Angels, Entrepreneurship, and Employment Dynamics: Evidence from Investor Accreditation Rules

Laura Lindsey*
W. P. Carey School of Business
Arizona State University

Luke C.D. Stein†
Babson College

February 2020

Abstract

This paper examines the effects of a shock to angel finance on entrepreneurial activity and employment. Using U.S. Census data, we estimate the state-level fraction of households that lost accreditation status from Dodd–Frank's elimination of housing wealth in determining accreditation. A larger reduction in the pool of potential investors reduces firm entry and employment at small entrants, particularly in areas with alternate sources of financing. Employment increases at small and young incumbents, and relative wages for the startup sector decline, especially for high-skilled workers and industries. These results suggest that angels are an important source of entrepreneurial finance to high-quality, competitive firms.

We are grateful for comments from Tania Babina, Allen Berger, Stefano Bonini, April Franco, Jessica Jeffers, Shane Johnson, Ramana Nanda, Paige Oiumet, Berk Sensoy, Xinxin Wang, Ting Xu, and participants in the 2017 Southern California Private Equity, 2017 SIT Emerging Trends in Entrepreneurial Finance, 2017 SITE Labor & Finance, 2017 European Finance Association, 2018 UNC Kenan Institute Frontiers of Entrepreneurship, 2018 Financial Intermediation Research Society, 2018 Texas A&M Young Scholars Finance Consortium, 2018 University of Kentucky, 2018 SFS Cavalcade, 2018 Chicago Fed/Notre Dame Labor Market Dynamism, 2019 Midwest Finance Association, 2019 SEC Financial Market Regulation Conferences, and presentations at Johns Hopkins University and Cambridge University. We thank Andra Ghent for extensive advice and Hong Zhao for valuable research assistance.

*Department of Finance, P.O. Box 873906, Tempe AZ 85287; laura.lindsey@asu.edu.

†Finance Division, Babson College, 231 Forest Street, Babson Park MA 02457; lcdstein@babson.edu.

1 Introduction

Access to capital has long been recognized as crucial to the entrepreneurial process of creative destruction (Schumpeter, 1911, 1942). While financial intermediaries such as banks and venture capital firms play an important role, much of the capital supplied to new firms takes place through the informal channel of direct investment from individuals, often referred to as "angel finance." Puri and Zarutskie (2012), for example, estimate that fewer than 0.2% of new companies raise venture capital, and bank lending often requires collateral or personal credit that is infeasible for many types of businesses or entrepreneurs. Estimates of the angel capital market's size are comparable in magnitude to that of the venture capital industry in the U.S. and many other countries (Fenn, Liang, and Prowse, 1997; OECD, 2011). Yet, largely due to data limitations, very little empirical evidence exists on angel investing or its economic impact.

In the U.S., many angel transactions require no disclosure, and where disclosure requirements *do* exist, enforcement is lax and compliance levels may be low. Therefore, much of what is known about the angel market relies on surveys of angel groups and well-known individual investors, or data on the subset of transactions that take place alongside organized venture capital or are reported to the SEC. Of transactions reported to the SEC claiming exemption from securities registration under Regulation D, 99% of investment takes place through Rule 506, which allows a company to issue securities to an unlimited number of accredited investors and a limited number of other purchasers, with a similarly high percentage even when the amounts raised would allow for use of other provisions. Approximately 90% of reported offerings are limited to accredited investors, meaning investors who meet minimum

wealth or income thresholds (Bauguess, Gullapalli, and Ivanov, 2018).[1]

In this paper, we exploit a rule change in 2010 that differentially reduced eligibility for accredited investor status across geographies in order to assess the importance of business angels for the entrepreneurial sector. We show that the availability of angel capital at the margin bears a causal relation to firm entry that is driven by smaller firms. The effect of angel capital on entry is stronger in states where alternative capital sources are more prevalent, suggesting these sources are poor substitutes for angel finance. Further, we provide evidence on how competing firms and entrepreneurial wages are affected by the decline in business entry associated with the rule change, demonstrating the economic importance of angels beyond the companies they directly fund.

Our measure of differential treatment derives from the public-use Survey of Income and Program Participation (SIPP), with geographic information available at the state level. A special wave of the survey conducted around the time of the rule change contains detailed information on households' net worth and income.[2] From these data, we calculate the fraction of the sampled accredited investor population that lost accreditation status when the value of equity in a primary residence could no longer be used to meet the net-worth standard for accreditation as a result of the passage of The Dodd–Frank Wall Street Reform and Consumer Protection Act (Dodd–Frank).

Using a continuous-treatment difference-in-differences framework, we first examine the impact of this shock to angel finance on new business creation using information from the

[1]Shane (2009) argues that while not all funds deployed in the angel market come from investors meeting accreditation standards, accreditation likely gives investors access to better investment opportunities. Entrepreneurs may prefer Rule 506 (and accredited investors) for a number of reasons, including lower disclosure requirements, automatic compliance with state securities laws, and the option to raise additional funds without exceeding a pre-determined limit. Notably, failure to notify the SEC has not been deemed to eliminate the ability to claim safe-harbor under the rule.

[2]We are unaware of any alternative representative source that contains the micro data necessary to assess investor accreditation at a finer geographic level.

Business Dynamics Statistics (BDS) database. We find a negative and statistically significant reduction in new businesses of about 2% on average. These results obtain controlling for state fixed effects, as well as measures of local economic conditions that vary over time. Thus, we demonstrate that even relatively small angels play a vital role in the entrepreneurial sector of the economy. Further, our results imply that the net-worth threshold for investor accreditation binds.

To determine how the reduction in new businesses may be affected by other forms of available finance, we segment states at median levels of venture capital investment, house-price appreciation, and availability of small business bank lending at the time of the shock. We find that effects are more pronounced in areas with higher availability of alternative funding, suggesting that these sources are imperfect substitutes for angel finance. We also examine differences in entry based on the size of the new business, as measured by the number of employees. Consistent with the notion that the declines in new business formation arise from a shock to the capital supply from smaller investors, effects are driven by smaller businesses.

We next study effects on employment. The entrepreneurial sector has been shown to have a disproportionate effect on the economy overall, with most new job creation stemming from young firms (Haltiwanger, Jarmin, and Miranda, 2013; Decker, Haltiwanger, Jarmin, and Miranda, 2014). In addition, survey estimates suggest that as many as six jobs are created for each angel investment (Sohl, 2011). Having fewer new businesses as a result of a reduction in funding from smaller investors should result in a reduction in total employment at smaller entrants. Incumbent firms may benefit from reduced competition in the labor or product markets, and funding sources that may have served as complements to angel finance may redeploy capital to existing firms.

We find that employment falls more for firms with 10 or fewer employees by the end of

their entering year in states that lost a greater percentage of potential angel investors, though we find no aggregate effects on employment levels for entrants or incumbents. For incumbent firms, we identify larger increases in employment at smaller and younger incumbents as a result of both increases in new job creation and decreases in job destruction. These results indicate that a reduction in capital availability resulting from stricter accreditation standards alters the observed distribution of employment across firms. Insofar as lost entrants were higher-quality firms, these competitive effects indicate a lower level of dynamism.[3]

In order to better understand the characteristics of firms that rely on informal financing networks, we turn to Quarterly Workforce Indicators (QWI) data that allow us to examine employment changes by industry and by worker demographics. We expect that angel finance has a larger impact in industries for which entry is easier, and partition our sample accordingly. First, we categorize industries by the level of venture-capital funding they receive, which may be correlated with return characteristics that make these industries likely to attract external finance. The decline in employment for entrants is driven by industries that attract a large amount of venture capital, demonstrating again that formal venture capital does not easily substitute for angel finance even in sectors in which it is relatively active. Next, we segment industries by their level of concentration. The decline in employment for entrants is driven by sectors with below-median employment shares for their top 50 firms, suggesting that effects are more pronounced in more competitive industries. We also segment industries by the typical amount of startup capital needed to enter. We find that employment effects are driven by industries with lower startup-capital requirements, suggesting that angels fund firms with relatively modest capital requirements at the margin, and again providing evidence consistent with forgone entry due to a reduction in capital availability from smaller investors.[4]

[3]We discuss efforts to assess the relative quality of lost entrants later in the analysis.

[4]When we examine younger incumbent firms, there is no measurable effect when segmenting industries by

To gain insight about the quality of firms that failed to enter due to a reduction in the pool of accredited investors, we also examine entrepreneurial wages, which can be affected by changes in labor composition and competition. We find evidence of lower wage levels at infant firms for states that are more affected by the investor accreditation rule change. This result is consistent both with the possibilities that angel-backed firms hire more highly paid workers and that they bid up the wages of startup employees more broadly. (If fewer new businesses enter and compete for workers, labor demand shifts downward, putting downward pressure on wages provided the labor supply curve is not flat). The effect is driven by industries that have a higher fraction of higher-skilled workers and for workers with higher skill. Taken together, these results suggest that the decline in new business entry resulting from the reduction in availability of angel capital comprised firms able to attract high-quality workers.

Our investigation sheds light on the impact of informal capital networks. To our knowledge, we provide the first empirical investigation of investor accreditation rules. We demonstrate that the net-worth threshold binds, constraining entrepreneurial activity. We also demonstrate the economic importance of informal capital networks in providing finance for new businesses. Our study is the first to identify causal economic effects from changes in capital availability in the informal angel financing market. In addition, we offer suggestive evidence that the marginal investor does not fund firms of lower quality: we observe measurable effects on the distribution of employment and wage patterns consistent with high levels of human capital at forgone entrants.

Understanding the impact of these rule changes is particularly important given the policy directive of Dodd–Frank that the Securities and Exchange Commission (SEC) review

high or low venture capital, concentration, or startup capital requirements. These results offer little support for the notion that additional financing for young incumbents becomes available, though the QWI data does not allow us to simultaneously segment these data by firm age and size.

accredited investor requirements every four years. Our results obtain based on what might be considered minor changes affecting primarily smaller participants. Notably, it is not the aggregate amount of capital removed from the market that is important for our study, but instead the number of individuals providing capital to businesses that lack alternative sources of start-up funding. Survey data from 2017 indicate that the interquartile range of investment amounts for angels who remain accredited under the new rules is $15,000 to $37,500 (Huang et al., 2017). Investors that lost accreditation may not necessarily have provided less capital per investment than investors who remain accredited, but may instead make fewer investments.[5]

While numerous studies examine the impact of organized venture capital on the firms they finance and their role in the economy, prior work on the consequences of angel finance is scarce.[6] Notable exceptions include Kerr, Lerner, and Schoar (2011) and Lerner, Schoar, Sokolinski, and Wilson (2018), who examine the impact of angel funding on firm outcomes using data from angel groups, and Bernstein, Korteweg, and Laws (2017), who conduct an experiment on an investment platform to determine which factors are important in attracting early stage funding. In contemporaneous working papers, Howell and Mezzanotti (2019) and Denes, Wang, and Xu (2019) study the efficacy of state tax credits in promoting angel finance. Additional treatments study the interaction of angel finance and formal VC (Hellmann and Thiele, 2008; Goldfarb, Hoberg, Kirsch, and Triantis, 2013; Hellmann, Schure, and Vo, 2017; Chemmanur and Chen, 2014). Our work contributes to the scant knowledge about

[5]It remains an open question to what extent Regulation CF, finalized in May 2016, may relax these constraints. However, Huang et al. (2017) also report that over 50% of angels access deals outside of angel groups, either through associates or directly with an entrepreneur, whereas fewer than 20% of angels had engaged with an online portal. Therefore, Regulation CF is unlikely to relax constraints from accreditation rules significantly, at least in the near term.

[6]See for example Hellmann and Puri (2000, 2002); Mollica and Zingales (2007); Sørensen (2007); Bottazzi, Da Rin, and Hellmann (2008); Lindsey (2008); Samila and Sorenson (2010); Puri and Zarutskie (2012); Bernstein, Giroud, and Townsend (2015); Gonzalez-Uribe (2020). An extensive review of the venture capital literature is available in Da Rin, Hellmann, and Puri (2012).

financial angels, providing a first causal glimpse into their effect on entrepreneurial activity and employment at the macro level.

Our work also relates to studies addressing alternative forms of finance for new firms. Both theoretical and empirical treatments suggest that banks are an imperfect substitute for venture or angel capital, particularly at the early stages (Ueda, 2004; Hellmann, Lindsey, and Puri, 2008).[7] Corporations also participate in new firm finance, but typically invest in companies that will complement their core businesses (Hellmann, 2002; Gompers and Lerner, 2000; Ma, 2020). Additional papers have explored the role and success of government programs (Lerner, 1999; Brander, Du, and Hellmann, 2014) and newer market participants such as accelerators (Fehder and Hochberg, 2014). We show that angel finance also plays an important role, and offer new evidence on its areas of impact.

We also contribute to a large literature that has sought to understand the role of financial constraints on entry, employment, and productivity. Local capital availability, whether measured at the country level or more finely, has been linked to the birth of new firms and the growth of economies more generally.[8] In particular, both theoretical and empirical work emphasize the role of personal wealth in the pursuit of entrepreneurship (Evans and Jovanovic, 1989; Hurst and Lusardi, 2004), and access to collateral in the form of housing wealth has also been shown to be a binding constraint on entry and growth (Adelino, Schoar, and Severino, 2015; Schmalz, Sraer, and Thesmar, 2017).

Several studies in this literature focus on bank lending, relying on changes in the structure of the banking industry to isolate causal effects. For example, following changes in bank branching restrictions, Black and Strahan (2002) document effects on new incorporations, and

[7]Note that recent evidence highlighting the role of debt is not necessarily indicative of business lending from banks; for example, Robb and Robinson (2014) categorize personal debt as outside debt in their study of new firms' capital structures.

[8]See Kerr and Nanda (2011) for a review.

Kerr and Nanda (2009) show evidence of increased entry and exit. Further, Kerr and Nanda (2010) and Krishnan, Nandy, and Puri (2015) focus on firms' size at entry and total factor productivity, respectively, highlighting effects from banking deregulation at the intensive margin as well.

Additional work focuses on the role of bank finance through recent financial or other crises (e.g., Goetz and Gozzi, 2010; Greenstone, Mas, and Nguyen, 2014; Cortes, 2014; Chodorow-Reich, 2014). Our work is similar in spirit in that we utilize a shock to to the pool of potential capital suppliers to measure differential effects across geographies and provide estimates for a new constraint on the supply of capital in the aftermath of the financial crisis.

Finally, our work relates to the literature that seeks to understand the relation between firm size, age, and employment growth in order to inform policy decisions. Haltiwanger, Jarmin, and Miranda (2013) document that the relation between firm size and growth is driven largely by firm age, and emphasize the role of entrants in new job creation. Adelino, Ma, and Robinson (2017) examine differences in how young and old firms take advantage of investment opportunities. Both papers suggest that promoting capital availability to entering firms may be more important than policies that focus on small firms and job creation per se.

Though we document adverse effects of tightened investor accreditation requirements on entry and job creation at entrants, our study does not attempt to assess any offsetting benefits that may have motivated the rule change or could influence SEC recommendations going forward. It is interesting to note that while the accreditation rules were originally rooted in investor *protection*, much of the current debate focuses on expanding investors' *access* to private securities issued by high-growth firms.[9] Our work shows that capital formation for

[9]See, for example, Michaels, Dave, "SEC Chairman Wants to Let More Main Street Investors In on Private Deals," Wall Street Journal, 30 October 2018; or the public comments of SEC commissioner Luis Aguilar on 17 December 2014 (available at https://www.sec.gov/news/statement/spch121714laa.html).

entrepreneurial ventures should also a first-order consideration, especially since it appears that other sources of funding are poor substitutes for informal angel networks.

2 Empirical approach

Our analysis relies on a difference-in-differences approach to isolate causal effects of angel finance on entrepreneurial activity. The refinement to the investor accreditation definition within Dodd–Frank had little to do with any prior activity in the entrepreneurial sector, and venture capital firms were exempt from registration under the the Act. Further, the differential impact across regions is based on housing wealth and incomes in the general population and can be taken as exogenous as long as the extent to which these characteristics may predict changes in local economic conditions can be controlled. A key assumption is that angel finance is sufficiently local such that state-level shocks to capital availability impact new and incumbent businesses within the state. The available data on angel investing indicate that it is, indeed, a local activity, with the vast majority of investments occurring within 50 miles of the angel investor (Shane, 2009). In this section, we provide background on the rules that govern accredited investors and describe the construction of our treatment measure.

2.1 Regulatory background

Angel finance usually involves the issuance of private (unregistered) securities. Until recently, the issuance of private securities was governed largely by rules set forth in the Securities Act of 1933 and modifications made in the 1980s under Regulation D.[10] Transactions commonly take place under various provisions of Regulation D, which set limits on the amount of capital that can be raised, require various disclosures to investors, and limit the

[10]These regulations apply to all securities, including debt, though borrowing from an institution such as a bank is not considered a securities issuance.

number of shareholders in certain circumstances. Under Rule 506, disclosure of extensive financial information is not required nor is the capital amount limited as long as investors are "accredited," a designation meant to proxy for financial sophistication sufficient to evaluate securities not covered by the Securities Act (or, alternatively, identify investors who are sufficiently wealthy so as not to require protection).[11]

To be accredited, investors must meet minimum wealth or income thresholds. Regulation D defined accredited investor status for an individual as having income in excess of $200,000 in the most recent two years (with an expectation of continued income at the same level in the current year), or a net worth over $1 million. In 1988, the income requirement was refined to include a $300,000 joint-income test with one's spouse (Regulation D Adopting Release).[12] Other than the Dodd–Frank-induced change we study, there were no changes regarding eligibility for investment in private offerings during our sample period.[13]

In the wake of the financial crisis, Section 413(a) of Dodd–Frank required that the value of a person's primary residence be excluded from the calculation of net worth used to determine investor accreditation status. The change to the net worth requirement was effective immediately upon passage when signed into law on July 21, 2010. SEC rules were later updated to reflect that positive home equity should not be included in the calculation,

[11]Rule 504 under Regulation D, which allowed companies to raise amounts up to $1 million in a 12-month period, effectively required either that investors have a prior relationship with the company, the offering be registered at the state level, or investors meet accreditation thresholds. Rule 505 allowed for amounts up to $5 million in a 12-month period from an unlimited number of accredited investors and up to 35 (affiliated) non-accredited investors. Importantly, Rules 504 and 505 did not preempt regulation at the state level as does Rule 506.

[12]Other thresholds apply to entities that are not natural persons such as business trusts or retirement accounts, and banks and investment companies are governed under separate rules.

[13]The Jumpstart Our Business Startups (JOBS) Act was signed in 2012, but its expansion of participation in private offerings by non-accredited investors did not take effect until after our sample period. Rules for the expansion of Regulation A were finalized in April 2015, and crowdfunding provisions were not effective until 2016. General solicitation, which allowed advertising to accredited investors, became effective in the last six months of our annual sample (after the SEC rule-making process in September of 2013).

and imposed restrictions on the use of cash-out mortgage refinances to meet the threshold. Importantly, Section 413(n)(2)(A) of Dodd–Frank directs the SEC to review the definition of accredited investors every four years. In the 2015 review, for example, recommendations to raise the income and net-worth thresholds (to $500,000 and $3 million, respectively) and to introduce investment limits for those meeting the current threshold but not the proposed levels were introduced, though no changes were made.

From the 2010 Survey of Consumer Finances (SCF), we can estimate the number of households that lost accredited investor status. Applying a $200,000 income threshold if the household responder is unmarried and a $300,000 threshold if married, we find that 3.6% of households qualify under the income test. For assets, 9.8% of households have a net worth of $1 million or more including the home equity of the primary residence, dropping to 7.5% with the value of the residence excluded. With the income *or* asset distinction, 10.4% of households qualify as accredited investors prior to the Dodd–Frank change, and 8.4% after, a reduction of almost 20%.[14] Of course, only a small subset of investors who meet the accreditation standards likely engage in angel activity, and it is likely that a larger proportion of angel capital comes from those households that remain accredited under Dodd–Frank's stricter standard. Nevertheless, a 20% change in the number of households that can provide private capital under Regulation D is sizable, and the treated (i.e., marginal) angel investors may have been more likely to fund businesses unable to raise funds from alternative sources. Moreover, the participation rate among marginal accredited investors is not directly relevant; instead, for the percentage reductions in the number of accredited investors and angel investors to be similar requires only that participation rates not be very different between marginal and

[14]The SCF also asks whether respondents' prior-year income is "unusually high." Excluding positive responders from the income qualification, about 10% of households qualify before Dodd–Frank, and approximately 8% after. All percentages are calculated using SCF sampling weights.

average accredited investors.

What are the demographics of these treated households? The mean age of the responder in a treated household is 63, with an interquartile range of 54 to 71. Over 72% are college graduates, with an additional 15% reporting some college. Approximately 61% of treated households report "Excellent" financial literacy and about 94% place themselves in either the "Excellent" or "Good" categories. These figures compare to 67% and 96% for those households that remain accredited after Dodd–Frank. Perhaps surprisingly, wealth and income standards may be a reasonable proxy for investor sophistication, and the treated population appears to be from the lower end of the distribution among this relatively sophisticated group.

Table 1 reports mean and median values of income, net worth, and home values for treated, accredited untreated, and never-accredited investors. For the treated group, average income is \$119K and net worth excluding home equity is over \$750K. Notably, home equity and home values are not too different between treated and untreated accredited households. The mean (median) home value is \$748K (\$550K) for households that remain accredited, and \$601K (\$500K) for those potentially affected by the Dodd–Frank rule change. Home equity figures are even more similar: mean (median) home equity is \$530K (\$380K) for households that remain accredited, and \$476K (\$377K) for treated households. Overall, the determination of treatment seems to relate largely to wealth outside of one's residence and income.

In Figure 1, we consider the probability that a household is treated across several characteristics related to the accreditation criteria. Consistent with the summary statistics in Table 1, treatment likelihood varies considerably with non-housing wealth (i.e., net worth excluding equity in the primary residence). Household income is less predictive of treatment except, of course, near the accreditation thresholds. Home equity and home value also appear uninformative of treatment likelihood for values between \$300K and \$1M. Interestingly,

households with homes worth more than $1M appear very unlikely to be treated, presumably because they have sufficient levels of income or non-housing wealth.

2.2 Measuring treatment

While the Survey of Consumer Finances has rich microdata on the determinants of investor accreditation status, it includes only limited information about respondent geography. Since our empirical strategy relies on geographic variation in the intensity of the Dodd–Frank-induced accreditation standards, we instead rely on the Survey of Income and Program Participation (SIPP). In particular, Wave 10 of the 2008 SIPP panel—conducted between September and December, 2011—included a special topical module with detailed questions about family income, assets, and liabilities that we can use to assess accreditation status. We are unaware of alternative data sources with microdata allowing us to determine geographic variation in accreditation under the pre- and Post-Dodd–Frank rules.

The SIPP is a household-level longitudinal survey covering 79,321 individuals in 34,216 families; our assessment of accreditation status is, therefore, at the family level. It should be noted that the SIPP is several times larger than the SCF, and, therefore, surveys a greater number of potentially accredited and treated households than the SCF despite its role in assessing program participation. The key variables we rely on are monthly earnings (*tpearn_waveavg*), home value (*tpropval*), amount owed on home mortgages (*thhmortg*), and net worth (*thhtnw*). Top-coding of variables means we can observe only an imperfect measure of accreditation and treatment status. Given that the SIPP's design goals include assessment of Americans' participation in income transfer programs, it oversamples lower-income households; given this, we do not rely directly on the *level* of treatment, but only on cross-state variation. In addition, the Census suggests some caution about using the SIPP to generate state-level

estimates.[15] To the degree that our state-level treatment measure is noisy, we would expect our analysis to be biased against finding results.

Accreditation under the *income* standard requires annual income of at least $200,000, or $300,000 if married. Income in the SIPP is reported at the individual level, with top-coding at $12,500 (equivalent to $150,000 per year). For each family, we consider the "reference person" and spouse (if any): if neither has top-coded income we say the family *does not* meet the income standard (97.6% of families); if one is top-coded; it *may* meet the income standard (2.3%); if both are top-coded, it *does* meet the income standard (0.1%).

Accreditation under the *asset* standard requires net worth of at least $1 million. After Dodd–Frank, home equity was no longer included in this calculation. Net worth is reported in the SIPP at the family level, calculated from a number of separately top-coded asset and liability amounts including home value (top-coded at $750,000) and home mortgage debt (top-coded at $420,000). For each family we consider the reported net worth, a calculated net worth excluding home equity, and which variables are top-coded, dividing families into four types based on accreditation under the old (pre-Dodd–Frank) asset standard and new asset standard: Families which

1. *Do not* meet the old standard and *do not* meet the new standard (95.4% of families): Less than $1 million in net worth and a non-top-coded home value.

2. *May* meet the old standard and *may* meet the new standard (0.9%): Less than $1 million in net worth and a top-coded home value; or greater than $1 million in net worth, less than $1 million in net worth excluding home equity, and a top-coded mortgage debt.

[15] Per the Census's SIPP Users' Guide, "2004 and 2008 SIPP Panels can be used to produce state estimates. The survey was designed to produce reliable low-income estimates for the 33 largest states." Therefore, states with larger samples in the supplemental survey are more likely to be representative.

3. *Do* meet the old standard and *do not* meet the new standard (1.6%): Greater than $1 million in net worth, less than $1 million in net worth excluding home equity, and a non-top-coded mortgage debt.

4. *Do* meet the old standard and *do* meet the new standard (2.1%): Greater than $1 million in net worth excluding home equity.

Combining the income standard with the old asset standard, we find that as many as 6.1% of families in the SIPP may have been accredited investors before the Dodd–Frank modification. Potentially treated households—an upper bound on those who were actually treated—are those who do not necessarily meet the income standard, but may have met the old but not the new asset standard (i.e., categories 2 and 3, above). Such families represent 2.4% of all families in the SIPP, similar in magnitude to (weighted) estimates from the SCF. We calculate the analogous fraction of potentially pre-Dodd–Frank-accredited investors who may have lost their accreditation at the state level. This measure is available for all states (and the District of Columbia), except that it is undefined for West Virginia, where the SIPP sample included no potentially accredited investors. Variation in this state-level measure of treatment intensity (Frac), which represents the key source of cross-sectional variation in our analysis, is mapped in Figure 2.

This measure of treatment is not based on changes in household incomes or balance sheets over the sample period, but rather uses the single September–December, 2011 snapshot to assess which households would have qualified as accredited investors under the old and new standards. Our measure of treatment bears little correlation to economic characteristics that might relate to business entry and employment. In Figure 3, we show scatter plots of the estimated state-level fraction treated with each of population, per-capita income, venture capital investment, and house-price appreciation measures for 2010. The highest correlation

is with venture capital investment (0.26).

3 Data

There are no comprehensive data sources covering angel investment or angel-backed firms. (For example, OECD (2011) suggests that only 3% of angel investment in the U.S. is "visible.") We, therefore, rely on data sources that capture the overall entrepreneurial landscape.

The primary data source for our analysis is the U.S. Census. In particular, we use the firm-level Business Dynamics Statistics (BDS) dataset that provides annual information on the number of businesses and jobs by state, year, (initial) firm size, and firm age. The BDS data forms the core of our state–year and state–year–size samples. We also employ the Census's Quarterly Workforce Indicators (QWI) data, which links the Longitudinal Employer-Household Dynamics data with the Quarterly Census of Employment and Wages. From the QWI, we obtain quarterly information on employment and earnings by state, industry, and firm age, which forms the core of our state–quarter–industry sample.

We supplement these data with annual state- and industry-level information from a variety of sources. We obtain information by geography on state populations and incomes from the Bureau of Economic Analysis, housing prices from Federal Finance Housing Agency, venture capital investment amounts from SDC's Venture Xpert, and bank branch data from the FDIC's Summary of Deposits. Industry information includes VC investment amounts from SDC's Venture Xpert, startup capital requirements from the public use microdata sample of the 2007 Survey of Business Owners, concentration from the 2007 Economic Census, and employee educational status from the QWI.

The time period of our study is centered around the modification to the accreditation rules. The state–year and state–year–size samples (from the BDS) cover the years (ending

March 12) 2008 to 2014. The state–quarter–industry sample (from the QWI) covers 2007q2 to 2013q1. (We provide a description of the variable definitions that follow in Appendix Table A1.)

3.1 Variable construction: Outcomes

In the BDS data, firms are categorized by their employment size at the beginning of the period unless they are entrants, in which case the firm is categorized by its ending size. As such, we study entrants and incumbents separately. We identify entrants as the number of firms with an age recorded as zero, which indicates the first year a firm reports employment. For use in the regressions, we normalize the number of entrants in each cell (state×year or state×year×firm size) by the total number of firms in the state at the beginning of the year. We construct the beginning total by subtracting the total number of age-zero firms from and adding the number of firm deaths to the total number of firms. We also examine net job creation (job creation less job destruction) at entrants. Similarly, this figure is normalized by the state–year total of the provided Davis–Haltiwanger–Schuh (DHS) denominator, which is the average of total employment in the current year and its lag.

We define young incumbents as non-entrants five years old or younger and old incumbents as firms older than 5 years. For each group of incumbents, we construct a measure of net job creation. For young incumbents, we also examine job creation and job destruction separately. Each of these measures is normalized by the (cell-level) DHS denominator. Thus, for incumbents, the normalization has the standard interpretation of a percent change in jobs for firms of a particular size, adjusted so that transitory shocks are smoothed. Note that for entrants, the average of lagged employment and ending employment would be half of ending employment, and so we choose a normalization that has the interpretation of the size of the

entrepreneurial sector relative to the economy in the state as a whole.

The QWI contains information on employment and earnings at the state–quarter–age–industry level, as well as information on worker characteristics. As before, we analyze entrants, with the caveat that the finest age category available also includes firms that are one year old. Using these data, we define entrant employment by normalizing the ending level of employment by total initial employment in each state–industry–quarter. We compute the average quarterly wage for entrants in each industry and state by dividing the total quarterly payroll by the average of the number of employees at the beginning and end of the quarter; thus, we analyze entrants' average wages relative to the state–quarter–industry average wage. In addition, we segment the sample to examine differential wage effects by worker age and education.[16]

A necessary condition for the validity of our approach is that the expectation of the error term conditional on the treatment intensity after the policy (After×Frac) is zero. While this assumption cannot formally be tested given the unobservability of the error, we can test for the absence of pre-treatment trends in the outcome variables correlated with treatment intensity. Figure 4 plots average annual firm entry rates within terciles of the state-level treated fraction of accredited investors as a result of Dodd–Frank; pre-treatment trends appear parallel. Though we do not include graphs for each outcome variable in the paper, we perform (unreported) parallel-trends tests for each dependent variable considered in our main analyses on Frac (or Frac and Frac^2) for the pre-Dodd–Frank period. The coefficients on Frac (and its square) are both economically small and statistically insignificant in all cases.

[16]Note that we cannot study new firm creation with these data since the number of employers is available only at the establishment level.

3.2 Variable construction: Controls

We construct a number of variables to control for state-level economic conditions that may affect new business entry and vary over time. Population growth is thought to be important for economic growth, and recent studies have suggested that regional population declines may be responsible for the secular declines in new business formation over the past several decades (Hathaway and Litan, 2014). Following Adelino, Schoar, and Severino (2015), we include the natural logarithm of population (measured in the middle of the year) as a control.

Given the importance of personal wealth for entry into entrepreneurship, we also control for per-capita income. This measure can also capture differential economic fluctuations through the sample period (Adelino, Ma, and Robinson, 2017). We define income per capita log as the natural log of total personal income for the year divided by the midyear population.

We calculate the percentage change in the seasonally adjusted house price index for each state using index values from the first calendar quarter of the year (to align with the BDS data). Not only might housing price changes also serve as a barometer for economic fluctuations, but several studies have documented the importance of housing wealth and the collateral channel more generally for the growth of small businesses (Adelino, Schoar, and Severino, 2015; Schmalz, Sraer, and Thesmar, 2017).

The availability of more organized startup capital may also affect entrepreneurial activity. We, therefore, construct controls for the amount of venture capital allocated in a state in each calendar year. We sum total venture capital disbursements in the Venture Xpert data for U.S. firms where the round date, the firm's location, and the amount of the round are available, i.e., we exclude stages coded as acquisitions, real estate, and other.[17] For use in the regressions, we take the natural log of (one plus) the total venture amount, in thousands.

[17]These data are compiled from legacy downloads.

In addition to using the log of the total amount of VC in a state–year as a control, we also define states as being high or low venture capital states at the time of treatment. Using 2010 measures, we categorize states as high VC if the VC volume was above the median level, and low VC otherwise. We segment states into high and low house-price appreciation using the same approach.

We also segment states according to the availability of small business lending. Numerous studies have documented the importance of banking industry characteristics for the availability of lending to smaller or more opaque firms (see, for example, Stein, 2002; Cetorelli and Strahan, 2006; Berger and Udell, 1995). We proxy for the availability of small business lending using a measure of the presence of small banks, under the assumption that smaller banks are more likely to lend to small business (Berger, Miller, Peterson, Rajan, and Stein, 2005; Berger, Black, Bouwman, and Dlugosz, 2017). Following the approach in Berger and Bouwman (2009), we use the FDIC's Summary of Deposits data to calculate the fraction of bank branches in each state that are from banks with less than $1B in total assets.[18] We define as high-lending states those with an above-median fraction of small bank branches in 2010.

We also construct a number of categorical variables across industries at the 2-digit NAICS level, i.e., sectors. We divide sectors into high or low capital needs based on the amount of startup capital needed as in Adelino, Schoar, and Severino (2015). The source information comes from the public use microdata sample for the 2007 Survey of Business Owners in response to the question about the amount of startup capital needed to start the business. Additionally, we recalculate the total amount of VC disbursements by sector (as of 2010). In order to assign company-level disbursements to a sector, we map 4-digit SIC codes to 6-digit NAICS and aggregate up to the 2-digit level. We also divide sectors according to

[18]This measure does not vary much over time for most states, so we omit it as a control in our regressions, although results throughout are robust to its inclusion.

high or low industry concentration based on the employment share of the largest firms in the sector. We define high industry concentration for sectors above the median share from the top 50 companies. Last, we use the QWI to extract education characteristics by industry at the national level. We define low-skilled industries as those with the highest percentage of workers with less than a high school education.

3.3 Summary statistics

Summary statistics for our various samples are reported in Table 2. In Table 2a, we show the mean, standard deviation, median, and interquartile range for variables by state–year. The mean firm entry rate is 7.42%, with an interquartile range of approximately 6.4% to 8.2%. Our measure of treatment, Frac, ranges from 26.7% to 47.1%, with a cross-sectional average of 36.6%.[19]

Table 2b reports statistics for the state–year-size sample. We suppress reporting of the variables that do not change from the state–year level. Here, the entry rate is exactly one-third of the state–year sample, reflecting the division of the sample into three size groupings of 1–4, 5–9, and 10 or more employees. The job creation rate for entrants averages 0.64% for each state, year, and size grouping, or just under 2% in the aggregate. For both young and old incumbent firms, the cross-sectional average of net job creation is negative. In Table 2c, we report quarterly net job creation rates at entrants for the QWI sample, which segments observations by 2-digit NAICS codes. Here, cross-sectional average net job creation is slightly positive, and entrant wages are 84% of the average wage.[20]

[19]These figures represent raw percentages. The correlation between the Frac measure with and without sampling weights is greater than 97%, and regression coefficients are almost identical across alternative measures.

[20]Studies such as Brown and Medoff (2003) document a wage discount at younger firms.

4 Results

In this section, we present results. Because the measure for the fraction of accredited investors treated is less reliable for smaller states, all reported regressions are weighted by the estimated number of potentially accredited households under the accreditation standards prior to Dodd–Frank in the SIPP. In unreported analysis, we weight by state population or employment with similar results. We first report analysis on entering and incumbent firms using BDS data, followed by analysis using industry information from the QWI.

4.1 Business formation

In Table 3, we present results for the difference-in-differences estimation of new business entry as a function the fraction of accredited investors affected by Dodd–Frank interacted with an indicator variable equal to one for BDS years 2011 and beyond. The unit of observation is a state–year, and the dependent variable is the number of entering firms normalized by the state total in the prior year. Control variables include population, per-capita income, venture capital invested, and the percentage change in house prices, as well as state and year fixed effects. Standard errors are heteroskedasticity-consistent and clustered at the state level. Column 1 presents results for the full sample, and Columns 2 through 7 present estimates for the various median-split subsamples.

In Column 1, the coefficient on the interaction effect (After×Frac) indicates a negative and statistically significant change in the number of new businesses for states more extensively affected by the change in investor accreditation standards. The coefficient estimate of -0.0055 translates to about a 2% reduction in entry at the mean (and median).[21] Control variables

[21]The coefficient estimate of -0.0055 times the mean Frac of 0.366 equals approximately -0.002, or 0.2 percentage points; a 0.2 percentage point decline divided by a 7% to 10% start rate equals 2% to 3%.

have intuitive signs. States with higher per-capita income and greater house-price appreciation see an increase in new businesses. Given that the measure of entry is already normalized by the prior number of businesses in the state, population bears a negative relation.

The implied magnitude of the causal effect—a roughly 2% reduction in new business entry—is both economically meaningful and plausible. For example, the effect is in line with estimated magnitudes of the effect of capital availability from the banking literature. Black and Strahan (2002) estimate that new incorporations per capita increased by as much as 3.8% in response to intrastate bank branching deregulation, and by 7.9% following elimination of interstate banking restrictions.[22]

Our estimates also suggest that the overall fraction of angel-backed companies affected is similar to the share of accredited investors that lost accreditation. Survey evidence suggests that angels fund over 60,000 businesses annually, with as many as 70% of deals in the seed and early stage (Sohl, 2011, 2014). Given the overall number of new employing businesses, this suggests that at least 10% of employing startups may be angel-backed. A 2% decline divided by the 10% in angel-backed starts implies (under) a 20% reduction in angel-backed entrants as a result of a decline in accredited households of similar magnitude.

We next explore to what extent alternative forms of finance for new business entry might substitute for a decline in the availability of angel capital. In Columns 2 through 7, we present estimates from subsamples split at median values of alternative forms of finance for new business entry. Median values for the state are calculated in the year prior to treatment.

Columns 2 and 3 report estimates for high and low-VC states. For high-VC states, the

[22]To the extent that the effect of bank deregulation was stronger for incorporated firms than for employing firms overall, these effects should be scaled down before comparing their magnitudes with our estimate. For example, if bank deregulation *only* affected incorporation, and if half of new employing firms incorporate, Black and Strahan (2002) suggest a 2–4% increase in new business entry. Corporations account for 66% of employer businesses with fewer than 500 employees (per https://www.sba.gov/sites/default/files/FAQ_Sept_2012.pdf); presumably, the proportion for entrants is lower.

coefficient is negative and larger in absolute value than in the full sample regression; for low-VC states, the sign on the coefficient is negative, but is not statistically different from zero. The overall effect on entry therefore appears driven by states with above-median levels of venture capital investment.[23]

Columns 4 and 5 report subsamples for high and low prior year house-price appreciation. Insofar as home values translate into collateral that enables financing for business entry or expansion, we again see a negative and significant coefficient for areas with more alternative finance that is larger in absolute value than for the full sample. There is no measurable effect for areas with lower appreciation.

The segmentation on small business lending availability is similar. In Column 6, for the estimation of the subsample for above-median small bank concentration, the coefficient is negative and of larger magnitude than in the full sample, with a statistically insignificant effect for the states with presumably lower levels of small business lending availability. Overall, it does not appear that these traditional channels to alleviate financial constraints act as substitutes for the decline in capital availability from angels. Nor do more specialized intermediaries like venture capital firms seem to serve the same population of potential entrants.[24] These results suggest that that informal financing networks help to overcome very particular frictions, which makes sense given the high degree of uncertainty and asymmetric information involved in new firm finance.

We verify that an omitted factor correlated with the measured treatment is unlikely to drive our baseline results. When we interact each of our control variables with an indicator for

[23]This sample split is not merely a proxy for state size; we get similar results if we segment states by per-capita VC investment volumes.

[24]Our findings on venture capital being a poor substitute for angel finance is consistent with Hellmann, Schure, and Vo (2017), which finds that angels and venture capitalists tend to substitute for one another in a given firm. Of course, this does not preclude geographic complementarity, as the presence of VC may influence the supply of entrepreneurs and angels (Hellmann and Thiele, 2008).

After, none of the estimated coefficients on these interaction terms are statistically significant, and our result for After×Frac remains. Further, when we examine rolling widows varying the timing of the treatment, none of the placebo coefficients (PlaceboAfter ×Frac) are statistically significant. We further check that our results are not driven by state-level housing market characteristics that are correlated with treatment propensity. We construct placebo treatment measures using data from the 2010 American Community Survey, replacing Frac with the median value of owner-occupied housing or the fraction of such houses with values above $1M, $750K, or $500K. For each of these four price-based placebo treatments, the coefficient on the interaction term with After is statistically insignificant. We also construct placebo treatment measures that replace Frac with changes in each state's house price index (from the Federal Finance Housing Agency) over 2002–07 and 2007–10 to check whether our results could be due to differential run-ups or declines surrounding the financial crisis. These measures also show no relation to firm entry in the post Dodd–Frank period. (Results of these house–price-based placebo tests are reported in Appendix Table A2.)

4.2 Entrants and incumbents by size

Given that angel investors are likely to fund smaller firms on average, and this is particularly true for angels affected by changes to the accreditation rules, we next examine entry and employment changes by firm size. We group firms according to the number of employees in categories of 1 to 4 employees, 5 to 9 employees, and 10 or more employees as the base category.

We explore differential effects on entry and employment at entrants, as captured by age zero firms. We segment previously existing firms into two groups depending on their age to isolate effects on job creation for these groups of firms separately.

4.2.1 Entering firms

We begin by examining business entry by firm size. The unit of observation is now a state, year, and size grouping. The dependent variable is the number of entering firms in each size group for the state and year, again normalized by the state–year total. Recall that the size groupings for entering firms are the ending sizes. The regressions contain the same controls as Table 3 and are augmented with firm-size fixed effects.

Table 4 reports the results. In Column 1, we report a baseline specification for this new unit of observation without any interactions with firm size. As before, the coefficient on After×Frac is negative and significant, with a value of -0.0018. In Column 2, we add the full set of interactions for firm size with the treatment variable Frac and the time indicator After.[25] The coefficient on After×Frac is now insignificant, but the coefficient on the 1 to 4 employee size grouping interacted with After×Frac is -0.0048 and significant at 90% confidence. The coefficient for the grouping of 5 to 9 employees interacted with After×Frac is significant at 99% confidence, with an estimated value of -0.0017. These results indicate that the effects are indeed more pronounced at small firms, with a monotonic pattern moving from the smallest category.

Next, we turn to employment. We expect that forgone entry will result in decreased employment at entrants overall. We report employment effects for entering firms in Columns 3 and 4 of Table 4. The dependent variable is net job creation, normalized by the state–year denominator. As before, we report a baseline specification with firm-size fixed effects but no interactions with firm size in Column 3. We note that there is no overall effect, meaning that

[25]With these data, we could include state-by-year fixed effects to test whether small firms experienced a reduction in entry and employment relative to larger firms controlling for any unobservable factors that vary within the state over time. Because none of our controls vary by firm size, however, the estimated coefficients of interest are identical to those reported. Inference remains qualitatively similar.

the rate of job creation for entrants does not change when all firm sizes are grouped together. Any jobs created or lost by very small firms, i.e., those we expect to be more reliant on angels, are swamped by a much smaller relative change at larger firms. Column 4 demonstrates the effects across firm sizes as a result of differential treatment. Here, the coefficient on After×Frac is positive and significant, indicating the effect for all firms. The coefficient for the small firms interacted with After×Frac is negative and statistically significant. The coefficient estimates for both the 1 to 4 grouping and the 5 to 9 grouping are similar in magnitude to one another, and are similar in absolute value (but of opposite sign) to the overall treatment coefficient. These results indicate that smaller firms, i.e., those likely more dependent on angel finance, contributed less job creation when compared to larger entrants.

4.2.2 Incumbent firms

A decrease in firm entry may have positive repercussions for incumbents, particularly those that would compete with angel-backed firms in the product, labor, or financing markets. We, therefore, analyze employment changes at incumbent firms. Given the level of detail in our data, we might expect these effects to manifest at younger or smaller firms.

We segment incumbent firms by age, with young incumbents being firms ages one to five, and older firms over five years old. In Table 5, we report specifications for job creation at young and old incumbents. The unit of observation remains a state, year, and firm-size grouping. The normalization for the job creation variables differ from before, however. Each unit of observation is normalized by the denominator for its state, year, and size. The variable of interest is After×Frac or, alternatively, its interaction with firm-size groupings. The regressions have the same time-varying controls as reported in Table 3, and contain state, year, and size grouping fixed effects. Columns 1 and 2 report baseline specifications for net job creation in young and old firms. Consistent with prior literature, we see that younger and

smaller firms create more jobs, though there is no statistically significant effect of After×Frac. This makes sense given that we do not necessarily expect angel financed entrants to have a large near-term effect on the economy as a whole.

In Columns 3 and 4, we report estimations for net job creation at young and old incumbents with the full set of interactions for After, Frac, and firm-size grouping. The coefficient on After×Frac remains statistically insignificant, but the interactions with smaller firm sizes are positive and significant in the specification for young incumbents (Column 3). There is no measurable effect for the firm-size interactions for old incumbents (Column 4). These results show that younger, smaller incumbents expand more rapidly in areas disproportionately affected by the decline in angel financing, which affected the rate of entry. In Columns 5 and 6 of Table 5, we see that the effect for young incumbents is driven roughly equally by an increase in job creation and a decrease in job destruction for the smallest firms, and is also driven by both creation and destruction for firms with 5 to 9 employees. While our preferred interpretation is one of decreased competition, insofar as accredited investors may provide capital to firms that are not entrants, the positive effect could be construed as surprising. Importantly, angels were allowed to make follow-on investments in portfolio companies even if an investor lost accreditation status under the new Dodd–Frank rules. Further, firms with a track record are presumably better able to access alternative financing sources.

4.3 Entrants by industry

Our analysis now turns to the state–quarter–industry sample, built from the Census's Quarterly Workforce Indicators (QWI) data. By considering within-industry effects, this sample allows us to more carefully control for changing differences in industry composition across states, as well as to consider heterogeneous effects across industry groupings. For

these regressions, we omit observations for NAICS code 92 (Public Administration) as well as state–industries with fewer than 250 employees.[26] We also require that measures are not "significantly distorted" by the Census in their efforts to preserve confidentiality.

Table 6 presents estimates of the effect of angels' loss of accreditation status on net job creation at entering firms (defined in the QWI as those aged 0–1 years), by industry. The dependent variable is the employment at entering firms in a given state and industry divided by the total number of employees in the state–industry at the beginning of the quarter. In Column 1, we include only the main treatment effect (After×Frac), together with the usual annual state-level controls and fixed effects at the state-, quarter-, and industry-levels. The sign of the coefficient on After×Frac is negative, with a p-value of approximately 11%. Though we cannot segment entrant employment by size as in the BDS data (Table 4), this result suggests that states with a higher treated fraction of angels saw lower employment growth at entering firms after Dodd–Frank when controlling for industry composition.

The following three columns of Table 6 consider heterogeneous effects on employment across industries. In particular, each column allows the treatment effect to differ between industries that lie above or below the median of a characteristic plausibly related to the importance of angel-funded entrants. In each case, the main coefficients of interest are on After×Frac (which shows the treatment effect in industries that lack the characteristic) and Characteristic×After×Frac (which shows the difference in treatment effects between industries with and without the characteristic).

Column 2 of Table 6 reports differential effects for industries more or less favored by VC. The coefficient on After×Frac is negative and statistically significant, whereas the coefficient

[26] In addition, our results are robust to excluding the finance and insurance sector and/or the real estate, rental, and leasing sector, offering assurance that our results are not driven by Dodd–Frank regulatory changes other than those to investor accreditation rules.

on Low-VC×After×Frac is positive and statistically significant, both at 90% confidence. Thus, we see a reduction in employment growth at entrants for states that lost a larger fraction of accredited investors in industries favored by traditional VC. This result again suggests imperfect substitutability between angel and venture capital finance.

In Column 3, we examine effects by levels of industry concentration. The coefficient on After×Frac is negative and statistically significant, and the coefficient on Concentrated industry×After×Frac is positive and statistically significant. We thus observe negative effects on entrant employment in less concentrated industries, with no statistical effects for more highly concentrated industries where the threat of entry is presumably less severe.

Column 4 documents differential effects for industries with varying levels of startup capital requirements. The coefficient on After×Frac is negative and statistically significant, whereas the coefficient on High-cap industry×After×Frac is positive, statistically significant at 90% confidence, and approximately equal in absolute value. Thus, the reduction in employment at entrants resulting from the change to accreditation standards is greater for industries with lower startup capital requirements, consistent with angels driving entrant employment growth in such industries. There is no overall effect for industries facing higher capital requirements.[27] Overall, these results underscore the particular importance of angel finance in competitive sectors with high growth potential and lower cost of entry, as one might expect.

[27]In untabulated results, we replicate the analysis from Table 6, but consider employment growth at incumbent firms aged 2–5 years rather than at entrants. To the degree that entrants compete with incumbents—particularly young ones—in product, finance, and labor markets, we expect that a reduction in angel finance availability will increase young incumbent employment, especially in industries where angel-funded entrants are likely to be important. While the signs on the coefficient estimates are consistent with this hypothesis, none of the coefficients of interest are statistically significant.

4.4 Wages

Our results thus far leave open the question of whether the decline in firm entry and resulting employment effects from the change to investor accreditation rules stem from firms that should have been funded in the first place. Prior literature emphasizes the role of private benefits for entrepreneurs, and many business owners do not strive to grow firm employment (Moskowitz and Vissing-Jørgensen, 2002; Hurst and Pugsley, 2011). While the more pronounced results in geographies and industries with higher levels of venture-capital funding suggest that angel-backed firms may be high quality, a natural question is to what extent these firms have the potential to be economically important. The QWI includes payroll data, allowing us to assess the impact on wages, which may help in determining whether affected workers are likely to be highly skilled.[28]

Entering firms compete for workers (with each other, and in the broader labor market), so we might expect that reduced angel funding—by reducing entry and employment at entrants—would lower relative wages for the entrepreneurial sector. Angel funding means there are more entrants (and potentially better capitalized ones) to bid wages up; we would expect these effects to be strongest for higher-skilled workers who may be more difficult to attract to angel-funded startups. In addition, not all would-be entrants are identical. If a drop in capital availability disproportionately affects startups that would attract higher quality workers, changes in the composition of firms may similarly affect the change in relative wages. Notably, recent work by Babina, Ma, Ouimet, and Zarutskie (2018) shows that almost all of the difference in wages paid by young firms can be explained by time-invariant firm or worker characteristics.

[28]Given the small measured base rate on changes in firm entry, we lack statistical power to detect changes in failure rates: differences in survival between forgone entrants and actual entrants would need to be implausibly large to detect an effect.

Table 7 presents estimates of the effect of angels' loss of accreditation status on the wages at entering firms (defined in the QWI as those aged 0–1 years), by industry. We compute wages both overall and separately across workers with characteristics likely correlated with skill: education and age. The dependent variable is the average wage per employee in the demographic category at entering firms in a given state and industry divided by the average wage per employee for firms in the same industry and state. In column 1 of Panel A, we include only our main treatment effect (After×Frac), together with our usual annual state-level controls and fixed effects at the state-, quarter-, and industry-levels. The negative coefficient on After×Frac, statistically significant at the 5% level, shows that states with a higher treated fraction of potential angels saw entrants pay their employees lower relative wages after the Dodd–Frank rule change. This is consistent with competition among angel-funded entrants bidding wages up as well as a decline in entrant quality as a result of a shock to funding availability.[29]

In columns 2 through 4, we examine relative wages separately for workers with less than a high school education, high school graduates without college degrees, and those with college degrees. We observe no statistical effect for workers with less than a high school education, and a monotonically increasing negative effect for those with high school and college degrees. In columns 5 through 8, we segment the working population by age (18–34, 35–44, 45–54, and 55–64). We observe similar negative wage effects for both groups of workers below age 45, though the coefficient is not statistically significant for those age 35–44. For older workers, effects are more pronounced, with the largest measured effect for the oldest workers. Thus, insofar as human capital accumulates over time and education is an indication of skill, effects are stronger for workers where labor supply elasticity is presumably lower.

[29]Here too, results are robust to excluding the finance and/or real estate sectors.

In Panel B of Table 7, we repeat the analysis of Panel A, but allow the treatment effects to differ between higher and lower-skill industries (as measured by the fraction of employees with less-than-high school education). The larger negative coefficients on After×Frac, and the positive coefficients on Lower-skill industry×After×Frac mean that the treatment effects are stronger in higher-skill industries both overall and across worker demographics; the similar magnitude and opposite signs of these coefficients means that the wage effect is concentrated only in higher-skill industries. Thus, effects are most pronounced for skilled workers in skilled industries, with no effects for industries that do not require skilled workers.

These results show that changing investor accreditation standards may have an effect even on the firms that still enter, whether through capital constraints or reduced competition for workers. The findings are also consistent with the notion that human capital is important in angel-funded firms, such that these firms have a greater chance at being economically important entrants.

5 Conclusion

In this paper, we provide the first causal empirical estimates of the marginal impact of financial angels in the economy. We demonstrate that a larger reduction in the pool of potential angels negatively affects firm entry and reduces employment levels at smaller entrants. Effects are concentrated in states that are more developed in terms of other available financing sources, suggesting that financial angels serve a unique role. The evidence also indicates that investor accreditation rules matter at the margin. It is the reduction in the pool of accredited investors that leads to these observed changes.

We find employment increases at small and young incumbents, either as workers are absorbed or competitive pressures in the product markets are reduced. Further, wages decline

in the entrepreneurial sector, suggesting a combination of reduced competition for workers and a change in the composition of entering firms. These effects are consistent with angels backing high-quality firms, and also demonstrate the importance of angels in the economy beyond the companies that they directly fund.

Our work highlights the importance of policies affecting the informal capital markets. As noted earlier, crowdfunding provisions of the JOBS Act were not final until 2016, after our sample period. It remains an open question to what extent the JOBS Act will relax constraints stemming from investor accreditation rules. While the definition of accredited investor has not changed since Dodd–Frank, there have been recommendations put forth suggesting that the net worth and income standards be raised from current levels, with limitations on investment amounts for individuals below the new thresholds. Though our study cannot speak to the investor-protection benefits of restrictions on angel investing, it suggests that investor accreditation standards do impose very real costs on potential entrants.

References

Adelino, M., S. Ma, and D. T. Robinson, 2017, Firm Age, Investment Opportunities, and Job Creation, *Journal of Finance* 72, 999–1038.

Adelino, M., A. Schoar, and F. Severino, 2015, House Prices, Collateral and Self-Employment, *Journal of Financial Economics* 117, 288–306.

Babina, T., W. Ma, P. Ouimet, and R. Zarutskie, 2018, Entrepreneurial Wages, Working paper, UNC Chapel Hill.

Bauguess, S., R. Gullapalli, and V. Ivanov, 2018, Capital Raising in the U.S.: An Analysis of the Market for Unregistered Securities Offerings, 2009–2017, SEC Division of Economic and Risk Analysis White Paper.

Berger, A. N., L. Black, C. H. S. Bouwman, and J. Dlugosz, 2017, Bank Loan Supply Responses to Federal Reserve Emergency Liquidity Facilities, *Journal of Fixed Income* 32, 1–15.

Berger, A. N., and C. H. S. Bouwman, 2009, Bank Liquidity Creation, *Review of Financial Studies* 22, 3779–3837.

Berger, A. N., N. H. Miller, M. Peterson, R. G. Rajan, and J. C. Stein, 2005, Does Function Follow Organizational Form? Evidence From the Lending Practices of Large and Small Banks, *Journal of Financial Economics* 76, 237–269.

Berger, A. N., and G. F. Udell, 1995, Relationship Lending and Lines of Credit in Small Firm Finance, *Journal of Business* 68, 351–381.

Bernstein, S., X. Giroud, and R. Townsend, 2015, The Impact of Venture Capital Monitoring: Evidence From a Natural Experiment, *Journal of Finance* 71, 1591–1622.

Bernstein, S., A. Korteweg, and K. Laws, 2017, Attracting Early-Stage Investors: Evidence From a Randomized Field Experiment, *Journal of Finance* 72, 509–538.

Black, S., and P. Strahan, 2002, Entrepreneurship and Bank Credit Availability, *Journal of Finance* 57, 2807–2833.

Bottazzi, L., M. Da Rin, and T. F. Hellmann, 2008, Who Are the Active Investors?: Evidence From Venture Capital, *Journal of Financial Economics* 89, 488–512.

Brander, J. A., Q. Du, and T. F. Hellmann, 2014, The Effects of Government-Sponsored Venture Capital: International Evidence, *Review of Finance* 19, 571–618.

Brown, C., and J. Medoff, 2003, Firm Age and Wages, *Journal of Labor Economics* 21, 677–698.

Cetorelli, N., and P. E. Strahan, 2006, Finance as a Barrier to Entry: Bank Competition and Industry Structure in Local U.S. Markets, *Journal of Finance* 61, 437–461.

Chemmanur, T., and Z. Chen, 2014, Venture Capitalists Versus Angels: The Dynamics of Private Firm Financing Contracts, *Review of Corporate Finance Studies* 3, 39–86.

Chodorow-Reich, G., 2014, The Employment Effects of Credit Market Disruptions: Firm-level Evidence from the 2008–09 Financial Crisis, *Quarterly Journal of Economics* 129, 1–59.

Cortes, K. R., 2014, Rebuilding After Disaster Strikes: How Local Lenders Aid in the Recovery, Working paper, Federal Resere Bank of Cleveland.

Da Rin, M., T. F. Hellmann, and M. Puri, 2012, A Survey of Venture Capital Research, In *Handbook of the Economics of Finance*. George Constantinides, Milton Harris, and René Stulz (eds) , vol 2, Amsterdam, North Holland.

Decker, R., J. Haltiwanger, R. S. Jarmin, and J. Miranda, 2014, The Role of Entrepreneurship in US Job Creation and Economic Dynamism, *Journal of Economic Perspectives* 28, 3–24.

Denes, M., X. Wang, and T. Xu, 2019, Financing Entrepreneurship: Tax Incentives for Early-Stage Investors, Kenan Institute of Private Enterprise Research Paper 19–31.

Evans, D. S., and B. Jovanovic, 1989, An Estimated Model of Entrepreneurial Choice Under Liquidity Constraints, *Journal of Political Economy* 97, 808–827.

Fehder, D. C., and Y. Hochberg, 2014, Accelerators and the Regional Supply of Venture Capital Investment, Working paper, SSRN.

Fenn, G. W., N. Liang, and S. Prowse, 1997, The Private Equity Market: An Overveiw, *Financial Markets, Institutions & Instruments* 6, 1–106.

Goetz, M. R., and J. C. Gozzi, 2010, Liquidity Shocks, Local Banks, and Economic Activity: Evidence From the 2007–2009 Crisis, Working paper, Goethe University Frankfurt.

Goldfarb, B. D., G. Hoberg, D. Kirsch, and A. J. Triantis, 2013, Are Angels Different? An Analysis of Early Venture Financing, Working paper, Robert H. Smith School.

Gompers, P., and J. Lerner, 2000, Can Corporate Venture Capital Succeed? Organizational structure, Complementaries, and Success, In *Concentrated Ownership*. National Bureau of Economic Research Conference Reports, R. Morck, ed., University of Chicago Press.

Gonzalez-Uribe, J., 2020, Exchanges of Innovation Resources Inside Venture Capital Portfolios, *Journal of Financial Economics* 135, 144–168.

Greenstone, M., A. Mas, and H.-L. Nguyen, 2014, Do Credit Market Shocks Affect the Real Economy? Quasi-Experimental Evidence from the Great Recession and "Normal" Economic Times, Working paper, National Bureau of Economic Research.

Haltiwanger, J., R. S. Jarmin, and J. Miranda, 2013, Who Creates Jobs? Small Versus Large Versus Young, *Review of Economics and Statistics* 95, 347–361.

Hathaway, I., and R. E. Litan, 2014, What's Driving the Decline in the Firm Formation Rate? A Partial Explanation, Economic Studies at Brookings The Brookings Institution.

Hellmann, T. F., 2002, A Theory of Strategic Venture Investing, *Journal of Financial Economics* 64, 285–314.

Hellmann, T. F., L. A. Lindsey, and M. Puri, 2008, Building Relationships Early: Banks in Venture Capital, *Review of Financial Studies* 21, 513–541.

Hellmann, T. F., and M. Puri, 2000, The Interaction Between Product Market and Financing Strategy: The Role of Venture Capital, *Review of Financial Studies* 13, 959–984.

Hellmann, T. F., and M. Puri, 2002, Venture Capital and the Professionalization of Start-up Firms: Empirical Evidence, *Journal of Finance* 57, 169–197.

Hellmann, T. F., P. Schure, and D. Vo, 2017, Angels and Venture Capitalists: Substitutes or Complements?, Working paper, Saïd Business School.

Hellmann, T. F., and V. Thiele, 2008, Friends or Foes? The Interrelationship Between Angel and Venture Capital Markets, *Journal of Financial Economics* 115, 639–653.

Howell, S., and F. Mezzanotti, 2019, Financing Entrepreneurship Through the Tax Code: Angel Investor Tax Credits, Working Paper.

Huang, L., A. Wu, M. J. Lee, J. Bao, M. Hudson, and E. Bolle, 2017, The American Angel, Whitepaper Angel Capital Association.

Hurst, E., and A. Lusardi, 2004, Liquidity Constraints, Household Wealth, and Entrepreneurship, *Journal of Political Economy* 112, 319–347.

Hurst, E., and B. W. Pugsley, 2011, What Do Small Businesses Do?, *Brookings Papers on Economic Activity* (2).

Kerr, W. R., J. Lerner, and A. Schoar, 2011, The Consequences of Entrepreneurial Finance: Evidence from Angel Financings, *Review of Financial Studies* 27, 20–55.

Kerr, W. R., and R. Nanda, 2009, Democratizing Entry: Banking Deregulations, Financing Constraints, and Entrepreneurship, *Journal of Financial Economics* 94, 124–149.

Kerr, W. R., and R. Nanda, 2010, Banking Deregulations, Financing Constraints, and Firm Entry Size, *Journal of the European Economic Association* 8, 582–593.

Kerr, W. R., and R. Nanda, 2011, Financing Constraints and Entrepreneurship, in D. Audretsch, O. Falck, and S. Heblich, eds.: *Handbook on Research on Innovation and Entrepreneurship* (Edward Elgar, Cheltenham).

Krishnan, K., D. K. Nandy, and M. Puri, 2015, Does Financing Spur Small Business Productivity? Evidence from a Natural Experiment, *Review of Financial Studies* 28, 1768–1809.

Lerner, J., 1999, The Government as Venture Capitalist: The Long-Run Effects of the SBIR Program, *Journal of Business* 72, 285–318.

Lerner, J., A. Schoar, S. Sokolinski, and K. Wilson, 2018, The Globalization of Angel Investments: Evidence Across Countries, *Journal of Financial Economics* 127, 1–20.

Lindsey, L. A., 2008, Blurring Firm Boundaries: The Role of Venture Capital in Strategic Alliances, *Journal of Finance* 63, 1137–1168.

Ma, S., 2020, The Life Cycle of Corporate Venture Capital, *Review of Financial Studies* 33, 358–394.

Mollica, M., and L. Zingales, 2007, The Impact of Venture Capital on Innovation and the Creation of New Businesses, Working paper, University of Chicago.

Moskowitz, Tobias, J., and A. Vissing-Jørgensen, 2002, The Returns to Entrepreneurial Investment: A Private Equity Premium Puzzle?, *American Economic Review* 92, 745–778.

OECD, 2011, Financing High-Growth Firms: The Role of Angel Investors, OECD Publishing, http://dx.doi.org/10.1787/9789264118782-en.

Puri, M., and R. Zarutskie, 2012, On the Lifecycle Dynamics of Venture Capital and Nonventure Capital Financed Firms, *Journal of Finance* 67, 2247–2293.

Robb, A. M., and D. T. Robinson, 2014, The Capital Structure Decisions of New Firms, *Review of Financial Studies* 27, 153–179.

Samila, S., and O. Sorenson, 2010, Venture Capital, Entrepreneurship, and Economic Growth, *Review of Economics and Statistics* 93, 338–349.

Schmalz, M. C., D. A. Sraer, and D. Thesmar, 2017, Housing Collateral and Entrepreneurship, *Journal of Finance* 72, 99–132.

Schumpeter, J. A., 1911, *The Theory of Economic Development*. Harvard University Press.

Schumpeter, J. A., 1942, *Socialism, Capitalism and Democracy*. Harper and Brothers.

Shane, S. A., 2009, *Fools Gold? The Truth Behind Angel Investing in America*. Oxford University Press New York.

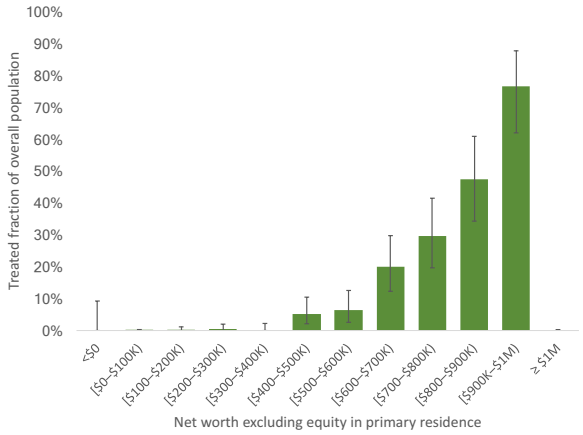
Sohl, J., 2011, The Angel Investor Market in 2010: A Market on the Rebound, Analysis report Center for Venture Research.

Sohl, J., 2014, The Angel Investor Market in 2013: A Return to Seed Investing, Analysis report Center for Venture Research.

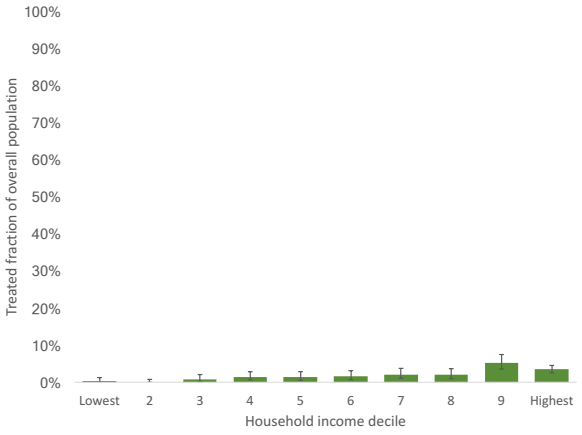
Sørensen, M., 2007, How Smart is Smart Money? An Empirical Two-Sided Matching Model of Venture Capital, *Journal of Finance* 62, 2725–2762.

Stein, J. C., 2002, Information Production and Capital Allocation: Decentralized Versus Hierarchical Firms, *Journal of Finance* 57, 1891–1921.

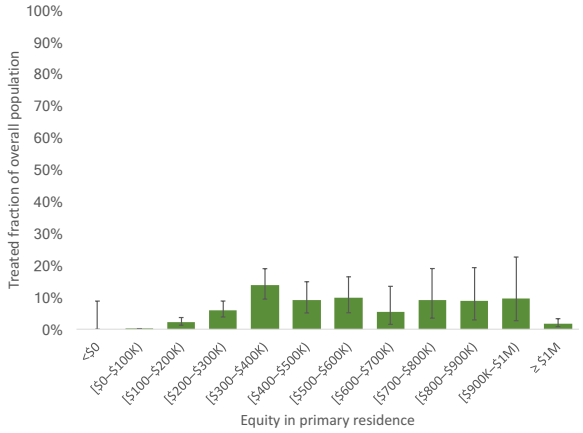
Ueda, M., 2004, Banks Versus Venture Capital: Project Evaluation, Screening, and Expropriation, *Journal of Finance* 59, 601–621.



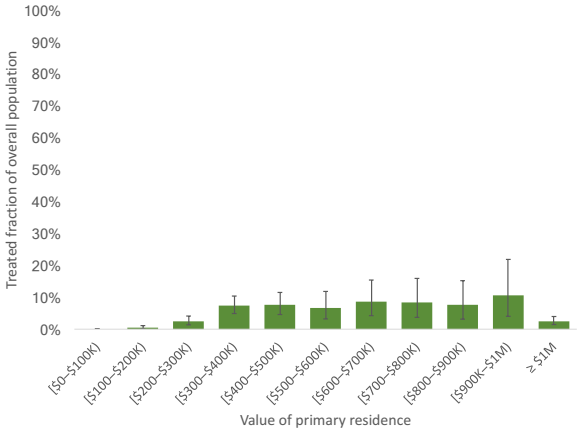
(a) Non-housing wealth



(b) Income



(c) Home equity



(d) Home value

Figure 1. Treated fraction of overall population by household attribute

Each graph plots the estimated fraction of the total population who are treated accredited investors across some household attribute: net worth excluding equity in primary residence, income, equity in primary residence, and value of primary residence. All estimates are calculated using the 2010 Survey of Consumer Finances, and reported with 95% Bernoulli confidence intervals.

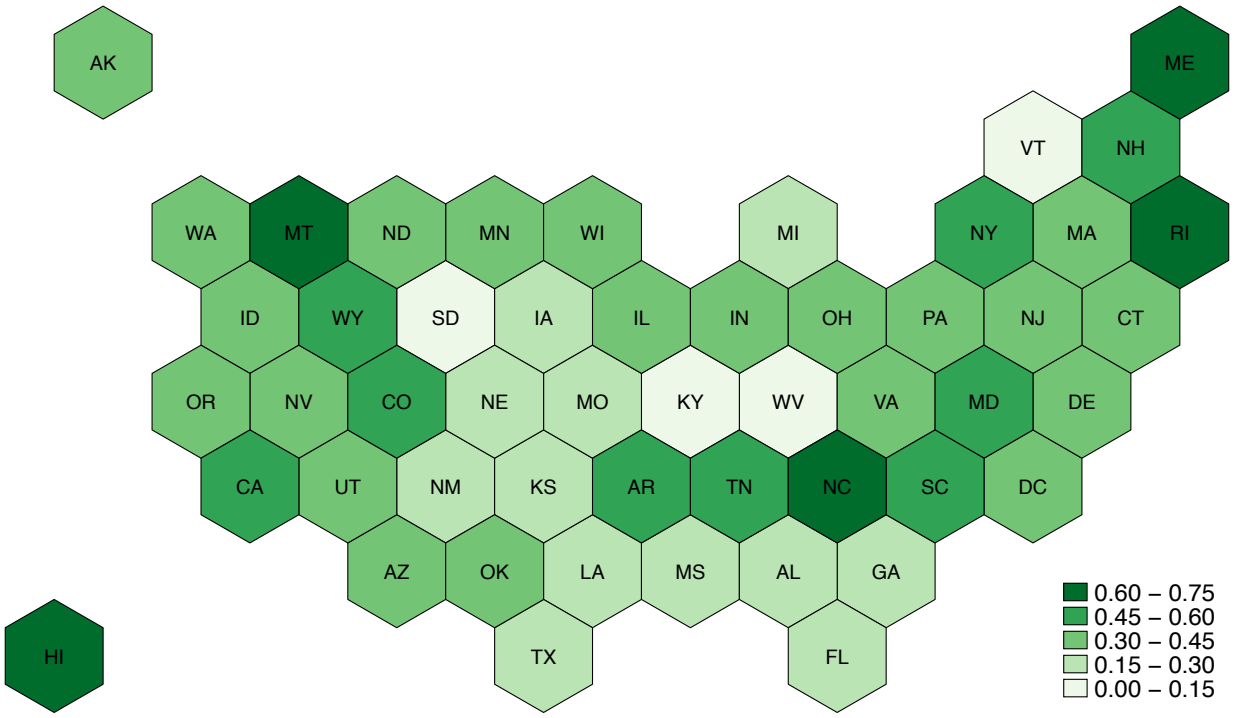
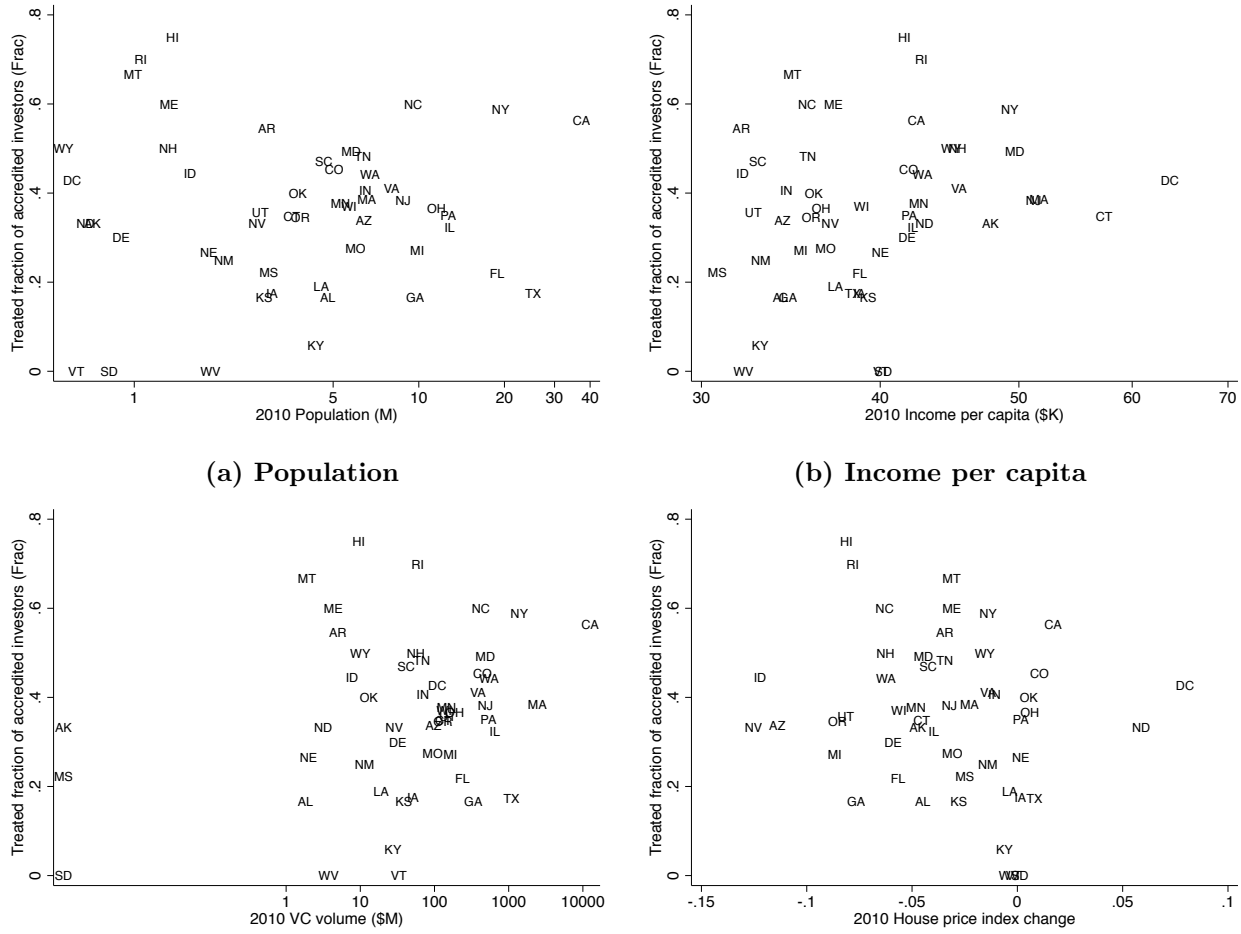


Figure 2. State-level treated fraction of accredited investors

This map shades each state and the District of Columbia according to the treated fraction of accredited investors as a result of Dodd–Frank (Frac). Note that the treatment is undefined for West Virginia.



(a) Population **(b) Income per capita**

(c) Venture capital **(d) House price index change**

Figure 3. Treated fraction of accredited investors and state attributes

Each graph plots the state-level treated fraction of accredited investors against a state attribute: 2010 log population (correlation = 0.01), 2010 log income per capita (0.23), 2010 log venture capital volume (0.26), and the 2010 change in house price index (-0.20). Time-varying analogues of these measures are included as annual state-level controls in our main regressions.

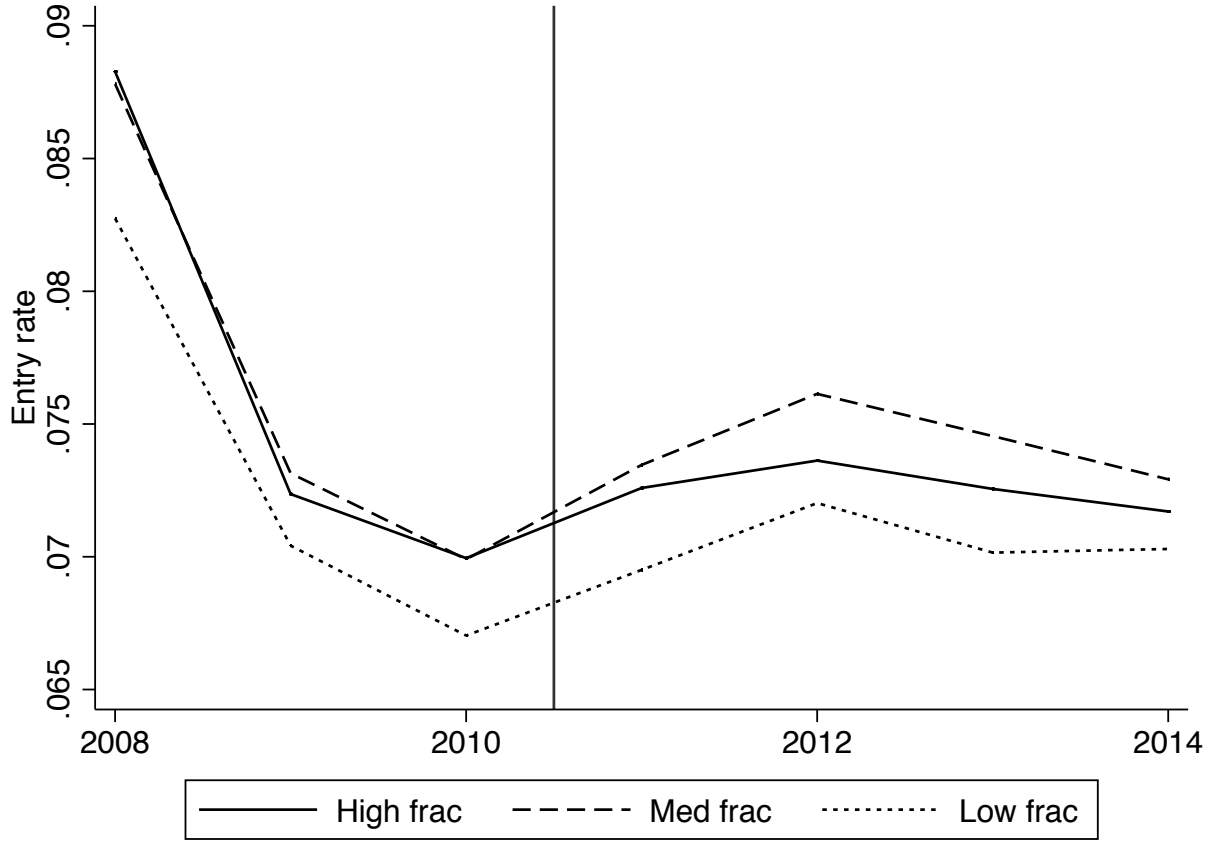


Figure 4. Parallel trends: Firm entry)

This graph plots annual firm entry rates (*entry*, as defined in Table A1) averaged within terciles of the state-level treated fraction of accredited investors (Frac) as a result of after Dodd–Frank. In unreported tests, we regress first-differences of each dependent variable considered in our analyses on Frac (or Frac and its square) for the pre-Dodd–Frank period; the coefficients on Frac (and its square) are both economically small and statistically insignificant in all cases.

Table 1. Household-level summary statistics by accreditation status

This table reports mean (and in square brackets, median) attributes for households in the 2010 Survey of Consumer Finances (SCF) by investor accreditation status, as described in Section 2.1. "Not Accredited" households are those not qualified under the pre-Dodd–Frank standard, "Accredited Untreated" households are those qualified both before and after the Dodd–Frank change, and "Treated" households are those qualified prior to the change but unqualified under the stricter, post-Dodd–Frank standard.

	Not Accredited	Accredited Untreated	Treated
Income ($K)	54	360	119
	[42]	[204]	[111]
Net worth ($K)			
Total	145	4,385	1,234
	[59]	[2,205]	[1,170]
Home equity	59	530	476
	[15]	[380]	[377]
Total excl. home equity	86	3,855	758
	[22]	[1,688]	[801]
Home value ($K)	121	748	601
	[90]	[550]	[500]
Fraction of population (SCF)	89.6%	8.4%	2.0%

Table 2. Summary statistics

This table reports distributional summary statistics for our main variables. Panel A reports summary statistics for the state–year sample (BDS); Panel B reports summary statistics for the state–year-size sample (BDS); Panel C reports summary statistics for the state–quarter–industry sample (QWI). For each variable in each dataset, we report the pooled mean, standard deviation (sd), median (p_{50}), first quartile (p_{25}), third quartile (p_{75}).The last row of each panel reports the total number of observations in the dataset.

(a) State–year sample

	mean	sd	p50	p25	p75
Firm entry rate (%)	7.42	1.36	7.22	6.40	8.19
Frac (%)	36.61	16.55	36.19	26.67	47.06
Population log	15.13	1.04	15.30	14.27	15.72
Income per capita log	10.62	0.16	10.60	10.49	10.71
VC log	10.75	3.14	11.23	9.59	12.72
House price index change (%)	-0.99	6.18	-1.04	-4.40	2.27
Observations	350				

(b) State–year–size sample

	mean	sd	p50	p25	p75
Firm entry rate (%)	2.47	2.26	1.05	0.86	4.68
Net job creation rate (%)					
Entrants	0.64	0.40	0.46	0.33	0.95
Younger incumbents	-3.54	7.11	-3.62	-8.20	1.47
Older incumbents	-1.42	3.65	-0.84	-3.97	1.00
JC (%): Younger incumbents	20.30	6.56	18.35	15.12	26.72
JD (%): Younger incumbents	23.84	3.58	23.51	21.52	25.70
Observations	1050				

(c) State–quarter–industry sample

	mean	sd	p50	p25	p75	Obs
Net job creation rate (%)						
Entrants	4.08	2.65	3.51	2.14	5.43	20869
Relative entrant wages (%)	84.20	15.39	83.93	75.59	91.85	17876

Table 3. Firm entry

This table reports coefficients, standard errors, and statistical significance from WLS regressions of the firm entry rate, estimated using the state–year sample described in Section 3. The key explanatory variable is the state-level treated fraction of accredited investors after Dodd–Frank (After×Frac); regressions also include annual state-level control variables (log population, log income per capita, log venture capital volume, and house price index change), state-fixed effects, and year-fixed effects. Variables are calculated as described in Section 3. Columns 2–7 are estimated separately on states with above- and below-median 2010 venture capital volume (columns 2–3), 2009–10 change in house prices (4–5), and 2010 fraction of branches at banks with less than $1B in assets (6–7). Standard errors clustered by state are reported in parentheses. Significance levels are indicated by *, **, *** for 10%, 5%, and 1% respectively.

	(1) Overall	(2) High VC	(3) Low VC	(4) High ΔHPI	(5) Low ΔHPI	(6) High loan	(7) Low loan
After×Frac	-0.00551**	-0.00736**	-0.00280	-0.00950*	0.000996	-0.0101**	-0.00448
	(0.00267)	(0.00328)	(0.00396)	(0.00467)	(0.00466)	(0.00453)	(0.00377)
Population log	-0.0604**	-0.0687**	0.00314	-0.0580	-0.110***	-0.0217	-0.0926**
	(0.0281)	(0.0311)	(0.0540)	(0.0404)	(0.0357)	(0.0335)	(0.0424)
Income per capita log	0.0429**	0.0532**	0.0450*	0.0426*	0.000109	0.0238	0.0424
	(0.0198)	(0.0257)	(0.0243)	(0.0221)	(0.0343)	(0.0288)	(0.0317)
VC log	0.000280	0.0000690	0.000261	0.000518**	0.0000288	0.000317	0.0000867
	(0.000234)	(0.000797)	(0.000189)	(0.000210)	(0.000264)	(0.000238)	(0.000678)
House price index change	0.0163***	0.0155***	0.0222***	0.0225***	0.0151**	0.00846	0.0162***
	(0.00361)	(0.00422)	(0.00651)	(0.00504)	(0.00591)	(0.0143)	(0.00467)
State FE	✓	✓	✓	✓	✓	✓	✓
Annual FE	✓	✓	✓	✓	✓	✓	✓
Observations	350	175	175	175	175	175	175

Table 4. Entry and employment at entrants by size

This table reports coefficients, standard errors, and statistical significance from WLS regressions of the firm entry rate by firm size (columns 1–2) and entering firms' net job creation rate by firm size (3–4), estimated using the state–year–size sample described in Section 3. The key explanatory variable is the state-level treated fraction of accredited investors after Dodd–Frank (After×Frac), and (in columns 2 and 4) its interaction with indicators for firm-size categories. Regressions also include annual state-level control variables (log population, log income per capita, log venture capital volume, and house price index change), state-fixed effects, year-fixed effects, and firm-size indicators (1–4 employees and 5–9 employees; 10+ employees is omitted). Columns 2 and 4 also include interactions of the firm-size indicators with a post-Dodd–Frank indicator and with the state-level treated fraction of accredited investors. Variables are calculated as described in Section 3. Standard errors clustered by state are reported in parentheses. Significance levels are indicated by *, **, *** for 10%, 5%, and 1% respectively.

	(1) Entry	(2) Entry	(3) Net job creation	(4) Net job creation
After×Frac	-0.00184**	0.000306	0.000496	0.00173*
	(0.000836)	(0.000585)	(0.000366)	(0.000967)
1-4	0.0502***	0.0459***	-0.00708***	-0.00893***
	(0.00185)	(0.00603)	(0.000379)	(0.00128)
5-9	0.00110***	-0.000409	-0.00886***	-0.0103***
	(0.000194)	(0.000330)	(0.000410)	(0.00138)
1-4×After		0.00148		0.00167***
		(0.00109)		(0.000304)
5-9×After		0.000867***		0.00158***
		(0.000243)		(0.000341)
1-4×Frac		0.0114		0.00329
		(0.0141)		(0.00296)
5-9×Frac		0.00349***		0.00234
		(0.000985)		(0.00319)
1-4×After×Frac		-0.00476*		-0.00185**
		(0.00264)		(0.000868)
5-9×After×Frac		-0.00167***		-0.00184*
		(0.000543)		(0.000999)
Annual state-level controls	✓	✓	✓	✓
State FE	✓	✓	✓	✓
Annual FE	✓	✓	✓	✓
Observations	1050	1050	1050	1050

Table 5. Employment at incumbents by size

This table reports coefficients, standard errors, and statistical significance from WLS regressions of the rates by firm size of net job creation at young incumbent firms aged 1–5 years (columns 1 and 3), net job creation at older incumbents aged ≥6 years (2 and 4), job creation at young incumbents (5), and job destruction at young incumbents (6). All are estimated using the state–year–size sample described in Section 3. The key explanatory variable is the state-level treated fraction of accredited investors after Dodd–Frank (After×Frac), and (in columns 3–6) its interaction with indicators for firm-size categories. Regressions also include annual state-level control variables (log population, log income per capita, log venture capital volume, and house price index change), state-fixed effects, year-fixed effects, and firm-size indicators (1–4 employees and 5–9 employees; 10+ employees is omitted). Columns 3–6 also include interactions of the firm-size indicators with a post-Dodd–Frank indicator and with the state-level treated fraction of accredited investors. Variables are calculated as described in Section 3. Standard errors clustered by state are reported in parentheses. Significance levels are indicated by *, **, *** for 10%, 5%, and 1% respectively.

	(1) NJC, Young	(2) NJC, Old	(3) NJC, Young	(4) NJC, Old	(5) JC, Young	(6) JD, Young
After×Frac	0.0131	0.00271	-0.0185	-0.00245	0.00435	0.0229
	(0.0148)	(0.0101)	(0.0142)	(0.00598)	(0.00869)	(0.0145)
1-4	0.109***	0.0240***	0.148***	0.0374***	0.154***	0.00636
	(0.00497)	(0.00214)	(0.0150)	(0.00724)	(0.00986)	(0.00916)
5-9	0.0214***	-0.0366***	0.0261***	-0.0365***	0.0347***	0.00862
	(0.00308)	(0.00118)	(0.00945)	(0.00529)	(0.00413)	(0.00767)
1-4×After			-0.0517***	-0.0259***	-0.0283***	0.0235***
			(0.00836)	(0.00755)	(0.00633)	(0.00758)
5-9×After			-0.0189*	-0.00146	-0.00851**	0.0104
			(0.00951)	(0.00588)	(0.00353)	(0.00878)
1-4×Frac			-0.0569	-0.00650	-0.0258	0.0310
			(0.0383)	(0.0175)	(0.0258)	(0.0243)
5-9×Frac			-0.00571	0.00310	0.00389	0.00960
			(0.0220)	(0.0108)	(0.00846)	(0.0181)
1-4×After×Frac			0.0586***	0.0174	0.0293**	-0.0293*
			(0.0182)	(0.0161)	(0.0145)	(0.0164)
5-9×After×Frac			0.0364*	-0.00189	0.0118	-0.0246
			(0.0190)	(0.0123)	(0.00845)	(0.0199)
Annual state-level controls	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓
Annual FE	✓	✓	✓	✓	✓	✓
Observations	1050	1050	1050	1050	1050	1050

Table 6. Employment at entrants by industry

This table reports coefficients, standard errors, and statistical significance from WLS regressions of the net job creation rate at firms aged 0–1 years by industry, estimated using the state–quarter–industry sample described in Section 3. The key explanatory variable is the state-level treated fraction of accredited investors after Dodd–Frank (After×Frac), and (in columns 2–4) its interactions with indicators for various industry characteristics (less funded by venture capital, highly concentrated, requiring high startup capital). Regressions also include annual state-level control variables (log population, log income per capita, log venture capital volume, and house price index change), state-fixed effects, quarter-fixed effects, and industry-fixed effects. Columns 2–4 also include interactions of indicators for various industry characteristics with a post-Dodd–Frank indicator and with the state-level treated fraction of accredited investors. Variables are calculated as described in Section 3. Standard errors clustered by state are reported in parentheses. Significance levels are indicated by *, **, *** for 10%, 5%, and 1% respectively.

	(1)	(2)	(3)	(4)
After×Frac	-0.00671	-0.0106*	-0.0118**	-0.0116**
	(0.00416)	(0.00549)	(0.00563)	(0.00574)
Low-VC industry×After		-0.00267		
		(0.00197)		
Low-VC industry×Frac		-0.0263*		
		(0.0143)		
Low-VC industry×After×Frac		0.00821*		
		(0.00459)		
Concentrated industry×After			-0.00329*	
			(0.00171)	
Concentrated industry×Frac			-0.00997	
			(0.0144)	
Concentrated industry×After×Frac			0.00726**	
			(0.00325)	
High-cap industry×After				-0.00466*
				(0.00248)
High-cap industry×Frac				-0.0229
				(0.0194)
High-cap industry×After×Frac				0.0110*
				(0.00628)
Annual state-level controls	✓	✓	✓	✓
State FE	✓	✓	✓	✓
Quarterly FE	✓	✓	✓	✓
Industry FE	✓	✓	✓	✓
Observations	20869	20869	15768	20869
p-val: $\beta_{\text{Aft}\times\text{Frac}} + \beta_{\ldots\text{industry}\times\text{Aft}\times\text{Frac}} = 0$		0.522	0.355	0.887

Table 7. Wages at entrants

This table reports coefficients, standard errors, and statistical significance from WLS regressions of the ratio of the average wage per worker (of the worker type defining each column) at firms aged 0–1 years to the average wage per worker for workers of the same type at all firms in the same industry, state, and quarter, estimated using the state–quarter–industry sample described in Section 3. The key explanatory variables are the state-level treated fraction of accredited investors after Dodd–Frank (After×Frac), and (in Panel B) its interactions with an indicator for lower-skill industries (those with an above-median fraction of total employees with less than a high school diploma). All regressions also include annual state-level control variables (log population, log income per capita, log venture capital volume, and house price index change), state-fixed effects, quarter-fixed effects, and industry-fixed effects. Panel B also includes interactions of the lower-skill industry indicator with a post-Dodd–Frank indicator and with the state-level treated fraction of accredited investors. Variables are calculated as described in Section 3. Standard errors clustered by state are reported in parentheses. Significance levels are indicated by *, **, *** for 10%, 5%, and 1% respectively.

(a) Worker demographics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Education			Age			
	Overall	<HS	<College	College	18–34	35–44	45–54	55–64
After×Frac	-0.0641**	-0.0370	-0.0516*	-0.0742*	-0.0450**	-0.0437	-0.0972**	-0.138***
	(0.0253)	(0.0222)	(0.0257)	(0.0414)	(0.0193)	(0.0365)	(0.0434)	(0.0458)
Controls (incl. FE)	✓	✓	✓	✓	✓	✓	✓	✓
Observations	17876	8773	15965	11726	14345	12408	11769	8891

(b) Worker demographics and industry skill

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Education			Age			
	Overall	<HS	<College	College	18–34	35–44	45–54	55–64
After×Frac	-0.159**	-0.100*	-0.127*	-0.316**	-0.106**	-0.143	-0.312*	-0.415***
	(0.0661)	(0.0595)	(0.0662)	(0.148)	(0.0517)	(0.128)	(0.157)	(0.132)
Lower-skill industry×After	-0.0513*	-0.0571**	-0.0632**	-0.171**	-0.0283	-0.0475	-0.115*	-0.157***
	(0.0266)	(0.0217)	(0.0248)	(0.0691)	(0.0190)	(0.0523)	(0.0639)	(0.0545)
Lower-skill industry×Frac	-0.143**	-0.0594*	-0.0605	-0.200	-0.0770*	-0.259**	-0.292**	-0.327***
	(0.0576)	(0.0309)	(0.0440)	(0.138)	(0.0394)	(0.122)	(0.126)	(0.108)
Lower-skill industry×After×Frac	0.176**	0.102	0.138*	0.449**	0.109*	0.177	0.380*	0.477**
	(0.0850)	(0.0705)	(0.0796)	(0.223)	(0.0621)	(0.170)	(0.210)	(0.183)
Controls (incl. FE)	✓	✓	✓	✓	✓	✓	✓	✓
Observations	17876	8773	15965	11726	14345	12408	11769	8891
p-val: $\beta_{\text{Aft}\times\text{Frac}} + \beta_{\text{Lower}\times\text{Aft}\times\text{Frac}} = 0$	0.525	0.924	0.610	0.132	0.868	0.479	0.249	0.355

Appendix

Table A1.
Variable definitions

This table describes the variables used in our analysis and explains their construction.

Variable	Description	Calculation
Outcome variables: State–year sample and state–year–size sample		
Entry	Firm entry rate	The number of age-zero firms (perhaps of a given size) in a state, divided by the total number of firms in the state at the beginning of the year (calculated as firms minus firm entry plus firm deaths).
NJC	Net job creation rate	For entering firms (age zero): Net job creation by age-zero firms (perhaps of a given size) in a state divided by the average of the total number of employees in the state at the beginning and end of the year. For incumbent firms (age $\geq$1): Net job creation by firms of a given age (and perhaps of a given size) in a state, divided by the average of the number of employees firms of the same age (and perhaps size) had in the state at the beginning and end of the year.
JC	Job creation rate	Job creation by firms of a given age (and perhaps of a given size) in a state, divided by the average of the number of employees firms of the same age (and perhaps size) had in the state at the beginning and end of the year.
JD	Job destruction rate	Job destruction by firms of a given age (and perhaps of a given size) in a state, divided by the average of the number of employees firms of the same age (and perhaps size) had in the state at the beginning and end of the year.
Outcome variables: State–quarter–industry sample		
NJC	Net job creation rate	For entering firms (age zero and one): Ending employment for infant firms in a state, divided by the number of employees in the state and industry at the beginning of the quarter.

(continued)

Table A1.
Variable definitions (cont.)

Variable	Description	Calculation
Relative entrant wages	Relative wage per worker at entrants	Wage per employee at firms aged 0–1 year of a given industry and demographic category in a state (defined as payroll divided by the average of beginning and ending employment), divided by wage per employee for firms in the same industry in the state.
Main explanatory variables		
After	Post-Dodd–Frank indicator	In the state–year sample and state–year–size sample: Years ending March 12, 2011 and later. In the state–quarter–industry sample: Quarter ending June 30, 2010 and later.
Frac	State-level treated fraction of accredited investors	The number of families in a state who may have been accredited investors under the pre-Dodd–Frank standard but not the post-Dodd–Frank standard, divided by the number who may have been accredited under the pre-Dodd–Frank standard. Calculated using Wave 10 of the 2008 SIPP panel as described in Section 2.2.
1–4/5–9/10+ employees	Number of employees	Categorized using end-of-year employment for entrants and beginning-of-year employment for incumbents.
Annual state-level variables		
Population log	Population	The natural log of population measured in the middle of the prior calendar year, from the Bureau of Economic Analysis.
Income per capita log	Income per capita	The natural log of total personal income in the prior calendar year divided by the midyear population, from the Bureau of Economic Analysis.
VC log	Venture capital volume	The natural log of (one plus) the total venture amount, in thousands, from SDC's Venture Xpert. The round date, the firm's location, and the amount of the round must be available, and we exclude stages coded as acquisitions, real estate, and other.
House price index change	House price change	The annual percentage change in the seasonally-adjusted house price index measured as of the first quarter of the year, from the Federal Finance Housing Agency.

(continued)

Table A1.
Variable definitions (cont.)

Variable	Description	Calculation
State-level variables		
High VC	Above-median 2010 venture capital volume	State has above-median levels of venture capital invested in 2010, calculated from SDC's Venture Xpert. (AZ, CA, CO, CT, DC, FL, GA, IL, IN, MA, MD, MI, MN, MO, NC, NJ, NY, OH, OR, PA, TX, UT, VA, WA, WI.)
High ΔHPI	Above-median 2009–10 house price index change	State has above-median house-price appreciation from 2009 to 2010 based on the percentage change in the Federal Finance Housing Agency house price index. (CA, CO, DC, IA, IN, KS, KY, LA, MA, ME, MO, MS, MT, ND, NE, NM, NY, OH, OK, PA, SD, TX, VA, VT, WY.)
High loan	Above-median 2010 fraction of small-bank branches	State has above-median fraction of bank branches at banks with less than $1B in assets per FDIC Summary of Deposits data. (AL, AR, CO, IA, IL, KS, KY, LA, ME, MN, MO, MS, MT, ND, NE, NH, NM, OK, SC, SD, TN, TX, VT, WI, WY.)
Industry-level variables		
Low-VC industry	Below-median venture capital	Two-digit NAICS industry has below-median venture capital volume, calculated from SDC's Venture Xpert. (NAICS 11, 21, 42, 53, 55, 56, 61, 71, 72.)
Concentrated industry	Above-median employment concentration	Two-digit NAICS industry has above-median fraction of total employment at 50 largest firms, from the 2007 Economic Census. (NAICS 22, 44–45, 48–49, 51, 52, 56, 72. Note: Data is not available for NAICS 11, 21, 23, 31–33, 55.)
High-cap industry	Above-median start-up capital	Two-digit NAICS industry has above-median reported amount of start-up capital, from the 2007 Survey of Business Owners. (NAICS 21, 22, 31–33, 44–45, 51, 53, 55, 71, 72.)
Lower-skill industry	Above-median fraction of low-skill workers	Two-digit NAICS industry has above-median fraction of 2010q2 total industry employees with a less-than-high school education, calculated from the QWI. (NAICS 11, 23, 31–33, 44–45, 48–49, 53, 56, 72, 81.)

Table A2. Firm entry: House-price placebo treatments

This table reports estimates from WLS regressions of the firm entry rate using a variety of "placebo" treatment measures, replicating the analysis of Table 3 (column 1). The sample consists of state–year observations as described in Section 3. Column 1 is our baseline result, where the key explanatory variable is the state-level fraction of households that lost accreditation status after passage of Dodd–Frank (After×Frac). Columns 2–7 replace the Frac measure with state-level house price measures. Columns 2–5 use the 2010 median value of owner-occupied housing units and the fraction of such houses valued above $1M, $750K, and $500K, respectively, from 2010 American Community Survey. Columns 6–7 use the percentage change in the seasonally-adjusted house price index measured as of the first quarter of the year (from the Federal Finance Housing Agency) over 2002–07 and 2007–10. Regressions include annual state-level control variables (log population, log income per capita, log venture capital volume, and house price index change), state-fixed effects, and year-fixed effects. Variables are calculated as described in Section 3. Standard errors clustered by state are reported in parentheses. Significance levels are indicated by *, **, *** for 10%, 5%, and 1% respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Fraction of house prices above			House price index change	
	Frac	Median HP ($K)	$1M	$750K	$500K	2002–07	2007–10
After×Measure	-0.00551**	0.00000321	0.000801	0.000825	0.00128	0.000486	0.00388
	(0.00267)	(0.00000612)	(0.0200)	(0.00961)	(0.00484)	(0.00213)	(0.00584)
Population log	-0.0604**	-0.0575*	-0.0577*	-0.0578*	-0.0580*	-0.0591*	-0.0582*
	(0.0281)	(0.0314)	(0.0313)	(0.0315)	(0.0315)	(0.0340)	(0.0296)
Income per capita log	0.0429**	0.0436**	0.0427**	0.0427**	0.0428**	0.0440**	0.0359*
	(0.0198)	(0.0206)	(0.0205)	(0.0206)	(0.0205)	(0.0188)	(0.0191)
VC log	0.000280	0.000215	0.000223	0.000222	0.000218	0.000224	0.000226
	(0.000234)	(0.000242)	(0.000241)	(0.000242)	(0.000243)	(0.000244)	(0.000240)
House price index change	0.0163***	0.0131***	0.0142***	0.0141***	0.0137***	0.0137**	0.0179**
	(0.00361)	(0.00487)	(0.00527)	(0.00518)	(0.00509)	(0.00552)	(0.00715)
After	-0.0141***	-0.0171***	-0.0163***	-0.0163***	-0.0165***	-0.0166***	-0.0151***
	(0.00338)	(0.00332)	(0.00312)	(0.00312)	(0.00311)	(0.00280)	(0.00305)
State FE	✓	✓	✓	✓	✓	✓	✓
Annual FE	✓	✓	✓	✓	✓	✓	✓
Observations	350	350	350	350	350	350	350