number of crops goes down in periods with a bankruptcy law, pointing towards stronger specialization.

Overall, this section has shown that indebtedness shapes farmers' responses to adverse shocks along both the extensive and intensive margins: higher indebtedness levels increase the number of farms and shift investment away from capital and toward labor.

7. Liquidity and the adjustment of the agricultural sector to drought shocks

Next, we explore the economic mechanism behind our findings, focusing on whether access to liquid assets helps explain farmers' responses. We exploit variation in liquidity generated by differences in state-level homestead exemption amounts, which matter differently depending on whether bankruptcy regulation was in place.

Without bankruptcy, exemptions affect whether farmers retain any assets after a drought. When exemptions were low, farmers could lose their land to creditors and be left with neither debt nor assets. When exemptions were high, they retained their homestead, leaving them with more debt but also a potentially productive asset. However, since selling the homestead would trigger repayment of the remaining debt, this asset was illiquid.

With bankruptcy, the implications of exemptions change. Low exemptions meant farmers exited bankruptcy without debt but also without assets. High exemptions allowed them to keep their potentially productive homestead and eliminate their debt. In this case, the homestead became a liquid asset, since it could be sold without being seized by creditors. Thus, exemptions generated a liquid, productive asset.

In summary, high homestead exemptions generate illiquid assets when no bankruptcy law is in place, and liquid assets when bankruptcy is available. In the following, we estimate whether access to illiquid or liquid assets create differential responses to droughts and indebtedness.

Variation in state-level homestead exemption laws may be endogenous. For examples, historians have identified states with generous exemptions to also have less developed credit markets, be more exposed to risky environments, or have more demand for settlers. We thus think of the next part of the evidence as more suggestive rather than causal.

To estimate the relationship between access to liquid or illiquid assets and farmers' adjustment to

droughts, we extend our baseline regression (2) to include interactions with state-level homestead exemption amounts:

$$\begin{aligned}\ln \#farms_{ct} = \beta_1 PastDrought_{ct} \cdot BankruptcyLaw_t \\ + \beta_2 PastDrought_{ct} \cdot BankruptcyLaw_t \cdot ExemptionAmount_{st} \\ + \beta_3 PastDrought_{ct} \cdot NoBankruptcyLaw_t \\ + \beta_4 PastDrought_{ct} \cdot NoBankruptcyLaw_t \cdot ExemptionAmount_{st} \\ + FE_{zt} + FE_c + FE_{st} + \epsilon_{ct}\end{aligned} \tag{3}$$

Exemption amounts are defined as described in Section 3 and refer to the harvest year, i.e., the year before the census year.[20] Fixed effects are specified as before.

Figure 11 presents the results. Figure 11(a) illustrates the case with a bankruptcy law in place, i.e., where farmers are debt-free following bankruptcy, and higher exemption amounts translate into liquid assets. The larger the exemption, the greater the reduction in the number of farms, consistent with liquid assets facilitating exit from farming. In contrast, Figure 11(b) shows the case without bankruptcy protection, i.e., where farmers remain indebted, and higher exemption amounts imply the retention of an illiquid, potentially productive asset, but also larger outstanding debt. There is a small positive, but insignificant effect of the illiquid asset on the number of farms.

One possible explanation consistent with this pattern involves the existence of moving costs, whether geographic or occupational. Under bankruptcy protection, farmers are debt-free and, with higher exemptions, may also have liquid assets that can help cover the costs of relocation or starting a new occupation or business. In contrast, without bankruptcy protection, indebted farmers face more difficulty in obtaining credit for such transitions. If they also hold an illiquid asset—while carrying even more debt—this may further tie them to their current situation, limiting adjustment.

This suggests that homesteads were illiquid assets, tying farmers to the agricultural sector and making them less likely to exit. Figure 13 shows that this pattern is also reflected in farmer inflows: States with more generous exemption amounts see larger inflows into farming from residents of the same county. In contrast, when a federal bankruptcy law is in place, exemption amounts have no effect on these inflows.

Farmer outflows, however, reveal a different pattern. With bankruptcy protection in place, the reduction in the number of farms was driven by states with more generous homestead exemptions. Figure 14 shows that this is reflected in farmer outflows into urban counties: States with more generous exemption amounts see larger outflows of farmers to urban counties. Without a bankruptcy law, the pattern reverses (although effects are not significant).

Overall, the analysis of farmer inflows and outflows supports the view that geographic mobility is constrained by financial frictions. When bankruptcy protection is available, farmers are freed from debt obligations and, in states with higher homestead exemptions, can use their liquid homestead to finance migration to manufacturing jobs in urban counties, thereby contributing to the structural transformation of the US economy. In contrast, in the absence of such protection, indebted farmers remain tied to their farms, as their illiquid homestead limits mobility, slowing structural transformation.

[20] As there is little variation in homestead exemption amounts over time, results are not sensitive to the specific year chosen for the interaction term.

8. Conclusions

How economies recover from adverse shocks depends not only on the severity of the shock itself but also on the institutional environment that shapes individuals' ability to respond. This paper provides causal evidence on one dimension of that environment: the role of bankruptcy institutions in enabling occupational and geographic reallocation. Exploiting the repeated introduction and repeal of federal bankruptcy law in 19th-century United States, combined with granular drought exposure and linked individual census records, we show that indebtedness is a first-order determinant of how farmers adjust to negative shocks — and that institutions which reduce indebtedness can dramatically alter the course of structural transformation.

Our aggregate findings reveal a striking contrast. In periods without bankruptcy protection, droughts increase the number of farms: large holdings are broken up and parceled out to local young entrants, while distressed farmers remain on the land. When bankruptcy protection is available, droughts instead reduce farm counts, as insolvent farmers exit agriculture and the sector consolidates. This pattern holds across subsamples, specifications, and is tightly tied to the timing of the law's introduction and repeal rather than to secular trends.

The individual-level evidence deepens this picture. In the absence of bankruptcy protection, distressed farmers remain in place, selling portions of land to locals — particularly young individuals who might otherwise have entered non-agricultural employment. Under bankruptcy protection, affected farmers are significantly more likely to migrate, and to do so to urban counties where they take up manufacturing jobs. This suggests that bankruptcy law did not merely reshuffle workers within agriculture, but contributed to the structural transformation of the U.S. economy by redirecting labor toward the emerging industrial sector.

A key mechanism behind these findings is the role of liquidity. Homestead exemptions — which shielded a farmer's dwelling and surrounding land from creditors — had fundamentally different effects depending on the bankruptcy regime. Without bankruptcy, the protected homestead was an illiquid asset: selling it would trigger creditor claims on the proceeds, so insolvent farmers had little incentive to convert it to cash and move. Under bankruptcy, discharged debts eliminated this trap. The homestead became a liquid asset that farmers could sell to finance relocation or a new occupation. States with more generous exemptions therefore saw larger outflows of farmers to urban counties when bankruptcy protection was available, and larger inflows of locals into farming when it was not. Institutions that protect assets, in other words, may simultaneously constrain mobility — unless paired with mechanisms that resolve the underlying debt.

Indebtedness also shapes behavior along the intensive margin. Without bankruptcy protection, farmers respond to drought by increasing manual labor investment — improving land through their own effort — while reducing capital investment in machinery and livestock. With bankruptcy protection, the pattern reverses: capital investment rises while labor-intensive improvements decline. This suggests that financial distress shifts not just whether farmers exit, but how surviving farmers operate, with potentially lasting consequences for agricultural productivity.

Our findings speak to several broader questions. They contribute to the literature on mobility frictions by showing that indebtedness — alongside the more commonly studied moving costs — can be a significant barrier to geographic and occupational reallocation. They add to work on structural transformation by demonstrating that credit market institutions shape the speed and direction of labor reallocation across sectors. And they offer a historical precedent for understanding why debtor-friendly institutions may be particularly consequential in economies where shocks are frequent, asset markets are illiquid, and the

transition out of subsistence agriculture is incomplete.

More generally, our results suggest that the design of bankruptcy institutions carries consequences well beyond individual debt relief. By determining whether distressed individuals can exit their current situation and reallocate their labor and assets productively, these institutions influence the aggregate capacity of an economy to adjust to adversity. In contexts where shocks are recurrent and mobility is desirable — whether in developing economies today or in historically agrarian ones — the availability of orderly debt discharge may be as important as the shocks themselves in determining long-run outcomes.

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FIGURES

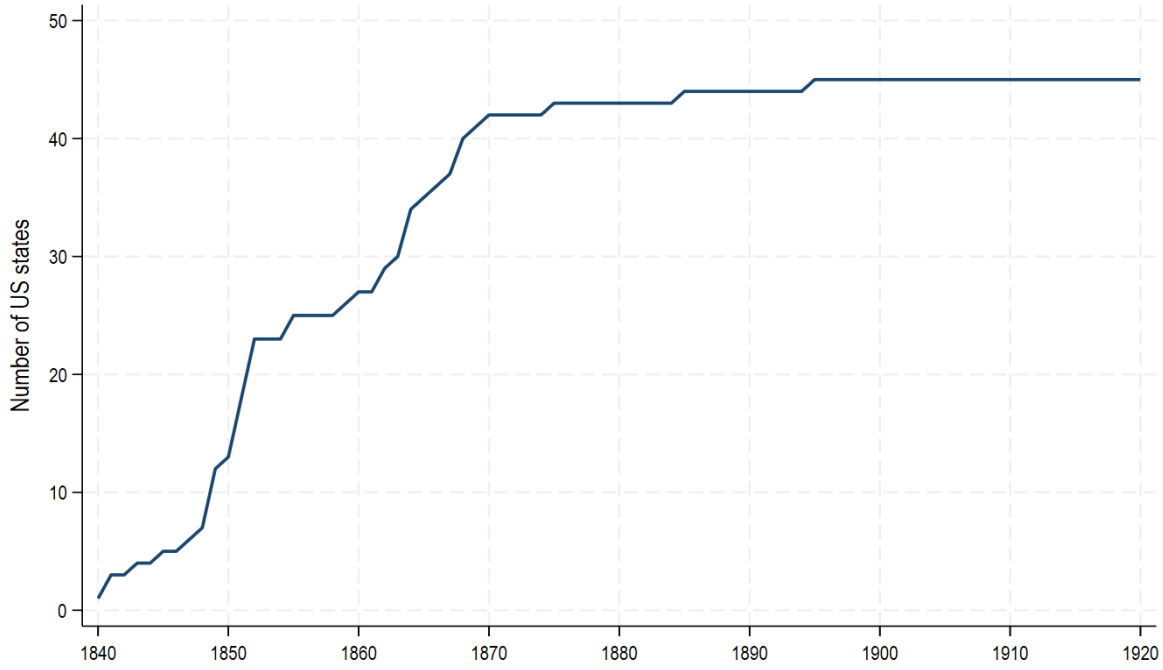


FIGURE 1: Number of US states with homestead exemption laws, 1830-1897

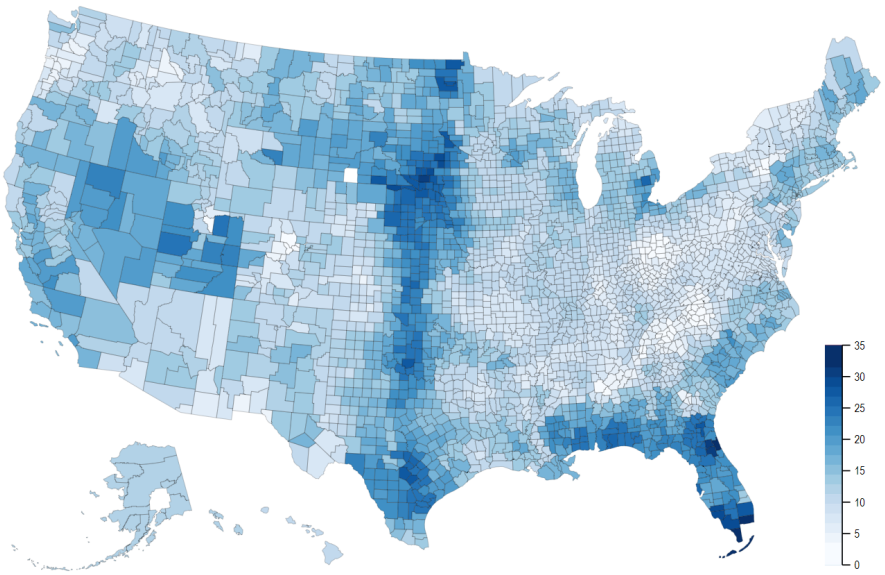


FIGURE 2: Map of climate zones

Notes: We define climate zones based on the number of droughts a county experienced in the decade preceding our analysis, 1730-1830. Droughts are defined as PMDI$\leq$-2 in a given year, as in our main analysis.

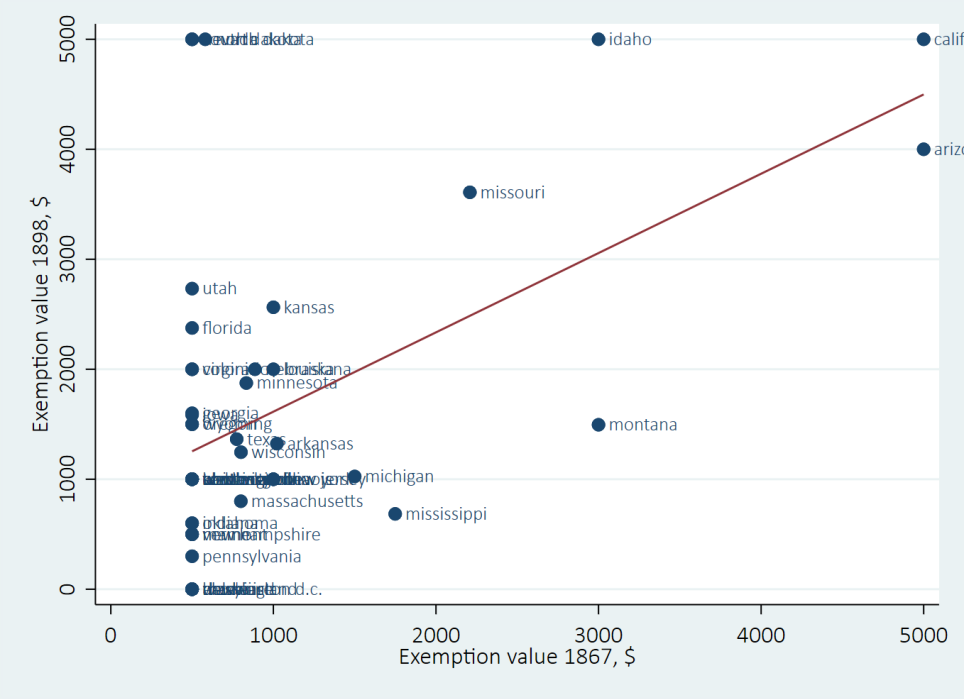
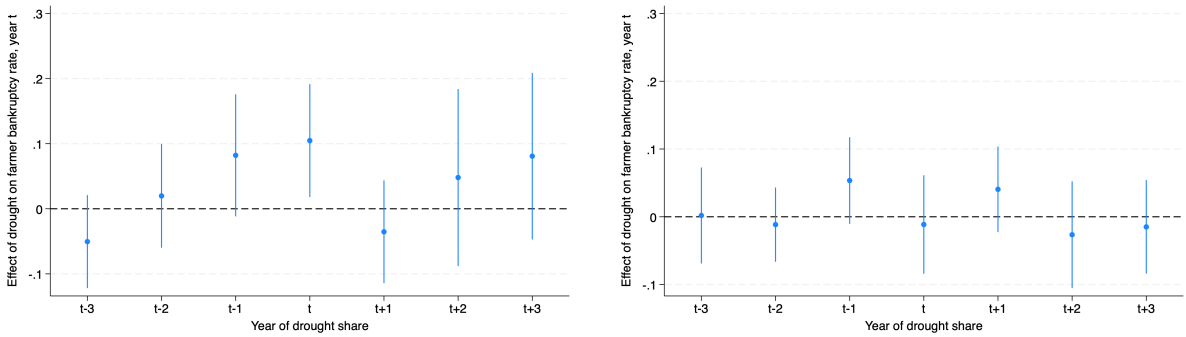


FIGURE 3: Homestead exemption amounts (dollar equivalent), 1867 and 1898

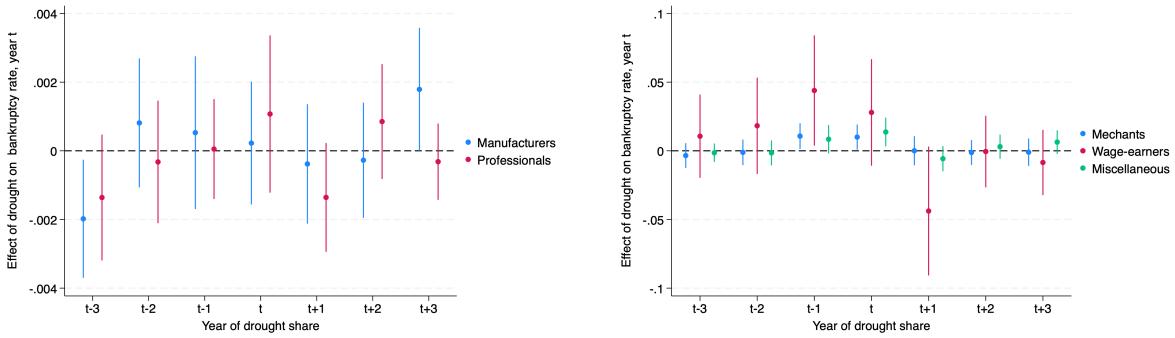
Notes: The red line shows the linear fit between exemption values in 1867 and 1898.



(A) Main sample (B) Placebo: low take-up states

FIGURE 4: Effect of droughts on farmer bankruptcies

Notes: The figures report the coefficients estimated in regression (A.1).



(A) Professionals and Manufacturers (B) Other professions

FIGURE 5: Effect of droughts on non-farmer bankruptcies

Notes: The figures report the coefficients estimated in regression (A.1).

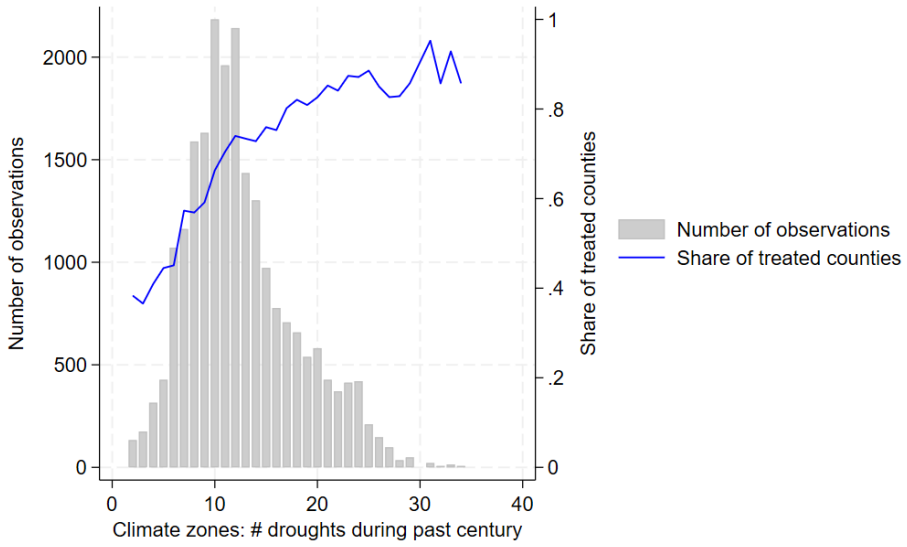


FIGURE 6: Distribution of treated counties across climate zones

Notes: Treatment indicates whether a county experienced at least one drought during the decade preceding the census year.

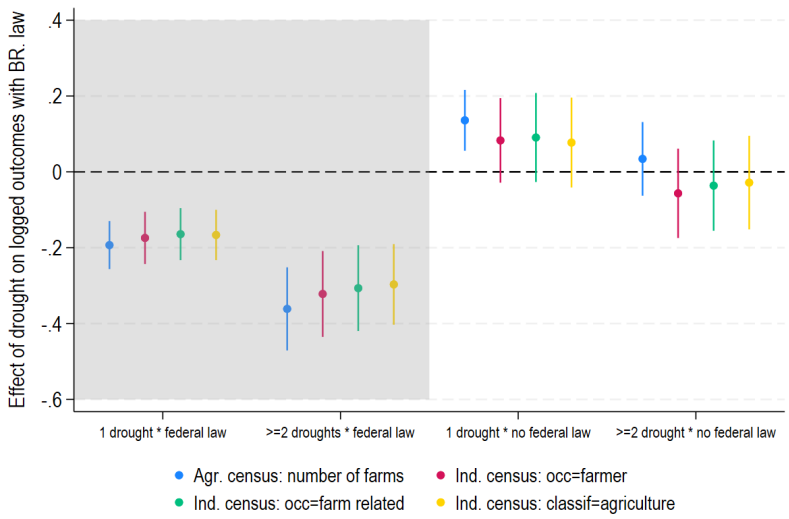


FIGURE 7: Number of farms after droughts, by bankruptcy law—by number of droughts

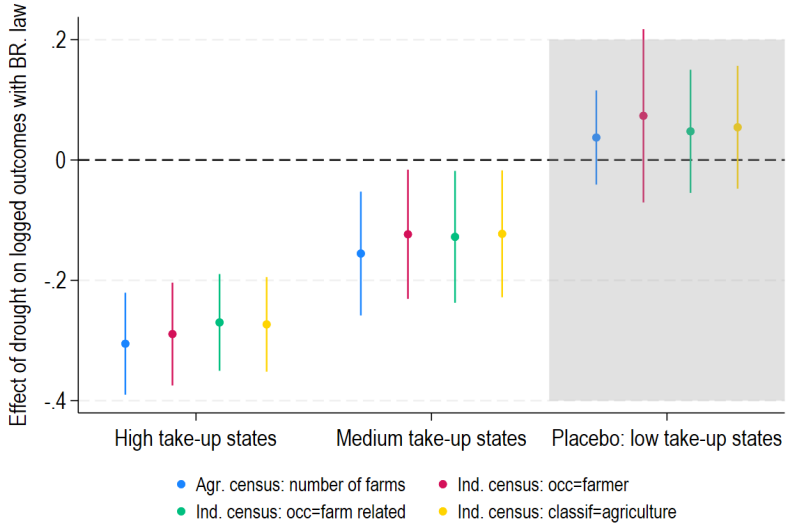


FIGURE 8: Effect of bankruptcy law by take-up

Notes: Estimated effects are based on a single regression that interacts $PastDrought_{ct} \cdot BankruptcyLaw_t$ with three dummy variables for states with high, medium, and low farmer bankruptcy take-up rates, replacing the baseline interaction term in equation (2).

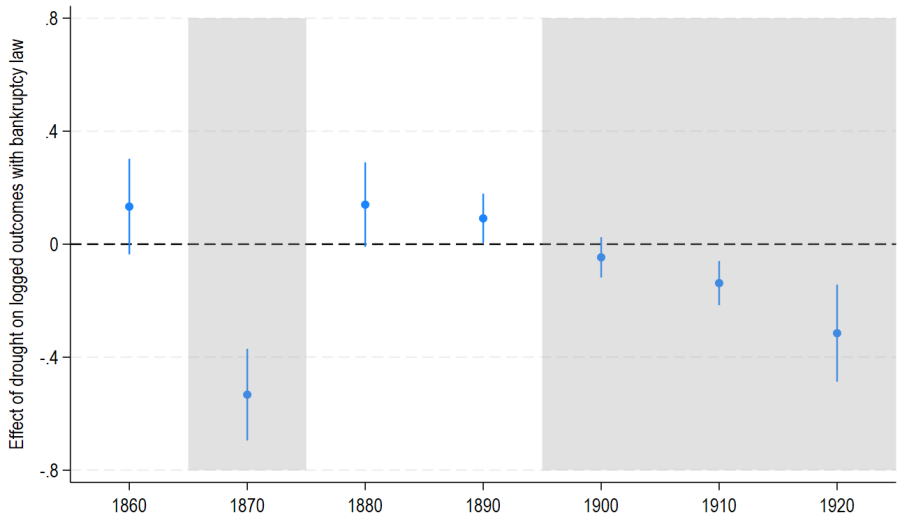


FIGURE 9: Effect of drought depends on bankruptcy law

Notes: Estimated effects are based on a single regression that interacts $PastDrought_{ct}$ with seven decade dummies, replacing the baseline interaction terms with the bankruptcy law indicators in equation (2).

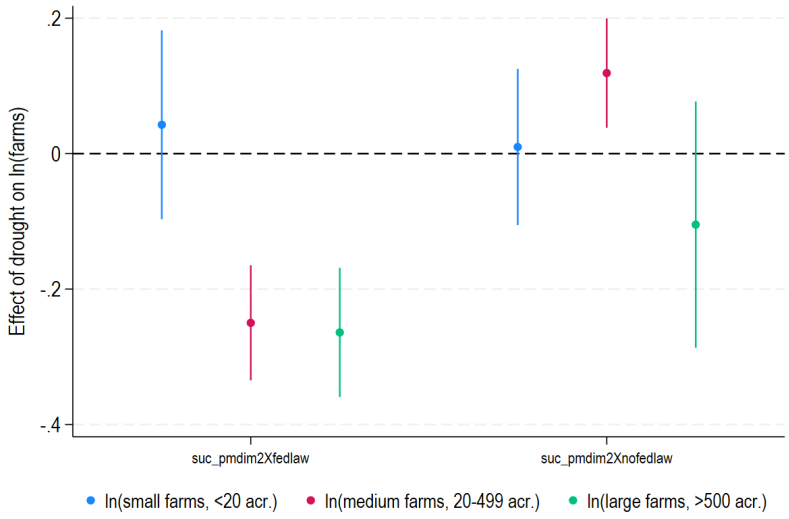
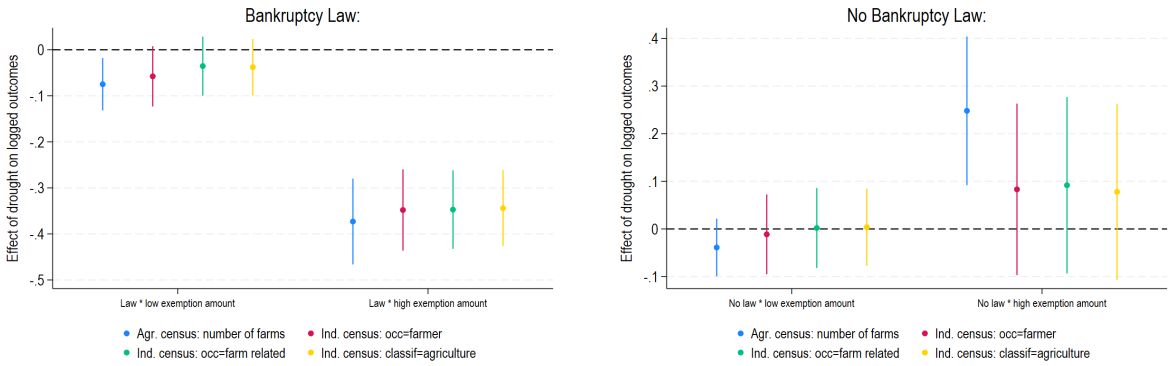


FIGURE 10: Effect of droughts on size distribution of farms



(A) With bankruptcy law (B) Without bankruptcy law

FIGURE 11: Differential effects of homestead exemptions on the number of farms

Notes: The figure plots the effects for states with high and low homestead exemption amounts estimated in regression equation (3).

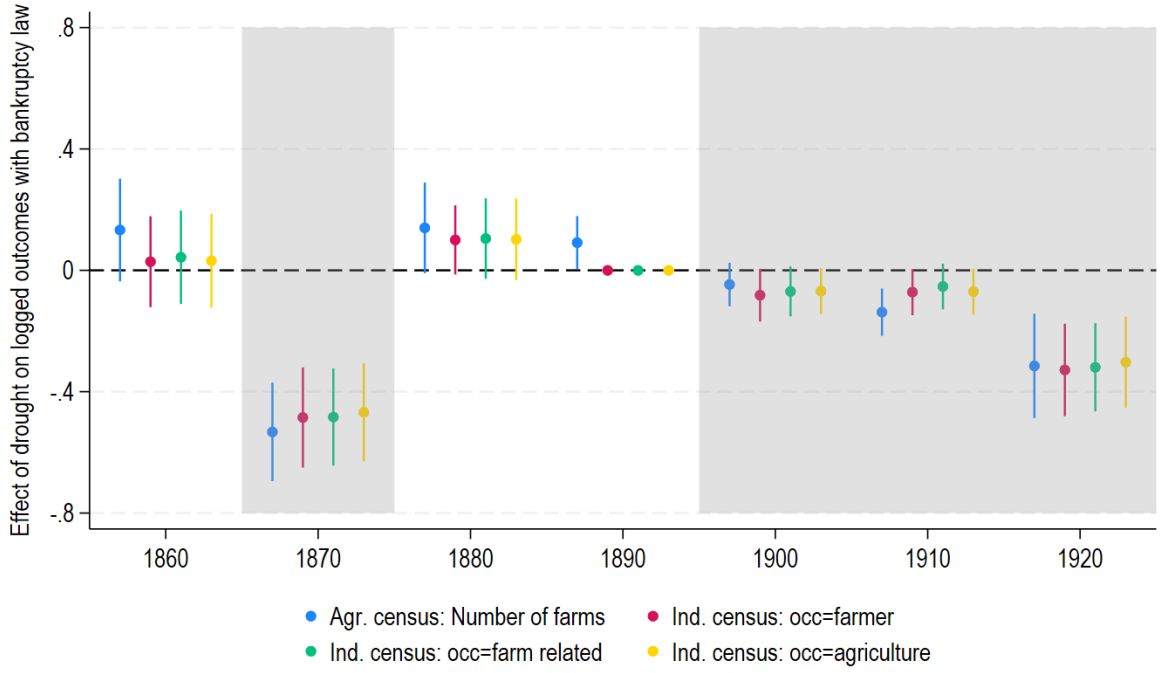
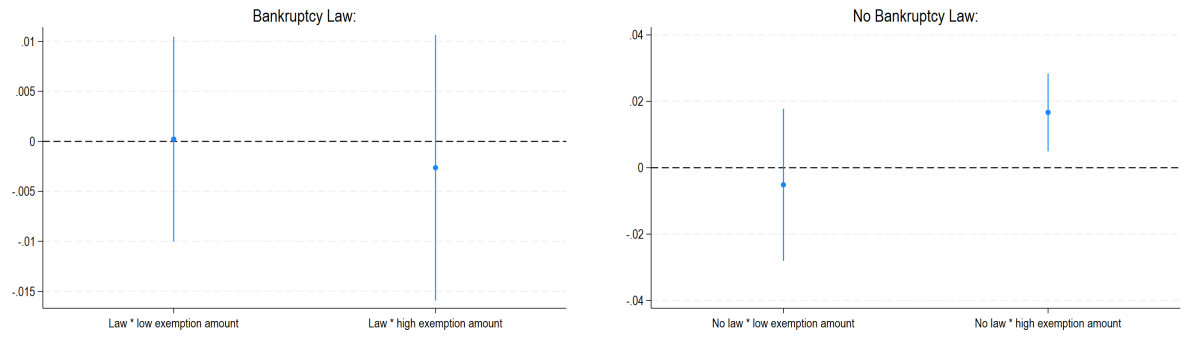


FIGURE 12: Effect of droughts on the number of farmers, based in the agricultural and aggregated individual census

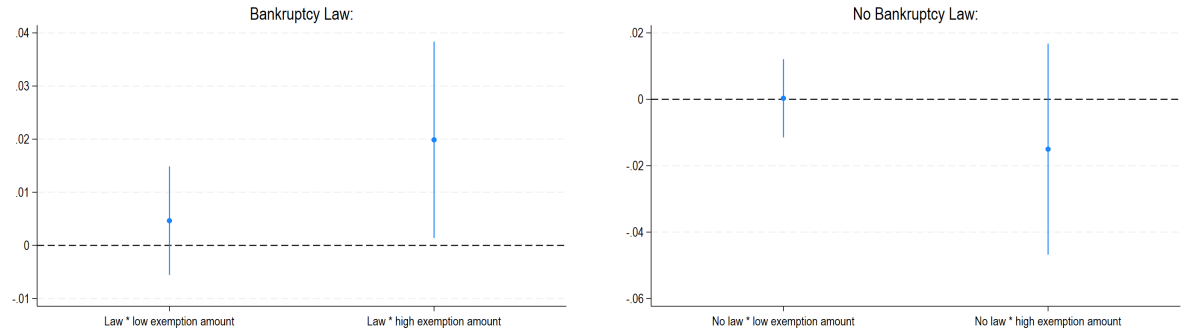
Notes: Individual census data for 1890 are not available.



(A) With bankruptcy law (B) Without bankruptcy law

FIGURE 13: Differential effects of homestead exemptions on farmer inflows from same county

Notes: The figure plots the effects for states with high and low homestead exemption amounts estimated in regression equation (3), using as outcome the share of farmer inflows from the same county, as in column 2 in Table 9.



(A) With bankruptcy law (B) Without bankruptcy law

FIGURE 14: Differential effects of homestead exemptions on farmer outflows to urban counties

Notes: The figure plots the effects for states with high and low homestead exemption amounts estimated in regression equation (3), using as outcome the share of farmer outflows to urban counties, as in column 4 in Table 10.

TABLES

TABLE 1: Effect of droughts on agricultural yields

	(1) ln(yield)	(2) ln(yield)	(3) ln(yield)	(4) ln(yield)	(5) ln(yield)	(6) ln(yield)	(7) ln(yield)	(8) ln(yield)	(9) ln(productivity)
Drought dummy	-0.258***	-0.278***	-0.265***	-0.258***	-0.297***	-0.243***	-0.257***	-0.134***	-0.089***
	(0.047)	(0.050)	(0.049)	(0.048)	(0.058)	(0.047)	(0.049)	(0.047)	(0.017)
Lagged drought dummy		-0.034	-0.028	-0.027	-0.046	-0.035	-0.038	-0.055	0.004
		(0.043)	(0.044)	(0.043)	(0.052)	(0.045)	(0.045)	(0.046)	(0.012)
Lead of drought dummy		-0.054							
		(0.040)							
Flood dummy							0.147**		
							(0.064)		
Observations	122,410	122,410	122,410	122,412	92,767	122,143	122,410	83,643	83,643
R-squared	0.852	0.852	0.852	0.851	0.851	0.852	0.852	0.885	0.953
Lagged log improved acres	yes	yes	yes	yes	yes	yes	yes	yes	
Climate zone-decade FE	yes	yes	yes		yes	yes	yes	yes	yes
Climate zone decile-decade FE				yes					
Crop-decade FE	yes	yes	yes	yes	yes	yes	yes	yes	yes
Crop-county FE	yes	yes	yes	yes	yes	yes	yes	yes	yes
Sample					drop extreme climate zones	drop extreme droughts		productivity (≥1880)	productivity (≥1880)

Notes: Lags and leads refer to values in the previous or the following census year. Standard errors are two-way clustered at county-level and climate zone-decade-level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 2: Summary statistics, county level

	Obs.	Mean	Std. Dev.	Min	Max
Panel A: Agricultural census, 1860-1920					
Number of farms	19,205	1,636	1,349	1	12,808
Acres per farm	19,205	384	4,181	3	450,000
Total acres	19,205	237,900	181,070	8	3,246,335
Share of improved acres in total acres	19,205	0.49	0.24	0.00	1.00
Value of farmland and buildings per acre ($)	19,205	34.35	102.65	0.07	7,235.24
Value of farmland and buildings per farm ($)	19,205	5,181.84	10,361.64	28.23	899,999.69
Value of machinery and livestock per farm ($)	19,205	1,409.07	5,304.85	11.11	403,386.88
Value of farmland, buildings, machinery and livestock per farm ($)	19,205	6,590.91	14,358.96	203.79	1,303,386.50
Total value of farmland, buildings, machinery, livestock (1,000 $)	19,205	9,661.5	15,537.6	0.7	396,910.8
Number of cultivated crops	19,205	7	1	0	10
Homestead exemption amount ($ equivalent)	19,205	2,013	2,240	0	14,109
Panel B: Aggregated individual census, 1860-1920 (excl. 1890)					
Population	16,248	23,693	70,463	6	3,022,513
Farmers	16,248	1,740	1,394	1	15,671
Farm-related occupations	16,248	3,002	2,603	1	29,298
Agricultural occupations	16,248	3,151	2,926	1	60,321
Panel C: Aggregated linked individual census, 1860-1920 (excl. 1890)					
Population, linked	16,205	3,996	11,091	1	512,688
Farmers, linked	16,205	636	552	1	6,293
Farm-related occupations, linked	16,205	973	828	1	9,743
Agricultural occupations, linked	16,205	1,019	905	1	15,438

Notes: The individual census includes all individuals recorded in the full-count IPUMS census, aggregates them to counties, and reports summary statistics for our sample of counties and years. The linked individual census includes all linked individuals recorded in IPUMS census, aggregates them to counties, and reports summary statistics for our sample of counties and years.

TABLE 3: Balance test: pre-period outcomes and drought exposure

	(1) Across all counties	(2) Within climate zone-decades
Panel A: Agricultural census		
ln(number of farms)	-0.011	0.049
	(0.083)	(0.074)
ln(acres per farm)	-0.009	-0.058
	(0.046)	(0.037)
ln(total acres)	-0.029	-0.014
	(0.052)	(0.061)
Share of improved acres in total acres	0.018	0.018
	(0.013)	(0.012)
ln(value of farmland and buildings per acre)	0.179***	0.064
	(0.065)	(0.053)
ln(value of farmland and buildings per farm)	0.173***	0.007
	(0.038)	(0.040)
ln(value of machinery and livestock per farm)	0.146***	-0.023
	(0.034)	(0.034)
ln(value of farmland, buildings, machinery and livestock per farm)	0.164***	-0.005
	(0.035)	(0.036)
ln(total value of farmland, buildings, machinery, livestock)	0.144	0.041
	(0.093)	(0.080)
Number of cultivated crops	-0.279***	0.055
	(0.080)	(0.081)
Panel B: Aggregated individual census		
ln(population)	-0.047	0.000
	(0.091)	(0.077)
ln(farmers)	-0.082	0.009
	(0.091)	(0.088)
ln(farm-related occupations)	-0.017	0.030
	(0.098)	(0.086)
ln(agricultural occupations)	0.001	0.027
	(0.101)	(0.085)
Panel C: Aggregated linked individual census		
ln(population, linked)	-0.034	-0.031
	(0.105)	(0.080)
ln(farmers, linked)	-0.079	-0.007
	(0.104)	(0.094)
ln(farm-related occupations, linked)	-0.026	0.003
	(0.112)	(0.093)
ln(agricultural occupations, linked)	-0.009	-0.000
	(0.115)	(0.092)

Notes: Each row reports the coefficient from a separate regression of the respective variable on a dummy indicating that the county experienced at least one drought in the following decade. Column (1) includes no fixed effects. Column (2) includes climate zone-by-decade fixed effects. Standard errors are two-way clustered at the county level and climate zone-decade level in both regressions. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

TABLE 4: Number of farms after droughts, by bankruptcy law

	(1) ln(farms)	(2) ln(farms)	(3) ln(farms)	(4) ln(farms)	(5) ln(farms)	(6) ln(farms)
Past drought * bankruptcy law	-0.232***	-0.227***	-0.200***	-0.232***	-0.229***	-0.245***
	(0.035)	(0.041)	(0.036)	(0.069)	(0.034)	(0.036)
Past drought * no bankruptcy law	0.112***	0.115**	0.125**	0.101	0.092**	0.082*
	(0.039)	(0.045)	(0.054)	(0.084)	(0.039)	(0.042)
Observations	12,466	12,471	12,466	12,471	10,886	12,466
R-squared	0.865	0.862	0.783	0.857	0.878	0.866
County FE	yes	yes	yes	yes	yes	yes
Climate zone-decade FE	yes		yes		yes	yes
Climate zone decile-decade FE		yes				
One-decade-lagged drought * bankruptcy law						yes
Sample					drop extreme droughts	

Notes: One-decade-lagged drought × bankruptcy law" includes two additional interaction terms as controls: (i) a dummy indicating whether the county experienced a drought between 10 and 20 years before the census year, interacted with a dummy for the presence of a federal bankruptcy law in the census year; and (ii) the same lagged drought dummy interacted with a dummy for the absence of a federal bankruptcy law. Standard errors are two-way clustered at county-level and climate zone-decade-level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 5: Number of farms after droughts, by bankruptcy law—robustness across different samples

	(1) ln(farms)	(2) ln(farms)	(3) ln(farms)	(4) ln(farms)	(5) ln(farms)	(6) ln(farms)
Past drought * bankruptcy law	-0.232***	-0.304***	-0.227***	-0.189***	-0.125***	-0.296***
	(0.035)	(0.041)	(0.035)	(0.036)	(0.027)	(0.042)
Past drought * no bankruptcy law	0.112***	0.137***	0.130***	0.221***	0.051**	0.138***
	(0.039)	(0.045)	(0.047)	(0.048)	(0.021)	(0.045)
Observations	12,466	10,760	10,908	9,870	11,380	10,577
R-squared	0.865	0.865	0.852	0.866	0.914	0.867
County FE	yes	yes	yes	yes	yes	yes
Climate zone-decade FE	yes	yes	yes	yes	yes	yes
State-decade FE	yes	yes	yes	yes	yes	yes
Sample		drop southern states	drop urban counties	drop states with some insolvency law	drop frontier counties	drop cotton-growing counties

Notes: Standard errors are two-way clustered at county-level and climate zone-decade-level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 6: Manual vs. capital investment

	(1) **manual investment**	(2) **capital investment**	(3) **diversification**	(4)
	share of improved acres/total acres	ln(machinery and livestock per farm)	number of crops	ln(number of crops)
Drought in past decade × bankruptcy law	-0.012*	0.024	-0.192***	-0.039***
	(0.006)	(0.016)	(0.037)	(0.007)
Drought in past decade × no bankruptcy law	0.015***	-0.052**	0.104**	0.022**
	(0.005)	(0.025)	(0.048)	(0.010)
Observations	12,466	12,466	12,466	12,458
R-squared	0.881	0.910	0.649	0.597
County FE	yes	yes	yes	yes
Climate zone-decade FE	yes	yes	yes	yes
State-decade FE	yes	yes	yes	yes

Notes: Standard errors are two-way clustered at county-level and climate zone-decade-level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 7: Aggregated individual census compared to agricultural census

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Agric. census		**Aggr. indiv. census**			**Population and race**			**Aggr. linked indiv. census**		
	ln(farms)	ln(farms)	ln(occ=farmer)	ln(occ=farm rel.)	ln(occ=agric.)	ln(pop)	ln(white)	ln(black)	ln(occ=farmer)	ln(occ=farm rel.)	ln(occ=agric.)
Drought in past decade *	-0.232***	-0.233***	-0.211***	-0.196***	-0.189***	-0.192***	-0.210***	-0.099	-0.208***	-0.197***	-0.197***
bankruptcy law	(0.035)	(0.036)	(0.036)	(0.035)	(0.033)	(0.030)	(0.030)	(0.073)	(0.038)	(0.037)	(0.036)
Drought in past decade *	0.112***	0.124**	0.101*	0.086	0.071	0.119*	0.122*	-0.044	0.051	0.062	0.054
no bankruptcy law	(0.039)	(0.063)	(0.054)	(0.054)	(0.055)	(0.061)	(0.063)	(0.116)	(0.054)	(0.056)	(0.057)
Observations	12,466	10,509	10,486	10,487	10,488	10,500	10,500	9,263	10,475	10,479	10,483
R-squared	0.865	0.862	0.860	0.872	0.875	0.888	0.889	0.855	0.882	0.886	0.887
County FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Climate zone-decade FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
State-decade FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Sample		drop 1890	drop 1890	drop 1890	drop 1890	drop 1890	drop 1890	drop 1890	drop 1890	drop 1890	drop 1890

Notes: Standard errors are two-way clustered at county-level and climate zone-decade-level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 8: Farmer inflows and outflows, county level

	Obs.	Mean	Std. Dev.	Min	Max
Panel A: Inflows by previous occupation and location, 1860-1920 (excl. 1890), in % of current farmers					
Farmers in same county	14,986	0.28	0.15	0.00	0.75
From other occupations in same county	14,986	0.23	0.12	0.00	1.00
From different county	14,986	0.49	0.24	0.00	1.00
From other occupations in same county: <16 yrs	14,986	0.15	0.09	0.00	0.55
From other occupations in same county: ≥ 16 yrs	14,986	0.08	0.07	0.00	1.00
Panel B: Outflows from farm-related occupations, 1860-1920 (excl. 1890), in % of previous farmers					
Continuing farmers	13,780	0.41	0.13	0.00	1.00
To other occupation in same county	13,780	0.12	0.08	0.00	1.00
To different county	13,780	0.47	0.15	0.00	1.00
To different urban county	13,780	0.34	0.12	0.00	1.00
To different non-urban county	13,780	0.14	0.09	0.00	1.00
To different urban county in manufacturing occupation	13,780	0.05	0.04	0.00	1.00
To different urban county in agricultural occupation	13,780	0.16	0.09	0.00	1.00
To different urban county in service occupation	13,780	0.07	0.05	0.00	1.00

Notes: The sample uses farm-related occupations as the definition of farmers.

TABLE 9: Farmer inflows, by previous occupation and location

	(1)	(2)	(3)	(4)	(5)
	in % of all farmers				
		Farmer inflows by origin			
	Continuing	From same	From other	From same county	
	farmers	county	counties	$\leq$ 16 yrs	$\geq$ 16 yrs
Past drought * bankruptcy law	-0.001	-0.001	0.003	-0.004	0.003
	(0.004)	(0.005)	(0.007)	(0.003)	(0.003)
Past drought * no bankruptcy law	0.007	0.008	-0.015	0.008**	0.000
	(0.009)	(0.006)	(0.010)	(0.003)	(0.004)
Observations	9,843	9,843	9,843	9,843	9,843
R-squared	0.832	0.711	0.848	0.721	0.717
County FE	yes	yes	yes	yes	yes
Climate zone-decade FE	yes	yes	yes	yes	yes
State-decade FE	yes	yes	yes	yes	yes

Notes: "Continuing farmers" are individuals who were already farmers in the previous census and remained in the same county. "From same county" refers to individuals who were not farmers but lived in the same county in the previous census. "From other counties" includes individuals who were located in a different county in the previous census, regardless of their prior occupation. Columns (4) and (5) split farmer inflows from same county by age of individuals in the previous census. All values are expressed as percentages of farmers observed in the census year, excluding individuals who did not appear in the previous census (as their prior occupation or location is unknown). Standard errors are two-way clustered at the county and climate zone-decade levels. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 10: Farmer outflows, by current occupation and location

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	in % of previous farmers							
		Farmer outflows by destination						
	Continuing	To other occ.	To other	To other counties		To other urban counties		
	farmers	in same county	counties	urban	non-urban	manuf occ.	agr. occ.	serv. occ.
Past drought	-0.005	-0.006**	0.010**	0.012**	-0.001	0.005**	0.006	-0.001
* bankruptcy law	(0.005)	(0.003)	(0.005)	(0.005)	(0.004)	(0.002)	(0.004)	(0.003)
Past drought	0.008	0.007	-0.015	-0.008	-0.007	0.000	0.001	-0.003
* no bankruptcy law	(0.008)	(0.006)	(0.009)	(0.009)	(0.005)	(0.004)	(0.007)	(0.004)
Observations	9,117	9,117	9,117	9,117	9,117	9,117	9,117	9,117
R-squared	0.690	0.685	0.771	0.688	0.746	0.498	0.622	0.540
County FE	yes	yes	yes	yes	yes	yes	yes	yes
Climate zone-dec. FE	yes	yes	yes	yes	yes	yes	yes	yes
State-decade FE	yes	yes	yes	yes	yes	yes	yes	yes

Notes: "Continuing farmers" are individuals who were already farmers in the previous census and remained in the same county. "To other occ. in same county" refers to individuals who stopped farming but continue to live in the same county in the current census. "To other counties" includes individuals who move to a different county in the current census, regardless of their current occupation. Urban and non-urban refer to the destination county and are defined as described in Section 3. All values are expressed as percentages of farmers observed in the previous census, excluding farmers who do not appear in the current census (as their current occupation or location is unknown). Standard errors are two-way clustered at the county and climate zone-decade levels. *** p<0.01, ** p<0.05, * p<0.1.

A. Appendix

A.1 Analysis underlying Figure 4: Farmer bankruptcies increased after droughts

We use data on the annual number of farmer bankruptcies at the federal district court level from the *Reports of the General Attorney of the United States* for the period 1899–1920 (digitized version available in Hansen et al. (2016)). To measure drought intensity at the district court-year level, we compute the weighted share of county-level drought dummies as in our main analysis (see Section 3), using the number of farms per county in 1900 as weights.

To estimate the relationship between drought shocks and farmer bankruptcy rates, we estimate the following regression model:

$$\begin{aligned} \text{BankruptcyRate}_{dt} \quad = \quad & \sum_{l=-3}^{3} \beta_l \, \text{Drought}_{d,t-l} \cdot 1_{s \notin L} + \sum_{l=-3}^{3} \gamma_l \, \text{Drought}_{d,t-l} \cdot 1_{s \in L} \\ & + \text{FE}_d + \text{FE}_{zt} + \epsilon_{dt} \end{aligned} \tag{A.1}$$

where $\text{BankruptcyRate}_{dt}$ denotes the number of farmer bankruptcies per 1,000 farms in district court d and year t, and $\text{Drought}_{d,t-l}$ refers to the lth lead or lag of the district court-level drought share. The specification includes district court fixed effects FE_d. To ensure comparison of states within climate-zones, we group states by climate zone based on their mode of county-level climate zone deciles, and add climate-zone-by-year fixed effects FE_{zt}. We estimate the effects of droughts separately for states in our main sample (yielding β_l), and the set of states in low bankruptcy take-up regions denoted as L (yielding γ_l). The latter are defined as regions where farmer bankruptcy rates remained below 1 per 1,000 farms, which includes the West South Central (Louisiana, Arkansas, Texas, Oklahoma) and the South Atlantic region (Delaware, Georgia, South Carolina, Virginia, Maryland, North Carolina, Florida, West Virginia, District of Columbia), see Wickens (1936). Standard errors are clustered at the state level.

Figure 4(a) plots the estimated coefficients on leads and lags of drought shares for the district courts in our main sample. We find that the number of farmer bankruptcies rises in years after a drought shock and to some extent in the year of a drought shock but does not respond to earlier drought shocks (years $t-2$, $t-3$) nor anticipate future ones (years $t+1$, $t+2$ or $t+3$). Figure 4(b) shows the coefficients for the low take-up states, where, as expected, farmers did not resort to bankruptcies to deal with droughts, as they were unfamiliar with this legal possibility.

Figure 5(a) and (b) plots the estimated coefficients on leads and lags of drought shares for the district courts in our main sample, using as dependent variable the bankruptcy rate for other non-farming professions. The bankruptcy rate is computed as (cases/1900 population in district court). We find that the number of bankruptcies for non-farming professions such as manufacturing and other professional occupations does not respond to drought shocks (panel a). For other occupations that may be indirectly related to farming, we only see the bankruptcies of wage-earners responding to a drought shock in the year before (panel b).

A.2 Farm mortgages

Figure A.1 uses data from the agricultural census to plot the share of owned farms with mortgages and the average mortgage interest rate (measured as mortgage payments in percent of the mortgage value) for the counties in our sample, aggregated across the US. Mortgage payment data are not available for 1900 and 1910.

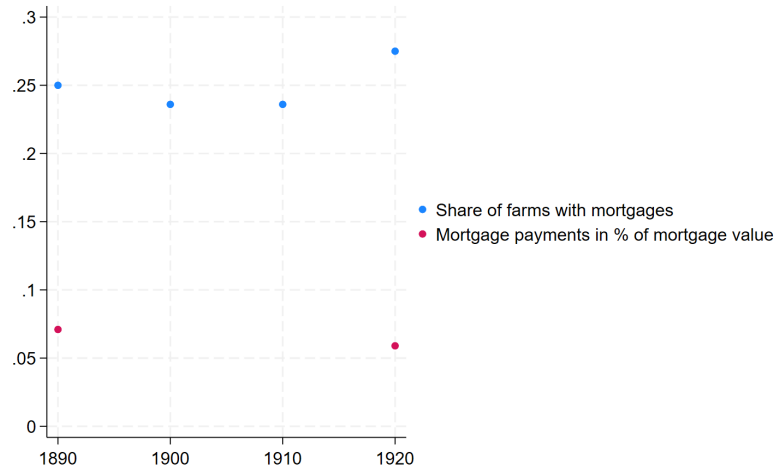


FIGURE A.1: Farm mortgages over time

Notes: Mortgage payment data are not available for 1900 and 1910.

A.3 Long-term correlation of droughts across centuries

Figure A.2 plots our definition of climate zones—measured as the number of droughts a county experienced between 1730 and 1830—on the x-axis, against the number of droughts the same county experienced in the subsequent century (1830–1930), which includes our main analysis period, on the y-axis. Each blue circle represents a county, and the red line indicates the 45-degree line.

The figure shows a strong correlation in drought frequency across the two centuries, suggesting that county-level drought exposure remained relatively stable over time. There is no indication of climate change across these two centuries.

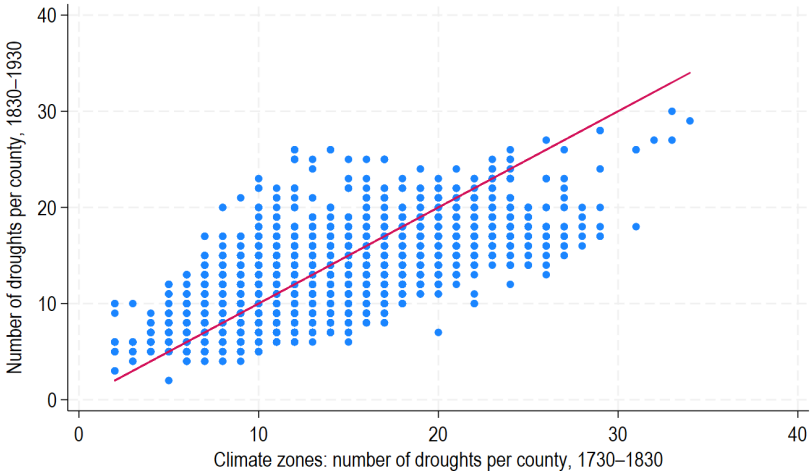


FIGURE A.2: Correlation of drought occurrence across decades

A.4 Correlation between individual and agricultural census

To assess the consistency between the individual census and the agricultural census, Table A.1 reports the correlation between the number of farms per county, as recorded in the agricultural census (dependent variable), and the number of farmers per county, as measured in the individual census (regressor). For

the latter, we use the three complementary definitions outlined in Section 3. All specifications include county and state-decade fixed effects to account for correlated time variation within states.

The results in Table A.1 indicate a strong positive correlation between the two sources, suggesting close alignment in how farming activity is captured across the two datasets.

	(1)	(2)	(3)	(4)	(5)	(6)
	Aggregated raw individual census			**Aggregated linked individual census**		
	ln(occ=farmer)	ln(occ=farm rel.)	ln(occ=agric.)	ln(occ=farmer)	ln(occ=farm rel.)	ln(occ=agric.)
Agricultural census:						
ln(farms)	0.721***	0.681***	0.649***	0.773***	0.733***	0.700***
	(0.042)	(0.040)	(0.038)	(0.048)	(0.046)	(0.045)
Observations	19,141	19,142	19,143	19,047	19,058	19,113
R-squared	0.942	0.950	0.946	0.946	0.954	0.954
County FE	yes	yes	yes	yes	yes	yes
State-decade FE	yes	yes	yes	yes	yes	yes

TABLE A.1: Correlation between aggregated individual census and agricultural census

A.5 State-level homestead exemption laws

The first homestead regulation was adopted in Texas, in 1839, and spread across the Northeast and Midwest between the 1840s and 1850s and to the South from 1840s to the 1860s. The Homestead regulation reached the Far West mostly by the 1860s, and by 1870 it was present in all states except in New Mexico and Oklahoma. The generosity and type of property exempt varied widely from state to state but it generally included the homestead, some acres of land and some monetary amount as protected from creditors (Goodman, 1993). Many states increased the generosity of its homestead law at different points in time, providing additional sources of variation. Homestead exemptions are still in place today and still vary widely across the U.S.