BANK FOR INTERNATIONAL SETTLEMENTS

BIS Working Papers
No 823

Unintended Side Effects: Stress Tests, Entrepreneurship, and Innovation

by Sebastian Doerr

Monetary and Economic Department

November 2019

JEL classification: G20, G21, L26

Keywords: stress tests, small business lending, entrepreneurship, innovation, productivity slowdown

BIS Working Papers are written by members of the Monetary and Economic Department of the Bank for International Settlements, and from time to time by other economists, and are published by the Bank. The papers are on subjects of topical interest and are technical in character. The views expressed in them are those of their authors and not necessarily the views of the BIS.

This publication is available on the BIS website (www.bis.org).

ISSN 1020-0959 (print)
ISSN 1682-7678 (online)

Unintended Side Effects: Stress Tests, Entrepreneurship, and Innovation*

Sebastian Doerr
Bank for International Settlements
sebastian.doerr@bis.org

November 15, 2019

Abstract

Post-crisis stress tests have helped to enhance financial stability and to reduce banks' risk-taking. In order to quantify their overall impact, regulators have turned to evaluating the effects of stress tests on financing and the real economy. Using the U.S. as a laboratory, this paper shows that stress tests have had potentially unintended side effects on entrepreneurship and innovation at young firms. Banks subject to stress tests have strongly cut small business loans secured by home equity, an important source of financing for entrepreneurs. Lower credit supply has led to a relative decline in entrepreneurship during the recovery in counties with higher exposure to stress tested banks. The decline has been steeper in sectors with a higher share of young firms using home equity financing, i.e. where the reduction in credit hit hardest. Counties with higher exposure have also seen a decline in patent applications by young firms. I provide suggestive evidence that the decline in credit has negatively affected labor productivity, reflecting young firms' disproportionate contribution to growth. My results do not imply that stress tests reduce welfare, but highlight a possible trade-off between financial stability and economic dynamism. The effects of stress tests on entrepreneurship should be taken into account when evaluating their effectiveness.

JEL classification: G20, G21, L26.

Keywords: stress tests, small business lending, entrepreneurship, innovation, productivity slowdown

---

*Previous title: 'No Risk, No Growth: The Effects of Stress Testing on Entrepreneurship and Innovation'. I would like to thank Vimal Balasubramaniam, Tobias Berg, Jose Berrospide, Martin Brown, Stijn Claessens, Kristle Romero Cortés, Yuliya Demyanyk, Leonardo Gambacorta, Emilia Garcia-Appendini, Denis Gromb, David Hémous, Catherine Koch, Ralf Meisenzahl, Karsten Müller, Steven Ongena, José-Luis Peydró, Diane Pierret, Kasper Roszbach, Joel Shapiro, Roberto Steri, Javier Suárez, Nikola Tarashev, Joachim Voth, as well as conference and seminar participants at the SFI Research Days, Swiss Winter Conference on Financial Intermediation, Chicago Financial Institutions Conference, European System of Central Banks' Day-Ahead Conference, NYU, HEC Paris, HEC Lausanne, Oxford Saïd Business School, ETH Zurich, University of Amsterdam, Erasmus University Rotterdam, University of Bonn, Georgetown University, Bank for International Settlements, BI Norwegian Business School, Copenhagen Business School, University of Copenhagen, Bocconi University, Federal Reserve Board, University of Zurich, and UBS Switzerland. The views expressed here are those of the authors only, and not necessarily those of the Bank for International Settlements.

1 Introduction

The recent financial crisis led to the worst recession since the Great Depression and showed that banks had taken on excessive risk in the run-up. Preventing a total collapse of the financial system necessitated bail-outs and an unprecedentedly accommodative monetary policy. To avoid a recurrence, policy makers introduced new rules in a bid to increase banks' resilience against shocks and to make the financial system safer. Higher capital requirements have emerged as the most prominent tool: new regulation – such as Basel III or regulatory stress tests – prescribes significantly higher capital ratios for banks. These measures, and stress tests in particular, have achieved their goals of improving risk management and capital planning at individual institutions, as well as promoting the stability of the financial system (Quarles, 2019). However, since many observers have expressed concern about possible unintended side effects, regulators have directed their attention towards evaluating the effects of stress tests on credit supply and the real economy.[1]

This paper argues that U.S. stress tests have reduced the credit supply to entrepreneurs, thus contributing to the anemic recovery of young firms since the crisis. Exploiting a unique feature of geographically disaggregated data on mortgage lending, I establish that banks subject to post-crisis stress tests have strongly curbed lending secured by home equity to small businesses. Since home equity lending is an important source of financing for entrepreneurs, its contraction hurts firm entry and the real economy. Counties in which stress tested banks have a higher pre-crisis market share have seen a relative decline in employment at young firms during the recovery. Importantly, employment declines by more in industries in which a higher share of young firms relies on home equity financing. I also find that counties with a higher market share of stress tested banks see a relative decline in patent applications by young firms, as well as labor productivity – reflecting the outsized importance of young firms for innovation and growth (Haltiwanger, 2015). My paper provides a possible explanation for the persistent decline in entrepreneurship since the crisis.

Following the financial crisis, regulators introduced annual stress tests that monitor the investment portfolios of systemically relevant banks. Their primary objective is to enhance financial stability and reduce risky lending. Stress tests assume an adverse economic scenario, for example rising unemployment and falling house prices. Given banks' asset portfolios, an internal model predicts banks' hypothetical losses. These losses are then projected into minimum capital ratios for banks that would be required to withstand the downturn. If they fail a test, financial institutions must adjust their equity and may not distribute capital over the following quarters. While stress tests are a macroprudential tool designed to bolster financial stability, a common complaint is that they raise the cost of certain loan classes, in particular

[1] See, for example, "Evaluation of the effects of financial regulatory reforms on small and medium-sized enterprise (SME) financing" (Financial Stability Board 2019)

small business lending (Bordo, Cole and Duca, 2019).[2] One argument is that stress tests raise costs for pro-cyclical loan classes through higher implicit risk weights (Cortés, Demyanyk, Li, Loutskina and Strahan, forthcoming). This paper argues that stress tests have unintended negative side effects on small business loans secured by home equity, because they assume a strong decline in residential real estate prices and hence impose high risk weights on business-related home equity loans.

The empirical analysis begins by showing that banks subject to stress tests cut loan supply to small businesses that is secured by home equity. To identify business-related home equity loans in the data, I exploit a unique feature of loan-level data on residential mortgage lending, as provided by the Home Mortgage Disclosure Act (HMDA). In HMDA, if an applicant or co-applicant is not a natural person (i.e. the applicant is a company), banks have to report ethnicity and gender codes as not applicable (Avery, Brevoort and Canner, 2007). By focusing on refinanced loans, this allows me to isolate small business loans secured by home equity. HMDA thus provides detailed yearly data on business-related home equity lending, disaggregated at the bank-county level. To the best of my knowledge, this rich data source on business-related lending has so far been overlooked in the literature.

To estimate the effect of stress tests on loan supply, I compare lending by stress tested to non-stress tested banks during the pre- and post-crisis period in a difference-in-difference setting. Granular data on the bank-county level allow me to isolate loan supply by including time-varying fixed effects at the borrower-county level that control for unobservable county characteristics, for example, loan demand. I find that stress tested banks cut business-related home-equity lending by around 35.9% more than non-stress tested banks during the stress testing period. Including county*time fixed effects slightly decreases effect size, but coefficients remain economically and statistically significant − suggesting that banks cut loan supply.

To further separate demand and supply forces, I compare secured with unsecured small business lending by the same bank. Using balance sheet data at the aggregate bank level, I show that small business lending does not decline uniformly. While stress tested banks cut unsecured loans by an (insignificant) 12% relative to non-stress tested banks, they reduce small business loans secured by real estate collateral by more than three times as much. Within-bank comparisons allow me to control for time-varying confounding factors at the bank level through bank*time fixed effects. Their inclusion does not materially affect results. I argue that the fall in business-related home-equity lending is an unintended side effect of stress tests due to an

[2]For examples, see "How Dodd-Frank Stole The Recovery By Killing Small-Business Growth" (Investor's Business Daily), "How the Dodd-Frank Act hurts small businesses" (The Washington Post), and "Dodd-Frank has made banks safer but slowed economy, data show" (Financial Times). The importance of stress tests for lending is in line with a recent survey by the MIT Golub Center, in which senior officers of large banks report that post-crisis financial regulation, and in particular stress tests, are the main determinants of changes in their post-crisis risk management practices (See MIT GCFP and GrantThompson (2017): "The risk management function of the future".)

assumed collapse in house prices, which − although aimed at addressing risk in the residential mortgage market − constitutes an additional cost for secured small business loans.

In a second step, I analyze the consequences of the decline in credit supply for the real economy. For each U.S. county, I define pre-crisis *exposure* to stress tested banks as the share of county deposits held in branches of stress tested banks. Exposure reflects geographic variation in the importance of stress tested banks in local markets across the U.S. I first establish that aggregate business-related home-equity lending declines in exposed counties − in other words, non-stress tested banks do not make up for the contraction in secured small business loans by stress tested banks. Then, using detailed county by industry by age data on firm employment, I show that counties with higher exposure see a significant decline in employment among young firms during the post-crisis period, relative to counties with low exposure.

Young firms are opaque and inherently risky, and often rely on personal assets to secure loans (Steijvers and Voordeckers, 2009). Consequently, I find that the strong contraction in small business lending secured by home equity disproportionately affects industries in which home equity financing is more important. Using the 2007 Survey of Business Owners I compute the industry share of young firms that use home equity or personal assets to start or expand their business.[3] Within industries in the top tercile of home equity dependence, the negative effect of exposure on entrepreneurship is almost three times as large as for industries in the bottom tercile. In home equity-intensive industries, counties at the 75^{th} percentile of exposure see a 0.8% stronger decline in the share of young firm employment, relative to counties at the 25^{th} percentile. For the average county, the share of young firm employment out of total employment declined by 2.1% relative to the pre-crisis period. The employment decline in high-exposure counties hence corresponds to around one-third of the average cross-sectional decline.

The empirical analysis faces various challenges. I argue that stress tests are responsible for the decline in HMDA home equity loans, in line with findings on small business lending by Berrospide and Edge (2019) and Cortés, Demyanyk, Li, Loutskina and Strahan (forthcoming). Since the post-crisis period is riddled with regulatory initiatives and government programs, I undertake additional tests to isolate the role of stress tests. First, I restrict the sample to banks with at least $20bn in assets. Narrowing the control group does not materially affect results. Second, I show that secured and unsecured bank loans follow the same trend prior to the crisis for stress tested banks, and that there is also no differential pre-trend across stress tested and non-stress tested banks. Third, within the sample of stress tested banks I use the quasi-exogenous *capital gap* computed in Cortés, Demyanyk, Li, Loutskina and Strahan (forthcoming) between forecasted and regulatory minimum capital ratio and show that banks facing a smaller capital gap have a significantly lower growth rate of business-related home equity lend-

[3]The share exhibits little variation across survey vintages.

ing from 2012 to 2016. Finally, I compute a measure of county risk and find that the decline in lending by stress tested banks is stronger in riskier counties. This specification allows for the inclusion of bank*time fixed effects, so that unobservable time-varying bank characteristics are not confounding effects. Coefficients remain stable under saturated specifications.

On the county level, the key challenge for identification is to control for unobservable shocks that affect young firms and are correlated with county exposure to stress tested banks. I overcome this issue by including granular fixed effects. County*industry fixed effects exploit variation within the same county-industry pair over time and control for unobservable and time-invariant county and industry heterogeneity. County*time fixed effects allow shocks to affect each county at each point in time heterogeneously. Thereby I control for unobservable time-varying county fundamentals (such as house prices or unemployment). In addition to local shocks, industry-specific changes, for example the secular decline in manufacturing or changes in export competition, could affect young firms. Inclusion of industry*time fixed effects mitigates these concerns. Essentially, I am comparing employment at young firms in the same county and same industry for different levels of exposure and home equity dependence, exploiting only the within variation of each county-industry pair (Jiménez, Ongena, Peydró and Saurina, 2014). Estimated coefficients remain significant after adding the battery of fixed effects; if anything, effect size increases, which suggests that stress tested banks serve counties with stronger unobservables during the recovery.

In addition, I use an instrumental variable (IV) approach that predicts county exposure to stress tested banks with a gravity model of bank expansion, combined with staggered state level deregulation of interstate banking from 1994 to 2005. The instrument addresses the issue that banks' distribution of deposits could reflect strategic choices. Building on a large literature highlighting the importance of distance in banking (Degryse and Ongena, 2005; Agarwal and Hauswald, 2010), in a first step I predict banks' geographic distribution of deposits across counties. I employ a gravity model based on the distance between banks' headquarters and branch counties, as well as relative market size (Goetz, Laeven and Levine, 2013, 2016). Since the gravity model disregards state level policies on banking deregulation, in a second step I re-scale predicted deposits with an index of staggered interstate banking deregulation (Rice and Strahan, 2010). Re-scaling takes into account that, to different degrees, states have restricted out-of-state banks from entering. To the extent that distance and deregulation prior to 2005 are plausibly exogenous to county characteristics during the recovery, I use exposure based on predicted and deregulation-adjusted deposits as an instrument for observed exposure. IV regressions confirm OLS estimates and show that higher exposure significantly reduces employment at young firms. IV coefficients are similar in terms of sign and significance to OLS, but larger in magnitude. The increase in magnitude is in line with fixed effects regressions if the instrument addresses the issues that stress tested banks select into counties with stronger (unobservable) fundamentals.

In further robustness checks on the county level, I show that neither the pre-crisis housing boom, nor the decline of house prices during the recession, nor variation in home ownership across counties explain the results. As results are robust to factors related to the housing market, this rules out demand-driven explanations based on the collateral channel. Furthermore, when I control for industry risk or account for differences in the sensitivity of young and old firms to common shocks through fixed effects, results remain close to their baseline values. This suggests that any changes in risk among borrowers are not responsible for my findings. Likewise, controlling for the share of the four largest banks or restricting the sample to counties with no top-four presence does not materially affect results. Finally, I compute exposure based on CRA loan data instead of deposit data. Results are similar in terms of sign, size, and significance.

After establishing that banks cut their loan supply to entrepreneurs, who in turn employ fewer workers, I show that exposure to stress tested banks also negatively affects innovation. Using data on U.S. patents aggregated to the county level, I find that county exposure to stress tested banks has a strong and significant negative effect on innovation at young firms. Over the post-crisis period, counties at the 75^{th} percentile of exposure see a 16% decline in patent applications by young firms, relative to counties at the 25^{th} percentile.[4] There is no significant effect on patenting activity by old firms. The negative effect of exposure to stress tested banks on patenting of young firms increases in magnitude when I account for the quality of innovation by citation-weighting patents; this suggests that more productive firms get less financing.

In light of recent literature establishing that young firms contribute disproportionately to aggregate productivity growth (Haltiwanger, 2015; Alon, Berger, Pugsley and Dent, 2018) and the debate on the post-crisis productivity slowdown (Teulings and Baldwin, 2014; Fernald, 2016), my results raise the question whether regulation exacerbates these macroeconomic trends. I provide suggestive evidence that stress tests have negatively affected labor productivity during the recovery. Similar to findings for young firm employment, wages (a common proxy for labor productivity) decline significantly more in exposed counties and industries with higher reliance on home equity financing. This suggests that stress testing, through its effect on loan supply and entrepreneurship, could have slowed recovery from the recession.

My results do not imply that stress testing is bad for welfare in general. Stress testing seeks to to reduce risk-taking and bolster financial stability. Reducing volatility and the incidence of crises might require a reduction in risky lending to young firms. This paper highlights a possible trade-off between financial stability and growth: post-crisis financial regulation leads to a reallocation of credit away from risky borrowers, and these unintended side effects have reduced dynamism and innovation during the recovery from the Great Recession. Yet, I take no stance on the efficiency or long-run implications of the implemented policy.

[4]Accounting for the importance of patents and weighting by citations increases effect size.

Literature and Contribution The main contribution lies in providing an explanation for the persistent decline in entrepreneurship since the crisis. I thereby highlight a quantitatively important channel through which stress tests affect the real economy: they prevent young firms from getting credit and have unintended negative side effects on innovation and growth.

My paper speaks to two strands of literature. First, it connects to work that analyzes the effects of regulation, in particular stress testing, on loan supply. Several recent papers suggest that stress tests increase implicit capital requirements on riskier loans. Berrospide and Edge (2019) and Bordo, Cole and Duca (2019) show that stress tests reduce banks' C&I and small business lending. Cortés, Demyanyk, Li, Loutskina and Strahan (forthcoming) use banks' capital gap in each stress test, i.e. the distance of forecasted from required minimum capital ratio, and show that within the group of stress tested banks, those with smaller gaps reduce credit supply of and raise interest rates on small business loans. For large borrowers and syndicated loans, Acharya, Berger and Roman (2018) and Pierret and Steri (2018) show that banks act more prudently when subject to stress tests.[5]

Since young firms are inherently risky and require external finance to grow (Steijvers and Voordeckers, 2009; Ivashina and Scharfstein, 2010), my finding that stress tests reduce credit supply to risky borrowers is in line with previous findings. However, relative to the existing literature, this paper makes two main contributions. First, I establish sizeable real effects of regulation, while the results in previous literature on the effects of stress tests on employment or investment are ambiguous. By focusing on the crucial role of home equity lending as a source of financing for young firms, I show that stress tests have an economically meaningful effect on entrepreneurship and innovation.[6] Second, I construct a novel dataset on home equity lending to businesses on the granular bank-county level. A large literature analyses how home equity and the collateral channel matter for businesses.[7] Most papers rely on changes in aggregate house prices to indirectly infer the effect on firms' borrowing against real estate collateral. Disaggregated data on business-related home equity loans at the bank-county level provide complementary direct evidence on lending and help to disentangle demand and supply forces.

[5]For further papers on regulation and bank resolution, see Gambacorta and Mistrulli (2004); Aiyar, Calomiris, Hooley, Korniyenko and Wieladek (2014); Fraisse, Lé and Thesmar (2015); Jiménez, Ongena, Peydró and Saurina (2017); Granja and Leuz (2017); Gropp, Rocholl and Saadi (2017); Celerier, Kick and Ongena (2018); Connolly (2018); Gropp, Mosk, Ongena and Wix (2018); Shapiro and Zeng (2018); De Jonghe, Dewachter, Mulier, Ongena and Schepens (2019).

[6]A related paper is Chen, Hanson and Stein (2017), who document that the four largest U.S. banks contract their small business lending during and after the crisis. They also show that counties with a higher market share see a persistent drop in wage growth, despite the fact that employment rebounds. Bord, Ivashina and Taliaferro (2018) focus on the exposure of large banks the the collapse in real estate prices and show that banks with higher exposure contract small business lending by more. For real effects, they document that there is weaker employment growth among small firms in the post-crisis period. My results provide a complementary explanation: post-crisis financial regulation hurts entrepreneurs and thereby productivity. Note that both papers look at employment among small businesses, while the focus of my paper is on entrepreneurship. While almost all young firms are small, the reverse is not true (Haltiwanger, 2015).

[7]See, for example Chaney, Sraer and Thesmar (2012); Meisenzahl (2014); Adelino, Schoar and Severino (2015); Harding and Rosenthal (2017).

Second, my paper also relates to work on the decline in entrepreneurship and growth since the crisis. Siemer (2016); Moreira (2017) and Bassetto, Cagetti and De Nardi (2015) show that the decline in young firm employment during the Great Recession has lasting effects on growth. They build on work by Decker, Haltiwanger, Jarmin and Miranda (2014, 2016, 2018) that shows a secular decline in business dynamism, and work by Aghion (2017), as well as Haltiwanger (2015); Pugsley, Sedlacek and Sterk (2017); Alon, Berger, Pugsley and Dent (2018) that highlights the importance of creative destruction arising from firm entry and exit.[8] While there is convincing evidence that dynamism and growth have declined, there is little consensus on the underlying forces that are responsible. I provide a supply-side explanation for the anemic recovery from the Great Recession, based on cross-sectional evidence: changes in bank lending hurt start-ups above and beyond the immediate effects during the crisis. The decline in entrepreneurship due to lower credit supply potentially contributed to the post-crisis slowdown in productivity growth.

The paper proceeds as follows. Section 2 explains the main variables and provides descriptive statistics. Section 3 lays out the empirical strategy and reports main regression results for bank and county level regressions. Section 4 investigates how stress tests affect innovation and labor productivity. Section 5 concludes.

2 Data and Descriptive Statistics

This section first provides background information on post-crisis financial regulation and stress testing. It then explains data construction and reports descriptive statistics for the main bank and county variables.

Stress Testing Following the recent financial crisis in the U.S., regulators introduced stress tests for the largest financial institutions. The first stress test was carried out in 2009 under the Supervisory Capital Assessment Program (SCAP) and they became regular exercises under the Comprehensive Capital Analysis and Review (CCAR) in 2011. Their goal is to assess whether banks can withstand adverse shocks, including rising unemployment and falling house prices. Regulators provide banks with three scenarios: *baseline*, *adverse*, and *severely adverse*. Each scenario simulates an increasingly hostile economic environment. For example, the severely adverse scenario in 2014 stress tests assumes a rise in the unemployment rate to 12% and a fall

[8] The post-crisis slowdown in growth led to a debate on whether advanced economies have entered a period of secular stagnation, see Summers (2015); Gordon (2015). For papers on the slowdown in investment and growth, see Fernald (2014); Byrne, Fernald and Reinsdorf (2016); Fernald (2016); Fernald and Wang (2016); Bloom, Jones, Van Reenen and Webb (2017); Gutiérrez and Philippon (2017a,b); Fernald, Hall, Stock and Watson (2017). Note that productivity growth has been in steady decline since the mid-2000s. My results do not provide an explanation for the secular slowdown, but rather for the sharp additional decline since the Great Recession.

in house prices by 35%. Banks have to develop annual capital plans and maintain adequate capital to weather these scenarios. They submit their capital plans to the regulator for review, including planned capital issuance and distributions. Based on internal models, the Federal Reserve decides whether banks have passed or failed tests.[9]

The outcome of the decision is publicly disclosed in the CCAR summary report. If banks fail to meet supervisory criteria, they are not allowed to make any capital distribution in the following quarters. In essence, stress tests are forward-looking capital requirements with the goal of ensuring that banks have sufficient capital and will reduce their risk-taking. In 2009, 19 institutions with assets exceeding $100 billion were part of the stress tests. By 2014, tests included smaller institutions with assets over $50 billion and covered a total of 32 banks.[10] The thresholds were chosen such that stress tests cover the largest and systemically relevant financial institutions in the U.S. banking system. Stress tests are seen as effective in altering banks' lending behavior to reduce risk. For example, both regulators and the senior officers of large banks agree that "a large portion of [why banks implemented risk management practices after the financial crisis] is driven by [. . .] regulations" and that "stress tests were effective, but costly".[11]

However, small businesses and banks have complained that stress tests reduce banks' ability to supply credit to small firms. The Clearinghouse, a banking association and payments company owned by the largest commercial banks, argues that "the Federal Reserve's CCAR stress test is imposing dramatically higher capital requirements on [. . .] small business loans" (Clearinghouse, 2017a). In a similar vein, the National Small Business Association states "lending is much harder than it was before the great recession". There are two main reasons why small business loans are particularly sensitive to stress tests. First, small business employment is pro-cyclical and depends on local demand, which makes it sensitive to changes in aggregate macroeconomic conditions and hence riskier under adverse scenarios. Since banks can loosen their capital requirement by reducing risk in their loan portfolio, small business lending becomes more expensive for stress tested banks (Bordo, Cole and Duca, 2019; Cortés, Demyanyk, Li, Loutskina and Strahan, forthcoming). Second, young and small firms are risky and opaque, often with no credit history. To overcome asymmetric information about borrower quality, banks require collateral (Carpenter and Petersen, 2002; Berger, Espinosa-Vega, Frame and Miller, 2011; Berger and Bouwman, 2013; Berger, Imbierowicz and Rauch, 2016). Stress tests model a strong decline in house prices (collateral values) in addition to declining economic activity (weak local demand). Secured small business lending is thus not only assumed to be inherently pro-cyclical, but subject to both declining demand and falling collateral values,

[9] Since 2013, the Federal Reserve can give an objection, a conditional non-objection, or a non-objection to a bank's capital plans. Stress tests rest on a scenario and model that applies to all banks to a similar extent. What differs is the underlying composition of banks' assets.

[10] The decision to include smaller institutions was made in 2012.

[11] See MIT GCFP and GrantThompson (2017): "The risk management function of the future".

which implies high capital requirements and makes it less profitable for stress tested banks.[12]

Note that the modeled decline in residential real estate prices aims at addressing risk in the residential mortgage market. While bank lending is an important source of financing for young firms, loans to young firms – and especially business-related home equity loans – constitute only a tiny share of overall bank lending. Its impact on aggregate bank risk is therefore negligible; the effect of stress tests on business-related home equity lending likely reflects an unintended side effect, rather than the explicit goal.

Bank Data The bank level analyzes how stress tests affect banks' loan supply to small businesses, in particular when loans are secured by real estate collateral. The Federal Deposit Insurance Corporation (FDIC) provides detailed bank balance sheet data in its Statistics on Depository Institutions (SDI), as well as information on secured and unsecured small business loans. I define small business loans as loans outstanding with an origination amount of $1,000,000 or less. Secured commercial loans are defined as "Nonresidential loans (excluding farm loans) primarily secured by real estate held in domestic offices", and exclude loans for construction purposes. Each bank is assigned a dummy with value one if its bank holding company (BHC) is mandated to undergo stress tests. To control for annual differences in bank characteristics, I collect second quarter data for each year on banks' total assets, Tier 1 capital ratio, non-interest and total income, total investment securities, overhead costs (efficiency ratio), non-performing loans, share of C&I loans, return on assets, interest expense on deposits, and total deposits.

Since SDI provide data on the bank-time level, controlling for loan demand is imperfect. For example, stress tested banks could lend to counties with less overall demand for loans, in which case it is difficult to disentangle the effect of regulation on loan supply from changes in local demand. To analyze the loan supply effects of stress tests on secured commercial loans, I exploit a feature of loan-level data on residential mortgage lending, provided by the Home Mortgage Disclosure Act. HMDA collects home mortgage application data, covering the vast majority of applications and approved loans in the U.S. The data include application outcome, loan amount, and borrower income for each year. Additionally, they contain detailed information on applicant race, gender, and ethnicity. A peculiar characteristic of HMDA data recording is that, if an applicant or co-applicant is not a natural person (i.e. the applicant is a company), banks have to report ethnicity and gender codes as not applicable (Avery, Brevoort and Canner, 2007). This classification rule allows me to identify business-related loans secured by home equity by restricting the sample to refinanced loans for which applicant and/or co-applicant gender, race, and ethnicity are coded as not available.

To better capture loans to small or young businesses, I further exclude all loan observations

[12]Publications by The Clearing House argue that stress testing reduces small business loan growth and imposes up to 45% higher capital requirements on small business loans. (Clearinghouse, 2016, 2017a,b).

with loan amount above $1 million. I then aggregate the loan data to the bank-county-year level, where I only keep bank-county pairs with at least three observations (loans) per year; this eliminates noisy variation arising from insignificant amounts of loans originated locally by a given bank holding company.[13] Further, since HMDA has poor coverage in rural areas, I restrict the sample to borrowers located in Metropolitan Statistical Areas (MSAs). After merging HMDA data with data on bank balance sheets and county characteristics, I end up with a sample of 169,061 bank-county-year observations, in which 3,195 banks serve 2,373 counties. The average year has 11,953 bank-county observations.

[Table 1 about here]

Table 1, panels (a)-(b), report mean, standard deviation (sd), and additional statistics for key bank variables for the full sample from 2002-2016. The share of secured small business loans equals 58% on average, with a large variation of 0.24 as standard deviation. In terms of aggregate sample statistics, stress tested banks cover 65 (58)% of total bank assets (loans), and 35 (31)% of total (secured) small business loans (all values as of 2008).

County Data The county level analysis examines how county exposure to stress tested banks affects entrepreneurship and innovation during the post-crisis period. To calculate county exposure to stress tested banks, I use data from the FDIC's Summary of Deposits (SOD), which provide yearly information on bank deposits in each county. I compute exposure as of 2007 as

$$exposure_{c,07} = \sum_{b=1}^{B} \frac{deposits_{c,b}}{deposits_c} \times \mathbb{1}(stress\ tested_b), \tag{1}$$

where $deposits_{c,b}$ denotes bank b's deposits in county c in year 2007, and $\mathbb{1}(stress\ tested_b)$ is an indicator with value one if bank b belongs to a stress tested bank holding company. High exposure implies that a large share of county deposits is held in the offices of stress tested banks, while low exposure implies that a larger share of deposits is held in offices of non-stress tested banks. $exposure$ ranges from $[0, 1]$.[14]

To shed light on the role of young firms, I use data on the county-industry-year level on end-of-quarter employment by firm age groups, provided by the Quarterly Workforce Indicators (QWI). I follow the literature and define young firms or entrepreneurs as firms aged zero to one (Gourio, Messer and Siemer, 2016; Curtis and Decker, 2018). For each two digit industry in

[13] For more details on variable construction and sample size, see the Data Appendix.

[14] In robustness checks, I define county exposure based on small business CRA lending data. $\mathbb{1}(stress\ tested_b)$ takes on value one if a bank was stress tested at least once after 2009. In unreported regressions I show that the majority of variation in exposure (86% of R^2 in a Shorrocks-Shapely decomposition) stems from banks stress tested between 2009 and 2013. That being said, replacing exposure with either only the pre- or post-2014 group of banks yields a significant negative effect of exposure on young firm employment in the post-crisis period for both groups (unreported).

each county, I use 4^{th} quarter values. QWI is the only publicly available data set that provides information on county employment by firm age.[15] Comprehensive QWI data on firm formation is available from 2002 to 2016.

Yearly patent applications at the county-industry level are provided by PatentsView. 'Industry' refers to a patent's International Patent Classification (IPC).[16] I assign patents to the county of the inventor, and define young firms as assignees (organizations) that filed patents at most five years ago for the first time. The average sample share of patents by young firms out of total county patents equals 21%, which is close to their aggregate share of 25% (Goldschlag and Perlman, 2017). As a proxy for county labor productivity, I use data on average wages on the county-industry-year level, provided by the Quarterly Census of Employment and Wages (QCEW). For each two-digit NAICS industry, I obtain wage data for the 4^{th} quarter of each year. Finally, I sum HMDA data on business-related home equity loans to the county level to construct aggregate county-level secured small business lending.

County controls include log population, the share of black population and share of population older than 65 years, labor force participation rate, unemployment rate, house prices, and per capita income.[17]

As I will show, stress tested banks reduce small business lending to firms that secure loans by real estate collateral. Young firms often use home equity and personal assets to finance expansion of operations (Jensen, Leth-Petersen and Nanda, 2015; Adelino, Schoar and Severino, 2015; Harding and Rosenthal, 2017; Schmalz, Sraer and Thesmar, 2017; Bahaj, Foulis and Pinter, 2018). Hence, firms in industries that rely more on real estate collateral should be harder hit by the contraction in secured lending.[18] To this end, I use the 2007 Public Use

[15]For other papers using QWI data on firm age, see Adelino, Ma and Robinson (2017); Curtis and Decker (2018). Note that the data classifies subsidiaries of existing firms as start-ups whenever they are formed as separate legal entities. "For example, a new McDonalds franchisee opening her first McDonald's location is classified as a start-up, whereas a new location opened by an existing franchisee, or by corporate headquarters, would be an expansion" (Adelino, Ma and Robinson, 2017).

[16]IPCs do not correspond to NAICS industries, so patent industries cannot be classified into high/low home equity dependence.

[17]Data sources (in order): Census Bureau's Population Estimates, BLS LAUS, FHFA House Price Index (HPI), and BEA LAPI.

[18]I build on a large literature that establishes the importance of collateral for firms' access to finance, in particular for young and small firms (see (Hubbard, 1998; Beck, Demirgüç-Kunt, Laeven and Levine, 2008; Berger, Espinosa-Vega, Frame and Miller, 2011; Meisenzahl, 2016; Liberti and Petersen, 2019) and Gan (2007); Chaney, Sraer and Thesmar (2012)). Young firms and start-ups are often opaque and harder to monitor, and have no credit history; hence they are financially constrained To overcome information asymmetry about the quality of borrowers, banks require small firms to pledge collateral (see Jiménez, Salas and Saurina (2006); Benmelech and Bergman (2008); Bolton, Freixas, Gambacorta and Mistrulli (2016); Hollander and Verriest (2016); Stroebel (2016); Degryse, Karapetyan and Karmakar (2017); Prilmeier (2017)). This makes collateral crucial for small and young firms' access to finance (Carpenter and Petersen, 2002). For a recent survey on financing of small business, see Hoffer, Miller and Wille (2017). For example, recent surveys show that insufficient credit history is the second most cited reason for credit denial among small firms (*Small Business Credit Survey 2016*, Federal Reserve (2017)). The Federal Reserve's 2003 Survey of Small Business Finances states that 45% of small business loans are collateralized by real estate.

Survey of Business Owners (SBO), which provides firm-level data on sources of business start-up and expansion capital, broken down by two-digit NAICS industries. For each industry i I compute the average fraction of young firms f that reports using home equity financing or personal assets (*home equity* henceforth) to start or expand their business:

$$\begin{aligned} home\ equity_{i,07} &= \frac{\sum_{f=1}^{F_i} \mathbb{1}(uses\ home\ equity_f)}{\sum_{f=1}^{F_i} 1} \\ &= \%\text{ of firms using home equity in industry i.} \end{aligned} \tag{2}$$

Table 1, panels (c)-(d), provide summary statistics for main county variables across the full sample of 2,644 counties from 2002-2016. Panel (c) reports summary statistics for the county-year level, panel (d) for the county-industry-year level. Average exposure equals 0.36 with standard deviation of 0.24. Across the sample, the average share of young firm employment out of total employment (*share emp Y*) equals 7%. Figure 1 provides details on county exposure, as defined in equation (1). Panel (a) shows its distribution, panel (b) a map of U.S. counties, where darker areas indicate higher exposure. There is significant variation in county exposure across the U.S. as well as within individual states. 805 counties have zero exposure to stress tested banks. With respect to home equity, in the average industry, 17.6% of young firms use home equity, with a standard deviation of 4%. Note that the measure potentially underestimates the importance of home equity as business owners often provide personal guarantees implicitly using their private residence as collateral.

[Figure 1 about here]

3 Empirical Strategy and Results

This section lays out the empirical strategy and reports main results for the bank and county level. The empirical argument follows three steps, visualized in Table 10. First, I establish that stress tested banks reduce lending to small businesses secured by home equity, building on the fact that stress tests increase banks' cost of lending to small firms (Acharya, Berger and Roman, 2018; Cortés, Demyanyk, Li, Loutskina and Strahan, forthcoming). In a second step, I show that the reduction in credit supply to small businesses hurts entrepreneurs: counties with higher exposure to stress tested banks see a weaker recovery of employment at young firms, because entrepreneurs lack credit.[19]

[19] This step build on the assumptions that a) almost all young firms are small; b) young firms rely on bank financing and collateral; and c) substituting across banks or into other forms of lending is subject to frictions. These assumptions are well-established in the literature: more than 90% of start-ups have fewer than 20 employees (Haltiwanger, 2015). Carpenter and Petersen (2002) show that small firms are financially

3.1 Stress Tests and Home Equity Lending

I first establish on the bank-county-year level that stress tested banks reduce small business lending secured by home equity financing (henceforth secured small business lending) by more than non-stress tested banks. To estimate how stress tested banks adjust their lending relative to other banks during the recovery, I run the following panel specification from 2002 to 2016:

$$ln(amt)_{b,c,t} = \gamma^{hmda}\ stress\ tested_{b,t} + controls_{b,t-1} + \theta_{b,c} + \tau_{c,t} + \epsilon_{b,t}, \tag{3}$$

where $ln(amt)_{b,c,t}$ denotes log volume of HMDA home equity loans to small businesses by bank b to county c in year t. *stress tested* is a dummy with value one for each year a bank is undergoing stress tests, and zero otherwise. For example, banks that were stress tested from 2009 onward get value one for each stress tests on from 2009, while banks that were added to the group of stress tested banks by 2014 get value zero for all years prior to 2014, and value one from then on. Bank controls include log of total assets, return on assets, deposits to total assets, Tier 1 capital ratio, overhead costs, share of non-performing loans in total loans, the share of non-interest income, C&I loans over total loans, and bank liquidity (cash plus marketable securities over assets). All controls are lagged by one period to avoid endogeneity. Standard errors are clustered on the bank holding company (BHC) level to account for serial correlation. Each regression includes bank*county ($\theta_{b,c}$) fixed effects and exploits variation within each bank-county combination, relative to its average. This gives equation (3) an interpretation in terms of changes: $\gamma^{hmda} < 0$ indicates that stress tested banks reduce lending by more than non-stress tested banks, relative to their respective pre-crisis trends.[20] I also include time (τ_t) fixed effects to account for common shocks.

Coefficient γ^{hmda} in regression equation (3) reflects changes in banks' loan supply, but also loan demand. In counties where stress tested banks extend more loans small firms could demand less credit for reasons unrelated to stress testing. For example, a natural disaster or changes in zoning laws could reduce loan demand and lending, irrespective of bank performance. Disaggregated bank-county data allow me to include county*time fixed effects ($\tau_{c,t}$) that absorb any unobserved changes in borrower characteristics across counties. In other words, with county*time fixed effects I compare lending by bank b to the same county. Under the assumption that small businesses depend on local demand, including county*time fixed effects isolates changes in loan supply. Comparing coefficients with and without county fixed effects thus provides insights on magnitude and direction of the bias arising from imperfectly

constrained, and Steijvers and Voordeckers (2009) report that 53% of loans granted to small firms use personal collateral, that the share is even higher for young and risky firms, and that young firms have limited outside financing options. Bolton et al. (2016) show the importance of relationship lending for small firms.

[20] A specification in levels with fixed effects is also better suited to capture the cumulative effect of stress tests over a span of multiple years, rather than their immediate impact on growth in a given year.

controlling for county characteristics.

[Table 2 about here]

Table 2 shows that stress tested banks reduce their supply of secured small business credit. Column (1) shows that stress tested banks reduce business-related home equity lending by 35.9% more during the stress testing period than non-stress tested banks. Effects are conditional on bank controls, bank*county fixed effects, and year fixed effects. To isolate changes in credit supply, column (2) introduces county*time fixed effects. The coefficient of interest keeps sign and significance, which suggests that stress tested banks reduce their loan *supply*. The slight decrease in coefficient size suggests that stress tested banks lend to counties with generally weaker loan demand during the post crisis period. In column (2), stress tested banks reduce small business lending by 26.7% more than non-stress tested banks.

HMDA data cover a subset of banks' total small business lending secured by real estate collateral. It also covers only a subset of lenders – those reporting their HMDA filings. To see whether the decline in secured business loans also holds for aggregate bank lending, I use FDIC SDI data on total secured and unsecured small business lending on the bank year level. Figure 2 provides a first visual inspection of the two series in the pre- and post-crisis period. It shows that both types of loans have evolved in a strikingly different manner since the crisis. Panel (a) shows that, while stress tested banks have seen a decline in total small business lending (grey short-dashed line), by 2016 unsecured small business lending (red dashed line) had recovered, while loans secured by real estate (blue solid line) fell by over 40% relative to their pre-crisis peak. Secured and unsecured lending follow the same trend prior to the recession. Panel (b) shows that the contraction in secured small business lending leads to a strong fall in its share out of total small business lending for stress tested banks (blue solid line). For non-stress tested banks (grey dashed line), the share remains stable and similar to pre-crisis levels. There is no differential trend across groups before 2009.

[Figure 2 about here]

To investigate the pattern in Figure 2 in more detail, I estimate the following regressions:

$$y_{b,t} = \gamma \ \textit{stress tested}_{b,t} + \textit{controls}_{b,t-1} + \theta_b + \tau_t^1 + \epsilon_{b,t} \tag{4}$$

$$\begin{aligned} y_{b,l,t} = {} & \delta_1 \ \textit{stress tested}_{b,t} + \delta_2 \ \textit{secured}_l \\ & + \delta_3 \ \textit{stress tested}_{b,t} \times \textit{secured}_l + \textit{controls}_{b,t-1} + \theta_{b,l} + \tau_t^2 + \epsilon_{b,l,t}. \end{aligned} \tag{5}$$

In equation (4) $y_{b,t}$ denotes the log volume of secured small business loans by bank b in year t. Alternatively, y is the log volume of unsecured small business loans. After including baseline bank controls, as well as bank (θ_b) and time (τ_t^1) fixed effects, coefficient γ indicates whether stress tested banks reduce lending by more than non-stress tested banks, relative to their respective pre-crisis trends. Similar to regression equation (3), including bank fixed effects implies an interpretation in changes.

To tighten identification, equation (5) compares secured and unsecured lending by the *same* bank. To this end, I run regressions on the bank-loan type (l)-year level, where $y_{b,l,t}$ denotes log loan volume of type unsecured or secured by bank b in year t. Dummy *secured* takes on value one for secured small business loans. Coefficient δ_3 hence indicates whether a bank cuts secured loans by more ($\delta_3 < 0$) or less ($\delta_3 > 0$) than unsecured loans. $\theta_{b,l}$ denote bank*loan type fixed effects. τ_t^2 are either year, bank*year, or bank*year and loan type*year fixed effects. Bank*year fixed effects control for all observable and unobservable time-varying bank characteristics. Loan type*year fixed effects additionally control for forces affecting secured and unsecured loans differentially across all banks. Including the full battery of fixed effects, I compare secured to unsecured lending by the same bank, while holding all observable and unobservable bank characteristics constant, as well as controlling for differential shocks to either loan class over time. In regression equations (4) and (5) standard errors are clustered on the bank level to account for serial correlation.

Columns (3)-(7) in Table 2 shows that the contraction in stress tested banks' small business lending secured by home equity loans also holds on the aggregate bank level. For regression equation (4) in column (3), stress tested banks reduce unsecured small business lending by 12.5% more than non-stress tested banks since the introduction of stress tests in 2009. The coefficient is significant at the 10% level. Column (4) uses log secured business lending as dependent variable and shows a relative decline by 35.8% for stress tested banks, or more than three times as much as its unsecured counterpart. In line with the pattern in Figure 2, the contraction in small business lending of stress tested banks is concentrated in its secured segment. Note that the size of the coefficient in column (4) is close in magnitude to column (1), despite the difference in samples.

Columns (5)-(7) report regression results for regression equation (5). In column (5) stress tested banks cut secured small business lending by significantly more (37.7%) than unsecured lending. Including bank*time and loan type*time fixed effects in columns (6) and (7) does not affect the coefficient in any statistically or economically meaningful way. This is, after controlling for bank- and loan type-specific shocks that vary over time, secured lending declines by around 37% more than unsecured lending of the same bank. Taken together, results in Table 2 suggest that stress tested banks reduce their lending to small businesses, especially if it is secured by collateral (or home equity). The contraction in secured lending is not due to unobservable time-varying bank or county characteristics. I provide further robustness tests in Section 3.3. The following section analyzes the real effects of the contraction in loan supply.

3.2 County Exposure and Entrepreneurship

Job creation by young firms has disappointed since the Great Recession (Foster, Grim and Haltiwanger, 2016). Figure 3 shows that young firms (blue solid line) saw a 30% decline in gross job creation during the recent crisis, similar in magnitude to older firms (black dashed line). Since then, job creation by old firms is almost back to its pre-crisis levels, while young firms have not recovered. This section links the reduction in loan supply to the decline in young firms by showing that county exposure to stress tested banks depresses entrepreneurship. Since young firms have an outsized effect on aggregate innovation and productivity, in a second step I analyze how exposure to stress tested banks affects patent applications and wages.

[Figure 3 about here]

I estimate regressions on the county-year (C-Y) and county-industry-year (C-I-Y) level. On the county-year level, I estimate the following difference-in-difference specification:

$$y_{c,t} = \gamma^c \; exposure_c \times post_t + controls_{c,t-1} + \theta_c + \tau_t + \epsilon_{c,t}, \tag{6}$$

where $y_{c,t}$ is the respective outcome variable in county c in year t. *exposure* denotes county exposure to stress tested banks, as defined in equation (1). *post* is a dummy with value one for the stress-testing years 2009-2016. Baseline county controls include log population, labor force participation rate, unemployment rate, house prices, the share of black population and share of population older than 65 years, as well as log income per capita, all lagged by one period. Standard errors are clustered on the county level. $\gamma^c < 0$ indicates that counties with higher pre-crisis exposure to stress tested banks see a stronger decline in outcome variables than counties with low exposure during the stress testing period.

Based on bank level results, the strong contraction in secured loans should hurt young firms that use real estate collateral particularly hard. I test the hypothesis by estimating the following difference-in-difference-in-difference specification on the county-industry-year level:

$$\begin{aligned} y_{c,i,t} = &\gamma_1 \; exposure_c + \gamma_2 \; post_t + \gamma_3 \; exposure_c \times post_t \\ &+ \gamma_4 \; home\ equity_i + \gamma_5 \; exposure_c \times home\ equity_i + \gamma_6 \; home\ equity_i \times post_t \\ &+ \gamma_7 \; exposure_c \times home\ equity_i \times post_t + \theta_{c,i} + \tau^1_{c,t} + \tau^2_{i,t} + \epsilon_{c,i,t}, \end{aligned} \tag{7}$$

where $y_{c,i,t}$ is either log employment, the share out of total employment of firms aged zero to one, or log wages in county c and industry i in year t. *home equity*$_i$ denotes the share of young firms in industry i that uses home equity financing to expand operations, as defined in equation (2). I include county-industry fixed effects ($\theta_{c,i}$), which absorb coefficients γ_1, γ_4, and γ_5 and exploit *within* variation of each county-industry cell. To control for time-varying unobservables I include time fixed effects at the yearly level (τ_t, absorbs γ_2) or at the county*time and industry*time level ($\tau^1_{c,t}, \tau^2_{i,t}$, absorb γ_2 and γ_3). The inclusion of county*industry fixed effects, combined with a dependent variable in logs, implies an interpretation in changes. The coefficient of interest (γ_7) indicates whether counties with higher exposure see a stronger decline in the importance of young firms or wages (relative to counties with low exposure) in industries that rely more on home equity financing. If so, then $\gamma_7 < 0$.

Identification – fixed effects The underlying assumption in regression equations (6) and (7) is that counties with higher exposure see a relative decline in young firms and innovation because stress tested banks *supply* less credit to young firms. To ensure that *exposure* reflects loan supply effects, I need to control for confounding unobservable shocks that affect employment of young firms. Regressions on the bank-county-year level allow me to isolate the relative contribution of loan supply to small business lending by controlling for county-specific loan demand. However, unobservable county or industry characteristics could still lead to a bias in county level regressions.

The key identification challenge is thus to control for characteristics that affect young firms beyond the change in credit supply by stress tested banks. I overcome this issue in regression equation (7) by including granular fixed effects. First, county*industry fixed effects ($\theta_{c,i}$) exploit variation within the same county-industry combination over time and control for unobservable and time-invariant county and industry heterogeneity (for example location or sensitivity to the business cycle), as well as for unobservable time-invariant characteristics at the county-industry level, such as the importance of an industry within a county. Second, county*time fixed effects ($\tau^1_{c,t}$) allow shocks to affect each county at each point in time heterogeneously. Thereby I control for unobservable time-varying county fundamentals (such as house prices, unemployment, and other local characteristics) to identify credit supply. Third, industry*time fixed effects ($\tau^2_{i,t}$) absorb common shocks to two-digit industries that vary over time, for example the secular decline in manufacturing. Essentially, I am comparing employment at young firms in the same county and same industry for different levels of exposure, exploiting only the within variation of each county-industry pair (Jiménez, Ongena, Peydró and Saurina, 2014). After absorbing changes in local and industry demand, the remaining variation reflects the consequences of changes in loan supply.

Identification – gravity model and deregulation Identification of the effects of stress tests on entrepreneurship and innovation rests on geographical variation in exposure across counties. Exposure is constructed based on bank deposits in a given county, but banks' deposit share in a county is potentially endogenous to unobservable county characteristics – for example, if stress tested banks select into counties with a weaker recovery. While granular fixed effects on the county*industry and county*year level already address concerns about selection and unobservables, I additionally instrument local bank deposits through a gravity model of bank expansion (Goetz, Laeven and Levine, 2013, 2016), combined with an index of interstate banking deregulation as developed in Rice and Strahan (2010).[21] Intuitively, gravity models predict that distance and market size determine the degree of activity in a market. In the present context, a New York-based bank is more likely to have deposits in a county in nearby Pennsylvania than in Texas (given similar market size), and (given same distance) in a more populous county. I estimate the following equation:

$$deposit\ share_{b,B,c,t} = \gamma_1\ ln(distance_{b,c}) + \gamma_2\ ln\left(\frac{population_{c,t}}{population_{B,t}}\right) + \epsilon_{b,B,c,t}, \qquad (8)$$

where b denotes bank, B bank headquarter county, and c the destination (branch) county. The gravity model predicts that $\gamma_1 > 0, \gamma_2 > 0$. Following Goetz, Laeven and Levine (2016) I use a fractional logit model to estimate equation (8) for year 2007. I then predict the deposit share for each bank-county combination based on distance and market size. I set negative predicted values equal zero.

Table 3 shows results for the 'zero' stage regression. Column (1) uses a fractional logit model and shows a strong and significant negative effect of distance on banks' deposit share in a given county. Market size enters positively, suggesting that banks hold a higher share of deposits in larger markets. Columns (2)-(6) run OLS regressions and add fixed effects to check whether effects are sensitive to unobservable home (bank headquarter county) or host (bank branch county) market characteristics.

[Table 3 about here]

Column (2) shows that for distance results are similar in OLS to logit regressions. Column (3) adds host county fixed effects, column (4) adds home county fixed effects. Accounting for unobservable characteristics in either location does not materially affect the coefficient of interest. Column (5) goes one step further and includes home and host county fixed effects, as well as home state*host county fixed effects. It thus compares deposit shares by banks located in the same state lending to the same county. For example, it exploits variation only in the distance across banks headquartered in California that lend to Kings County, NY. The stability of coefficients suggests that the effect of distance on local deposits is orthogonal to local unobservable county characteristics, i.e. not due to economic factors in home or host markets.

While headquarter-branch distance is reasonably exogenous to the local recovery from the crisis, the gravity model does not take into account that states impose restrictions on entry

[21] For a detailed discussion, see Goetz, Laeven and Levine (2016). A large literature establishes that in banking distance matters (Degryse and Ongena, 2005; Agarwal and Hauswald, 2010). Goetz, Laeven and Levine (2013) show that banks 'are more likely to expand into geographically closer markets than into more distant ones'. The underlying argument is informational advantages and lower costs.

by out-of-state banks. Rice and Strahan (2010) show that even after de jure deregulation following the Interstate Banking and Branching Efficiency Act (IBBEA) in 1994, most states use different combinations of policy tools to protect domestic banks from outside competition. The regulation of local banking markets took one or more of the following forms:

a) minimum age of the targeted bank (5 years, 3 years or less)
b) de-novo branching without an explicit agreement by state authorities
c) acquisition of individual branches without acquiring the entire bank
d) state-wide deposit cap on the total amount of state-wide deposits controlled by a single bank or bank holding company.

Over time, 43 states relaxed protection of their local banking markets. Omitting the degree of local banking regulation will thus lead to a bias in predicted deposit shares. Given similar distance from headquarters, equation (8) assigns counties A and B the same deposit share, even if county A prohibits out-of-state banks from opening local branches.

For each state, I first construct a yearly index that ranges from 0 to 4 to capture each dimension of state level branching restrictions, similar to Celerier and Matray (2016). States with a value of zero regulate their banking sector in all four dimensions, states with a value of four are fully deregulated. I then define the state level variable $deregulation_s$ as the cumulative index for each state s from 1994 to 2007:

$$deregulation_s = \sum_{t=1994}^{2007} index_{s,t}. \tag{9}$$

For example, Alabama increased its index from 0 to 1 in 1997, so $deregulation_{AL} = \sum_{1994}^{1996} 0 + \sum_{1997}^{2007} 1 = 11$. Illinois increased its index from 0 to 1 in 1997 and from 1 to 4 in 2003, so $deregulation_{IL} = \sum_{1994}^{1996} 0 + \sum_{1997}^{2002} 1 + \sum_{2003}^{2007} 4 = 26$. I then re-scale $deregulation_s$ to lie in the range of $[0, 1]$. Since I use the instrument to predict the presence of stress tested banks in a given county, cumulative addition reflects that 'foreign' banks had more time to enter states that deregulated earlier.[22] I then scale predicted deposit shares $\widehat{deposit\ share}_{b,B,c}$ by $deregulation_s$ and construct local instrumented deposits by bank b in county c as $deposits_{c,b}^{IV} = \widehat{deposit\ share}_{b,B,c} \times deregulation_s \times total\ bank\ deposits_b$. If s equals a banks' headquarter state, I set $deregulation_s = 1$, since banks face no restrictions on expanding in their own state. Finally, I compute predicted exposure $exposure_c^{IV}$ according to equation (1), but based on $deposits_{c,b}^{IV}$.

[22] Rice and Strahan (2010) and Celerier and Matray (2016) provide several tests to show that deregulation occurred independently of economic or political considerations that could affect the real economy (for example GDP per capita, unemployment rate, or personal income of low income households). Both studies use deregulation with contemporaneous outcome variables. Since my paper uses deregulation before the crisis to study its effect on dynamism during the recovery, endogeneity issues arising due to the initial timing of deregulation *for the post-crisis period* are less of a concern. The majority of states deregulated from 1995-97, but to different degrees; several states deregulated more than once. Dick (2006) finds that deregulation has translated into a dramatic decrease in the number of regional banks and a strong appreciation of bank density.

Columns (6)-(8) in Table 3 report 'first stage' regressions of actual on predicted exposure, with and without state fixed effects and county controls. Across specifications, there is a strong positive effect of predicted on actual exposure. A higher level of predicted exposure to stress tested banks is positively associated with observed exposure to stress tested banks at the 1% level. Including fixed effects at the state level in column (7) increases R^2 by a factor of three, but the coefficient remains similar in size. Further adding pre-crisis county characteristics does not materially affect results. First-stage results show that the instrument explains the geographic distribution of local bank deposits across counties to a similar extent across and within states and does so largely independent of county characteristics. $exposure_c^{IV}$ hence provides plausibly exogenous variation in county level exposure to stress tested banks.

Results Table 4 shows that higher county exposure is associated with a relative decline in aggregate lending and employment among young firms. Columns (1) and (2) report regressions on the county-year level for regression equation (6), where the dependent variable is log HMDA home equity lending to small businesses, aggregated to the county level. Column (1) shows a negative and highly significant effect of exposure on aggregate lending during the stress testing period. Moving a county from the 25^{th} to the 75^{th} percentile in terms of exposure leads to a relative decline in aggregate credit by $(0.39 \times 0.267 =)$ 10.4%. Column (2) instruments actual with predicted exposure. IV results are in line with OLS regressions: higher exposure to stress tested banks reduces aggregate lending to small businesses that is secured by home equity. My results imply that non-stress tested banks are not able to fully offset the decline in lending by stress tested banks.[23]

[Table 4 about here]

Columns (3)-(7) analyze the effect of stress tests on employment. They report results for regression equation (7) on the county-industry-year (C-I-Y) level, the dependent variable is log employment of young firms. Column (3) shows that moving a county from the 25^{th} to the 75^{th} percentile in terms of exposure leads to a relative decline in employment of young firms by $(0.39 \times 0.045 =)$ 1.8%. Columns (4)-(5) add interaction terms of exposure with the share of firms using home equity in each industry (see regression equation (7)). Column (4) absorbs common shocks through year fixed effects, column (5) adds granular county*time and industry*time fixed effects to account for unobservable county and industry characteristics that

[23]This differs from results on small business lending covered in Community Reinvestment Act data (CRA) from 2012-2017 as in Cortés, Demyanyk, Li, Loutskina and Strahan (forthcoming). However, note that CRA data on small business lending includes secured and unsecured lending. Since bank level regressions in Table 2 show that stress tested banks reduce secured small business lending by more than three times as much than unsecured small business loans, this could explain why other papers find no effect on *aggregate* small business lending.

vary over time. The negative coefficient on the triple interaction term suggests that the decline in employees of young firms is stronger in industries with a higher share of firms using home equity to start or expand operations. Including fixed effects to control for demand effects in column (5) does not change sign or significance of the coefficient of interest (note that fixed effects absorb coefficients on interaction terms between exposure, home equity, and the *post* dummy). However, the coefficient increases in magnitude, which suggests that counties with higher exposure have stronger fundamentals and coefficients in regressions without granular fixed effects are biased towards zero.

Columns (6)-(7) instrument actual with predicted exposure. IV estimates confirm OLS findings: higher exposure to stress tested banks reduces employment among young firms, and the more so in home equity-intensive industries. IV estimates are slightly larger than OLS estimates. In line with fixed effects specifications, this suggests that high-exposure counties have stronger fundamentals. In column (7), moving a county from the 25^{th} to the 75^{th} percentile in terms of exposure leads to a relative decline in young firm employment by 11% in home equity-intensive industries. Table 4 hence shows that the contraction in business-related home equity loans by stress tested banks affects the real economy: there is a decline in aggregate credit and since young firms disproportionately depend on home equity financing, their employment declines. Before investigating the effects of the decline in dynamism on innovation and productivity, I provide additional robustness checks in the following section.

3.3 Robustness

Bank Robustness Table 5 provides further evidence on the effects of stress tests on banks' small business lending on the bank-county-year level. All regressions control for local county demand through county*time fixed effects. Columns (1) and (2) decompose the change in total loan volume into the change in the number (nr) of loans and average loan size (avg). While coefficients on dummy stress tested are negative in both regressions, stress testing only affects the number of loans significantly in column (1). While average loan size decreases, it does so insignificantly and to a smaller extent. Consequently, banks extend fewer loans instead of lending a lower amount for a given set of loans.

Stress tested banks comprise the largest banks with at least 50bn in total assets. To narrow the comparison group, column (2) restricts the control group to banks with at least 20bn in total assets as of 2008. Restricting the sample to larger banks leaves the coefficient of interest almost identical in terms of sign, size, and significance to baseline results, alleviating concerns that bank size alone explains findings. One potential concern regarding data construction of business-related home equity lending based on HMDA is that it includes transactions by developers. While these transactions fell after the crisis and stress test are likely to rate this activity as risky, they do not comprise loans to young and small businesses. To this end, I re-construct the sample of business-related home equity loans, but exclude all loans that were purchased over the previous year, which excludes, for example, 'flipping' activities. Column (4) shows that excluding repeat transactions does not materially affect sign, size, or significance of the coefficient on *stress tested*, despite the fact that the number of observations almost halves.

[Table 5 about here]

Stress tests increase capital requirements on risky loans. To shed light of the role of local risk, I follow Cortés, Demyanyk, Li, Loutskina and Strahan (forthcoming) and define *county risk* as the employment-weighted average industry beta.[24] Columns (5) and (6) show that the reduction in home equity lending to small businesses is particularly strong in riskier counties. While magnitude are similar in both columns, the inclusion of bank*time fixed effects to control for unobservable bank characteristics in column (6) improves the precision of the estimate.[25]

Columns (7)-(11) employ the log change in loan amount as dependent variable.[26] Columns (7) and (8) show that stress tested banks see slower loan growth during the stress testing period, and the more so, the riskier a county. Inclusion of bank*time fixed effects in column (8) does not materially affect results. Note that although R^2 almost doubles when including bank*time fixed effects, the coefficient on the interaction term remains significant and of similar magnitude across specifications. This suggests that the effect of stress testing in risky counties is orthogonal to a wide selection of unobservable and observable bank characteristics (Altonji, Elder and Taber, 2005; Oster, 2017).

As mentioned above, stress tested banks are the largest banks. To tighten identification, columns (9)-(11) restrict the sample to stress tested banks over the time period 2012-2016. Within this group, *stress test exposure* denotes banks' quasi-exogenous capital gap, i.e. the distance of forecasted from required minimum capital ratio (for details, see Cortés, Demyanyk, Li, Loutskina and Strahan (forthcoming)). Column (9) shows that among stress tested banks those that perform relatively worse in stress tests see a strong and significant decline in their growth of home equity lending to small businesses. As I include county*time fixed effects, the contraction in loan growth reflects a decline in loan supply. Columns (10)-(11) interact stress test exposure with county risk. As for the full sample, loan growth declines by most in risky counties. Adding bank*time fixed effects to control for observable and unobservable bank characteristics reduces effect size; yet, the effect of stress test exposure on loan growth remains negative and significant at the 5% level.

[24] Cortés, Demyanyk, Li, Loutskina and Strahan (forthcoming) define industry betas, or *industry risk*, as the long-run sensitivity of industry employment to changes in aggregate employment. Higher values indicate riskier industries; likewise, higher values of county risk imply that a county is more pro-cyclical and hence riskier.

[25] For a one unit increase in county risk, which is close to its average value of 0.89, stress tested banks cut lending by 40.7% (54.4%) more in column (5) ((6)).

[26] In change regressions, I can no longer include bank*county fixed effects without eliminating the relevant variation.

County Robustness Tables 6 and 7 provide additional robustness checks on the county level. Literature establishes the importance of the collateral channel: rising real estate prices increase collateral values and relax financial constraints.[27] While in baseline regressions county*year fixed effects control for common shocks to all firms within a county, changes in local real estate prices could affect industries heterogeneously, depending on their home equity intensity. For example, if house prices recovered more slowly from the Great Recession in counties with higher exposure, depressed collateral values would decrease loan demand by more in industries that rely on home equity. Likewise, literature has shown that the pre-crisis house price boom and ensuing bust had lasting effects on local economies (Mian, Sufi and Trebbi, 2015). If the boom-bust cycle of house prices affects industries differentially based on their financing patterns, my estimates could be biased.

To this end, Table 6 columns (1) and (2) include the change in county house prices from 2000 to 2007 during the boom, as well as the bust in house prices from 2007 to 2010. While both variables, interacted with *home equity* and the post dummy, enter significantly and with the expected sign (a more pronounced boom and bust depress young firm employment in the post-crisis period), the coefficient on *exposure* $\times$ *home equity* $\times$ $\mathbb{1}$*(2009-16)* remains significant and negative, and close in magnitude to the baseline specification. Similarly, including the share of home owners as of 2010 as interaction term in column (3) does not materially affect the coefficient of interest.[28]

[Table 6 about here]

To further rule out that the recovery in house prices since the recession explains results, Figure 4 plots the 2010-16 change in county house prices against exposure; panel (a) is unconditional, panel (b) conditional on the decline in house prices during the recession. There is an almost precise zero relationship between exposure to stress tested banks and the recovery in housing values, suggesting that employment of young firms in exposed counties does not suffer from a weaker housing recovery. The visual evidence is confirmed in column (4) of Table 6, which includes an interaction term with contemporaneous local house price growth. The coefficient on exposure does not change in any statistically or economically meaningful way when I control for contemporaneous house price changes. The fact that house price growth enters the regression significantly shows that house prices affect entrepreneurship – a well-established fact in the literature – but that their effect is orthogonal to county exposure to

[27] See Chaney, Sraer and Thesmar (2012); Adelino, Schoar and Severino (2015); Bahaj, Foulis and Pinter (2018); Doerr (2018).

[28] House prices, and hence home ownership, could have an effect on young firm employment due to a decline in home equity values and a corresponding fall in local demand since the recession (Mian and Sufi, 2014; Mian, Sufi and Trebbi, 2015). If the decline in demand varies systematically across industries, main coefficients will be biased. Data n home ownership is only available for 2000 or 2010,. The correlation between both series is 0.94 and results identical under both metrics.

stress tested banks. Finally, column (5) presents a horse race of exposure with all housing related variables in columns (1)-(4). Across specifications, the effect of exposure on young firm employment remains large and highly significant. The stability of the main effect suggests that exposure reflects a fall in loan supply by stress tested banks, irrespective of characteristics and performance of local housing markets. Results are not due to an impaired local collateral channel.

[Figure 4 about here]

Table 7 reports further robustness checks. Column (1) uses the employment share of young firms out of total employment in a county-industry-year cell as dependent variable. While falling employment among young firms could reflect a general decline in employment in counties with high exposure, the negative significant effect suggests this not to be the case. Young firm employment declines stronger than total employment in the same county and industry. Columns (2)-(3) control for the share of the four largest banks, since Chen, Hanson and Stein (2017) find that these banks cut back on small business lending since the crisis. Controlling for county exposure to top-four banks in column (2) or restricting the sample to counties with no top-four presence in column (3) does not materially affect the coefficient of interest. This suggests that the effects of exposure (and hence stress tests) do not hinge on the presence of the largest four banks and their contraction in lending.[29]

[Table 7 about here]

Another potential concern is that young firms have become more risky since the crisis, leading to a decline in loan supply irrespective of stress tests.[30] While it is difficult to assess changing risk among start-ups, column (4) interacts exposure with a measure of industry risk (Cortés, Demyanyk, Li, Loutskina and Strahan, forthcoming).[31] Under the assumption that the change in risk is correlated with overall industry risk, including *industry risk* controls for increasing risk among young firms that differs across two-digit industries. Including the

[29]Compared to Chen, Hanson and Stein (2017), my setup differs along two important dimensions: First, they use the fraction of small business establishments that expanded in a given year as outcome variable, while my paper uses employment in start-ups. Second, their level of aggregation is at the county level, while I use data at the county-industry level and focus on the effect of exposure on home equity-intensive industries. However, my results speak to their findings: presence of top-four banks has a negative effect on firm formation, but the effect is due to stress tests.

[30]Note that an increase in risk is not inconsistent with lower loan supply due to stress tests. Stress tests discourage risky lending, so the *same* increase in borrower risk could translate into a *stronger* contraction in loan supply if banks are being stress tested.

[31]Cortés, Demyanyk, Li, Loutskina and Strahan (forthcoming) define industry betas, or *industry risk*, as the long-run sensitivity of industry employment to changes in aggregate employment. Higher values indicate riskier industries.

additional interaction term increases the baseline coefficient in magnitude, suggesting that, if anything, riskier industries also use more home equity financing.[32]

To further account for unobservable trends that are specific to firms in different age groups, columns (5)-(6) include granular fixed effects. When employing county*time and industry*time fixed effects, the underlying assumption is that young and old firms react similarly to common shocks. In general, young firms could depend more on local demand than old firms, which had time to build a network of clients across multiple markets. To account for differences in the sensitivity of firm age cells to common shocks, I estimate regressions at the county-industry-age-year level, where I define dummy *young* with value 1 for firms age 0-1, and value 0 for firms age two and older. Column (5) shows that young firm employment declines by more than old firm employment in counties with higher exposure and in home equity intensive industries − mirroring the result in column (1). Column (6) then adds county*firm age*time and industry*firm age*time fixed effects to control for shocks that affect firms in different age groups heterogeneously within each county and industry over time. For example, they control for a local rise in house prices that increases consumer demand and hence sales of young firms by more than sales of old firms; or a decline in industry exports that affects old firms stronger than young firms who sell predominately local. Note that these highly granular fixed effects also absorb changes in risk that vary by *age group over time.* Coefficients do not change in any statistically meaningful way after including fixed effects in column (6), relative to column (5). Columns (7)-(8) compute exposure based on CRA loan data instead of deposit data (analogous to equation (1)). Results are similar in terms of sign, size, and significance. Finally, column (9) re-computes the home equity share at the state-industry level. One potential concern is that the share of young firms using home equity to start their business is correlated with state-level characteristics, such as foreclosure laws. If so, taking industry-level average would provide an incomplete picture of the geographic variation across the U.S. Column (9) shows a highly significant and negative effect of exposure on young firm employment in home equity intensive state-industry cells. This finding suggests that my results do not hinge on the chosen level of aggregation for industry-level home equity usage by young firms.

4 Innovation and Productivity

Section 3 showed that stress tested banks cut business-related home equity lending, which leads to a contraction in aggregate loan supply and employment of start-ups. Since a vast literature establishes the importance of entrepreneurs for disruptive innovation and aggregate productivity (Haltiwanger, 2015; Aghion, 2017), this section analyzes whether the decline in

[32]The correlation between industry risk and home equity financing is $\rho = 0.20$, or $\rho = 0.42$ when excluding arts and recreation (unreported). The positive correlation is in line with theories in which risky borrower are required to pledge collateral (Steijvers and Voordeckers, 2009; Berger, Espinosa-Vega, Frame and Miller, 2011).

entrepreneurship due to stress tests contributes to the slowdown in post-crisis growth.

Table 8 shows that counties with higher exposure to stress tested banks see a fall in patent applications of young firms. Each column uses log patents by young firms as dependent variable at the county-IPC-year level.[33] Column (1) includes year fixed effects and shows that patent applications by young firms decline significantly for counties with higher exposure. Adding county controls in column (2) and time-varying fixed effects at the state and industry (IPC) level in column (3) does not materially affect size or significance of the coefficient. Across specifications, there is a significant negative effect of exposure on patent applications by young firms.

[Table 8 about here]

Not all patents are used: if other companies do not use the underlying innovation (due to dummy patents or patent trolling), the decline in patents by young firms has no consequences. To account for incremental innovations, column (4) shows that the effect of exposure on patents is stronger when I weight patents by citations, i.e. account for how often they are used. Compared to column (3), effect size almost doubles in magnitude, suggesting that counties that generated highly-cited patents are particularly affected. This finding holds in column (5) that instruments observed with predicted exposure and reports a significant negative coefficient. In terms of magnitude, moving a county from the 25^{th} to the 75^{th} percentile in terms of exposure leads to a relative decline in citation-weighted patents by 16.2%. Finally, columns (6)-(7) replicate columns (4)-(5), but use log (weighted) patents by old firms as dependent variable. There is an insignificant and weakly positive effect of exposure on patents by old firms. In conclusion, Table 8 suggest that counties with higher exposure to stress tested banks see a decline in innovation by young firms, but no significant increase in patents of old firms. The decline in loan supply to young firms hence reduces their capability to innovate.

Following the financial crisis, the U.S. witnessed an unprecedented decline in growth. Output per capita growth fell by almost half, averaging a meager 1.3%. Research established that a major factor contributing to the slowdown is weak productivity growth (Fernald, 2016). Since young firms have an outsized importance for aggregate productivity, the decline in employment and innovation has potentially far reaching consequences for productivity.[34] In light of results on the decline in firm formation and innovation, Table 9 investigates the link between stress

[33] A well-known feature of patent data is that industries differ in their intensity to patent. To this end, I aggregate patent data to the county-industry-year level, where industry refers to patents' International Patent Classification (IPC, does not correspond to NAICS industry codes). This allows me to include time-varying fixed effects at the IPC*year level to account for unobservable differences across industries, for example patenting activity or a rise in the share of non-patented innovation.

[34] Firm entry contributes around one-third to aggregate TFP growth (Decker, Haltiwanger, Jarmin and Miranda, 2014; Haltiwanger, 2015; Alon, Berger, Pugsley and Dent, 2018; Curtis and Decker, 2018).

tests and labor productivity.[35] It shows that counties with higher exposure see a decline in wages, relative to counties with low exposure to stress tested banks. It reports results for regression equation (7) on the county-industry-year (C-I-Y) level, dependent variable is log average wage, a common proxy for labor productivity.

[Table 9 about here]

Column (1) shows that higher exposure is associated with a relative decline in wages during the stress testing period. Bank and county level results show that the contraction in lending is strongest for secured loans, and that the decline in young firm employment particularly strong in home-equity intensive industries. Columns (2)-(3) show that the same hold for labor productivity: the decline in average wages is concentrated in industries that rely more on home equity financing. This result holds without and with county*time and industry*time fixed effects that control for local and industry demand in columns (2) and (3), as well as when I instrument exposure with predicted exposure in columns (4)-(5). In column (5), moving a county from the 25^{th} to the 75^{th} percentile in terms of exposure leads to a relative decline in wages by 2.6% in home equity-intensive industries. Table 9 suggests that the decline in lending affects productivity in industries with a higher share of young firms using home equity financing − precisely the industries, where young firm employment declines most strongly in exposed counties (see Table 4).

Discussion An emerging literature examines how the drop in firm formation during the Great Recession affects productivity (Bassetto, Cagetti and De Nardi, 2015; Siemer, 2016). My results suggest that not only did employment among young firms decline during the crisis, but the contraction in lending due to stress tests hurts entrepreneurship even after the crisis has passed. The persistent collapse in lending to start-ups reduces innovation and potentially contributes to the productivity slowdown above and beyond the immediate effects of the crisis.

However, my results have to be viewed with caution. First, stress tests have the intention of increasing the resilience of the financial sector against shocks. They achieve this by increasing capital requirements during a transition period. Once the economy reaches its new steady state and banks adjust their portfolios accordingly, higher capital requirements need no longer have negative effects on entrepreneurship or growth. Second, a sounder financial system could reduce macroeconomic volatility and the probability of a future crisis. A lower probability of future negative shocks might be worth slower growth during 'calm' periods, and could translate into higher growth on average. This debate dates back to work on the trade-off between financial

[35] For literature on the effects of loan supply on productivity, see Duval, Hong and Timmer (2017); Manaresi and Pierri (2017); Doerr, Raissi and Weber (2018); for work showing how changes in the allocation of credit across borrower classes affect aggregate growth, see Acharya, Eisert, Eufinger and Hirsch (2017); Schivardi, Sette and Tabellini (2017); Blattner, Rebelo and Farinha (2018).

stability and growth, a theme well-explored for developing countries (Loayza, Ranciére, Servén and Ventura, 2007; Loayza, Ouazad and Ranciere, 2017).

With respect to productivity, my reduced form regressions attribute the overall effect of exposure on labor productivity exclusively to the decline in young firms. While a large literature highlights the importance of young firms for growth and productivity estimates in Table 9 are qualitatively in line with those for employment in Table 4, the nature of my data does prohibits to exactly estimate the magnitude of the effect of exposure on productivity. Local labor productivity is affected by the activity of young firms, but also reflects actions by, for example, older and large firms. Since stress tests potentially affect banks and the economy beyond credit supply to young firms, they could affect productivity through channels besides start-up financing in general equilibrium. For example, lower firm entry can reduce incumbents' incentives to innovate and exacerbate the initial effect (Aghion, 2017).

5 Conclusion

This paper provides new evidence that post-crisis financial regulation has stymied entrepreneurship. I show that stress tested banks strongly cut small business lending secured by collateral and that the contraction in loan supply had real effects. Exploiting geographic variation in county exposure to stress tested banks, I find that counties with a higher exposure have seen a relative decline in employment at young firms during the recovery. In line with the literature that highlights the sensitivity of young firms to changes in collateral values, I find that the contraction in secured lending has hit entrepreneurs disproportionately harder in industries that rely more on home equity financing. Granular county-industry data ensure that my results are not confounded by unobservable shocks to local demand or industries. Results are also robust to an instrumental variable approach that predicts county exposure with a gravity model of bank expansion.

Young firms have an outsized effect on aggregate innovation and growth. Consequently, I show that higher county exposure to stress tested banks reduces patent applications by young firms. The decline is more pronounced when I adjust for the quality of innovation and weight patents by citations. I also provide suggestive evidence that wages grow more slowly during the recovery in exposed counties.

Capital regulation in general, and stress tests in particular, are intended to reduce risk-taking. My findings suggest that post-crisis financial regulation has lead to a reallocation of credit away from risky borrowers: entrepreneurs have seen a fall in credit supply. The shift in bank credit across borrowers has had unintended side effects, having reducer dynamism and innovation during the recovery from the Great Recession. These effects should be taken into account when evaluating the overall consequences for the economy of financial regulation or higher capital requirements, especially in light of the current debate on declining dynamism and the post-crisis productivity slowdown. Yet, my results do *not* imply that stress testing is bad for welfare. Reducing volatility and the incidence of crises might require a reduction in risky lending to young firms. This paper highlights a possible trade-off between financial stability and growth, but does not take a stance on the efficiency or long-run implications of the implemented policy.

References

Acharya, Viral V., Allen N. Berger, and Raluca A. Roman (2018) "Lending implications of U.S. bank stress tests: Costs or benefits?", *Journal of Financial Intermediation*, 34 (January), pp. 58–90.

Acharya, Viral V., Tim Eisert, Christian Eufinger, and Christian W. Hirsch (2017) "Whatever It Takes: The Real Effects of Unconventional Monetary Policy", *Working Paper*.

Adelino, Manuel, Song Ma, and David Robinson (2017) "Firm Age, Investment Opportunities, and Job Creation", *Journal of Finance*, 72 (3), pp. 999–1038.

Adelino, Manuel, Antoinette Schoar, and Felipe Severino (2015) "House prices, collateral, and self-employment", *Journal of Financial Economics*, 117 (2), pp. 288–306.

Agarwal, Sumit and Robert Hauswald (2010) "Distance and private information in lending", *Review of Financial Studies*, 23 (7), pp. 2758–2788.

Aghion, Philippe (2017) "Entrepreneurship and growth: lessons from an intellectual journey", *Small Business Economics*, 48 (1), pp. 9–24.

Aiyar, Shekhar, Charles W. Calomiris, John Hooley, Yevgeniya Korniyenko, and Tomasz Wieladek (2014) "The international transmission of bank capital requirements: Evidence from the UK", *Journal of Financial Economics*, 113 (3), pp. 368–382.

Alon, Titan, David Berger, Benjamin Pugsley, and Robert Dent (2018) "Older and Slower: The Startup Deficit's Lasting Effects on Aggregate Productivity Growth", *Journal of Monetary Economics*, 93, pp. 68–85.

Altonji, Joseph G., Todd E. Elder, and Christopher R. Taber (2005) "Selection on Observed and Unobserved Variables: Assessing the Effectiveness of Catholic Schools", *Journal of Political Economy*, 113 (1), pp. 151–184.

Avery, Robert B., Kenneth P. Brevoort, and Glenn B. Canner (2007) "Opportunities and Issues in Using HMDA Data", *The Journal of Real Estate Research*, 29 (4), pp. 351–380.

Bahaj, Saleem, Angus Foulis, and Gabor Pinter (2018) "Home Values and Firm Behaviour", *Working Paper*.

Bassetto, Marco, Marco Cagetti, and Mariacristina De Nardi (2015) "Credit crunches and credit allocation in a model of entrepreneurship", *Review of Economic Dynamics*, 18 (1), pp. 53–76.

Beck, Thorsten, Asli Demirgüç-Kunt, Luc Laeven, and Ross Levine (2008) "Finance, firm size, and growth", *Journal of Money, Credit and Banking*, 40 (7), pp. 1379–1405.

Benmelech, Efraim and Nittai K Bergman (2008) "Collateral Pricing", *NBER Working Paper* (13874).

Berger, Allen N. and Christa H.S. Bouwman (2013) "How does capital affect bank performance during financial crises?", *Journal of Financial Economics*, 109 (1), pp. 146–176.

Berger, Allen N., Marco A. Espinosa-Vega, Scott W. Frame, and Nathan H. Miller (2011) "Why Do Borrowers Pledge Collateral? New Empirical Evidence on the Role of Asymmetric Information", *Journal of Financial Intermediation*, 20 (1), pp. 55–70.

Berger, Allen N., Björn Imbierowicz, and Christian Rauch (2016) "The Roles of Corporate Governance in Bank Failures during the Recent Financial Crisis.", *Journal of Money, Credit and Banking*, 48 (4), pp. 729–770.

Berrospide, Jose M. and Rochelle M. Edge (2019) "The Effects of Bank Capital Buffers on Bank Lending and Firm Activity: What Can We Learn from Five Years of Stress-Test Results?", *Finance and Economics Discussion Series* (050).

Blattner, Laura, Francisca Rebelo, and Luísa Farinha (2018) "When Losses Turn Into Loans: The Cost of Undercapitalized Banks", *Working Paper*, pp. 1–78.

Bloom, Nicholas, Charles Jones, John Van Reenen, and Michael Webb (2017) "Are Ideas Getting Harder to Find?", *NBER Working Paper* (23782).

Bolton, Patrick, Xavier Freixas, Leonardo Gambacorta, and Paolo Emilio Mistrulli (2016) "Relationship and Transaction Lending in a Crisis", *Review of Financial Studies*, 29 (10), pp. 2643–2676.

Bord, Vitaly M., Victoria Ivashina, and Ryan D. Taliaferro (2018) "Large Banks and Small Firm Lending", *NBER Working Paper* (25184).

Bordo, Michael D., Rebel A. Cole, and John V. Duca (2019) "The Impact of the Dodd-Frank Act on Small Business", *Working Paper* (24501).

Byrne, David M., John G. Fernald, and Marshall B. Reinsdorf (2016) "Does the United States have a Productivity Slowdown or a Measurement Problem", *Brookings Papers on Economic Activity*.

Carpenter, Robert E. and Bruce C. Petersen (2002) "Is the growth of small firms constrained by finance?", *The Review of Economics and Statistics*, 84 (2), pp. 298–309.

Celerier, Claire, Thomas K. Kick, and Steven Ongena (2018) "Taxing Bank Leverage: The Effects on Bank Capital Structure, Credit Supply and Risk-Taking", *Working Paper*.

Celerier, Claire and Adrien Matray (2016) "Bank Branch Supply and the Unbanked Phenomenon", *Working Paper*, pp. 1–52.

Chaney, Thomas, David Sraer, and David Thesmar (2012) "The Collateral Channel: How Real Estate Shock Affect Corporate Investment", *American Economic Review*, 102 (6), pp. 2381–2409.

Chen, Brian S., Samuel Hanson, and Jeremy C. Stein (2017) "The Decline of Big-Bank Lending to Small Business: Dynamic Impacts on Local Credit and Labor Markets", *Working Paper*.

Clearinghouse (2016) "TCH Research Note: 2016 Federal Reserve's Stress Testing Scenarios", March.

Clearinghouse (2017a) "Are the Supervisory Bank Stress Tests Constraining the Supply of Credit to Small Businesses?", May.

Clearinghouse (2017b) "The Capital Allocation Inherent in the Federal Reserve's Capital Stress Test", January.

Connolly, Michael F. (2018) "The Real Effects of Stress Testing", *Working Paper.*

Cortés, Kristle Romero, Yuliya S. Demyanyk, Lei Li, Elena Loutskina, and Philip E. Strahan (forthcoming) "Stress Tests and Small Business Lending", *Journal of Financial Economics.*

Curtis, E. Mark and Ryan A. Decker (2018) "Entrepreneurship and State Taxation", *Working Paper.*

De Jonghe, Olivier, Hans Dewachter, Klaas Mulier, Steven Ongena, and Glenn Schepens (2019) "Some Borrowers are More Equal than Others: Bank Funding Shocks and Credit Reallocation", *Review of Finance* (forthcoming).

Decker, Ryan A., John C. Haltiwanger, Ron S. Jarmin, and Javier Miranda (2014) "The Role of Entrepreneurship in US Job Creation and Economic Dynamism", *Journal of Economic Perspectives*, 28 (3), pp. 3–24.

Decker, Ryan A., John C. Haltiwanger, Ron S. Jarmin, and Javier Miranda (2016) "Declining Business Dynamism: Implications for Productivity?", *Brookings Institution, Hutchins Center Working Paper.*

Decker, Ryan A., John C. Haltiwanger, Ron S. Jarmin, and Javier Miranda (2018) "Changing Business Dynamism and Productivity: Shocks vs. Responsiveness", *NBER Working Paper* (24236).

Degryse, Hans, Artashes Karapetyan, and Sudipto Karmakar (2017) "To Ask or Not To Ask? Collateral versus Screening in Lending Relationships", *Working Paper.*

Degryse, Hans and Steven Ongena (2005) "Distance, Lending Relationships, and Competition", *Journal of Finance*, 40 (1), pp. 231–266.

Dick, Astrid A. (2006) "Nationwide Branching and Its Impact on Market Structure, Quality, and Bank Performance", *The Journal of Business*, 79 (2), pp. 567–592.

Doerr, Sebastian (2018) "Collateral, Reallocation, and Aggregate Productivity: Evidence from the U.S. Housing Boom", *Working Paper.*

Doerr, Sebastian, Mehdi Raissi, and Anke Weber (2018) "Credit-Supply Shocks and Firm Productivity in Italy", *Journal of International Money and Finance*, October (87), pp. 155–171.

Duval, Romain, Gee Hee Hong, and Yannick Timmer (2017) "Financial Frictions and the Great Productivity Slowdown", *IMF Working Paper*, 17 (129).

Fernald, John G. (2014) "Productivity and Potential Output before, during, and after the Great Recession", *NBER Macroeconomics Annual 2014*, pp. 1–51.

Fernald, John G. (2016) "Reassessing Longer-Run U.S. Growth: How Low?", *Federal Reserve Bank of San Francisco Working Paper* (18).

Fernald, John G., Robert E. Hall, James H. Stock, and Mark W. Watson (2017) "The Disappointing Recovery of Output after 2009", *NBER Working Paper* (23543).

Fernald, John G. and J. Christina Wang (2016) "Why Has the Cyclicality of Productivity Changed? What Does It Mean?", *Meeting Papers Society for Economic Dynamics* (1220).

Foster, Lucia S., Cheryl A. Grim, and John C. Haltiwanger (2016) "Reallocation in the Great Recession: Cleansing or not?", *Journal of Labor Economics*, 34 (1), pp. 293 – 331.

Fraisse, Henri, Mathias Lé, and David Thesmar (2015) "The Real Effects of Bank Capital Requirements", *Working Paper*.

Gambacorta, Leonardo and Paolo Emilio Mistrulli (2004) "Does bank capital affect lending behavior?", *Journal of Financial Intermediation*, 13 (4), pp. 436–457.

Gan, Jie (2007) "The Real Effects of Asset Market Bubbles: Loan- and Firm-Level Evidence of a Lending Channel", *Review of Financial Studies*, 20 (6), pp. 1941–1973.

Goetz, Martin R., Luc Laeven, and Ross Levine (2013) "Identifying the valuation effects and agency costs of corporate diversification: Evidence from the geographic diversification of U.S. banks", *Review of Financial Studies*, 26 (7), pp. 1787–1823.

Goetz, Martin R., Luc Laeven, and Ross Levine (2016) "Does the geographic expansion of banks reduce risk?", *Journal of Financial Economics*, 120 (2), pp. 346–362.

Goldschlag, Nathan and Elizabeth Perlman (2017) "Business Dynamic Statistics of Innovative Firms".

Gordon, Robert J. (2015) "Secular Stagnation: A Supply-Side View", *American Economic Review: Papers and Proceedings*, 105 (5), pp. 54–59.

Gourio, François, Todd Messer, and Michael Siemer (2016) "Firm entry and macroeconomic dynamics: A state-level analysis", *American Economic Review*, 106 (5), pp. 214–18.

Granja, Joao and Christian Leuz (2017) "The Death of a Regulator: Strict Supervision, Bank Lending and Business Activity", *Working Paper*.

Griliches, Zvi (1990) "Patent Statistics as Economic Indicators: A Survey", *NBER Working Paper* (3301).

Gropp, Reint, Thomas Mosk, Steven Ongena, and Carlo Wix (2018) "Bank Response to Higher Capital Requirements: Evidence from a Quasi-Natural Experiment", *Review of Financial Studies*, forthcomin.

Gropp, Reint, Joerg Rocholl, and Vahid Saadi (2017) "The Cleansing Effect of Banking Crises", *Working Paper*.

Gutiérrez, Germán and Thomas Philippon (2017a) "Declining Competition and Investment in the U.S.", *Working Paper*.

Gutiérrez, Germán and Thomas Philippon (2017b) "Investmentless Growth: An Empirical Investigation", *Brookings Papers on Economic Activity*, pp. 89–169.

Haltiwanger, John C. (2015) "Job Creation, Job Destruction, and Productivity Growth: The Role of Young Businesses", *Annual Review of Economics*, 7, pp. 341–358.

Harding, John P. and Stuart S. Rosenthal (2017) "Homeownership, housing capital gains and self-employment", *Journal of Urban Economics*, 99, pp. 120–135.

Hoffer, Adam, Stephen Matteo Miller, and David Wille (2017) "Small-business financing after the financial crisis – lessons from the literature", *Journal of Entrepreneurship and Public Policy*, 6 (3), pp. 315–339.

Hollander, Stephan and Arnt Verriest (2016) "Bridging the gap: The design of bank loan contracts and distance", *Journal of Financial Economics*, 119 (2), pp. 399–419.

Hubbard, R. Glenn (1998) "Capital-market imperfections and investment", *Journal of Economic Literature*, 36 (1), pp. 193–225.

Ivashina, Victoria and David Scharfstein (2010) "Bank lending during the financial crisis of 2008", *Journal of Financial Economics*, 97 (3), pp. 319–338.

Jensen, Thais, Søren Leth-Petersen, and Ramana Nanda (2015) "Home Equity Finance and Entrepreneurial Performance - Evidence from a Mortgage Reform", *Working Paper*.

Jiménez, Gabriel, Steven Ongena, José-Luis Peydró, and Jesús Saurina (2014) "Hazardous Times for Monetary Policy: What do Twenty-Three Million Bank Loans Say About the Effects of Monetary Policy on Credit Risk Taking?", *Econometrica*, 82 (2), pp. 463–505.

Jiménez, Gabriel, Steven Ongena, José-Luis Peydró, and Jesús Saurina (2017) "Macroprudential policy, countercyclical bank capital buffers and credit supply : Evidence from the Spanish dynamic provisioning experiments", *Journal of Political Economy*, 125 (6).

Jiménez, Gabriel, Vicente Salas, and Jesús Saurina (2006) "Determinants of collateral", *Journal of Financial Economics*, 81 (2), pp. 255–281.

Liberti, José María and Mitchell A. Petersen (2019) "Information: Hard and Soft", *Review of Corporate Finance Studies*, 8 (1), pp. 1–44.

Loayza, Norman V., Amine Ouazad, and Romain Ranciere (2017) "Financial Development, Growth, and Crisis - Is There a Trade-Off?", *World Bank Policy Research Paper* (8237).

Loayza, Norman V., RomainRanciére, Luis Servén, and Jaume Ventura (2007) "Macroeconomic volatility and welfare in developing countries: An introduction", *World Bank Economic Review*, 21 (3), pp. 343–357.

Manaresi, Francesco and Nicola Pierri (2017) "Credit Supply and Productivity Growth", *Working Paper*.

Matray, Adrien (2015) "The Local Innovation Spillovers of Listed Firms".

Meisenzahl, Ralf R. (2014) "Verifying the State of Financing Constraints: Evidence from US Business Credit Contracts", *Journal of Economic Dynamics and Control*, 43 (June), pp.

58–77.

Meisenzahl, Ralf R. (2016) "Can Financing Constraints Explain the Evolution of the Firm Size Distribution?", *Review of Industrial Organization*, 48 (2), pp. 123–147.

Mian, Atif and Amir Sufi (2014) "What Explains the 2007-2009 Drop in Employment?", *Econometrica*, 82 (6), pp. 2197–2223.

Mian, Atif, Amir Sufi, and Francesco Trebbi (2015) "Foreclosures, House Prices, and the Real Economy", *Journal of Finance*, 70 (6), pp. 2587–2634.

Moreira, Sara (2017) "Firm Dynamics, Persistent Effects of Entry Conditions, and Business Cycles", *Working Paper*.

Oster, Emily (2017) "Unobservable Selection and Coefficient Stability: Theory and Evidence", *Journal of Business and Economic Statistics*, pp. 1–18.

Peri, Giovanni (2005) "Determinants of Knowledge Flows and their Effect on Innovation", *The Review of Economics and Statistics*, 87 (2), pp. 308–322.

Pierret, Diane and Roberto Steri (2018) "Stressed Banks", *Working Paper*.

Prilmeier, Robert (2017) "Why do loans contain covenants? Evidence from lending relationships", *Journal of Financial Economics*, 123 (3), pp. 558–579.

Pugsley, Benjamin W., Petr Sedlacek, and Vincent Sterk (2017) "The Nature of Firm Growth", *Working Paper*.

Quarles, Randal K. (2019) "Stress Testing: A Decade of Continuity and Change Remarks".

Rice, Tara and Philip E. Strahan (2010) "Does Credit Competition Affect Small-Firm Finance?", *Journal of Finance*, 65 (3), pp. 861–889.

Schivardi, Fabiano, Enrico Sette, and Guido Tabellini (2017) "Credit Misallocation During the Financial Crisis", *Working Paper*.

Schmalz, Martin C., David A. Sraer, and David Thesmar (2017) "Housing Collateral and Entrepreneurship", *Journal of Finance*, 72 (1), pp. 99–132.

Shapiro, Joel and Jing Zeng (2018) "Stress Testing and Bank Lending", *Working Paper*.

Siemer, Michael (2016) "Firm Entry and Employment Dynamics in the Great Recession", *Working Paper*.

Steijvers, Tensie and Wim Voordeckers (2009) "Collateral and credit rationing: a review of recent empirical studies as a guide for future research", *Journal of Economic Surveys*, 23 (5), pp. 924–946.

Stroebel, Johannes (2016) "Asymmetric Information about Collateral Values", *Journal of Finance*, 71 (3), pp. 1071–1112.

Summers, Lawrence H. (2015) "Demand Side Secular Stagnation", *American Economic Review: Papers and Proceedings*, 105 (5), pp. 60–65.

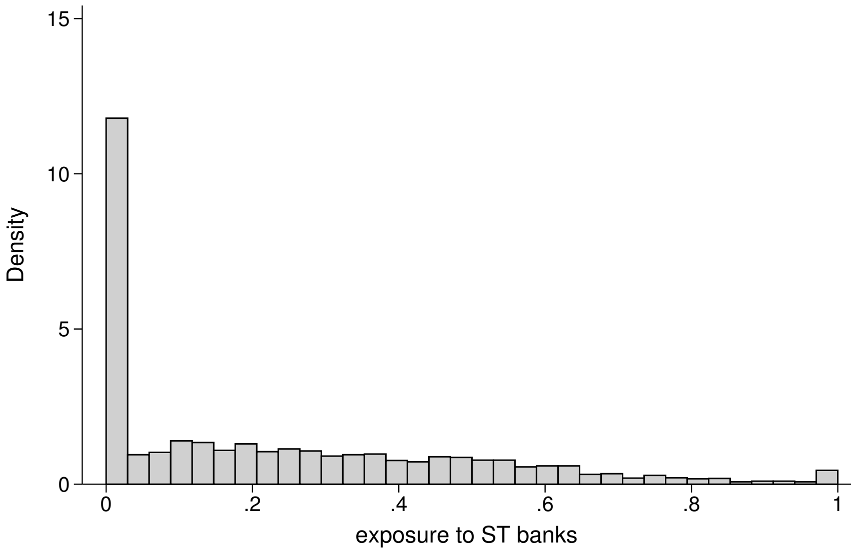
Teulings, Coen and Richard Baldwin (2014) *Secular Stagnation: Facts, Causes and Cures*, London, CEPR Press.

A Appendix

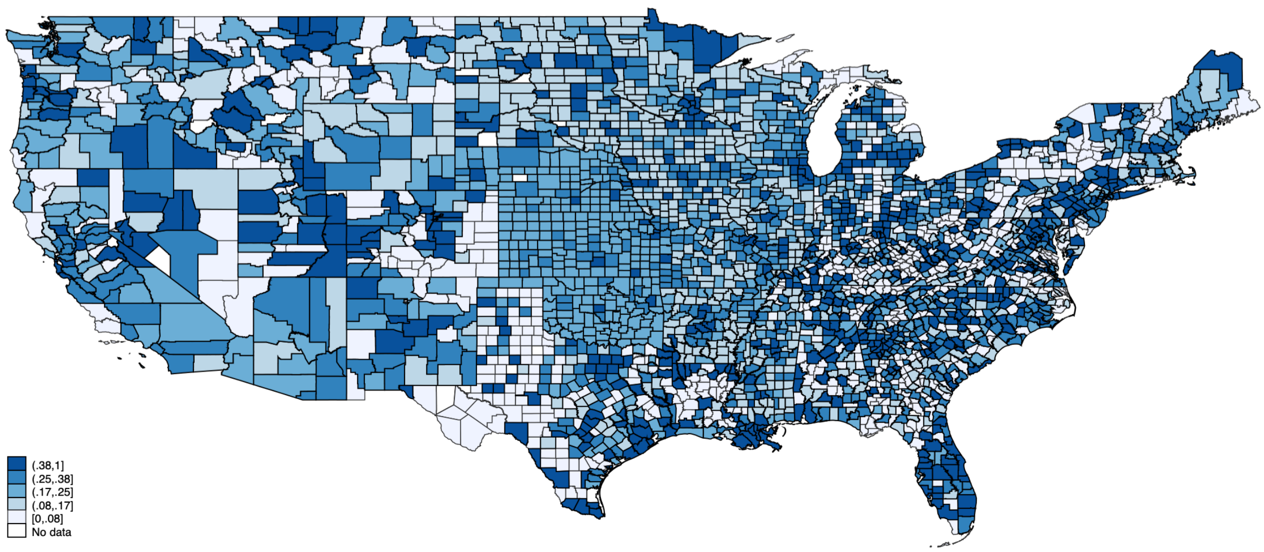
A.1 Figures

Figure 1: **County exposure**

(a) Histogram



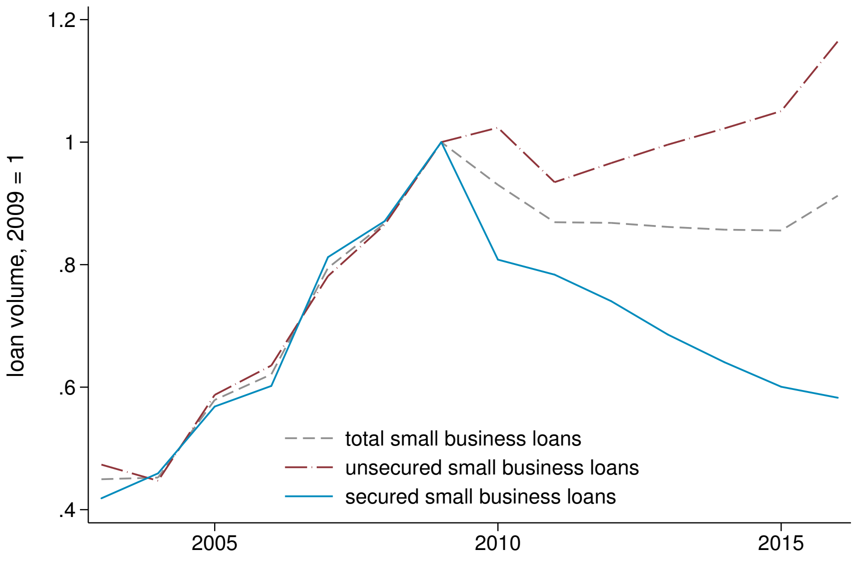
(b) Map



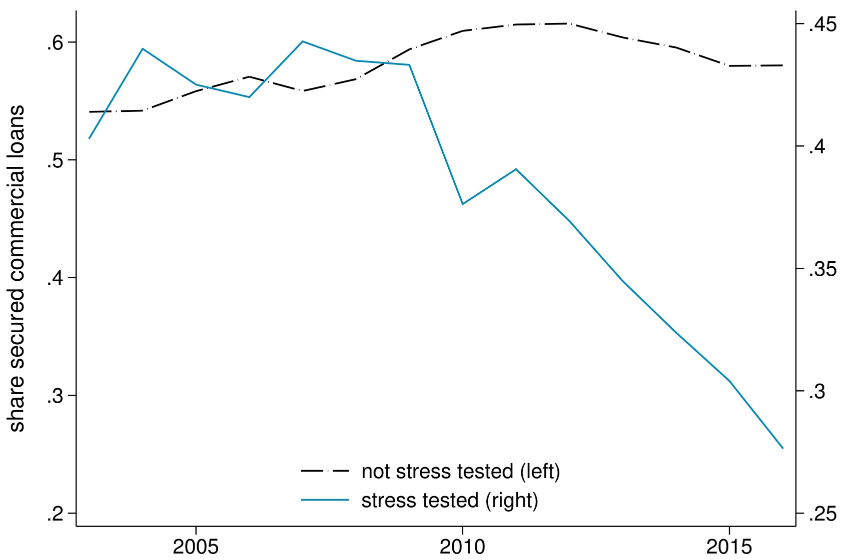
Note: Panel (a) shows the distribution of county *exposure* to stress tested banks. Panel (b) shows a map of U.S. counties, where darker areas indicate counties with higher exposure, where exposure is conditional on state fixed effects. There is significant variation in county exposure across the full sample, as well as within individual states.

Figure 2: **Secured small business lending**

(a) Small business lending by stress tested banks

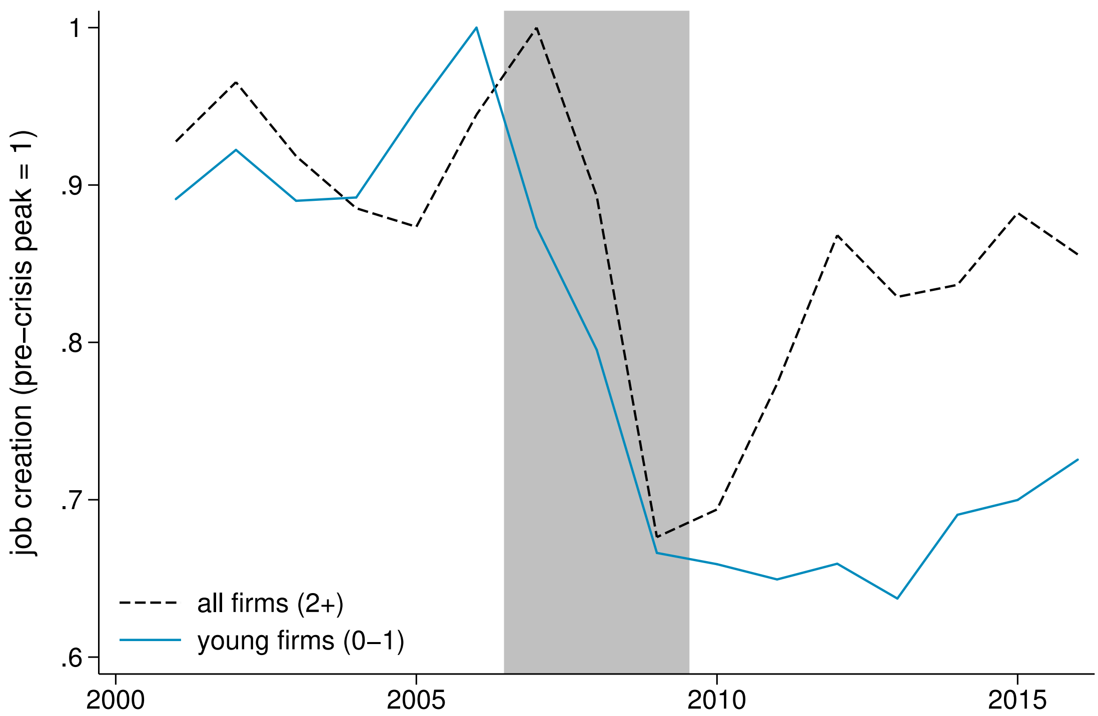


(b) Stress tested vs. other banks



Note: Panel (a) shows total (secured+unsecured), secured, and unsecured small business lending by stress tested banks, normalized to 1 in 2009. Panel (b) shows the share of secured out of total small business lending for stress tested (blue solid line) and non-stress tested (black dashed line) banks. Stress tested banks strongly and persistently reduced secured small business lending, panel (a). This is mirrored in a decline in the relative share of secured small business loans for stress tested banks that is not present for non-stress tested banks, panel (b).

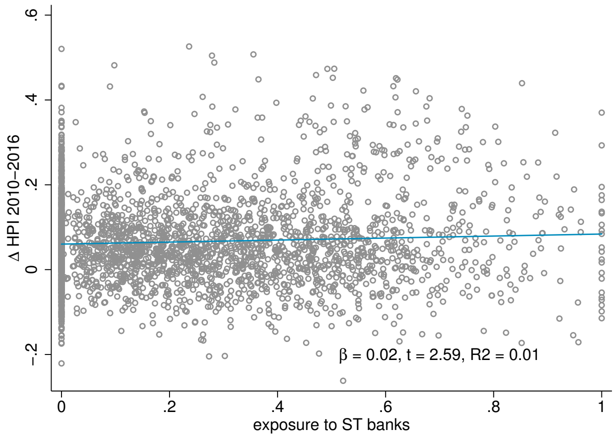
Figure 3: **Job creation by young firms**



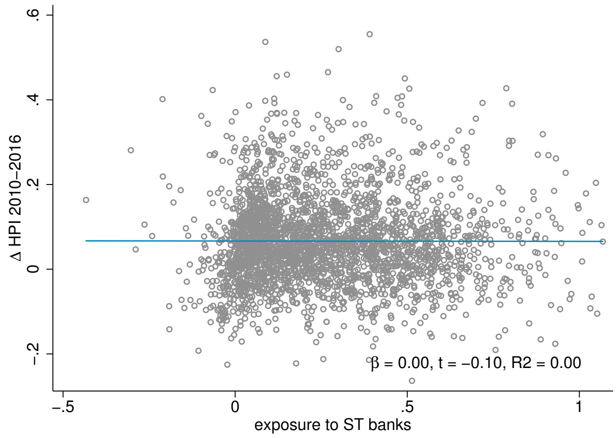
Note: This figure shows gross job creation of young firms (age 0-1) and all other firms (age 2+) during the Great Recession (shaded area) and recovery period. Series are normalized to their pre-crisis peak (Source: Census BDS Firm Characteristics Data Tables).

Figure 4: **House price recovery**

(a) Unconditional



(b) Conditional on $\Delta HPI\ 2007\text{-}2010$



Note: Panels (a) and (b) shows scatter plots of the change in county house prices from 2010 to 2016 on the y-axis and exposure, as defined in equation (1), on the x-axis. Panel (a) is unconditional, panel (b) conditional on the decline in house prices during the Great Recession from 2007 to 2010. There is no significant relationship between the recovery in house prices and county exposure to stress tested banks.

A.2 Tables

Table 1: **Descriptive statistics**

	mean	sd	min	max	count
Panel (a): Bank					
share secured SB loans	0.58	0.24	0.00	1.00	94510
log(assets)	14.93	1.38	9.72	24.14	94510
non-performing loans (%)	0.30	0.63	-0.75	3.58	94510
return on assets (%)	0.80	1.04	-4.40	4.23	94510
deposits/assets (%)	0.93	0.07	0.07	1.00	94510
liquidity (%)	0.07	0.05	0.01	0.30	94510
Tier 1 capital (%)	0.18	0.10	0.04	0.80	94510
non-interest income (%)	0.76	0.88	-0.56	6.67	94510
efficiency (%)	0.72	0.23	0.30	1.92	94510
Panel (b): Bank-county					
home equity amount	1487.53	1222.71	3.00	5349.00	169061
log(home equity amount)	6.95	0.89	1.10	8.58	169061
Panel (c): County					
exposure	0.36	0.24	0.00	1.00	91999
exposure (IV)	0.33	0.17	-0.02	0.94	91906
log(population)	2.45	0.11	1.81	2.78	91999
share black	0.10	0.12	0.00	0.70	91999
share elderly	0.14	0.04	0.02	0.33	91999
LF participation rate	0.94	0.02	0.81	1.06	91999
unemployment rate	0.06	0.02	0.01	0.19	91999
Δ house price index	0.02	0.07	-0.30	0.34	91999
log(income p.c.)	10.49	0.29	9.61	11.81	91999
Panel (d): County-industry					
log(emp Y)	3.55	1.64	0.00	10.99	293868
share emp Y	0.07	0.08	0.00	0.50	293868
log(wage)	6.84	0.85	4.34	8.77	293868

Note: This table shows descriptive statistics for main variables. For variable definitions see section B.

Table 2: **Small business lending**

VARIABLES	(1) ln(amt)	(2) ln(amt)	(3) log(unsecured)	(4) log(secured)	(5) ln(amt)	(6) ln(amt)	(7) ln(amt)
stress tested	-0.359***	-0.267***	-0.125*	-0.358***	-0.053		
	(0.088)	(0.094)	(0.075)	(0.074)	(0.047)		
stress tested × secured					-0.377***	-0.377***	-0.421***
					(0.092)	(0.092)	(0.096)
Observations	169,061	169,061	94,230	94,230	188,460	188,460	188,460
R-squared	0.567	0.752	0.906	0.895	0.896	0.958	0.959
Bank Controls	✓	✓	✓	✓	✓	✓	✓
Bank*County FE	✓	✓	-	-	-	-	-
Bank FE	-	-	✓	✓	-	-	-
Bank*Sec FE	-	-	-	-	✓	✓	✓
Time FE	T	C*T	T	T	T	B*T	B*T+S*T
Cluster	Bank	Bank	Bank	Bank	Bank	Bank	Bank

Note: This table shows regression results for regression equations (3) and (4) on the bank-county-year and bank-year level. The dependent variable in columns (1)-(2) is log HMDA small business lending at the bank-county level; in column (3) it is banks' log unsecured small business lending on the bank-year level. Columns (4) uses log small business lending secured by real estate collateral. Columns (5)-(7) are on the bank-loan type-year level and use log loan amount (secured vs. unsecured) as dependent variable. *stress tested* is a dummy with value if a bank was stress tested in a given year, $\mathbb{1}(2009-16)$ is a dummy with value 1 for years 2009 to 2016. *secured* is a dummy with value one if loan type equals secured small business loans, and zero if loan type equals unsecured small business loans. Column (1) includes bank controls, as well as bank*county and year (T) fixed effects. To absorb local unobservable county characteristics, column (2) includes county*time (C*T) fixed effects. Columns (3)-(4) include bank and year fixed effects. Columns (5)-(7) use bank*loan type fixed effects and add bank*time (B*T) and loan type*time (S*T) fixed effects. All specifications cluster standard errors on the bank holding company (BHC) level. Values in parentheses denote standard errors. Key: *** p<0.01, ** p<0.05, * p<0.1.

Table 3: **IV: gravity equation and first stage**

VARIABLES	(1) logit deposit share	(2) OLS deposit share	(3) OLS deposit share	(4) OLS deposit share	(5) OLS deposit share	(6) First Stage exposure	(7) First Stage exposure	(8) First Stage exposure
log(1+distance)	-0.960***	-0.136***	-0.138***	-0.138***	-0.159***			
	(0.006)	(0.004)	(0.003)	(0.003)	(0.002)			
log(population ratio)	0.267***	0.003	-0.008**					
	(0.009)	(0.003)	(0.003)					
exposure (predicted)						0.277***	0.284***	0.182***
						(0.034)	(0.018)	(0.015)
Observations	27,601	27,601	27,470	27,105	22,619	3,112	3,112	2,624
R-squared		0.729	0.785	0.830	0.850	0.221	0.571	0.598
Host County FE	-	-	✓	✓	-	-	-	-
Home County FE	-	-	-	✓	✓	-	-	-
Home State*Host County FE	-	-	-	-	✓	-	-	-
State FE	-	-	-	-	-	-	✓	✓
County Controls	-	-	-	-	-	-	-	✓
Cluster	Home County	Home County	Home County	Home County	Home County	State	State	State

Note: Columns (1)-(5) are on the bank-county level for 2007. Dependent variable is the deposit share of bank b in county c (out of total bank deposits). *log(1+distance)* denotes log of one plus distance between the bank headquarter county and bank branch county. *log(population ratio)* is the log ratio of home (bank HQ) to host (bank branch) county population. Column (1) runs a fractional logit model. Columns (2)-(4) add fixed effects to account for unobservable county characteristics in banks' HQ and branch counties. Adding fixed effects does not materially change the coefficient of interest on distance. Column (5) exploits variation between banks located in the same state lending to the same county, i.e. differences in distance only reflect differences in banks' HQ locations within a given state. Columns (6)-(8) run 'first stage' regressions on the county level (as of 2007) of actual county exposure on county exposure based on predicted deposits. Column (6) is unconditional, column (7) adds state fixed effects, and column (8) state fixed effects plus pre-crisis county controls. Values in parentheses denote standard errors. Key: *** p<0.01, ** p<0.05, * p<0.1.

Table 4: **Young firm employment**

VARIABLES	(1) ln(amt)	(2) IV ln(amt)	(3) log(emp Y)	(4) log(emp Y)	(5) log(emp Y)	(6) IV log(emp Y)	(7) IV log(emp Y)
exposure × $\mathbb{1}$(2009-16)	-0.267***	-0.859***	-0.045***	0.206***		-0.154***	
	(0.040)	(0.113)	(0.014)	(0.060)		(0.021)	
home equity × $\mathbb{1}$(2009-16)				-2.419***			
				(0.147)			
exposure × home equity × $\mathbb{1}$(2009-16)				-1.504***	-2.052***		-3.307***
				(0.336)	(0.338)		(0.499)
Observations	24,638	24,638	293,868	293,868	293,868	293,534	293,534
R-squared	0.778	0.038	0.809	0.810	0.817	-	-
County FE	✓	✓	-	-	-	-	-
County*Industry FE	-	-	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	-	✓	-
County*Time FE	-	-	-	-	✓	-	✓
Industry*Time FE	-	-	-	-	✓	-	✓
Cluster	County	County	County	County	County	County	County

Note: This table shows regression results at the county-year level and county-industry-year level (see equation (7)). Dependent variable is the log home equity lending in columns (1)-(2) and log employment of young firms (age 0-1) in columns (3)-(7). *exposure* denotes pre-crisis county exposure to stress tested banks as defined in equation (1). $\mathbb{1}(2009-16)$ is a dummy with value one for years 2009 to 2016, i.e. the years after the introduction of stress tests. *home equity* is the share of young firms within each two-digit industry that uses home equity financing to start or expand operations. Column (5) adds granular fixed effects to control for unobservable shocks that could affect local or industry demand over time. Counties with higher exposure see a decline in the number of young firms during the post-crisis period, and the decline is concentrated within industries that rely more on home equity financing. Columns (2) and (6)-(7) instrument county exposure with predicted exposure based on the gravity model. Values of Anderson-Rubin F-statistics in columns (2), (6), and (7) are 67.38, 53.00, and 44.27, respectively. Standard errors are clustered on the county level. Values in parentheses denote standard errors. Key: *** p<0.01, ** p<0.05, * p<0.1.

Table 5: **Bank robustness: more evidence on stress tests**

VARIABLES	(1) ln(nr)	(2) ln(avg)	(3) $> 20bn$ ln(amt)	(4) no repeat ln(amt)	(5) ln(amt)	(6) ln(amt)	(7) Δ amt	(8) Δ amt	(9) Δ amt	(10) Δ amt	(11) Δ amt
stress tested	-0.204**	-0.063	-0.278***	-0.289**	0.188		0.385**				
	(0.081)	(0.049)	(0.100)	(0.127)	(0.359)		(0.187)				
stress tested × county risk					-0.595	-0.544***	-0.398**	-0.428***			
					(0.439)	(0.162)	(0.190)	(0.158)			
stress test exposure									-0.114**	-0.043	
									(0.046)	(0.056)	
stress test exposure × county risk										-0.079***	-0.059**
										(0.020)	(0.021)
Observations	169,061	169,061	131,295	83,735	117,870	112,815	111,396	111,396	21,344	21,344	21,344
R-squared	0.713	0.803	0.867	0.737	0.868	0.948	0.532	0.835	0.347	0.348	0.535
Bank Controls	✓	✓	✓	✓	✓	-	✓	-	✓	✓	-
Bank*County FE	✓	✓	✓	✓	✓	✓	-	-	-	-	-
County*Time FE	✓	✓	✓	✓	✓	-	✓	✓	✓	✓	✓
Bank*Time FE	-	-	-	-	-	✓	-	✓	-	-	✓
Cluster	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank

Note: This table shows robustness checks for bank-county level regressions. The dependent variable in columns (1)-(5) is log HMDA small business lending at the bank-county level; in columns (6)-(10) it is the log change in HMDA small business lending at the bank-county level. *stress tested* is a dummy with value for each year a bank underwent stress tests. *stress test exposure* denotes banks' capital gap based on stress test results, i.e. the distance of forecasted from required minimum capital ratio. Columns (3)-(5) restrict the sample to banks with total asset size (as of 2008) of at least 20bn. Columns (8)-(10) restrict the sample to stress test years 2012-16. All specifications cluster standard errors on the bank holding company (BHC) level. Values in parentheses denote standard errors. Key: *** p<0.01, ** p<0.05, * p<0.1.

Table 6: **County robustness: the collateral channel**

VARIABLES	(1) log(emp Y)	(2) log(emp Y)	(3) log(emp Y)	(4) log(emp Y)	(5) log(emp Y)
exposure × home equity × $\mathbb{1}$(2009-16)	-1.697***	-1.415***	-2.071***	-2.116***	-1.405***
	(0.349)	(0.356)	(0.351)	(0.338)	(0.367)
Δ HPI 00-07 × home equity × $\mathbb{1}$(2009-16)	-2.089***				-0.786
	(0.411)				(0.547)
Δ HPI 07-10 × home equity × $\mathbb{1}$(2009-16)		3.677***			2.842***
		(0.555)			(0.722)
ownership 10 × home equity × $\mathbb{1}$(2009-16)			0.005		0.005
			(0.010)		(0.010)
Δ HPI × home equity × $\mathbb{1}$(2009-16)				3.173***	3.206***
				(0.251)	(0.258)
Observations	276,889	287,127	287,314	286,847	276,833
R-squared	0.814	0.815	0.815	0.815	0.814
County*Industry FE	✓	✓	✓	✓	✓
County*Time FE	✓	✓	✓	✓	✓
Industry*Time FE	✓	✓	✓	✓	✓
Cluster	County	County	County	County	County

Note: This table shows regression results for regressions on the county-industry-year level (see regression equation (7)). Dependent variable is log employment of young firms. *exposure* denotes pre-crisis county exposure to stress tested banks as defined in equation (1). *home equity* is the share of young firms within each two-digit industry that uses home equity financing to start or expand operations. $\mathbb{1}(2009-16)$ is a dummy with value one for years 2009 to 2016, i.e. the years after the introduction of stress tests. ΔHPI $00-07$ and ΔHPI $07-10$ are changes in county house prices from 2000 to 2007 and 2007 to 2010; *ownership 10* is the county share of home owners in 2010; Δ HPI is growth in yearly county house prices. Growth rates are log-differences. Standard errors are clustered on the county level. Values in parentheses denote standard errors. Key: *** p<0.01, ** p<0.05, * p<0.1.

Table 7: **County robustness: alternative exposure**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
			no top4						S-I HE
VARIABLES	share emp Y	log(emp Y)	log(emp Y)	log(emp Y)	log(emp)	log(emp)	log(emp Y)	share emp Y	log(emp Y)
exposure × home equity × $\mathbb{1}$(2009-16)	-0.078***	-1.897***	-2.467***	-2.016***	-1.629***	-1.581***			-1.087***
	(0.026)	(0.408)	(0.625)	(0.366)	(0.160)	(0.155)			(0.317)
top-4 exposure × home equity × $\mathbb{1}$(2009-16)		-0.720							
		(0.933)							
exposure × industry risk × $\mathbb{1}$(2009-16)				-0.003					
				(0.008)					
young × $\mathbb{1}$(2009-16)					-0.193***				
					(0.028)				
exposure × young × $\mathbb{1}$(2009-16)					0.072				
					(0.062)				
home equity × young × $\mathbb{1}$(2009-16)					-0.586***				
					(0.153)				
exposure × home equity × young × $\mathbb{1}$(2009-16)					-0.674**	-0.692**			
					(0.339)	(0.342)			
exposure (CRA) × home equity × $\mathbb{1}$(2009-16)							-2.310***	-0.085**	
							(0.477)	(0.036)	
Observations	293,868	293,868	151,676	293,868	570,996	570,913	293,868	293,868	255,894
R-squared	0.523	0.817	0.684	0.817	0.953	0.954	0.817	0.523	0.808
County*Industry FE	✓	✓	✓	✓	× age	× age	✓	✓	✓
County*Time FE	✓	✓	✓	✓	✓	× age	✓	✓	✓
Industry*Time FE	✓	✓	✓	✓	✓	× age	✓	✓	✓
Cluster	County	County	County	County	County	County	County	County	County

Note: This table shows regression results for regressions on the county-industry-year level (see regression equation (7)). Dependent variables is the employment share of young firms in columns (1) and (8), and log employment of young firms in columns (2)-(7) and (9). *exposure* denotes pre-crisis county exposure to stress tested banks as defined in equation (1). *home equity* is the share of young firms within each two-digit industry that uses home equity financing to start or expand operations. *top-4-exposure* denotes county exposure (in terms of local deposits) to the four largest U.S. banks in terms of total assets as of 2007. *industry risk* denotes industry betas from Cortés et al. (forthcoming) for each 2-digit Naics industry. Columns (5)-(6) are on the county-industry-age-year level and include time-varying fixed effects at the county-age and industry-age level. *young* is a dummy with value one for firms age 0-1 and zero for firms age 2 and older. *exposure CRA* denotes pre-crisis county exposure to stress tested banks as defined in equation (1), but based on CRA small business loans instead of deposits. Column (9) uses *home equity* computed at the state-industry level as explanatory variable. Standard errors are clustered on the county level. Values in parentheses denote standard errors. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: **Patents**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
					IV		IV
				cit.	cit.	cit.	cit.
VARIABLES	log(PT Young)	log(PT Young)	log(PT Young)	log(PT Young)	log(PT Young)	log(PT Old)	log(PT Old)
exposure $\times$ $\mathbb{1}$(2009-16)	-0.116***	-0.150***	-0.133***	-0.254***	-0.440***	0.044	0.070
	(0.025)	(0.027)	(0.031)	(0.041)	(0.094)	(0.030)	(0.110)
Observations	91,999	91,999	91,999	91,999	91,906	91,999	91,906
R-squared	0.795	0.799	0.801	0.521	0.521	0.727	0.614
County Controls	-	✓	✓	✓	✓	✓	✓
County*Industry FE	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	-	-	-	-	-
State*Time FE	-	-	✓	✓	✓	✓	✓
Industry*Time FE	-	-	✓	✓	✓	✓	✓
Cluster	County	County	County	County	County	County	County

Note: This table shows regression results on the county-IPC-year level. Dependent variable is log patent applications (PT) by young firms. *exposure* denotes pre-crisis county exposure to stress tested banks as defined in equation (1). $\mathbb{1}(2009-16)$ is a dummy with value one for years 2009 to 2016, i.e. the years after the introduction of stress tests. *cit. wt.* denotes citation weighted patents. Columns (1)-(2) present baseline regressions, columns (3)-(5) use time-varying fixed effects on the state and industry level. Columns (5) instruments county exposure with predicted exposure based on the gravity model. Columns (6)-(7) replicate columns (4)-(5) but use log (weighted) patents by old firms as dependent variable. Standard errors are clustered on the county level. Values in parentheses denote standard errors. Key: *** p<0.01, ** p<0.05, * p<0.1.

Table 9: **Wages**

VARIABLES	(1) log(wage)	(2) log(wage)	(3) log(wage)	(4) IV log(wage)	(5) IV log(wage)
exposure × $\mathbb{1}$(2009-16)	-0.016*	0.276***		-0.052***	
	(0.009)	(0.050)		(0.014)	
home equity × $\mathbb{1}$(2009-16)		1.149***			
		(0.121)			
exposure × home equity × $\mathbb{1}$(2009-16)		-1.623***	-1.493***		-0.926**
		(0.257)	(0.260)		(0.390)
Observations	293,868	293,868	293,868	293,534	293,534
R-squared	0.710	0.710	0.723	-0.000	0.000
County*Industry FE	✓	✓	✓	✓	✓
Time FE	✓	✓	-	✓	-
County*Time FE	-	-	✓	-	✓
Industry*Time FE	-	-	✓	-	✓
Cluster	County*Time	County*Time	County*Time	County*Time	County*Time

Note: This table shows regression results for equation (7) on the county-industry-year level. Dependent variable is log average wage. *exposure* denotes pre-crisis county exposure to stress tested banks as defined in equation (1). $\mathbb{1}(2009-16)$ is a dummy with value one for years 2009 to 2016, i.e. the years after the introduction of stress tests. *home equity* is the share of young firms within each two-digit industry that uses home equity financing to start or expand operations. *low/high HE* denotes industry below/above the median in terms of home equity. Columns (1)-(2) present baseline regressions with county*industry and year fixed effects. Column (3) adds granular fixed effects to control for unobservable shocks that could affect local or industry demand over time. Counties with higher exposure see a relative decline in wages during the post-crisis period, and the decline is concentrated within industries that rely more on home equity financing. Columns (4)-(5) instrument county exposure with predicted exposure based on the gravity model. Standard errors are clustered on the county level. Values in parentheses denote standard errors. Key: *** p<0.01, ** p<0.05, * p<0.1.

B Data Appendix

Table 10: **Steps of the mechanism**

Banks and Stress Tests	Entrepreneurs	Innovation & Productivity
1. Stress tests increase cost of (secured) small business lending ⇒ Stress tested banks reduce (secured) small business lending (Section 3.1, Table 2)	2. Entrepreneurs are small and use collateral to receive financing ⇒ Decline in loan supply by stress tested banks hurts young firms (Section 3.2, Table 4)	3. Entrepreneurs are important for innovation and productivity ⇒ Decline in young firms reduces innovation and productivity (Sec. 3.2, Tables 8+9)

Note: This table summarizes the steps of the empirical argument. Since stress tests make (secured) small business lending expensive for banks (in terms of capital requirements), I show that (a) stress tested banks reduce their credit supply to small firms, especially if firms use real estate collateral. Entrepreneurs are mostly small and opaque, so they rely on real estate collateral and home equity to start or expand their business. Consequently, (b) in counties where stress tested banks are more important, entrepreneurs are hit harder by the reduction in credit supply, and the more so in home equity intensive industries; and (c) entrepreneurs have an outsized importance for innovation and aggregate growth, so the decline in young firms due to the contraction in loan supply hurts innovation and productivity in counties with higher exposure to stress tested banks.

Identifying business-related loans in HMDA data My main data source for loan level data is the Home Mortgage Disclosure Act (HMDA), Ultimate Loan Application Register (LAR). To construct the HMDA baseline sample, I follow what is standard in the literature:

- restrict the sample to originated, approved, or purchased loans
- restrict the sample to conventional or FHA insured loans
- exclude multifamily properties
- drop loans with no information on the borrower county fips code
- drop loans with missing borrower county fips code
- drop loans with missing or zero applicant income

Since I am interested in loans to small businesses, I also drop all loans with loan amount greater or equal $1,000,000. These conditions reduce the number of bank-borrower observations (loans) on average by around one-third in a given year. I then identify home equity loans for business purposes ("business-related loans") by restricting the sample to refinanced loans for which applicant and/or co-applicant gender, race, and ethnicity are coded as not available.[36] As

[36] As suggested in Avery et al. (2007), in 2004 I only use information on gender.

detailed in Avery, Brevoort and Canner (2007), "[...] if an applicant or co-applicant is not a natural person, then HMDA requirements stipulate that the race, ethnicity, and gender codes for the applicant (or co-applicant) should be reported as not applicable or "N/A." Business loans with personal guarantees would come under this rule." On average, around 3.5% of yearly bank-borrower observations (loans) represent business-related home equity loans. For example, in 2008 there were 560,000 business-related loans that were financed with home equity. I then aggregate to the bank-county level. Before computing main outcome variables, I identify and exclude clear outliers in terms of loan amount, defined as observations with loan amount at least five times above the inter-quartile range.

PatentsView Data on patent applications and patent citations is provided by *PatentsView*. It provides monthly data on granted patent applications, the inventor(s) and assignee(s) of each individual patent, as well as the location of inventors and assignees. Inventor refers to individuals, assignees can be individuals or corporations. For example, Sebastian Doerr could be inventor and assignee, but I could file the patent application in the name of my company Sebastian Inc. In the latter case, Sebastian Doerr is still the inventor, but Sebastian Inc. the assignee. I keep patent applications by U.S. and foreign companies or corporations, as well as individuals. I drop patent applications by universities and governments, and aggregate patent data at the inventor's county. If there are more than one inventor from the same county, the patent is counted once for the respective county. If there are more than one inventor in different counties, the patent is counted equally in each county (I split the patent on a pro-rata basis per inventor). Finally, I define patents by 'young firms' as patent application by assignees (i.e. corporations) that did not file a patent application more than five years ago. For example, suppose Sebastian Inc. files its first ever patent application in 2005. All patents filed until 2010 are then classified as patents by young firms, all patents from 2011 onward as old. Patents are often used as a proxy for innovative activity. However, they have two important drawbacks. First, not all firms patent their ideas, and if and how they do so can vary across industries and areas. Yet, the correlation between R&D and the number of patents in the cross-section of firms is usually high (Griliches, 1990). Second, patents can contain a different amount of ideas. Aggregating to the county level mitigates this problem, because differences in the amount of ideas across patents 'average out' in the aggregate and level effects can be addressed via fixed effects (Peri, 2005; Matray, 2015).

Table 11: **Variable definitions: bank level**

Variable	Comment	Unit	Source
business-related home equity amount	in 1000s	USD	HMDA
secured small business loans	lnrenres1+lnrenres2	USD	FDIC SDI
unsecured SB loans	lnci1+lnci2	USD	FDIC SDI
share secured SB loans	sec./(sec.+unsec.)	%	FDIC SDI
log(assets)	ln(asset)	-	FDIC SDI
return on assets	roa	%	FDIC SDI
deposits to assets	dep/asset	%	FDIC SDI
capital ratio (Tier 1 to RW assets)	rbc1rwaj	%	FDIC SDI
overhead (efficiency ratio)	eeffr	-	FDIC SDI
non-performing loans (over tot. loans)	ntlnlsr	%	FDIC SDI
non-interest income (over avg. assets)	noniiay	%	FDIC SDI
liquidity	(chbal+iglsec)/asset	%	FDIC SDI

Table 12: **Variable definitions: county level**

Variable	Comment	Unit	Source
exposure (to stress tested banks)	deposit share	[0-1]	FDIC SOD
employment by firm age		unit	QWI
patent applications		unit	PatentsView
wages	average wage	USD	QCEW
population		unit	Cen. Bureau
% black population	share of tot- pop.	%	Cen. Bureau
% elderly population	share of tot- pop.	%	Cen. Bureau
labor force participation rate	civilian labor force	%	BLS LAUS
unemployment rate		%	BLS LAUS
Δ house prices	growth rate	%	FHFA HPI
income per capita		USD	BEA LAPI

Table 13: **Stress tested banks**

Bank Holding Company	2009	2011	2012	2013	2014	2015	2016
Ally Financial Inc.	†	✓	†	†	✓	✓	✓
American Express Company	✓	✓	†	✓	✓	✓	✓
Bank Of America Corporation	†	✓	✓	✓	✓	†	✓
The Bank Of New York Mellon Corporation	✓	✓	✓	✓	✓	✓	✓
BB&T Corporation	✓	✓	✓	†	✓	✓	✓
Capital One Financial Corporation	✓	✓	†	✓	✓	✓	✓
Citigroup Inc.	†	✓	✓	✓	†	✓	✓
Fifth Third Bancorp	†	✓	✓	✓	✓	✓	✓
The Goldman Sachs Group, Inc.	✓	✓	✓	†	✓	✓	✓
JPMorgan Chase & Co.	✓	✓	✓	†	✓	✓	✓
KeyCorp	†	✓	✓	✓	✓	✓	✓
Metlife, Inc.	✓	✓	†				
Morgan Stanley	†	✓	✓	✓	✓	✓	†
The PNC Financial Services Group, Inc.	†	✓	✓	✓	✓	✓	✓
Regions Financial Corporation	†	✓	✓	✓	✓	✓	✓
State Street Corporation	✓	✓	✓	✓	✓	✓	✓
SunTrust Banks, Inc.	†	✓	✓	✓	✓	✓	✓
U.S. Bancorp	✓	✓	✓	✓	✓	✓	✓
Wells Fargo & Company	†	✓	✓	✓	✓	✓	✓
Banco Bilbao Vizcaya Argentaria, S.A.					✓	✓	✓
Banco Santander, S.A.					†	†	†
BancWest, Inc.							✓
Bank Of Montreal					✓	✓	✓
Citizens Financial Group, Inc.					✓	✓	✓
Comerica Incorporated					✓	✓	✓
Deutsche Bank						†	†
Discover Financial Services					✓	✓	✓
HSBC Holdings PLC					†	✓	✓
Huntington Bancshares Incorporated					✓	✓	✓
M&T Bank Corporation					✓	✓	✓
Mitsubishi UFJ Financial Group, Inc.					✓	✓	✓
Northern Trust Corporation					✓	✓	✓
The Toronto-Dominion Bank							✓
Zions Bancorporation					†	✓	✓

Note: This table lists stress tested banks for each round of stress tests (SCAP in 2009, CCAR from 2011-2016). ✓ indicates that a bank was stress tested in the respective year, † indicates that a bank did not pass the test (results for the 2011 stress tests are not publicly available.

Previous volumes in this series

All volumes are available on our website www.bis.org.