# Do Bankruptcy Exemptions Increase Entrepreneurship? Evidence from State Policy Variation and the Universe of U.S. Startups

## Extended Abstract

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## 1 Motivation

States implement bankruptcy exemptions with the goal of protecting assets such as a person's home if they file for bankruptcy. There is substantial variation in the reach of these policies across states. While debtor-friendly bankruptcy laws encourage risk taking by borrowers and facilitate a fresh start after bankruptcy, they may give rise to moral hazard and make it more difficult to obtain credit from lenders (White, 2007; Fossen and König, 2015). Thus, an important question is whether bankruptcy exemptions affect entrepreneurship. Do they increase the number of entrants by reducing the potential losses taken by entrepreneurs? Or, do they increase risk taking resulting in higher startup exit rates?

Bankruptcy laws offering some level of protection for assets such as the debtor's home limit the downward risk of entrepreneurship like an insurance policy. In this respect, more debtor-friendly bankruptcy laws, such as regulations providing larger homestead exemptions, make entrepreneurship more attractive. However, at the same time, bankruptcy exemptions shift risk to lenders, who expect to recover less of their credit

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in case of a debtor's insolvency. Banks may react by charging higher interest rates as a risk compensation, demanding more collateral, or rejecting credit applications. Because entrepreneurs rely on external credit to finance their investments, this constraint on credit supply may hinder entrepreneurship. It is theoretically ambiguous whether the insurance effect or the borrowing cost effect dominates and how bankruptcy law affects entrepreneurship on balance.

The theoretical literature studies this trade-off in equilibrium models and mostly predicts that the insurance effect dominates over the borrowing cost effect, such that more debtor-friendly bankrutcy laws increase entrepreneurial activity (Jia, 2015; Mankart and Rodano, 2015). A divergent result by Akyol and Athreya (2011) may be due to the omission of secured credit in their model, as secured credit mitigates the borrowing cost effect (Mankart and Rodano, 2015).

Empirical literature supports the view that more debtor-friendly personal bankruptcy laws increase entrepreneurship rates. Using data from the Survey of Income and Program Participation (1993-1998), Fan and White (2003) report that the probability of owning a business is 35% higher in states with unlimited rather than low homestead exemptions. Rohlin and Ross (2016) obtain qualitatively similar results based on zip-code level data from D&B Marketplace. Armour and Cumming (2008) find that entrepreneur-friendly bankruptcy laws increase entrepreneurship rates in a sample of 15 countries in Europe and North America (1990-2005). Fossen (2014) analyzes a legislative reform in Germany, which made personal bankruptcy law more debtor-friendly, as a natural experiment and finds that entrepreneurship became more attractive for individuals with relatively low wealth levels. More forgiving bankruptcy laws may also incentivize debtors to file for bankruptcy. Agarwal et al. (2005) report supporting evidence, as they find that the probability of small business owners filing for bankruptcy is higher in US states with higher homestead exemption levels.

An examination of the effects of bankruptcy exemptions on entrepreneurship contributes to the broader economic questions of adverse selection and moral hazard (Akerlof, 1970; Holmström, 1979). Does it draw in lower quality and thus higher risk entrepreneurs than what is optimal (i.e. adverse selection)? Does it increase risky behavior among entrepreneurs or startups that is not optimal (i.e. moral hazard)? Adverse selection and moral hazard among entrepreneurs might in turn lead lenders such as banks and investors to restrict the supply of loans and credit, especially if there is incomplete information on the likelihood of debt repayment among entrepreneurs. In this case, the bankruptcy exemptions might have a negative effect on startup activity and outcomes.

We propose to study the net effect of these opposing forces using state-level data on bankruptcy exemptions linked to microdata startups. We consider homestead bankruptcy exemptions, which determine how much home equity a debtor can protect from unsecured creditors in personal bankruptcy (Chapter 7 of U.S. personal bankruptcy law). For example, Florida and Texas provide unlimited homestead exemptions,

while other states have no or low homestead exemptions. Personal bankruptcy law is very relevant for entrepreneurs, because entrepreneurs are personally liable for business debt except for corporate forms with limited liability (which are rare among startups), and because business loans tend to be large in comparison to consumer credits (White, 2007).[1]

We plan to also use newly available administrative microdata—the Comprehensive Startup Panel (CSP)—covering the universe of U.S. startups and tracking them from initial formation (Fairlie et al., 2023). Commonly used aggregate data or employer-only records for studying entrepreneurship exclude 89 percent of startups and code "birth" as the first year an employee was paid rather than when the firm began. Because the CSP tracks firms from true formation, we can measure effects on both the extensive margin (entry and exit) and the intensive margin (employment growth), and examine whether bankruptcy policy shifts the composition of entry between employer and nonemployer startups. Fairlie et al. (2025) use the CSP to study the effects of a comprehensive set of tax policies on startup outcomes. We build on that work by studying a very different type of policy that has added implications for risk taking, moral hazard and adverse selection.

The proposed study will provide the first examination of the effects of bankruptcy exemptions using microdata covering the universe of startups in the United States. It will also be the first to use event study methods accounting for differential timing of treatment to investigate a large number of startup outcomes including the number of nonemployer and employer startups, employment, closure rates, and mobility. We will examine extensive and intensive margin adjustments by startups, including employment in startups and interstate mobility. The proposed study will build on previous studies of ours which use the CSP to examine the effects of taxes and tax incentives (Fairlie et al., 2025) and national levels of job creation and startup survival rates (Fairlie et al., 2023). The proposed study will also contribute to the limited previous literature which is based on survey data, non-representative proprietary data or cross-country comparisons, and considers only a few outcomes such as business ownership or exit.

## 2 Data

We combine newly available data on the universe of U.S. startups with state information on bankruptcy exemptions to examine how these policies affect entrepreneurial activity. Our entrepreneurial outcomes come from the Comprehensive Startup Panel (CSP), which tracks all business startups in their first eight years. Bankruptcy exemption data are sourced from Pattison and Hynes (2020). We supplement these

[1] Even if the startup is a limited liability company, this is often undermined in practice as lenders demand contracted personal guarantees from the entrepreneur.

with demographic and economic controls from the Bureau of Economic Analysis, the American Community Survey, and U.S. Census Bureau intercensal datasets. This analysis covers the period 2004–2017.

## 2.1 Measurement of Entrepreneurial Outcomes

The CSP provides the first administrative panel database on the *universe* of U.S. business startups—including both employer and nonemployer firms—tracked from founding through their first eight years (Fairlie et al., 2023).[2] This comprehensive coverage addresses a critical limitation of prior entrepreneurship research: existing datasets focus exclusively on employer firms (excluding 89 percent of startups) and define the startup year as the first year with employees, significantly mistiming founding dates for firms that begin as nonemployers.

The CSP integrates two Census Bureau data sources: the Longitudinal Business Database (LBD), covering all non-farm employer establishments annually since 1976, and the Integrated Longitudinal Business Database (ILBD), covering all nonemployer establishments with quinquennial data from 1977 to 1992 and annual data thereafter. By linking these universes, the CSP enables us to track nonemployer startups and observe when they transition to employer status, which is critical for understanding the full spectrum of entrepreneurial responses to policy changes.[3] The CSP includes detailed information on businesses, such as geographic location, establishment year, employment, payroll, legal form, industry, and closure.

In our main analysis, we examine six startup outcomes at the county-year level. Our primary extensive margin measures are the counts of new nonemployer startups (in their first year) and employer startups (in their first eight years). To assess survival and geographic mobility, we also measure startup exit rates (the share of startups that cease operations) and interstate move-out and move-in rates (the shares of startups that relocate across state borders). For intensive margin analysis, we examine total employment across all startups in their first eight years. These outcomes allow us to decompose policy effects across different margins of adjustment and distinguish between effects on firm formation, survival, and growth within surviving firms.

## 2.2 Measurement of Homestead Bankruptcy Exemption

Our key policy variable is the state homestead exemption, which determines how much home equity a debtor can protect from unsecured creditors in personal bankruptcy. Homestead exemptions vary widely across states and over time, ranging from very low protection in some states to effectively unlimited protection

[2]The CSP was recently released for use at Federal Statistical Research Data Centers.

[3]The CSP identifies startups using an algorithm that prioritizes the oldest establishments, resolving ties by considering employment, payroll, and random selection among equals. The data allow for accurate tracking of business entities as they evolve over time, including when they relocate across state borders or undergo ownership changes. A business enters a startup cohort only if it has not appeared in either the nonemployer or employer universes in the previous seven years. See Fairlie et al. (2023) for further details.

in others.

We construct a state-year panel of homestead exemption levels using the statutory exemption series compiled by Pattison and Hynes (2020). For each state and year, we record the dollar amount of home equity protected for a single filer under the applicable exemption regime. In states that allow debtors to choose between state and federal exemptions, we follow Pattison and Hynes (2020) and use the more generous option.

### 2.3 Measurement of Control Variables

We include demographic and economic controls shown in prior literature to explain entrepreneurship. County-level population data are obtained from the Bureau of Economic Analysis, part of the U.S. Department of Commerce. Demographic variables—including shares of the working-age population (ages 25-64), educational attainment (high school graduates and college graduates), and racial and ethnic composition (shares of female, African American, Asian, and Hispanic populations)—are sourced from the American Community Survey (ACS) for the years 2010 to 2017.

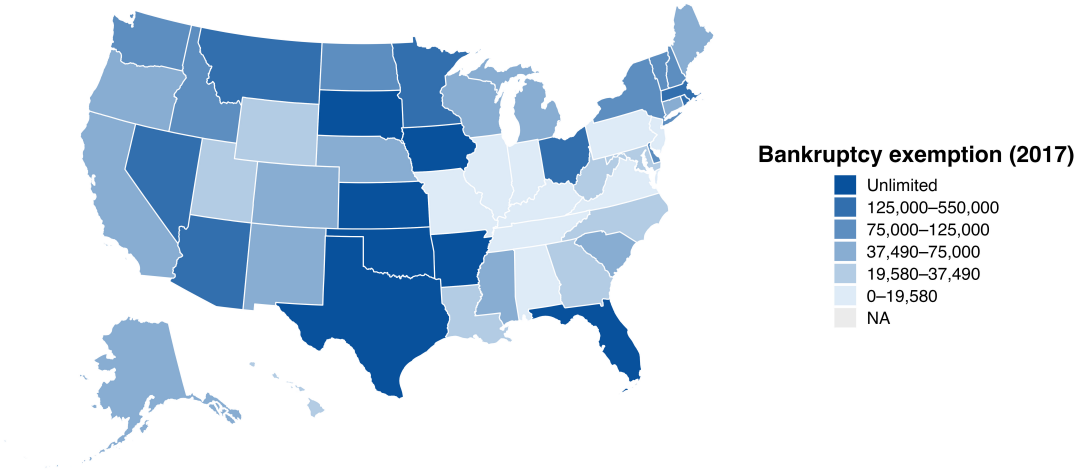
The ACS does not provide detailed demographic data consistently for years prior to 2010. To ensure complete coverage of our 2001-2017 analysis period, we supplement the demographic data for 2004-2009 using the County Intercensal Datasets provided by the U.S. Census Bureau for demographic shares and the USDA Economic Research Service for educational attainment variables. We also control for the average age of startups in the county and its square, calculated from the CSP (except when we estimate the number of new nonemployer businesses).

## 3 Descriptive Statistics

State-level homestead exemption levels in 2017 are shown in Figure 1. They range from less than $20,000 in states such as Illinois and Pennsylvania to unlimited exemptions in states such as Texas and Florida.

Table 1 presents descriptive statistics for key startup-level and county-level variables used in this study. We have 201.1 million startup-year observations in the CSP. Average employment at the startups in the sample, which are up to eight years old, is 1.37 workers. While transitions rates from nonemployer to employer and interstate mobility are low, the average annual closure rate is high at 30.15%. The average homestead bankruptcy exemption is $56,100. There are 5,600 startups up to eight years old in an average county, 4,700 of them are nonemployers. Startups employ a total of 8,000 workers in an average county.

Figure 1: Homestead Bankruptcy Exemptions by State (2017)




*Note:* This map displays state-level homestead exemption levels in 2017, which determine how much home equity a debtor can protect in personal bankruptcy. States with unlimited homestead exemptions are indicated accordingly. *Data Source:* Pattison and Hynes (2020).

Table 1: Descriptive Statistics

| **Panel A: Startup-Level Variables** | **Mean** | **Standard Deviation** |
|---|---|---|
| Employment | 1.37 | 80.44 |
| ΔEmployment | 0.04 | 62.6 |
| Transition from Nonemployer to Employer (%) | 0.83 | 0.09 |
| Closure Rate (%) | 30.15 | 0.46 |
| Move-out Rate (%) | 1.21 | 0.15 |
| Move-in Rate (%) | 2.32 | 0.11 |
| Firm Age | 2.06 | 1.97 |
| **Panel B: County-Level Variables** | **Mean** | **Standard Deviation** |
| Bankruptcy Exemption ($1,000s) | 56.10 | 87.89 |
| Total Employment | 8,000 | 30,500 |
| Nonemployer Startups | 4,700 | 17,500 |
| Employer Startups | 900 | 3,300 |
| Total Startups | 5,600 | 21,000 |
| Transition from Nonemployer to Employer (%) | 0.86 | 0.62 |
| Exit Rate (%) | 29.85 | 4.68 |
| Move-out Rate (%) | 0.97 | 0.71 |

This table reports summary statistics covering 2001 to 2015. The bankruptcy homestead exemption data cover 2004 to 2017. Unlimited homestead exemptions are not used in the calculation of these descriptive statistics, but will be included in the analysis of large policy changes. Panel A presents startup-level variables from the Comprehensive Startup Panel (CSP), which tracks the universe of U.S. startups from founding through their first eight years; the sample includes 201.1 million startup-year observations. Panel B presents the homestead bankruptcy exemption, measured at the state-year level and merged to counties by state, as well as county-level aggregates of startup outcomes. Startup counts and employment figures are rounded for Census Bureau disclosure requirements.

*Main data source*: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020, and CBDRB-FY25-P2936-R12587).

# 4 Proposed Analysis

We will examine the effects of homestead bankruptcy exemptions on several startup outcomes including the number of new nonemployer and employer startups in a county, total employment in startups in a county, closure rates, and the shares of startups moving out and in across state borders. To do so, we will aggregate the firm-level CSP microdata to county-year panel data. To identify causal effects, we will exploit large, discrete changes in bankruptcy exemptions using difference-in-differences designs. We will employ recent advances in difference-in-differences methods (Callaway and Sant'Anna, 2021) to address well-documented biases in traditional two-way fixed effects models with staggered adoption. Our approach will use only not-yet-treated or never-treated counties as controls, avoiding contamination from units already affected by treatment dynamics. We will use event study analysis to inspect parallel pre-trends between treatment and control groups, check whether effects coincide with the timing of policy changes, and estimate dynamic treatment effects. We will assess the robustness of our results, e.g., by using alternative treatment definitions and controlling for other contemporaneous policy changes.

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