Is There a Trade-off Between Protecting Investors and Promoting Entrepreneurial Activity? Evidence from Angel Financing *

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Abstract

This paper studies how changes in investor protection regulations affect local entrepreneurial activity, relying on the heterogeneous impact of a 2011 SEC regulation change on the definition of accredited investors across U.S. cities. Using a difference-in-differences approach, I show that cities more affected by the regulation change experienced a significantly larger decrease in local angel financing, entrepreneurial activity, innovation output, employment, and sales. I find that small business loans and second-lien mortgages became entrepreneurs' partial substitutes for angel investment. My cost-benefit analysis suggests that the costs of protecting angel investors through the 2011 regulation change outweigh its benefits.

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1 Introduction

Small businesses, which account for two-thirds of new jobs created in the US, are the basis for innovation and crucial for economic growth.[1] Raising capital for small businesses is important but not easy in a market where there is high information asymmetry and high search costs of potential investors.[2] Regulators like the Securities and Exchange Commission (SEC) have called lack of investor access to private companies a growing challenge.[3] However, there is often a trade-off between promoting entrepreneurial activity and investor protection. The concern is that smaller investors may lose a significant amount of money by investing in entrepreneurial firms that turn out to be unsuccessful.[4] There have been policy attempts to protect small investors, including a 2011 SEC amendment to the definition of accredited investors. In this paper, I exploit this regulation change to empirically analyze the aforementioned trade-off in the context of angel financing.

Angel investors drive a large portion of the financing for entrepreneurial firms (Denes, Howell, Mezzanotti, Wang, & Xu, 2020; W. R. Kerr, Lerner, & Schoar, 2014; Shane, 2008). Many firms were backed by angel investors at their early stage, with some famous examples being Google, Amazon, Facebook, Paypal, Costco, and The Home Depot. Angel investors are individual investors, as distinguished from institutional investors like venture capital (VC) and private equity (PE) firms. Angel investors may be more vulnerable to investing in frauds and scams, have less risk-bearing ability, and are more likely to make irrational investment decisions compared to institutional investors (Collewaert & Fassin, 2013; Drover

[1] President Barack Obama, Proclamation, National Small Business Week, 2014 (May 9, 2014), "Small businesses represent an ideal at the heart of our Nation's promise – that with ingenuity and hard work, anyone can build a better life. They are also the lifeblood of our economy, employing half of our country's workforce and creating nearly two out of every three new American jobs."

[2] There is large strand of literature discussing of such frictions, see examples in Leland and Pyle (1977), Grinblatt and Hwang (1989), and Conti, Thursby, and Rothaermel (2013).

[3] In SEC press release on June 18, 2019, "The Securities and Exchange Commission today requested public comment on ways to simplify, harmonize, and improve the exempt offering framework to expand investment opportunities while maintaining appropriate investor protections and to promote capital formation."

[4] The JOBS Act of 2012 on equity crowd-funding is another example that illustrates the potential trade-off between protecting investors and promoting entrepreneurship: It took the SEC more than three years to finalize rules on equity crowd-funding, which became legal in the U.S. only at the beginning of 2016.

et al., 2017). The concerns about protecting individual investors increased rapidly after the 2008 financial crisis, in which a lot of individuals went bankrupt and lost their home residence. On December 21, 2011, the SEC adopted amendments to the definition of accredited investors, requiring that the value of a person's primary residence be excluded when determining whether the person qualifies as an "accredited investor" on the basis of having a net worth in excess of $1 million.[5] The regulation change is estimated to eliminate more than 20% of previously eligible households in the U.S. (Hudson, 2014). I use the above regulation change to study the balance between protecting investors and promoting local angel financing and entrepreneurial activity.

The data set for this study is compiled from various sources. Data on angel investments are complied from the private placement records documented by the SEC Form D filings, Crunchbase, and VentureXpert. I obtain housing value data from Zillow. The regional average household net worth is estimated by using data from the Individual Income Tax Data from the Internal Revenue Service (IRS) and Survey of Income and Program Participation (SIPP). Data on firms' investor composition and investors' successful exits are collected from Crunchbase and VentureXpert. The patent data are from the United States Patent and Trademark Office (USPTO). I retrieve firm employment and sales data from the National Establishment Time-Series (NETS). The small business loan data are collected from the Small Business Administration and the data on second-lien mortgages are obtained through the Home Mortgage Disclosure Act (HMDA). I compile all the above data (either aggregate or average) at the city level. The final sample includes 3,896 cities from 2009 to 2013.

Making use of the heterogeneous impact of the SEC regulation change of 2011 across U.S. cities, I apply a difference-in-difference (DiD) approach to show that the above SEC

[5]On December 21, 2011, the SEC issued an announcement for immediate release (No. 2011-274), "the Securities and Exchange Commission has amended its rules to exclude the value of a person's home from net worth calculations used to determine whether an individual may invest in certain unregistered securities offerings. The changes were made to conform the SEC's definition of an 'accredited investor' to the requirements of the 2010 Dodd-Frank Wall Street Reform and Consumer Protection Act." The announcement can be found at `https://www.sec.gov/news/press/2011/2011-274.htm`. The final rule release can be found at `https://www.sec.gov/rules/final/2011/33-9287.pdf`.

regulation change had a significantly negative impact on local angel financing. Cities with a higher home-value-to-net-worth (HV/NW henceforth) ratio were more affected by the SEC regulation change, experienced significantly larger decreases in both the number and amount of angel financing after the regulation change. Specifically, a 10% higher HV/NW ratio prior to the regulation change on average leads to a 2.28% larger decrease in the amount and a 0.26% larger decrease in the number of angel investments after the regulation change. By performing several additional tests, I show that the above results are unlikely to be explained by alternative explanations such housing price growth, driven by outlier cities with bottom 10% and top 10% of home values or net wealth levels, or by regions where many angel investors reside in (i.e., San Francisco, New York, Boston, and their nearby cities). The findings are robust to using the top-tier home value to the net worth of top-class income group in a city (HV_top/NW_NW) instead of the HV/NW ratio as the measurement of treatment. I also provide evidence that angel investors invest locally and the previous results hold after controlling spillover effects from nearby regions. The placebo test shows that the 2011 regulation change had no significant effect on non-angel investments. In another placebo test where different pseudo event-times replace the actual event-time, no significant effect is found.

Next, I show that the above SEC regulation change had a significantly negative effect on local entrepreneurial activity measured by subsequent financing and successful exits (i.e., acquisitions or IPOs) generated by firms receiving angel financing (hereafter, angel-backed firms). My results suggest that a 10% higher HV/NW ratio prior to the regulation change, on average, led to a 0.15% larger decrease in the number of firms that received angel financing and later receive next-round financing and a 0.08% larger decrease in the number of angel-backed firms that later receive VC investment after the SEC regulation change. I also find that the number of angel-backed firms that later have successful exits decreased to a significantly greater extent in cities more affected by the SEC regulation change. The rate of receiving subsequent financing and having successful exits, however, did not have a

significant increase, consistent with the discussion in Hall and Lerner (2010) that it can be hard to screen start-up firms at an early stage when the prospects of these firms are highly uncertain.

Then, I provide evidence that the SEC regulation change, which aimed at protecting individual investors, imposed a non-negligible cost on the local economy. In particular, I examine the impact of the SEC regulation change on the generation of innovation, employment, and sales by firms that received angel investments in the local area. I show that when a city had a 10% higher HV/NW ratio in 2011, it, on average, experienced a 0.2% larger decrease in the number of patents, 0.01% larger decrease in the number of patent citations generated by the angel-backed firms in the city. The same city also experienced a 2.26% greater decrease in sales generated and a 0.65% greater decrease in the number of jobs supported by the angel-backed firms after the above regulation change.

To validate the above findings and to study the potential indirect impact of reducing angel financing on entrepreneurial activity, I examine the impact of the SEC regulation change on two alternative financing sources for small firms, namely, small business loans guaranteed by the Small Business Administration (SBA) and second-lien mortgages. I show that the number and amount of small business loans and second-lien mortgages increased significantly more in cities that were more affected by the SEC regulation change. The results suggest that the SEC amendment indeed reduced the supply of angel financing and pushed some entrepreneurs to borrow from taxpayers or to mortgage their own home. However, given the differences between debt and equity financing, these two alternative financing sources can not serve as perfect substitutes for angel financing (Schwienbacher, 2007; Winton & Yerramilli, 2008). Furthermore, even though credit provided from alternative financing sources may partially solve the financial constraints faced by entrepreneurs, the increased use of these two alternative financing sources may also generate concerns related to the efficient usage of government funding (Babina, He, Howell, Perlman, & Staudt, 2020; Brown & Earle, 2017) and the rising financial risk for both entrepreneurs and the economy (Elul, Souleles,

Chomsisengphet, Glennon, & Hunt, 2010).

Finally, I conduct a cost-benefit analysis of the 2011 SEC regulation change by estimating the present value of the costs and benefits of investor protection for the economy. The benefit is estimated by calculating the reduced amount of angel investment (due to the SEC regulation change) in entrepreneurial firms that would have turned out to be unsuccessful. The costs are measured by the present value of reduced sales generated by entrepreneurial firms that did not received angel financing (i.e., the present value of lost sales). I also show that the costs of reduced patents and employment generated by these entrepreneurial firms are non-negligible. The results of the above analysis provide suggestive evidence that from a pecuniary perspective, the costs of the 2011 SEC regulation change seem to be larger than its benefits. The above results are robust to various assumptions on growth rates and discount rates.

The rest of the paper is organized as follows. Section 2 discusses how my paper contributes to the related literature. Section 3 introduces the institutional background of angel investors and private placements in the U.S. Section 4 describes data sources and the construction variables used in this study. Sections 5 to 7 explain the empirical strategy and show the results of how the SEC regulation change impacted local angel financing, local entrepreneurial activity, and the local economy. Section 8 analyzes the substitution effects of the SEC regulation change on alternative financing sources. Section 9 presents the results of a cost-benefit analysis of the regulation change. Section 10 presents the policy implications from my study. I conclude the paper in Section 11.

2 Related Literature and Contribution

This paper contributes to several strands in the literature. First, it contributes to the literature on early-stage investors in entrepreneurial firms and their effects on firm performance. Previous studies have examined how angel groups (W. R. Kerr, Lerner, & Schoar, 2014;

Lerner, Schoar, Sokolinski, & Wilson, 2018), accelerators (Yu, 2020), and crowd-funding (Xu, 2018) impact firms' survival and performance. In terms of angel investors, studies have examined the relationship between angel investors and venture capitalists both theoretically (Chemmanur & Chen, 2014; Hellmann & Thiele, 2015) and empirically (Hellmann, Schure, & Vo, 2015). Venugopal and Yerramilli (2017) examine how seed-round successes of angel investors impact the evolution of the network of investors. Bernstein, Korteweg, and Laws (2017) study what firm characteristics attract early-stage investors. A contemporaneous and independent paper, Lindsey and Stein (2020), is the only other paper of which I am aware that studies the same regulation change. However, their paper is different from mine in many dimensions. First, they focus mainly on the negative impact of the above regulation change on aggregated small business employment. In contrast, the focus of my paper is to analyze the trade-off between investor protection regulations and the promotion of entrepreneurial activity and estimate both the costs and benefits of the 2011 SEC regulation change on the accreditation standard of angel investors. Second, they study the state-level aggregated business formation and employment for small firms (but not necessarily on angel-backed firms). In contrast, using mostly different and more micro-level data sets, I examine how the regulation change directly affected angel financing received by local firms at the city level, how it reduced the innovation, sales, and employment generated by angel-backed firms, and these firms' subsequent financing and successful exits.[6,7] Finally, their paper suggests that angel financing is complementary to alternative financing sources. On the contrary, I show that the decreased angel financing has significant substitution effects on alternative financing sources such as small business loans and second-lien mortgages, even though these sources

[6]I obtain firm-level angel financing data from SEC Form D filings, Crunchbase, and VentureXpert, patent-level data from the USPTO, firm-level annual sales and employment from the NETS, and firms' successful exits and financing histories from the VentureXpert and Crunchbase. I match these firm-level data sets and compile them at the city level. In Lindsey and Stein (2020), they mainly use the state-level aggregated data on the number of businesses and employment from Census's Business Dynamics Statistics and Quarterly Workforce Indicators. A discussion on the differences between their and my measurement of the treatment is in footnote 27.

[7]My paper is also related to the literature on the effect of VC-backing on corporate innovation, see, e.g., Chemmanur, Loutskina, and Tian (2014); Tian and Wang (2014).

may not serve as perfect substitutes for angel financing.

Second, my paper contributes to the literature on the impact of investor protection regulations on firm performance and policies. Existing literature has studied how institutional features shape investor protection laws across countries and their impact on external financing, corporate governance, corporate valuation, and dividend payout policies (Claessens, Djankov, Fan, & Lang, 2002; Claessens, Djankov, & Lang, 2000; La Porta, Lopez-de Silanes, Shleifer, & Vishny, 2000, 2002, 1997; Shleifer & Wolfenzon, 2002). Agrawal (2013) shows that investor protection has a causal impact on public firms' performance using the staggered passage of blue-sky laws in the U.S. However, there has been no study analyzing effects of investor protection regulations on the private equity market. In this paper, I empirically analyze the impact of investor protection regulation in the private market on local entrepreneurial activity and on the local economy for the first time in the literature.

Third, my paper is related to the literature on the role of government in promoting entrepreneurship and innovation. Lerner (2000) and Audretsch, Link, and Scott (2002) show that the U.S. Small Business Innovation Research (SBIR) positively impacts firms' R&D investment, commercialization, and subsequent firm growth. Howell (2017) causally estimates that an award from the U.S. Department of Energy's SBIR program approximately doubles the probability of receiving subsequent VC investments and has a positive impact on firms' innovation output and revenue growth. Da Rin, Nicodano, and Sembenelli (2006), however, find no evidence that public R&D spending has a positive effect on innovation using European data. Babina et al. (2020) compare government funding with private funding and find industry grants lead to greater appropriation of intellectual property. Brander, Du, and Hellmann (2015) and Denes (2017) study the impact of government-sponsored VC funding on the performance of entrepreneurial firms and its relationship with private VCs. Tian and Xu (2018) show that a place-based policy in China, the implementation of national high-tech zones, had a significant positive effect on local innovation and entrepreneurship. Denes et al. (2020) show that, although investor tax credits increase angel financing, they do not have

a significant effect in promoting high-growth entrepreneurship. However, existing literature has not examined the impact of investor protection regulations on entrepreneurial activity. In this study, I provide evidence that these regulations can negatively affect entrepreneurship and the real economy.

Fourth, my paper contributes to the recent debate about effects of the JOBS Act on the funding of small businesses in the U.S. and effects on entrepreneurship. Most of the existing studies have focused only on the impact of the JOBS Act on the initial public offerings (IPO). While the JOBS Act boosted IPO volume in subsequent years (Dambra, Field, & Gustafson, 2015), it also has brought unintended costs including higher IPO underpricing (Chaplinsky, Hanley, & Moon, 2017) and larger information uncertainty (Barth, Landsman, & Taylor, 2017) for emerging growth companies. These studies, however, have not looked into the crucial trade-off between protecting investors and promoting capital raising by small businesses, which is one of the main objectives of the JOBS Act. My paper empirically analyzes the above trade-off and provides policy implications for regulators.

3 Institutional Background

The financing of early-stage firms relies largely on investment from non-institutional investors. Angel investors, who are also known as accredited investors, provide about 90% of external equity raised by entrepreneurial firms.[8] Angel investors invested $24.8 billion in 70,730 deals in 2013, compared to venture capital, which invested $29.6 billion in 4,050 deals in 2013.[9] Angel investors usually invest at an earlier stage with a smaller amount of investment per firm than institutional investors like VCs. Many successful firms, like Google, Facebook, Amazon, and Costco, received angel investment at an early stage.

[8]Marianne Hudson, Executive Director, Angel Capital Association, Presentation to SEC Advisory Committee on Small and Emerging Companies, Washington, DC (December 17, 2014).

[9]The statistics on angel investors are from the annual angel report produced by the Center for Venture Research at the University of New Hampshire, which is available at `https://paulcollege.unh.edu/sites/default/files/resource/files/2013-analysis-report.pdf`. The statistics on VC are from NVCA 2014 Yearbook, which is available at `https://nvca.org/research/nvca-yearbook/`.

Unlike VC investors, the geographic distribution of angel investors is more diverse. 63% of angel investors are located outside of San Francisco, Boston, and New York City, with 16.2% in the Great Lakes region, 15.4% in the Southeast, and 10.7% in the Mid-Atlantic (Huang et al., 2017). Like other types of early-stage investment which tend to be distance sensitive (Agrawal, Catalini, & Goldfarb, 2011; Michelacci & Silva, 2007; Stuart & Sorenson, 2005), most angel investors invest locally. As illustrated in Figure 1, 60% of 8,832 angel investments in the U.S. have a distance of fewer than 100 miles between the angel and the funded company.

To receive money from investors, companies can sell securities either through a public offering or a private placement. To conduct a public offering, firms need to be registered with the SEC to make sure that all investors have enough information about what they are buying. Private placements, which are regulated by SEC rules collectively known as Regulation D, are offerings of unregistered securities to a limited pool of investors. Under Regulation D, companies may issue varying amounts of securities based on the type of investor they are selling them to—accredited or non-accredited investors—without registering those securities with the SEC.[10] Firms conducting private placements need to file a notice of an exemption to the SEC by using Form D within 15 days after the first sale of securities in the offering (Internet Appendix C shows the first two pages of the Form D). Although there are three rules under Regulation D, Rule 504, Rule 505, and Rule 506, 99% of the Form D filings file under SEC Rule 506. Rule 506 requires that most of the offering to be given only to accredited investors and can be given to at most 35 non-accredited investors. According to Ivanov and Bauguess (2013), more than 90% of private placements were sold only to accredited investors, which underscores the importance of defining who can become accredited investors.

As discussed above, investors in private placements consist mainly of accredited investors. Thus, the definition of accredited investors is crucial for capital access to the

[10]More information is available from the website of Financial Industry Regulatory Authority (FINRA), `http://www.finra.org/investors/private-placements-explained`.

private market. According to the SEC, an accredited investor is a person—or a married couple—with a net worth of at least $1 million, or an individual who earned an income of at least $200,000, or more than a combined income of $300,000 in the case of a married couple, for each of the last two years, and reasonably expects the same for the current year.

On December 21, 2011, the SEC amended its rules under the Securities Act of 1933 as required by the 2010 Dodd-Frank Wall Street Reform and Consumer Protection Act to exclude the value of a person's home from net worth calculations used to determine whether an individual may invest in certain unregistered securities offerings. The amendment became effective on February 27, 2012.[11] The regulation change is estimated to eliminate more than 20% of eligible households in the US, according to the survey conducted by the Angel Capital Association (Hudson, 2014).

In this paper, I study how the above SEC regulation change to the definition of accredited investors impacted local angel financing and subsequently affected firms' entrepreneurial activity and the local economy by exploiting the heterogeneity in the ratio of home value to net worth across U.S. cities. I also analyze the economics costs and benefits of the above regulation change.

4 Data

4.1 Data Sources

The data in this paper are compiled from various sources. Angel investments are difficult to observe and previous studies had to mainly rely on estimates from surveys (Shane, 2008). Following Denes et al. (2020), I combine data from SEC Form D filings, Crunchbase, and

[11] More information about the SEC regulation change is available on the website of the SEC, `https://www.sec.gov/news/press/2011/2011-274.htm`. Even though the Dodd Frank Act required the change to the net worth standard to be effective upon passage on July 21, 2010 and required the SEC to revise the definition of accredited investors, it was not until the late 2011 when the SEC officially adopted amendment to the rules under the Securities Act of 1933, which governs the security issuance. After the SEC amendment, the detailed definitions on net worth and primary home value became clear to the public. In Table B10 in Internet Appendix B, I assume the event time to be the first half or the second half in 2010 instead of the second half of 2011, and I find no significant results on angel financing.

Thomson Reuters VentureXpert to overcome the data challenge.

A Form D is used to file a notice of an exempt offering of securities with the SEC when firms do private placements.[12] Form D filings provide information such as the name, location, industry, incorporation year of filing firms, and the date and total offering amount of each filing. I include only the first-time Form D filing of every firm to capture the "entrepreneurial" property of economic activity and to avoid the potential bias driven by the differences in the frequency of firms doing private placements. Filings from firms in the industries of financial services or energy are excluded. I exclude SEC Form D amendments and include one filing per day for a firm to exclude duplicate filings.

The Form D observations are then supplemented with angel investments from Crunchbase and VentureXpert.[13] Crunchbase is a leading open-source database collecting information on start-ups and their round-by-round financing (Wang, 2018; Yu, 2020). VentureXpert provided by Thomson Reuters is a commercial database that has a better coverage on deals made by institutional investors such as VC and PE firms (Chemmanur et al., 2014; Ozmel, Robinson, & Stuart, 2013). I identify angel investments based on the round type and investor identity.[14] These identified angel investments from Crunchbase and VentureXpert are then matched with identified angel investments from Form D filings based on firm name, location, and the announcement date within three months of the filing date of the Form D. Only non-matched observations are then combined with the first-time Form D filings to form the angel-investment database. I also use the above matching procedure to exclude first-time Form D filings if they are regarded as VC/PE rounds using information from Crunchbase and

[12]The federal securities laws require the notice to be filed by companies that have sold securities without registration under the Securities Act of 1933 in an offering made under Rule 504 or 506 of Regulation D or Section 4(a)(5) of the Securities Act.

[13]This procedure is to address the issue that even though firms may face legal troubles if they do not file a Form D to the SEC, Ewens and Malenko (2020) show that some early-stage investments have never filed Form D.

[14]I include round types "pre-seed," "seed," and "angel" in Crunchbase and investor type identified as "angel," "individual," and "angel group" in VentureXpert. This procedure is similar as in Denes et al. (2020) with the only difference that I do not include round type "equity crowdfunding" or investor type "accelerator," "incubator," or "micro" as angel investments.

VentureXpert. Finally, I aggregate the angel investments at the city level semiannually.[15]

To measure the extent of a city being affected by the SEC regulation change, I construct the mean home-value-to-net-worth ratio. I collect ZIP code-level home value data from Zillow and then weighted-average the home value using population in a ZIP code at the city level semiannually.[16] The ZIP code-level household net worth is estimated by combining data from the Survey of Income and Program Participation (SIPP) and Internal Revenue Service (IRS) following the procedure suggested by Chenevert, Gottschalck, Klee, and Zhang (2017). Internet Appendix A illustrates the procedure of estimating the city-level average household net worth weighted by population.

To examine the impact on local entrepreneurial activity, I look at the subsequent financing and successful exits (i.e., IPO or acquisition) of firms that received angel investments. Specifically, the subsequent financing is measured by whether a firm conduct another SEC Form D filing or have received next-round of financing as documented in Crunchbase and VentureXpert databases after they received angel financing. I collect data on firms' investor identity and successful exits from Crunchbase and VentureXpert. I match firms in the Crunchbase, VentureXpert, and SEC Form D filings based on firm name and location. I then aggregate the entrepreneurial activity generated by angel-backed firms to the city level.

To examine the impact on local economic activity, I look at the generation of innovation, employment, and sales. For innovation output, I use data from the United States Patent and Trademark Office (USPTO) and calculate the number of patents and the number of patent citations. Data on the employment and sales are obtained from the National Establishment

[15] I set the unit of the analysis to be a city instead of other geographical units for several reasons. First, I did not choose ZIP codes because they are set up for the postal services and sometimes can be too small to be counted as a complete economic cluster. For example, ZIP code 02203 only covers a block in the center of Boston in Massachusetts. Second, I did not choose counties because they can be too large to include many economic clusters like the County of Los Angeles and their boundaries can cut through a economic cluster as in many cases listed here: `https://en.wikipedia.org/wiki/List_of_U.S._municipalities_in_multiple_counties`.

[16] Zillow home value data have been used in many studies (e.g., Bailey, Cao, Kuchler, and Stroebel (2018); Di Maggio et al. (2017); Giroud and Mueller (2017, 2019); Kaplan, Mitman, and Violante (2016); Mian, Sufi, and Trebbi (2015)).

Time-Series (NETS). I match firms in the USPTO database, the NETS database, and the SEC Form D filings based on their name and location.

Finally, to examine the potential substitution effects of reduced angel financing on entrepreneurs' demand for alternative financing sources. I use data on small business loans guaranteed by the Small Business Administration (including both 7(a) and 504 loans). I also examine the impact on second-lien mortgages using data collected under the Home Mortgage Disclosure Act (HMDA).

The unit of analysis in my study is at the city level.[17] I match all the variables using city names and manually check for matching accuracy.[18] To make sure that the results of my study reflect changes in local angel financing, I require sample cities to have at least one angel investment during the sample period. The final sample of this study has 3,896 cities during the time period of 2009 to 2013.

4.2 Variable Construction

4.2.1 Construction of Outcome Variables

The first set of outcome variables in the analysis are related to local angel financing in a city. Two variables are constructed, the natural logarithm of one plus the number of angel investments in city i and time t (*ln(Num+1)*) and the natural logarithm of one plus the amount of angel investments in city i and time t (*ln(Amount+1)*).

To examine the impact of the SEC regulation on local entrepreneurial activity, I use the natural logarithm of one plus the number of firms who have received angel financing in city i and time t (hereafter, angel-backed firms) and received a next-round financing (*ln(Num_next_financing+1)*) later and the number of angel-backed firms that later receive VC investment (*ln(Num_later_VC+1)*) as the outcome variables for subsequent financing (t

[17] The reasons for choosing the city level are stated in footnote 15.

[18] When both ZIP code and city names are provided in a data set, I adjust city names based on the ZIP code-city link table, provided at `https://simplemaps.com/data/us-cities`, to make sure the territory that a name of a city refers to, remains the same during the sample period.

is the time when a firm receives the angel investment not the time when the firm receives next-round financing). Similarly, I use the natural logarithm of one plus the number of angel-backed firms that have an acquisition (*ln(Num_Acq+1)*) later, the natural logarithm of one plus the number of angel-backed firms that have an IPO (*ln(Num_IPO+1)*), and the natural logarithm of one plus the number of angel-backed firms that have either an acquisition or an IPO (*ln(Num_Acq_or_IPO+1)*) as the outcome variables for successful exits. To account for the potential bias that may be created by the truncation problem in the data, I restrict all the above subsequent financing events or successful exits to be observed within five years after the angel investment.

To study the real economic impact of the SEC regulation change on the local economy, I look at the innovation, employment, and sales generated by the angel-backed firms. For innovation output, I use the natural logarithm of one plus the number of patents (*ln(Num_Patents+1)*), the natural logarithm of one plus the number of citations (*ln(Num_total_cites+1)*), and the natural logarithm of one plus the number of citations per patent (*ln(Num_cites_per_patent+1)*) generated by firms who received their angel investments in city i and time t (t is the time when a firm receive the angel investment not the time of the generation of patent, sales, or employment). The above three variables related to patents are adjusted for truncation biases following Hall, Jaffe, and Trajtenberg (2001). For employment and sales, I use the natural logarithm of one plus the number of jobs supported by angel-backed firms in the next year (*ln(Employment+1)*) and the natural logarithm of one plus the amount of sales generated by angel-backed firms in the next year (*ln(Sales+1)*).

To evaluate the impact of the SEC regulation change on small business loans, I construct *ln(Num_SBL+1)*, the natural logarithm of one plus the number of small business loans approved by the SBA, *ln(Amount_SBL+1)*, the natural logarithm of one plus the amount of small business loans approved by the SBA, and *ln(Guaranteed_Amount_SBL+1)*, the natural logarithm of one plus the amount of small business loans guaranteed by the SBA in city i and time t. To examine the impact on second-lien mortgage, I use the number of second-lien mort-

gages (*ln(2ndlien_num+1)*) and the amount of second-lien mortgages (*ln(2ndlien_amnt+1)*) in city i annually.

4.2.2 Construction of the Treatment Variables and Control Variables

I examine how the SEC regulation change in 2011 of removing primary residence from the net worth qualification standard of accredited investors impacted local entrepreneurial activity and the local economy. In this paper, the key variable, which measures the extent of a city being affected by the above SEC regulation change, is a city's home-value-to-net-worth ratio (hereafter, the *HV/NW* ratio). The *HV/NW* ratio is calculated by dividing the weighted-average home value by the weighted-average household net worth in a city. The weighted average of home value in city i is calculated by taking the mean of the Zillow home value index across all ZIP codes in city i using ZIP-code population as the weights. The weighted average net worth in city i is estimated following the procedure specified in Internet Appendix A.

Following the existing literature, I control for a vector of city characteristics that would affect a city's angel financing and economic activity. Control variables include the natural logarithm of a city's population (*Population*), the natural logarithm of a city's average income per person (*Income_per_person*), and the natural logarithm of a city's average home value (*Home_value*). Data on population and income are collected from the IRS and data on home value are collected from Zillow.[19]

4.3 Summary Statistics

Summary statistics are reported in Table 1. To alleviate the concern that the results may be driven by outliers, I winsorize all variables at the 1st and 99th percentiles in the regressions.

[Insert Table 1 about here]

[19] I calculate the income per person by dividing the gross income by the total number of personal exemptions, which approximates the population in the ZIP code according to IRS. I then obtain the city-level income per person by averaging the income per person at the ZIP code level and aggregate the ZIP code-level population to the city level.

As shown in Panel A Table 1, the median of the key variable in the study, the *HV/NW* ratio, which reflects the extent of a city being affected by the above SEC regulation change, is 1.029. This statistic suggests that for a median city in the sample, the average home value is about the same as the average household net worth. Figure 2 shows the geographical variance of the *HV/NW* ratio across the U.S. in 2011.[20] The darkness of the color in the figure reflects the *HV/NW* ratio, with darker colors indicating higher values and reflecting the larger extent of being affect by the regulation change. One can observe from Figure 2 that there is a large variation in the *HV/NW* ratio across U.S. cities: The *HV/NW* ratio is quite high along the west coast (especially in the Bay Area and around Los Angeles) and in cities like Boston and the New York City, but is relatively low in other places like many cities around the Great Lakes.[21]

Panel B of Table 1 reports the statistics on the outcome variables related to local angel financing. On average, a sample city has 1.2 (=0.558*2) angel investments per year and around $5.7 (=2.852*2) million amount of angel investments per year. Panel C of Table 1 reports statistics on variables related to the subsequent financing and the successful exits of the firms that received angel investments. Panel D shows statistics related to the innovation generated, employment supported, and sales generated by the firms that received angel investments. Panel E of Table 1 shows statistics related to small business loans and second-lien mortgages. As reported in Panel F of Table 1, sample cities on average, has a population of 50,000 per year with $38,000 annual income per person and a housing value of $251,000.

[20]Note that the figure is used to illustrate the geographical variation of *HV/NW* ratio across the U.S. and not all cities that have a HV/NW ratio in the figure enter the sample for the later analysis. As stated in section 4.1, I require all cities in the sample to have at least one angel investment during the four-year sample period to address the concern that cities never had any angel investments may contaminate the results. This step excludes many cities with low net worth from the sample.

[21]To show that the impact of the regulation change is not merely a metropolitan phenomenon, I show the *HV/NW* ratio of cities that are within top-30 metropolitan statistical areas (MSA) in Figure B2. Top-30 MSAs are chosen based on the total populations in 2011. We observe that even for these large cities located within MSAs, they have great variation in terms of the extent impacted by the regulation change: Cities in MSAs such as *Minneapolis-St. Paul-Bloomington*, *Chicago-Naperville-Joliet*, and *Detroit-Warren-Livonia* have relatively low *HV/NW* ratios while the ratio is much higher for cities located in MSAs such as *Los Angeles-Long Beach-Santa Ana*, *Orlando-Kissimmee*, and *New York-Northern New Jersey-Long Island*.

5 Impact on Local Angel Financing

5.1 Baseline Results

In this section, I examine whether the 2011 SEC regulation change of removing primary residence from net wealth in the qualification standard for accredited investors has generated impact on local angel financing. I use a DiD approach with a continuous treatment by estimating the following equation:

$$Y_{i,t} = \alpha + \beta ln(HV/NW)_i * Post_t + Controls_{i,t} + \delta_t + \eta_i + \epsilon_{i,t}. \quad (1)$$

where i represents a city and t represents a semiannual time period. $Y_{i,t}$ are the two dependent variables, $ln(Num + 1)_{i,t}$, the natural logarithm of one plus the number of angel investments, and $ln(Amount + 1)_{i,t}$, the natural logarithm of one plus the amount of angel investments in city i and time t. $ln(HV/NW)_i$, the natural logarithm of the HV/NW ratio. $Post_t$ is a dummy that equals one if period t is after 2011 and equals zero otherwise.[22] $Controls_{i,t}$ include $Population_{i,t}$, the natural logarithm of population in city i and time t, $Income\_per\_person_{i,t}$, the natural logarithm of average income per person in city i and time t, and $Home\_value_{i,t}$, the natural logarithm of the average home value in city i and time t. To account for time-specific shocks and time-invariant city unobservable characteristics that may affect the estimation, I include city fixed effects and time fixed effects. In all regressions,

[22] As discussed in footnote 11, there are two important dates regarding the regulation change: July 21, 2010 when the Dodd-Frank Act was passed and December 21, 2011 when the SEC officially announced the amendment to its rules under Securities Act of 1933 as required by the Dodd-Frank Act. I chose the latter date for the following reason. Even though the Dodd-Frank Act could have aroused immediate attention from law firms and institutional investors, the Act requires time for individual investors to learn its provisions, especially for the marginal small angel investors in my study. Also, it was not until late 2011 that detailed definition on net worth and primary home value became clear to the public. Indeed, in Table B10 in Internet Appendix B where the treatment date is moved forward from the second half of 2011 to the second half of 2010 and other dates, I fail to observe any significant impact on angel financing.

I cluster standard errors both at the city level and at the time level.[23]

[Insert Table 2 about here]

Table 2 shows the results. In column (1) and column (3), the dependent variable is the quantity variable of angel financing, *ln(Num+1)*. In column (2) and column (4), the dependent variable is replaced with the amount variable of angel financing, *ln(Amount+1)*. Column (1) and column (2) show the results when controlling for city fixed effects and time fixed effects. Column (3) and column (4) show results with additional demographic control variables. The coefficient estimates on $ln(HV/NW) * Post$ in Table 2 are all negative and significant at least at the 5% level. The magnitude of these estimates suggest that when the HV/NW ratio of a city is 10% higher than the mean in 2011, it on average would experience 0.26% greater decrease in the number of angel investments and a 2.28% greater decrease in the amount of angel investments after the regulation change.

To further ensure that my results satisfy the parallel trend assumption required by the DiD approach, I examine the dynamics of the impact of the SEC regulation change in a regression framework illustrated by the following equation:

$$Y_{i,t} = \alpha + \sum_{t=-5, t\neq 0}^{4} \beta_t ln(HV/NW)_i * Period_t + Controls_{i,t} + \delta_t + \eta_i + \epsilon_{i,t}, \tag{2}$$

where I use a set of dummies that represent each semiannual period ($Period_t$) to replace the time dummy ($Post_t$) in equation (1). The dummy for the event period (i.e., the second-half year of 2011) is dropped to avoid the multicollinearity problem. I control for the same set of variables as in equation (1) with city fixed effects and time fixed effects. Standard errors are double-clustered at the city level and at the time level.

[Insert Figure 3 about here]

Figure 3 plots the coefficient estimates of β_t in equation (2). The left panel of Figure

[23]The significances of coefficient estimates with standard errors clustered only at city level are similar to those double-clustered at city and time level, with some estimates become more statistically significant and some become less but still significant at the 10% level. The results of estimation with standard errors clustered only at the city level are available upon request.

3 shows the results when the dependent variable is *ln(Num+1)* and the right panel shows the results when the dependent variable is *ln(Amount+1)*. In both panels of Figure 3, one can observe that there is no significant trend prior to the regulation change: all of the coefficient estimates of β_t are not statistically less than zero at the 10% significance level. After the regulation change, there is a downward trend in both panels, indicating that the SEC regulation change indeed has a negative effect on angel financing after it took place. The results shown in Figure 3 suggest that the DiD examination satisfies the parallel trend assumption and confirm the finding in Table 2 that cities more affected by the SEC regulation change have experienced a greater decrease in local angel financing.

In Table B1 and Figure B1 in Internet Appendix B, I show the results of using a classic DiD approach to estimate the impact of the SEC regulation change on local angel financing. Specifically, I replace the continuous variable in equation (1), $ln(HV/NW)$, with a dummy variable, $Treat$, which equals one if city i's HV/NW ratio is larger than the median of the HV/NW ratio in the sample in 2011 and equals zero otherwise. The results and conclusion are similar as those using the DiD approach with a continuous treatment variable.

5.2 Additional Tests

Although the above DiD analysis with a continuous treatment has provided suggestive evidence that the 2011 regulation change led to a decrease in local angel financing, there could be concerns that unobservable factors and measurement error affecting the baseline results. In this subsection, I perform several additional tests to address these concerns.

5.2.1 Addressing Concern I: Housing Market Recovery as an Alternative Explanation

The first concern is that the 2011 SEC regulation change was implemented during the recovery of housing market after the Great Recession. One may worry that the decline in angel financing may not be driven by the regulation change but by potential entrepreneurs

switching from obtaining angel financing to mortgaging their housing equity to relax their financial constraints (Corradin & Popov, 2015; S. P. Kerr, Kerr, & Nanda, 2015; Schmalz, Sraer, & Thesmar, 2017).

The first approach to address the concern is to control for short-term housing price changes in the main tests, where the level of housing price has already been controlled.[24] Specifically, I add two control variables, *Home_value_growth_6M*, the changes in the natural logarithm of the housing price in a city in the last six months ($ln(Home\_value_{i,t}) - ln(Home\_value_{i,t-1})$), and the *Home_value_growth_12M*, the changes in the natural logarithm of the housing price in the last year ($ln(Home\_value_{i,t})$-$ln(Home\_value_{i,t-2})$).

The results are shown in Table B2 in Internet Appendix B. After adding *Home_value_growth_6M* or *Home_value_growth_12M* as a control variable (either putting with $Home\_value$ or replacing $Home\_value$), the coefficient estimates on $ln(HV/NW) * Post$ are all significantly negative at 1% significance level when the dependent variable is the number of angel investments and all significantly negative at 5% level when examining the amount of angel investments. The results suggest that the previous results remain after additionally controlling for short-term housing price changes.

The second approach I take to address the concern is to split all the sample cities into two groups based on their housing market growth from the end of 2008 to the end of 2011 (i.e., the period from approximately the end of the crisis to the implementation of the regulation change) and run a sub-sample test. If the alternative explanation were true, I should observe that the baseline results are stronger in cities with a higher housing price growth given that entrepreneurs can borrow more against their housing equity.

In Table B3 in Internet Appendix B, I show that the results of the sub-sample test where the sample cities are sorted into two groups based on their housing price growth from

[24] I want to emphasize that the previous tests have already controlled for the general trend in housing prices from the end of 2008 to the observation time by putting in city fixed-effects and the level of housing prices of a city in a time period (*Home_value*) in the regressions. The results in the previous analyses indicate that after controlling for the trend of housing prices in a city from the end of 2008, the 2011 SEC regulation had a negative impact on cities' angel financing.

the end of 2008 and the end of 2011. I run the sample regressions as in equation (1). The first two columns show the results of cities with an average housing price growth below the median and the last two columns show cities with the growth above the median. When examining the impact on the number of angels, one observes that the coefficient estimate on $ln(HV/NW) * Post$ is -0.029 for lower-price-growth cities and significant at 1% significance level, while the estimate has smaller magnitude (-0.024) and less significance (5%) for higher-price-growth cities. When the dependent variable is the amount of angel investments, the coefficient estimate on $ln(HV/NW) * Post$ is significant at 10% for lower-price-growth cities but insignificant for higher-price-growth cities.

The results of the above two tests suggest it is unlikely that differences in housing market recovery across cities drive the baseline results.

5.2.2 Addressing Concern II: Measurement of the Treatment

The second concern readers may have is that the HV/NW ratio is not a perfect proxy to measure the extent of a city being affected by the SEC regulation change and the measurement error may bias the baseline results.

Specifically, there are two types of questions related to this concern. First, some cities may have both a low net worth level and a low housing price, and hence a high HV/NW ratio, but unlikely to be affected by the regulation change because residents there are not wealthy enough to become accredited investors; in other cases, cities may have both a high net worth level and a high housing price, and hence a high HV/NW ratio, but these very wealthy cities are also less likely to be affected by the SEC regulation change because they already have a large pool of accredited investors to fund entrepreneurial firms such as cities in the Northeast Region and around the Bay Area. I address the above concern by performing tests on samples excluding cities with extreme net worth or housing values. Second, some people may worry that the ratio of mean home value to mean net worth in a city may not reflect the extent that a city being affected by the regulation change given that the wealth of

angel investors is usually at the right tail of the distribution in a city. I address this concern using an alternative measure of the treatment, which is defined as the ratio of top-tier home value to the average net worth of individuals with top-bracket income in a city.

Table B4 in Internet Appendix B shows the results of the robustness test by excluding outlier cities based on their average household net worth in 2011. I use the same empirical specification (the DiD analysis with continuous treatment) as in Table 2. The dependent variable in columns (1)-(3) is *ln(Num+1)* and is *ln(Amount+1)* in columns (4)-(6). In column (1) and column (4), I exclude cities that have the largest 10% of average household net worth in the sample in 2011. In column (2) and column (5), I exclude cities that have the smallest 10% of net worth in the sample in 2011. In column (3) and column (6), cities that either have the largest 10% or have the smallest 10% of net worth in the sample in 2011 are excluded. One can observe that the coefficient estimates on $ln(HV/NW) * Post$ in all columns in Table B4 are significantly negative at least at the 10% significance level. Results shown in Table B4 suggest that the previous results are not driven by cities that have extreme values of net worth.

Table B5 in Internet Appendix B shows the results of the robustness test by excluding outlier cities determined by the average housing value in 2011. The dependent variable in columns (1)-(3) is *ln(Num+1)* and is *ln(Amount+1)* in columns (4)-(6). In column (1) and column (4), I exclude cities that have the largest 10% of housing values in the sample in 2011. In column (2) and column (5), I exclude cities that have the smallest 10% of housing values in the sample in 2011. In column (3) and column (6), cities that have either the largest 10% or have the smallest 10% of housing values in 2011 are excluded. The coefficient estimates on $ln(HV/NW) * Post$ in all columns in Table B5 are significantly negative at least at the 10% significance level. These results show that the baseline findings are not driven by cities that have very high and/or very low average home value.

In Table B6 in Internet Appendix B, I use an alternative measure of the HV/NW ratio. Specifically, I replace the HV/NW ratio with the HV_top/NW_top ratio, which is defined

as the top-tier home value to the net worth of people with top-bracket annual income. I use the top-tier Zillow Home Value Index of a city (typical home value in dollars within 65th to 95th percentile range in a city) as HV_top. NW_top is estimated using a similar methodology as NW, with the only difference that the statistics of the top-bracket income group (i.e., annual gross income of $200,000 or more) are used.[25] The HV/NW ratio and the HV_top/NW_top ratio are highly correlated: they have a correlation coefficient of 0.8. The empirical specification in Table B6 is the same as in the baseline regressions as shown in Table 2. The coefficient estimates on $ln(HV\_top/NW\_top) * Post$ in all columns in Table B6 are all negative statistically significant (at 1% level when the dependent variable is *ln(Num+1)* and at 10% level when the dependent variable is *ln(Amount+1)*). The magnitude of the estimates are also very similar as in Table 2.[26]

Even though the HV/NW ratio is not a perfect measure of the treatment, I use three tests to address the concerns that measurement error of the treatment may bias the baseline results. The results of these tests show that the baseline results are not driven by cities with extreme values of housing prices and/or net worth. I also show that HV/NW ratio is highly correlated with the HV/NW ratio for people with top-tier home values and net worth (the HV_top/NW_top ratio). Using the HV_top/NW_top ratio in the DiD analysis

[25]The Statistics of Income provided by the IRS are listed in two formats: statistics of all gross income classes and statistics of six different gross income classes (under $25,000, $25,000 under $50,000, $50,000 under $75,000, $75,000 under $100,000, $100,000 under $200,000, and $200,000 or more). In my analysis, NW is calculated using the statistics in the first format and NW_top is calculated using those in the second format. The caveat of using the statistics of the top-class income is that when there are less than 20 tax returns for a particular income class, the observations of that class are combined with the next class within the same ZIP code due to privacy concerns. In my analysis, if the top class of a ZIP code is combined as indicated in the data (i.e., tax returns for classes of $100,000 under $200,000 and $200,000 or more are combined), I drop the ZIP code from calculating the city-level weighted-average net worth of the top-income class to make sure that the statistics reflect the wealth of people with annual income of $200,000 or more.

[26]The number of city decreases in Table B6 from Table 2 because of the data feature I mentioned in footnote 25: Some cities do not have enough tax returns in the top-class income group and therefore, do not have an estimate of the net worth of the income group. This issue is unlikely to affect my results because cities without enough tax returns in the top-class are also unlikely to have people qualified as angel investors.

provides similar results of using the HV/NW ratio.[27]

5.2.3 Addressing Concern III: Angel investors Invest Non-Locally

Previous research has shown that entrepreneurial investments tend to be distance-sensitive (Agrawal et al., 2011; Michelacci & Silva, 2007; Stuart & Sorenson, 2005). However, one may worry that angel investors travel to invest in non-local entrepreneurial firms or that entrepreneurs may travel to pitch to non-local angel investors. The concern is that if the capital constraint faced by entrepreneurs can be mostly solved by the traveling of angel investors or entrepreneurs, then the local effects may disappear after controlling for nearby region's effects and the reduction in local angel investments should not be a serious issue. In this subsection, I first show that most of angel investors in the U.S. invest locally. Then, I show that the previous findings still hold after controlling for geographical spillovers from nearby regions (within 25, 50, and 100 miles) in regressions.

Figure 1 shows the distances between angel investors and their portfolio firms using data from Crunchbase. The sample include 8,832 U.S. investor-firm pairs prior to 2014. The figure shows that around 60% of the sample angel-firm pairs have a distance between angel investors and firms of less than 100 miles. Figure 1 suggests that most angel investors in the U.S. invest locally.

Next, I control for geographical spillovers from nearby cities in regressions. The results

[27]The ideal way to identify the treatment is to obtain data on individual or household balance sheet and deed records. However, given the difficulty in obtaining these sensitive data, my paper and Lindsey and Stein (2020) take different approaches to measure the treatment. Their paper uses survey data and estimate the fraction of household affected at the state level. The advantage of their approach is that they can measure the treatment at the state-level relatively accurately (if the survey design and sampling procedure are believed to reflect the properties of the actual population distribution). The main drawback of their approach is that they can only perform the analysis at a macro level without controlling for any local changes or shocks. In addition, they examine aggregated business formation and employment of small firms, but only a small fraction of these firms are angel-backed and these changes may due to other state-level or macro shocks. Mine approach, using the city-level mean HV/NW ratio, although would generate the concerns that I have discussed in this section and tried to address in the paper, enables my analysis to have much more variation across the U.S. and control for other local shocks that may affect the results. Furthermore, most of my analysis focuses on firms that received angel investments and their later performances, therefore, provides more direct evidence of the impact of the regulation change.

are shown in Table B7 in Internet Appendix B. The dependent variable in all columns is $ln(Num+1)$, the natural logarithm of one plus the number of angel investments in city i and time t. In addition to the interaction term $ln(HV/NW) * Post$, I add the interaction terms of the time dummy with the natural logarithm of the average HV/NW ratio in other cities within a 25, 50, and 100 mile radius around city i, $ln(HV/NW)_{25(50,100)Miles} * Post$. I observe that the coefficient estimates on $ln(HV/NW) * Post$, the main DiD estimator of interest, in column (1), (2), and (3) are all significantly negative at least at 10% significance level. The coefficient estimates on the additional interaction terms are also significantly negative at 10% level within 100 miles. The results suggest two findings: first, there are geographical spillover effects of the SEC regulation change on local angel financing from nearby cities; second, even controlling for the above spillover effects, I still find that the SEC regulation change has significantly negative impact on local angel financing.

5.2.4 Addressing Concern IV: Major Cities Angel Investors Located in Driving the Results

Given 37% of angel investors locate in three cities, San Francisco, New York, and Boston (hereafter, "the three" cities; Huang et al. (2017)) and many highest quality entrepreneurial firms located outside these cities receive financing from angels located in these cities, one may question whether the SEC regulation change, which in theory should only affect marginal angel investors, would have enough power to drive the results.

Although angel investors located in "the three" cities consist of a significant portion of the angel investments in the U.S, angel investments are prevalent (outside "the three" cities) across the U.S., partly thanks to the angel tax credit program put forward by several state governments over the past decades (Denes et al., 2020). Furthermore, the focus of this paper is on the marginal angel investors (who are not wealthy enough to make themselves immune to the regulation change) and the marginal entrepreneurial firms (who cannot clearly signal their quality and receive non-local angel or VC investments) that would be affected by the

regulation change when excluding home value from the net worth calculation of accredited investors. If the SEC regulation change indeed had impact on angel investment, one should not observe "the three" cities or cities near them driving the results. In other words, the previous results should hold if I exclude "the three" cities or cities near them when performing the same analysis as in equation (1).

The results are shown in Table B8 in Internet Appendix B. In columns (1) and (2), I exclude San Francisco, New York, and Boston in the sample. In columns (3) and (4), I also exclude cities within 100 miles to "the three" cities. The dependent variables are the number and the amount of angel investments in a city in a time period. The coefficient estimates on $ln(HV/NW) * Post$ are all negative statistically significant with similar magnitude as in Table 2 in all four columns.[28] These results suggest that the 2011 SEC regulation change indeed had negative impact on local angel investments, especially in regions that are not within the radius of San Francisco, New York, and Boston.

5.2.5 Placebo Test: Impact on Non-Angel or Later-stage Investments

I perform a placebo test to examine whether the SEC regulation change had any impact on non-angel or later-stage investments. The reason for doing this test is that the 2011 SEC regulation change which mainly affected marginal angel investors should not have any significant impact on non-angel investments.

The results of the placebo test are shown in Table B9 in Internet Appendix. The dependent variable in column (1) is the number of investments made by VC firms in city i and time t.[29] The dependent variables in columns (2) and (3) are the natural logarithm of

[28] When I change the radius to 50 miles or 150 miles, the coefficient estimates are all significant at least at 10% significance level. The results are not shown in the paper due to space limit, but are available upon request.

[29] The data on VC investments are combined from Crunchbase and VentureXpert.

one plus the number or the amount of non-first SEC Form D filings.[30] All of the coefficient estimates on $ln(HV/NW) * Post$ in Table B9 are not statistically significant at 10% level, suggesting that the SEC regulation change did not have significant impact on later-stage financing for entrepreneurial firms.

5.2.6 Placebo Test: Using Pseudo Event Times

Some people may worry that other contemporaneous events which happened at the same time as the 2011 SEC regulation change may contaminate the previous finding. Below I conduct a placebo test where different pseudo event times that are prior to the actual event are used in the baseline regressions.

The results are shown in Table B10 in Internet Appendix B. The previous *Post* variable is replaced with four dummy variables, *Post_09H2*, *Post_10H1*, *Post_10H2*, and *Post_11H1*. *Post_09H2* equals one if a time period is after second half of 2009 and zero otherwise. Using *Post_09H2* instead of *Post* assumes that the event time is moved two years forward (from the second half of 2011 to the second half of 2009). The other three dummy variables are defined in a similar way, assuming the event time is moved prior to the actual event time. The dependent variables are the number and amount of angel investments in a city in a time period. In all the eight columns in Table B10, the coefficient estimates on the interaction term between $ln(HV/NW)$ and the pseudo event-time dummy are not statistically significant at 10% level. The results suggest that falsely assumed regulation change does not exhibit any significant effect on local angel financing.

[30] Although I can not fully rule out the possibility that non-first Form D filings also have angel investors participated in, the difficulty of getting capital will decreases significantly after firms have already approached investors once. Therefore, I am using the non-first SEC Form D filings as a dependent variable for the placebo test.

5.2.7 Heterogeneous Impact by Firm Age

The SEC regulation change may have heterogeneous impact on the financing of firms of different ages and industries. In Table B11 in Internet Appendix B, I categorize firms into three age groups, less than three years, three-to-five years old, or more than five years when they received the angel investments and examine the impact the SEC regulation change of these three groups of firms. The results suggest that the SEC regulation change had a negative impact on angel financing for firms of all age groups. The negative impact is larger for firms that are either very young or over five years old.

Overall, the above analyses suggest that the 2011 SEC regulation change of removing primary residence from net wealth in the qualification standard for accredited investors generated a heterogeneous and negative impact on local angel financing across cities in the U.S.

6 Impact on Local Entrepreneurial Activity

Next, I examine the impact of 2011 SEC regulation change on local entrepreneurial activity using the same empirical specification as illustrated by equation (1). Specifically, I look at the subsequent financing and successful exits for investors (i.e., IPO or Acquisition) of firms that received angel investments.

[Insert Table 3 about here]

Table 3 examines whether the SEC regulation has generated impact on local entrepreneurial activity in terms of the subsequent financing of angel-backed firms. The dependent variable in column (1) is *ln(Num_next_financing+1*, the natural logarithm of one plus the number of firms that received their angel investments in city i and time t, and received next-round financing within five years. The dependent variable in column (2) is *ln(Num_later_VC)+1*, the natural logarithm of one plus the number of firms that received their angel investments in city i and time t, and received at least one investment from VC within five years after.

The coefficient estimates on $ln(HV/NW) * Post$ in both columns are significantly positive at the 5% significance level and at the 10% significance level, respectively. The magnitude of the above coefficient estimates suggests that when a city has a 10% higher HV/NW ratio, it on average experiences a 0.15% greater decrease in the number of angel-backed firms that received next-round financing and 0.08% greater decrease in the number of angel-backed firms that later received VC investment after the SEC regulation change.

[Insert Table 4 about here]

Table 4 shows the results of how the SEC regulation has affected local entrepreneurial activity in terms of successful exits of firms that received an angel investment. The dependent variable in column (1), *ln(Num_Acq+1)*, is the natural logarithm of one plus the number of firms that received their angel investments in city i and time t and have an acquisition within five years after. The dependent variable in column (2) is $ln(Num\_IPO + 1)$, the natural logarithm of one plus the number of firms that received their angel investments in i and time t and have an IPO within five years after. The dependent variable in column (3) is *ln(Num_Acq_IPO+1)*, the natural logarithm of one plus the number of firms that received their angel investments in city i and time t and have an acquisition or an IPO within five years after. The coefficient estimates on $ln(HV/NW)_i * Post_t$ in all columns are significantly negative at the 5% significance level. The magnitude of the above coefficient estimates suggests that when a city has a 10% higher home-value-to-net-worth ratio, it on average experiences a 0.06% greater decrease in the number of angel-backed firms that have an acquisition, a 0.05% greater decrease in the number of angel-backed firms have an IPO, and a 0.08% greater decrease in the number of angel-backed firms have an acquisition or an IPO after the regulation change.

In Table B12 in Internet Appendix B, I examine how the SEC regulation change have affected the rate of receiving subsequent financing and the rate of having a successful exit conditional on firms that have received an angel investment. The coefficient estimates on $ln(HV/NW) * Post$ are all negative, providing suggestive evidence that the SEC regulation

change did not successfully select firms based on their potential for future successful exit for their investors. The results are consistent with the discussion in Hall and Lerner (2010) that the prospects of start-up firms are highly uncertain and thus hard to screen at their early stages.

The above results suggest that the regulation change of restricting the definition of accredited investors had a negative impact on local entrepreneurial activity generated by angel-backed firms.

7 Real Economic Impact

I then examine how the 2011 regulation change has impacted the local economy in terms of innovation, employment, and sales generated by the local firms that received angel financing.

[Insert Table 5 about here]

Table 5 presents the results of examining whether the SEC regulation change has impacted the innovation generated by local angel-backed firms. In column (1), the dependent variable is the natural logarithm of one plus the number of patents generated by firms that received their angel investments in city i and time t, *ln(Num_patents+1)*. The coefficient estimate on $ln(HV/NW) * Post$ in column (1) is significantly negative at the 1% significance level. In column (2), I replace the dependent variable with the natural logarithm of one plus the number of patent citations received by angel-backed firms in city i and time t, *ln(Num_total_cites+1)*. In column (3), the dependent variable is the natural logarithm of one plus the average number of citations per patent received by firms that received their angel investments in city i and time t, *ln(Num_cites_per_patent+1)*. The coefficient estimates on $ln(HV/NW) * Post$ in columns (2) and (3) are both negative and significant at the 5% level. The magnitudes of the coefficient estimates suggest that when a city has a 10% higher HV/NW ratio, it on average experiences a 0.2% greater decrease in the total number of patents, a 0.01% greater decrease in the total number of patent citations received, and a

0.004% greater decrease in the number of citations per patent received by firms that received an angel investment after the 2011 regulation change than those firms receive investments before the regulation change.

[Insert Table 6 about here]

Table 6 presents the results of examining whether the SEC regulation change has affected the total employment supported and total sales generated by local angel-backed firms. The dependent variable in column (1) is the natural logarithm of one plus the number of jobs supported in the next year by firms that received their angel investments in city i and time t, *ln(Employment+1)*. The coefficient estimate in column (1) is significantly negative at the 1% significance level. The magnitude of the estimate in column (1) suggests that when a city has a 10% higher HV/NW ratio, it exhibits a 0.65% greater decrease in the number of jobs supported in the next year by local angel-backed firms after the regulation change. In column (2), I replace the dependent variable with the natural logarithm of one plus the amount of sales in the next year generated by angel-backed firms in city i and time t, *ln(Sales+1)*. The coefficient estimate on $ln(HV/NW) * Post$ in column (2) is both negative and significant at the 5% level. The magnitude of the estimate suggests that when a city has a 10% higher HV/NW ratio, it exhibits a 2.26% greater decrease in the amount of sales generated in the next year by local angel-backed firms after the regulation change.

The results provide evidence that the SEC regulation change imposed a real economic cost on the local economy in terms of innovation, employment, and sales generated by firms that received angel financing.

8 Impact on Demands for Alternative Financing Sources

After establishing the fact that the 2011 SEC regulation has indeed generated a negative impact on angel financing, a natural question would be whether there are any substitution effects of the reduction in angel financing on entrepreneurs' demand for other financing

sources, among which I specifically focus on small business loans and second-lien mortgages. Addressing this question has two purposes: First, it validates my previous findings that entrepreneurs searched for alternatives when the availability of angel financing declined; second, it shows the potential unintended consequences of the regulation change through these alternative financing channels.

8.1 Small Business Loans

When the supply of angel financing is reduced, one important alternative financing source for entrepreneurs is the small business loans guaranteed by the Small Business Administration. In this section, I test whether the 2011 SEC regulation change on the definition of accredited investors had impact on small business loans. I collect small business loan data from Small Business Administration during the sample period of 2009 to 2013. I identify the location of borrowers and aggregate the loan data at the city-semiannual level. I use the same empirical specification as illustrated by equation (1) and look at whether the SEC regulation change had generated a substitution effect on small business loans.

[Insert Table 7 about Here]

Table 7 shows the results. The dependent variable in column (1) is the natural logarithm of one plus the number of approved small business loans applied in city i and time t, *ln(Num_SBL+1)*. The coefficient estimate on $ln(HV/NW)_i * Post_t$ is both positive and significant at the 5% significance level. The magnitude suggesting that when a city had a 10% larger HV/NW ratio prior to the SEC regulation change, the number of small business loans increased 4.24% in the city after the SEC regulation change. In column (2) and column (3), I replace the dependent variables with the natural logarithm of one plus the amount of small business loans, *ln(Amount_SBL+1)*, and the natural logarithm of one plus the amount of small business loans guaranteed by the Small Business Administration, *ln(Guaranteed_Amount_SBL+1)*, respectively. The coefficient estimates on $ln(HV/NW) * Post$ in both columns are positive and significant at least at the 5% signifi-

cance level, suggesting that cities more affected by the SEC regulation change experienced larger increases in both the total amount of small business loans and the amount of these loans guaranteed by the government after the regulation change. However, the above results need to be carefully interpreted as small business loans can only partially substitute angel financing and they also bear costs. More discussion is in subsection 8.3.

8.2 Second-Lien Mortgages

Previous literature has shown the importance of the availability of bank loans (Rice & Strahan, 2010) and housing mortgages (Corradin & Popov, 2015; S. P. Kerr et al., 2015; Schmalz et al., 2017) for entrepreneurship. Entrepreneurs can seek a second mortgage (or a second-lien mortgage) provided by local financial institutions as an alternative financing source when it is hard to obtain angel financing. Second-lien mortgages tap into the equity of a house, which is the market value of a home minus loan balances.

I examine the impact 2011 regulation change, which reduced local angel financing, on the demand for second-lien mortgages. The mortgage data are collected under the Home Mortgage Disclosure Act (HMDA). I aggregate mortgage applications with a lien status specified as "subordinate lien" in the HMDA data to the city-year level from 2009 to 2013.[31] Specifically, I construct two variables from the HMDA data: *ln(2ndlien_num+1)*, the natural logarithm of one plus the number of second-lien mortgages applied in city i and time t, and *ln(2ndlien_amnt+1)*, the natural logarithm of one plus the amount of second-lien mortgages applied in city i and time t.

[Insert Table 8 about here]

Results are reported in Table 8. The dependent variable is *ln(2ndlien_num+1)* in column (1) and is *ln(2ndlien_amnt+1)* in column (2). The coefficient estimate on $ln(HV/NW)_i * Post_t$ is positive and significant at the 1% significance level in column (1). The magnitude suggests that when a city had a 10% larger home-value-to-net-worth ratio, it on average

[31] I switch the frequency of observations from semiannual to annual because HMDA only reports the year of the mortgage application during the sample period.

experienced an 1.84% increase in the number of second-lien mortgage applications after the SEC regulation change in restricting the definition of accredited investors. In column (2), the coefficient estimates on $ln(HV/NW) * Post$ is positive and significant at the 5% significance level, suggesting that when a city had a 10% larger home-value-to-net-worth ratio, it on average experiences a 2.64% increase in the amount of second-lien mortgage applications after the SEC regulation change. More discussion on the interpretation of the above results is in the subsection below.

8.3 Discussion on the Alternative Financing Sources

The above results provide suggestive evidence that the 2011 SEC regulation change had impact on alternative financing sources such as small business loans and second-lien mortgages. However, there are two points that need a discussion when examining these two alternative sources.

First, given the differences between debt and equity financing, borrowing either from government-sponsored loans or home equity loans is by no means the same as financing through angel capital (Schwienbacher, 2007; Winton & Yerramilli, 2008). One difference is that creditors usually require a firm or an entrepreneur to have good credit, clear ability to repay, and an operating history.[32] In other words, firms with higher risks such as those in the technology sector could have a hard time finding a substitute for angel financing. In addition, previous literature has shown that early-stage investors such as VC and angels differentiate themselves from creditors as they provide value-added services and perform monitoring on their portfolio firms (Hellmann & Puri, 2002; W. R. Kerr et al., 2014). Hence, more than just providing funds to a firm, angel investors can also influence the growth and the outcome of a firm. The above reasons explain why the two alternative financing sources may not perfectly substitute angel investments.

Second, even though credit provided from alternative financing sources can help en-

[32]One example illustrating what the lenders of Small Business Admission Loan Program seek can be found at `https://www.sba.gov/sites/default/files/SDOLoanFactSheet_Oct_2011.pdf`

trepreneurs partially loosen financial constraints, these loans also present potential concerns. One concern relates to the efficient usage of government funding (Babina et al., 2020). Taxpayers pay for the cost if firms borrowing from the government-sponsored loans turn out to be unsuccessful. Brown and Earle (2017) estimate that the taxpayer cost per job created from small business loans is at least $21,000. Another concern regarding the second-lien mortgages is that they can make borrowers more vulnerable when they encounter financial difficulties and increase financial risks for the economy (Elul et al., 2010). This concern is related to a broader strand of literature discussing systemic risks accumulated in the housing market and the financial system (Bullard, Neely, & Wheelock, 2009; Geanakoplos et al., 2012), which is beyond the scope of this paper but important to consider.

9 Costs and Benefits of the 2011 SEC Regulation Change

The previous results in this paper suggest that increasing investor protection induced by a 2011 regulation change lead to a reduction in angel financing and entrepreneurial activity, which, in turn, imposed real costs on the economy. In this section, I first look at the other side of the trade-off between investor protection and the promotion of entrepreneurial activity. Specifically, I attempt to estimate the benefits of the above regulation change in terms of avoiding losses of angel investors through investment in unsuccessful entrepreneurial firms. I also provide an estimation of costs of the above SEC regulation change in terms of lost sales, innovation, and employment generated by entrepreneurial firms that did not received angel financing. Finally, I perform a cost-benefit analysis under different assumptions and discuss the results.

9.1 Estimation of Benefits of the 2011 Regulation Change

The main pecuniary benefit of the 2011 SEC regulation change is that it can prevent the later-unqualified angel investors from investing in firms that would have turned out to be unsuccessful. I estimate this benefit for each city by calculating a city's reduced amount of

investment due to the 2011 regulation change multiplied by the failure rate of angel-backed firms in the city as following:

$$Benefit_{i,t} = \Delta(Amount)_{i,t} * Failure\_rate_{i,t} \tag{3}$$

The failure rate in city i and time t, $Failure\_rate_{i,t}$, is calculated by dividing the number of angel investments in city i and time t that did not receive next-round financing within the next five years by the total number of angel investments in city i and time t.[33] The reduced amount of angel investment of city i in time t, $\Delta(Amount)_{i,t}$, is the difference between the estimated amount of investment if there was no SEC regulation change and the actual amount of investment with the above regulation change. Specifically, the reduced amount of investment is estimated as below:

$$\Delta(Amount)_{i,t} = exp\left[|\hat{\beta}| * \frac{HV}{NW}_i + ln(Amount+1)_{i,t}\right] - exp\left[ln(Amount+1)_{i,t}\right], \tag{4}$$

and $\hat{\beta}$ in equation (4) is obtained from the estimation of the following equation:[34]

$$ln(Amount+1)_{i,t} = \alpha + \beta \frac{HV}{NW}_i * Post_t + Controls_{i,t} + \delta_t + \eta_i + \epsilon_{i,t}, \tag{5}$$

where the dependent variable is the natural logarithm of one plus the amount of angel investments in city i and in time t ($ln(Amount+1)_{i,t}$) with other variables are defined in section 4. The estimated β in equation (5) is shown in Table B13 column (1) in Internet Appendix B. After obtaining the estimated benefits for each city in each time period, I aggregated these benefits to the national level annually.

Following the above procedure, the estimated benefits of preventing marginal angel

[33] Although a firm could still be operating without receiving next-round financing within the next five years, it is considered as a failure for angel investors because they can not successfully exit the investment.

[34] I use the *HV/NW* ratio instead of the natural logarithm of the ratio ($ln(HV/NW)$) as in equation (1) simply for illustration purpose: When the *HV/NW* ratio is less than one, *ln(HV/NW)* is negative and hard to interpret in equation (4). The estimated amount of reduced angel investment, however, is very similar when I use *ln(HV/NW)* and it does not affect the conclusion of the cost-benefit analysis.

investors from investing in firms that would have turned out to be unsuccessful is $3.19 billion in 2012 and $3.08 billion in 2013 nationally. The estimated benefits account for 8.2%(=$3.19 billion/$38.9 billion) of the total amount of angel investments in 2012 and 4.4%(=$3.08 billion/$69.8 billion) of the total amount of angel investments in 2013. It is worth-noting that the above estimate is likely to be the upper bound of the actual benefit because the failure rate of firms that received angel financing (i.e., observable firms) is used for firms that did not receive angel financing (i.e., unobservable firms) in the estimation. However, the unobserved failure rate of firms that did not get angel financing due to the regulation is likely to be lower: According to Table B12, the rate of successful exits (one minus the failure rate) for firms that received angel financing decreased due to the regulation change, thus a higher failure rate was used in the estimation.

[Insert Table 9 about here]

Next, I calculate the present value of the benefits of the SEC regulation change in the following years at the end of 2011. I use the previously estimated benefits in 2012 and 2013 and assume the impact of the regulation change will last for 10 years, 5 years, or 3 years. The estimation of the present value of benefits is shown in Panel A of Table 9 with different assumptions on the discount rate ranging from 5% to 30%. The estimated present value of benefits takes a value from $5.68 billion (in the lower right corner of Panel A, assuming the discount rate is 30% and the impact of the regulation change lasts for 3 years), to $23.89 billion (in the upper left corner of Panel A, assuming the discount rate is 5% and the impact of the regulation change lasts for 10 years).

9.2 Estimation of Costs of the 2011 Regulation Change

Following the same strategy, I estimate the costs of the SEC regulation change in terms of reduced sales generated by firms that did not receive angel financing due to the regulation change. Specifically, I estimate equation (4) and equation (5) with replacements of the

variable $Amount_{i,t}$ with $Sales_{i,t}$.[35] The estimated reduced sales due to the SEC regulation change are \$0.73 billion for in 2012 and \$1.05 billion in 2013.[36]

If assuming these affected firms would operate for 10 years without the regulation change, we can obtain the present value of the reduced sales in each year. For example, with additional assumptions of a discount rate of 15% and a growth rate of sales of 5% per year, the present value of forgone future sales is \$4.74 billion in 2013.[37] The estimated costs are likely to be a lower bound of the actual costs of the regulation change. The reason is similar as what has been discussed in subsection 9.1: The quality of firms received angel financing after the regulation change is assumed to be the same as the quality before the change, while the quality of firms actually declined after the regulation change according to Table B12. Therefore, the actual foregone sales, innovation, and employment of firms that did not receive angel financing could be larger than the estimation given above.

Similar to the estimation of benefits, I then calculate the present value of costs of the SEC regulation change in terms of reduced sales at the end of 2011. I use the previously estimated costs in 2012 and 2013 and assume that annual reduced sales in years after 2013 are the same as in 2013 to simplify the analysis. Panel B of Table 9 shows the estimation results with different assumptions on the discount rate (ranges from 5% to 30%), growth rate (ranges from 0% to 25%), and the length of the regulation change lasts (3, 5, or 10 years).

I also estimate the reduced innovation output and employment generated or supported by entrepreneurial firms, using the above strategy with a replacement of the variable $Amount_{i,t}$ with $Num\_patents_{i,t}$ and $Employment_{i,t}$.[38] I estimate that the SEC regulation change led to 292 fewer patents generated by the angel-backed firms in 2012 and 289 fewer patents in

[35] The results of the estimation of equation (5) are shown in Table B13 column (2) in Internet Appendix B.

[36] The reduced sales of \$1.05 billion that would be generated by firms that did not get angel financing in 2013, but not include those who were affected in 2012. Therefore, when calculating the total present value of reduced sales, all years of reduced sales need to be discounted and aggregated (not only the last year).

[37] The \$4.74 billion is calculated from the formula: $\frac{P}{r-g} * \left(1 - \frac{(1+g)^n}{(1+r)^n}\right) = \frac{1.05}{0.15-0.05} * \left(1 - \frac{(1+0.05)^{10}}{(1+0.15)^{10}}\right)$.

[38] The estimates of β in equation (5) are shown in Table B13 column (3) and column (4) in Internet Appendix B.

2013, and reduced about 3,770 jobs supported by the angel-backed firms in 2012 and around 4,392 jobs in 2013. These reduced patents and employment by entrepreneurial firms are also costs brought by the 2011 SEC regulation change in the private equity market.

9.3 Cost-Benefit Analysis and Discussion

I then perform an analysis using the above estimated present values of the costs of reduced sales and the benefits of preventing angel investment in unsuccessful firms for the 2011 SEC regulation change under different assumptions.

The estimated net benefits of the SEC regulation change are shown in Table 9 Panel C. To ensure that the conclusion of the analysis is not driven by a specific set of assumptions, I show results under various combinations of discount rates (5%, 10%, 15%, 20%, 25%, 30%) and growth rates (0%, 5%, 10%, 15%, 20%, 25%) for entrepreneurial firms with different lengths of the impact would last (10 years, 5 years, and 3 years). One can observe from Table 9 Panel C that the estimated net benefits of the SEC regulation change are negative in 58 out of 63 scenarios. For example, assuming the discount rate is 15%, the growth rate of sales is 10%, and the impact of the SEC regulation change would last for 10 years, the present value of net benefits is -20.27 billion dollars at the end of 2011. The only 5 scenarios where the net benefits are positive are those when the discount rate is relatively high and the growth rate is near zero, which is unlikely to be the case for entrepreneurial firms who usually enjoy very high growth rates.

As mentioned in the previous two subsections, the estimated benefits of the 2011 SEC regulation change are likely to be an upper bound of the actual benefits while the estimated costs tend to be a lower bound of the actual cost. Therefore, it is safe to draw the conclusion from the above analysis that the cost exceeds the benefit of the SEC regulation change in most cases from a pecuniary viewpoint, not to mention the costs in terms of the reduced innovation output and employment supported by entrepreneurial firms that would have received angel financing without the regulation change.

It is important for readers to notice two major limitations of the above analysis and carefully interpret its results.[39] First, the above cost-benefit analysis mainly focuses on the pecuniary aspect due to data and measurement limitations. There can be other costs and benefits of investor protection regulations that are not included in the analysis but also important to take into consideration when making policies. For example, other benefits of the 2011 SEC regulation change may include the prevention of bad social consequences for small investors when they lose their primary residence due to investing in unsuccessful firms. Other costs may include the loss of technological spillover effects from high-tech start-ups to ordinary firms because there are less start-ups being funded by angel investors. Second, the above analysis is a partial-equilibrium analysis and it ignores the feedback effects from other players in the market that might also affect the performance and failure rate of entrepreneurial firms.

10 Policy Implications

This paper adds to the debate about the trade-off between investor protection in the private market and promotion of entrepreneurial activity. How can the government potentially encourage entrepreneurship? What are the important aspects that need to be considered when making policies and regulations related to entrepreneurs and early-stage investors? The policy implications from this paper are as follows.

First, the government could encourage more private investment into entrepreneurial firms by allowing more angel investors to invest in these firms. However, there is always a cost arising from potential losses of angel investors through the failure of their portfolio firms. The results in this study show that the 2011 SEC regulation change reduced local angel financing received by entrepreneurial firms, and, in turn, led to reductions in the innovation, sales, and employment generated by entrepreneurial firms.

[39] Even though a cost-benefit analysis can be tentative as it relies on many assumptions, it is still important for providing policy evaluations and implications. Other studies have conducted cost-benefit analyses similar to mine (but in very different contexts): see, e.g., Hombert, Schoar, Sraer, and Thesmar (2020).

Second, the government could provide more funding to small businesses through government-lead VCs or direct lending through agencies like the Small Business Administration (SBA). This study shows that the 2011 SEC regulation change has a substitution effect on small business loans guaranteed by the SBA. The government should be aware of these potential substitution effects when developing policies regarding protecting investors or promoting entrepreneurial activity. Also, promoting debt financing and equity financing may have different compositional effect on the industries and riskiness of firms being funded.

Third, the government needs to be aware of the potential financial risks incurred by mortgages applied for by entrepreneurs when angel financing is reduced. The reason is that starting up a business is inherently risky and entrepreneurs bear higher risks than people with other occupations (Vereshchagina & Hopenhayn, 2009). Therefore, it is important for regulators to monitor the loans applied for by entrepreneurs and be aware of the potential for higher systemic risks in the mortgage market.

11 Conclusion

This paper studies how an SEC investor protection regulation change in 2011 required by the Dodd-Frank Act affected local angel financing and its real economic consequences in the local economy. Relying on the heterogeneous impact of the SEC regulation change of removing the primary residence from net wealth standard for accredited investors, I use a DiD approach and find that cities more affected by the SEC regulation change, experienced a significantly larger decrease in local angel financing and local entrepreneurial activity generated by angel-backed firms. I further show that the SEC regulation change imposed a real cost on the local economy in terms of the innovation, employment, and sales generated by angel-backed firms. A number of additional tests suggest that the results are likely to be causal. I also show that there were substitution effects between reduced angel financing and alternative financing sources such as small business loans guaranteed by the SBA and second-lien mortgages.

Additionally, I provide an estimation of the pecuniary benefits of the regulation change by avoiding angel investors' losses through investment in unsuccessful firms and an estimation of the costs of the above regulation change in terms of the reduced sales, patents, and employment by angel-backed firms. The cost-benefit analysis suggests that the monetary costs of protecting angel investors seem to outweigh its benefits in most scenarios. My paper contributes to the literature on early-stage investors, investor protection in the private equity market, and governments' role in promoting entrepreneurial activity. It provides new evidence to the debate about the trade-off between protecting investors and promoting entrepreneurial activity.

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Figures and Tables

Figure 1. Distances Between Angel Investors and Their Portfolio Firms

This figure shows the distribution of distances in miles between angel investors and their portfolio firms. Data are collected from Crunchbase. I include all U.S. firms that are available in the Crunchbase dataset and have received investments from angel investors in the U.S. prior to 2014. The sample contains 8,832 investor-firm pairs in total.

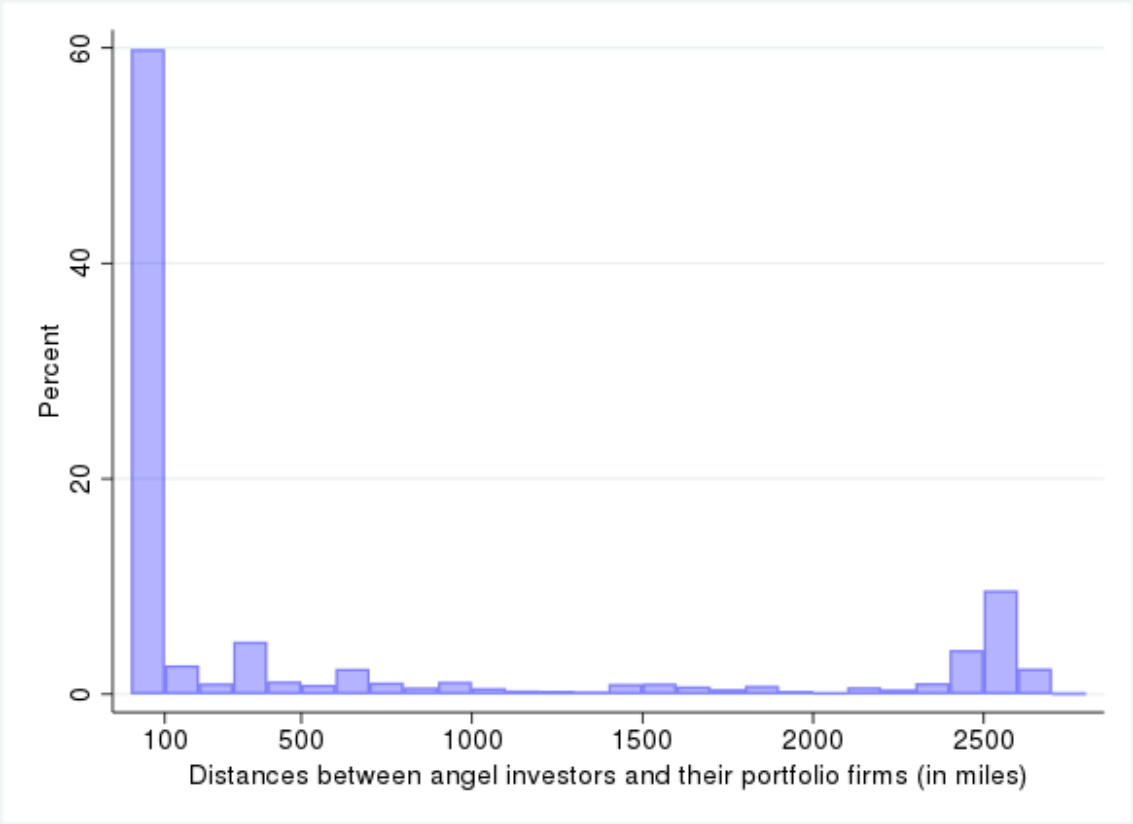


Figure 2. Geographical Variation of the Home-Value-To-Net-Worth Ratio in 2011

This figure shows the geographical variance of the HV/NW ratio across the U.S. in 2011. The darker the color represents a higher HV/NW ratio. The HV/NW ratio is calculated by diving the average home value in a city by the average household net worth in the city. The average home value in city i is calculated by averaging the Zillow home value index across all ZIP codes in city i. The average net worth in city i is estimated by combining data from SIPP and IRS following the procedure specified in Appendix A.

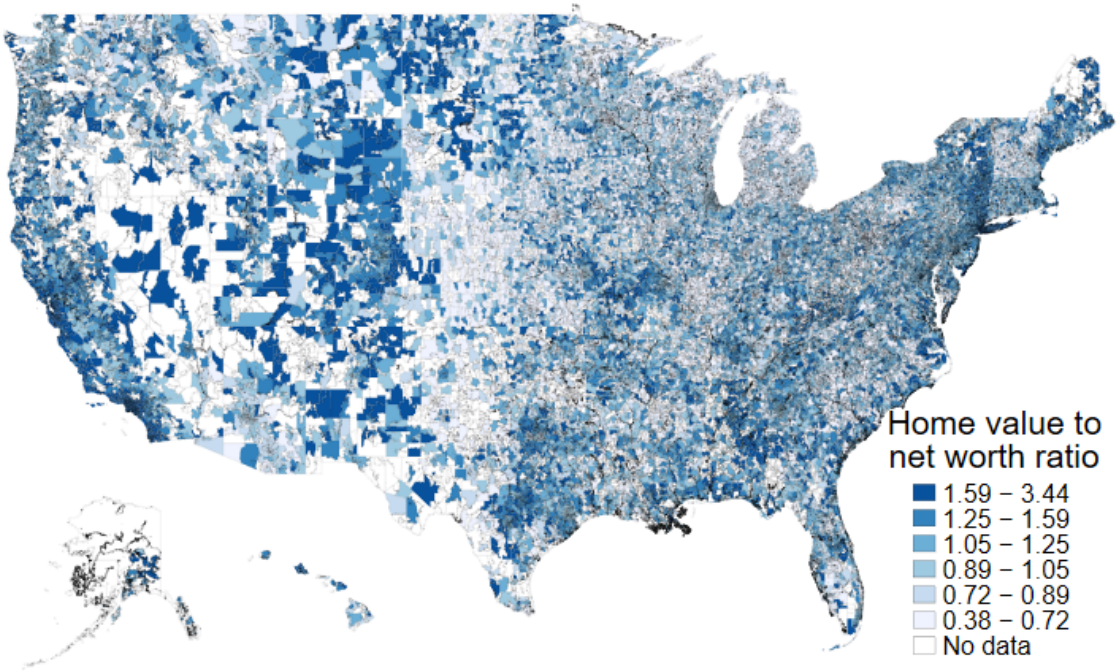


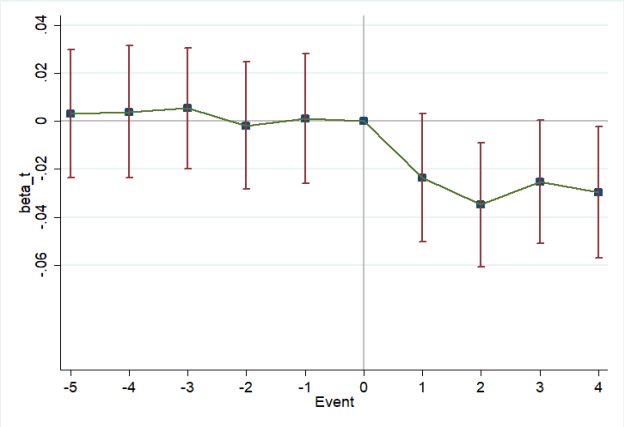
Figure 3. Plot of Coefficients Around the Event Time

The figure shows the coefficients plot around the SEC regulation change in 2011 by estimating the following model:

$$Y_{it} = \alpha + \sum_{t=-5, t\neq 0}^{4} \beta_t ln(HV/NW)_i * Period_t + Controls_{i,t} + \delta_t + \eta_i + \epsilon_{it}$$

where $Period_t$ is a set of dummy variables that equals one if a city-half-year observation is from the time unit t. For example, $Period_1$ equals one if observations are from the first-half year of 2012. The benchmark group comprises of observations from the event period (the second half of 2011, $t = 0$). Panel (a) shows the plot of estimates of β_t when the outcome variable is the natural logarithm of one plus the number of angel investments. Panel (b) shows the plot of estimates of β_t when the outcome variable is the natural logarithm of one plus the amount of angel investments. The center points show the point estimates of β_t and the vertical lines denote the 90% confidence intervals of β_t estimates.

(a) Number of Angel Investments



(b) Amount of Angel Investments

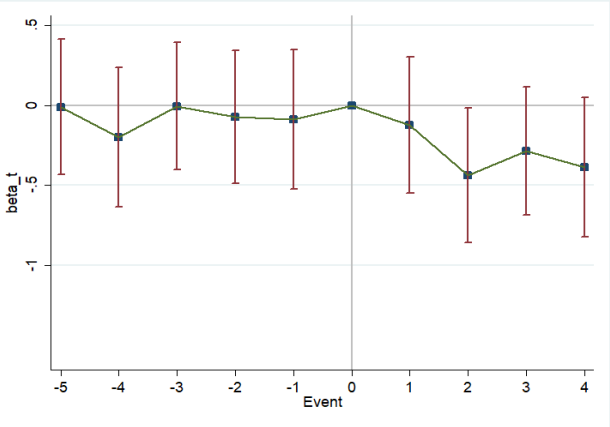


Table 1. Summary Statistics

This table displays the summary statistics for the variables used in this study. Panel A reports statistics on the treatment variable. Panel B reports statistics on the outcome variables related to local angel investments. Panel C reports statistics on the subsequent financing received by and the successful exits of firms obtained angel investments. Panel D shows statistics on the innovation generated, employment supported, and sales generated by firms obtained angel investments. Panel E shows statistics related to small business loans and second-lien mortgages. Panel E reports statistics on control variables. Data sources are introduced in section 4.1. All observations are at the city-semiannual level within the period from 2009 to 2013.

	N	Mean	Std. Err.	Min	Median	Max
Panel A: Treatment variable						
HV/NW	38,960	1.147	0.532	0.378	1.029	3.440
Panel B: Angel investments						
Num	38,960	0.558	1.661	0.000	0.000	12.000
Amount ($million)	38,960	2.852	12.586	0.000	0.000	100.488
Panel C: Entrepreneurial activity (subsequent financing and successful exits)						
Num_next_financing	38,960	0.173	0.605	0.000	0.000	4.000
Num_later_VC	38,960	0.045	0.252	0.000	0.000	2.000
Num_IPO	38,960	0.005	0.087	0.000	0.000	4.000
Num_Acq	38,960	0.018	0.216	0.000	0.000	11.000
Num_Acq_or_IPO	38,960	0.022	0.251	0.000	0.000	12.000
Panel D: Economic activity (innovation, employment, and sales)						
Num_patents	38,960	0.068	0.375	0.000	0.000	3.017
Num_total_cites	38,960	0.001	0.009	0.000	0.000	0.076
Num_cites_per_patent	38,960	0.001	0.006	0.000	0.000	0.045
Employment	38,960	6.907	50.408	0.000	0.000	3,306.044
Sales ($million)	38,960	0.662	6.067	0.000	0.000	494.742
Panel E: Small business loans and second-lien mortgages						
Num_SBL (million)	38,960	0.088	0.165	0.000	0.037	1.150
Amount_SBL ($million)	38,960	1.797	3.666	0.000	0.379	24.000
Guaranteed_Amount_SBL ($million)	38,960	1.003	2.135	0.000	0.140	13.724
2ndlien_num (000s)	19,375	0.063	0.170	0.000	0.023	5.660
2ndlien_amnt ($million)	19,375	3.499	10.545	0.000	1.217	447.397
Panel F: Control variables						
Population (million)	38,214	0.050	0.127	0.000	0.022	2.756
Income_per_person ($million)	38,214	0.038	0.031	0.009	0.030	0.746
Home_value ($million)	38,960	0.251	0.206	0.025	0.189	2.998

Table 2. Impact on Local Angel Financing

This table shows the results of the DiD analysis by estimating the following model:

$$Y_{i,t} = \alpha + \beta ln(HV/NW)_i * Post_t + Controls_{i,t} + \delta_t + \eta_i + \epsilon_{i,t}$$

where i represents a city and t represents a semi-annual time period. $Y_{i,t}$ are the two dependent variables that represent local angel financing: *ln(Num+1)*, the natural logarithm of one plus the number of angel investments, and *ln(Amount+1)*, the natural logarithm of one plus the amount of angel investments in city i and time t. $ln(HV/NW)$ is the natural logarithm of city i's home-value-to-net-worth ratio in 2011, *Post* is a dummy that equals one if period t is after 2011 and equals zero otherwise. Control variables, *Population*, *Income_per_person*, and *Home_value*, are described in section 4.2. I also control for time and city fixed effects. In all regressions, I double-cluster standard errors at the city level and at the time level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) *ln(Num+1)*	(2) *ln(Amount+1)*	(3) *ln(Num+1)*	(4) *ln(Amount+1)*
*ln(HV/HW)*Post*	-0.027***	-0.245**	-0.026***	-0.228**
	(0.006)	(0.096)	(0.006)	(0.099)
Population			0.009	0.280
			(0.057)	(0.967)
Income_per_person			0.038	0.613
			(0.060)	(0.835)
Home_value			-0.016	0.328
			(0.040)	(0.533)
Constant	0.242***	3.471***	-0.039	-9.651
	(0.000)	(0.001)	(1.198)	(17.290)
Observations	38,960	38,960	38,214	38,214
R-squared	0.667	0.432	0.668	0.433
City FE	YES	YES	YES	YES
Semi-annual FE	YES	YES	YES	YES
# of cities	3896	3896	3822	3822

Table 3. Impact on Local Entrepreneurial Activity: Subsequent Financing of Firms Received Angel Investments

This table shows how the SEC regulation change impacted local entrepreneurial activity measured by subsequent financing of firms received angel investments. I use the same empirical specification as described in Table 2. The dependent variable in column (1), $ln(Num\_next\_financing + 1)$, is the natural logarithm of one plus the number of firms that received an angel investment in city i and time t and receive next round financing in the future. The dependent variable in column (2), $ln(Num\_later\_VC + 1)$, is the natural logarithm of one plus the number of firms that received an angel investment in city i time t and later receive investments from venture capitals. $ln(HV/NW)$ is the natural logarithm of city i's home-value-to-net-worth ratio in 2011, *Post* is a dummy that equals one if period t is after 2011 and equals zero otherwise. Control variables, *Population*, *Income_per_person*, and *Home_value*, are described in section 4.2. I also control for time and city fixed effects. In all regressions, I double-cluster standard errors at the city level and at the time level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) *ln(Num_next_financing+1)*	(2) *ln(Num_later_VC+1)*
*ln(HV/HW)*Post*	-0.015**	-0.008*
	(0.005)	(0.004)
Population	0.024	-0.012
	(0.034)	(0.014)
Income_per_person	0.016	-0.008
	(0.033)	(0.015)
Home_value	-0.066**	-0.046***
	(0.021)	(0.011)
Constant	0.502	0.792**
	(0.611)	(0.315)
Observations	38,214	38,214
R-squared	0.581	0.490
City FE	YES	YES
Semi-annual FE	YES	YES

Table 4. Impact on Local Entrepreneurial Activity: Successful Exits of Firms Received Angel Investments

This table shows how the SEC regulation change impacted local entrepreneurial activity measured by investors' successful exits of firms received angel investments. I use the same empirical specification as described in Table 2. The dependent variable in column (1), $ln(Num\_Acq + 1)$, is the natural logarithm of one plus the number of firms that received angel investments in city i and time t and have an acquisition later. The dependent variable in column (2), $ln(Num\_IPO + 1)$, is the natural logarithm of one plus the number of firms that received angel investments in city i and time t and have an IPO later. The dependent variable in column (3), $ln(Num\_Acq\_IPO + 1)$, is the natural logarithm of one plus the number of firms received angel investments in city i and time t and have an acquisition or an IPO later. $ln(HV/NW)$ is the natural logarithm of city i's home-value-to-net-worth ratio in 2011, *Post* is a dummy that equals one if period t is after 2011 and equals zero otherwise. Control variables, *Population*, *Income_per_person*, and *Home_value*, are described in section 4.2. I also control for time and city fixed effects. In all regressions, I double-cluster standard errors at the city level and at the time level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) *ln(Num_Acq+1)*	(2) *ln(Num_IPO+1)*	(3) *ln(Num_Acq_or_IPO+1)*
*ln(HV/HW)*Post*	-0.006**	-0.005**	-0.008**
	(0.002)	(0.002)	(0.002)
Population	-0.014	-0.014	-0.017
	(0.009)	(0.008)	(0.012)
Income_per_person	-0.035**	-0.031**	-0.037**
	(0.011)	(0.014)	(0.011)
Home_value	-0.039***	-0.022**	-0.045***
	(0.011)	(0.008)	(0.012)
Constant	0.980***	0.738**	1.117***
	(0.228)	(0.270)	(0.276)
Observations	38,214	38,214	38,214
R-squared	0.351	0.261	0.362
City FE	YES	YES	YES

Table 5. Impact on the Local Economy: Innovation Generated by Firms Received Angel Investments

This table shows the impact of SEC regulation change on the local economy in terms of innovation generated by the filing firms. The dependent variable in column (1), $ln(Num\_patents + 1)$, is the natural logarithm of one plus the number of patents generated by firms that received angel investments in city i and time t. The dependent variable in column (2), $ln(Num\_total\_cites + 1)$, is the natural logarithm of one plus the number of patent citations received by firms who obtained their angel investments in city i and time t. The dependent variable in column (3), $ln(Num\_cites\_per\_patent + 1)$, is the natural logarithm of one plus the average number of citations per patent received by firms who obtained angel investments in city i and time t. $ln(HV/NW)$ is the natural logarithm of city i's home-value-to-net-worth ratio in 2011, *Post* is a dummy that equals one if period t is after 2011 and equals zero otherwise. Control variables, *Population*, *Income_per_person*, and *Home_value*, are described in section 4.2. I also control for time and city fixed effects. In all regressions, I double-cluster standard errors at the city level and at the time level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) *ln(Num_patents+1)*	(2) *ln(Num_total_cites+1)*	(3) *ln(Num_cites_per_patent+1)*
*ln(HV/HW)*Post*	-0.020***	-0.001**	-0.0004**
	(0.005)	(0.000)	(0.0002)
Population	-0.038	-0.002	-0.0013*
	(0.025)	(0.001)	(0.0007)
Income_per_person	-0.051	-0.004**	-0.0014*
	(0.029)	(0.001)	(0.0007)
Home_value	-0.086**	-0.003**	-0.0017*
	(0.030)	(0.001)	(0.0008)
Constant	1.999***	0.099***	0.0498**
	(0.598)	(0.027)	(0.0157)
Observations	38,214	38,214	38,214
R-squared	0.427	0.375	0.3158
City FE	YES	YES	YES
Semi-annual FE	YES	YES	YES

Table 6. Impact on the Local Economy: Employment and Sales Generated by Firms Received Angel Investments

This table shows the impact of SEC regulation change on the local economy in terms of employment supported and sales generated by the filing firms. The dependent variable in column (1), $ln(Employment + 1)$, is the natural logarithm of one plus the number of jobs supported in the next year by firms who received angel investments in city i and time t. The dependent variable in column (2), $ln(Sales + 1)$, is the natural logarithm of one plus the amount of sales generated in the next year by firms who received angel investments in city i and time t. $ln(HV/NW)$ is the natural logarithm of city i's home-value-to-net-worth ratio in 2011, *Post* is a dummy that equals one if period t is after 2011 and equals zero otherwise. Control variables, *Population*, *Income_per_person*, and *Home_value*, are described in section 4.2. I also control for time and city fixed effects. In all regressions, I double-cluster standard errors at the city level and at the time level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) *ln(Employment+1)*	(2) *ln(Sales+1)*
*ln(HV/HW)*Post*	-0.065***	-0.226**
	(0.016)	(0.088)
Population	-0.066	0.202
	(0.162)	(0.813)
Income_per_person	0.012	0.426
	(0.077)	(0.453)
Home_value	-0.064	0.258
	(0.052)	(0.368)
Constant	1.797	-7.023
	(2.239)	(12.374)
Observations	38,214	38,214
R-squared	0.540	0.452
City FE	YES	YES
Semi-annual FE	YES	YES

Table 7. The Substitution Effect Between Angel Financing and Small Business Loans

This table shows the substitution effect between reduced angel financing and the demand for small business loans. The dependent variable in column (1), *ln(Num_SBL+1)*, is the natural logarithm of one plus the number of approved small business loans applied in city i and time t. The dependent variable in column (2), *ln(Amount_SBL+1)*, is the natural logarithm of one plus the approved amount of small business loans applied in city i and time t. The dependent variable in column (3), *ln(Guaranteed_Amount_SBL+1)*, is the natural logarithm of one plus the amount of small business loans applied in city i and time t guaranteed by the Small Business Administration. $ln(HV/NW)$ is the natural logarithm of city i's home-value-to-net-worth ratio in 2011, *Post* is a dummy that equals one if period t is after 2011 and equals zero otherwise. Control variables, *Population*, *Income_per_person*, and *Home_value*, are described in section 4.2. I also control for time and city fixed effects. In all regressions, I double-cluster standard errors at the city level and at the time level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) *ln(Num_SBL+1)*	(2) *ln(Amount_SBL+1)*	(3) *ln(Guaranteed_Amount_SBL+1)*
$ln(HV/NW) * Post$	0.424**	0.536***	0.496**
	(0.134)	(0.162)	(0.184)
Population	0.596	0.931	0.127
	(0.611)	(0.891)	(0.835)
Income_per_person	0.438	0.718	0.708
	(0.496)	(0.581)	(0.639)
Home_value	0.104	-0.239	-0.079
	(0.642)	(0.801)	(0.776)
Constant	-3.781	-4.037	1.185
	(14.738)	(18.875)	(15.124)
Observations	38,784	38,784	38,784
R-squared	0.591	0.591	0.573
City FE	YES	YES	YES
Semi-annual FE	YES	YES	YES

Table 8. The Substitution Effect Between Angel Financing and Second-Lien Mortgages

This table shows the substitution effect between reduced angel financing and the demand for second-lien mortgages. The dependent variable in column (1), *ln(2ndlien_num+1)*, is the natural logarithm of one plus the number of second-lien mortgages applied in city i and time t. The dependent variable in column (2), *ln(2ndlien_amnt+1)*, is the natural logarithm of one plus the amount of second-lien mortgages applied in city i and time t. $ln(HV/NW)$ is the natural logarithm of city i's home-value-to-net-worth ratio in 2011, *Post* is a dummy that equals one if period t is after 2011 and equals zero otherwise. Control variables, *Population*, *Income_per_person*, and *Home_value*, are described in section 4.2. I also control for year and city fixed effects. In all regressions, I double-cluster standard errors at the city level and at the time level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) *ln(2ndlien_num+1)*	(2) *ln(2ndlien_amnt+1)*
*ln(HV/HW)*Post*	0.184***	0.264**
	(0.037)	(0.064)
Population	0.282	0.523
	(0.273)	(0.577)
Income_per_person	0.183	0.451
	(0.116)	(0.281)
Home_value	0.401	0.653*
	(0.197)	(0.268)
Constant	-6.715	-11.811
	(4.109)	(5.695)
Observations	19,002	19,002
R-squared	0.947	0.927
City FE	YES	YES
Semi-annual FE	YES	YES
# of cities	3801	3801

Table 9. Cost-Benefit Analysis of the 2011 SEC Regulation Change

This table shows the estimation of the benefits and costs of the 2011 SEC regulation change under different assumptions. Panel A of the table shows the estimation of the present value (in billion dollars) of the benefits of the above regulation change at the end of 2011 with assumptions on the length of the policy impact will last and on the discount rate. Panel B of the table shows the estimation of the present value (in billion dollars) of the cost of the above regulation change at the end of 2011 under different assumptions on the growth rate, the discount rate, and the length of the policy impact will last. Panel C of the table shows the net benefit (*i.e.,* benefits minus costs) under different assumptions. r stands for the discount rate and g stands for the growth rate. The details of the estimation is described in the section 9.

Panel A. Estimation of Benefits

Assumption \ r=	5%	10%	15%	20%	25%	30%
Assuming the impact of SEC regulation change lasts for 10 years	23.89	19.03	15.55	13.00	11.09	9.61
Assuming the impact of SEC regulation change lasts for 5 years	13.44	11.78	10.42	9.30	8.37	7.59
Assuming the impact of SEC regulation change lasts for 3 years	8.49	7.76	7.13	6.58	6.10	5.68

Panel B. Estimation of Costs

	g= \ r=	5%	10%	15%	20%	25%	30%
Assuming the impact of SEC regulation change lasts for 10 years	**0%**	60.25	37.85	25.05	17.33	12.47	9.27
	5%		45.83	29.81	20.31	14.41	10.58
	10%			35.82	24.03	16.80	12.17
	15%				28.66	19.75	14.13
	20%					23.41	16.52
	25%						19.46
	g= \ r=	**5%**	**10%**	**15%**	**20%**	**25%**	**30%**
Assuming the impact of SEC regulation change lasts for 5 years	**0%**	32.74	22.66	16.26	12.04	9.17	7.14
	5%		27.44	19.36	14.11	10.59	8.15
	10%			23.26	16.69	12.35	9.38
	15%				19.91	14.52	10.88
	20%					17.21	12.73
	25%						14.99
	g= \ r=	**5%**	**10%**	**15%**	**20%**	**25%**	**30%**
Assuming the impact of SEC regulation change lasts for 3 years	**0%**	19.72	14.25	10.63	8.15	6.40	5.13
	5%		17.26	12.65	9.55	7.40	5.86
	10%			15.20	11.30	8.62	6.74
	15%				13.48	10.14	7.82
	20%					12.02	9.14
	25%						10.77

Panel C. Estimation of Net Benefits

	r= / g=	5%	10%	15%	20%	25%	30%
Assuming the impact of SEC regulation change lasts for 10 years	**0%**	-36.36	-18.83	-9.49	-4.33	-1.38	0.33
	5%		-26.80	-14.26	-7.31	-3.32	-0.97
	10%			-20.27	-11.02	-5.71	-2.57
	15%				-15.66	-8.67	-4.52
	20%					-12.32	-6.91
	25%						-9.85
	r= / g=	**5%**	**10%**	**15%**	**20%**	**25%**	**30%**
Assuming the impact of SEC regulation change lasts for 5 years	**0%**	-14.42	-7.88	-3.78	-1.15	0.56	1.69
	5%		-12.13	-6.64	-3.13	-0.85	0.66
	10%			-10.25	-5.61	-2.59	-0.59
	15%				-8.69	-4.74	-2.12
	20%					-7.40	-4.01
	25%						-6.32
	r= / g=	**5%**	**10%**	**15%**	**20%**	**25%**	**30%**
Assuming the impact of SEC regulation change lasts for 3 years	**0%**	-11.23	-6.49	-3.50	-1.57	-0.30	0.55
	5%		-9.50	-5.53	-2.97	-1.30	-0.18
	10%			-8.08	-4.72	-2.52	-1.06
	15%				-6.90	-4.04	-2.14
	20%					-5.92	-3.47
	25%						-5.09

Internet Appendix

A Net Worth Calculation

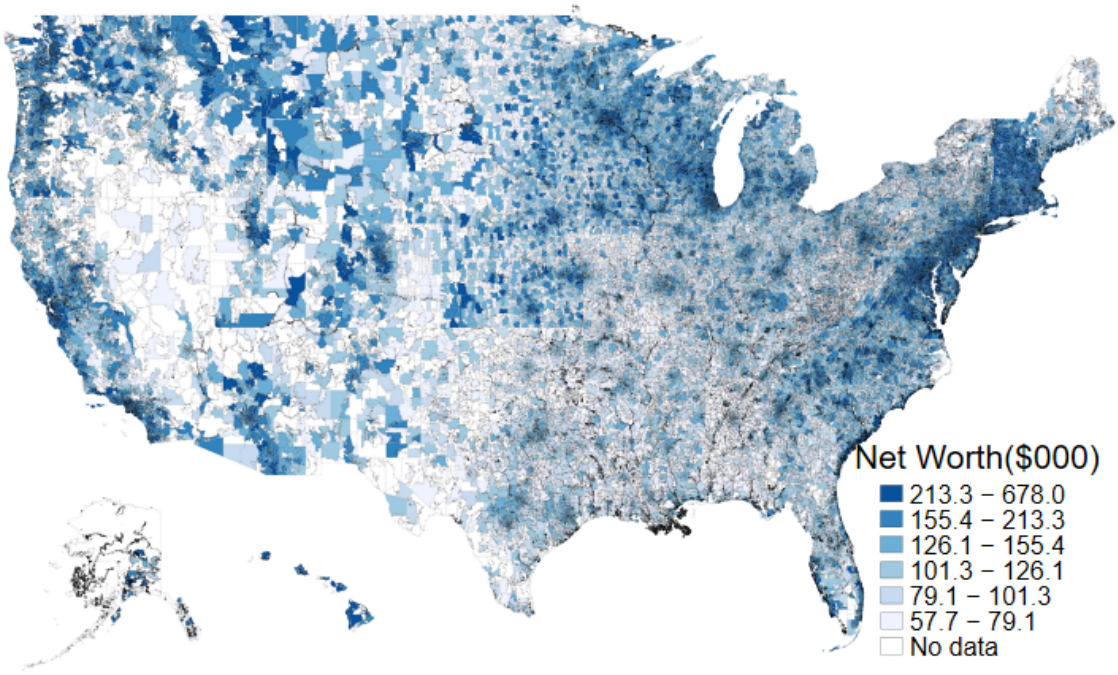
This section describes the procedure of calculating the mean value of household net worth in a city following Chenevert et al. (2017). Two data sets are used in the calculation. The first data set is the Wave 10 of the 2008 Survey of Income and Program Participation (SIPP), which was conducted during the September to December in 2011. The second one is the 2011 personal income tax data from the Internal Revenue Service (IRS). The SIPP data only provides the geography of respondents at the state level. To obtain the net worth information at the city level, I combine the SIPP data with both the state-level and the ZIP code-level data from the IRS. Specific steps of calculating the mean value of a city's household net worth are as follows.

Step 1: I collect the state-level mean values of household net worth from the Wave 8 of the 2008 SIPP which was conducted in 2011. In addition, I obtain the state-level average value of household net worth of five categories of assets: (1) interest paying assets (investment in banks and financial institutions); (2) dividend paying assets (investment in stocks, mutual funds, and equity in business); (3) retirement accounts; (4) real estate assets; (5) other assets that are not included in the above four categories.

Step 2: Using state-level personal income tax data from IRS, I calculate the state-level average household gross income in 2011. I also calculate the average of the income generated from the five categories of assets as listed in Step 1. Dividing the mean values of net worth for each type of assets obtained from Step 1 by the mean values of income obtained from Step 2, I obtain the net-worth-to-income ratio for each of the five types of assets at the state level.

Step 3: Using the net-worth-to-income ratios obtained from Step 2 multiplied by the income generated from each type of assets using ZIP-code level data from the IRS, I get

Figure A1. Geographical Variation of the Net Worth in 2011



the household net worth for each type of assets at the ZIP-code level. Adding up the net worth for the five types of assets, I obtain the mean value of net worth at the ZIP code level. Finally, the mean values of household net worth at the city level are obtained by averaging the mean values of net worth at the ZIP code level weighted by ZIP code-level population.[40]

Figure A1 shows the geographical variance of the estimated average net worth across U.S. cities in 2011. The darker the color represents a higher net worth in a city. One can observe that the net worth in large cities along the east coast and west coast is relatively higher. Cities in Colorado and Illinois also enjoy high net worth.

[40]Using the value weighted by the population in each ZIP code or the simple mean (not weighted) does not affect the results and conclusions of this study.

B Additional Tests

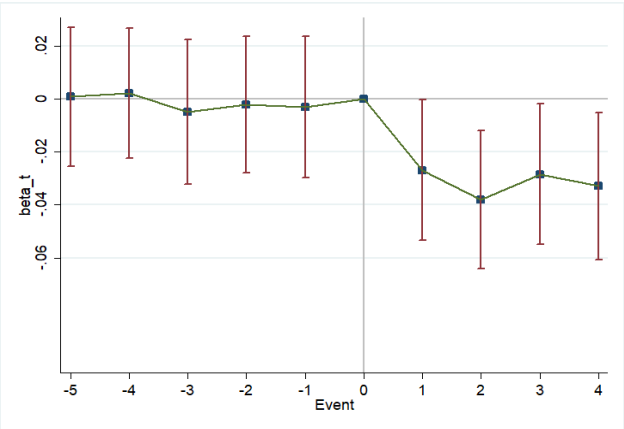
Figure B1. Plot of Coefficients Around the Event Time

The figures show the coefficients plot around the regulation change by estimating the following model:

$$Y_{it} = \alpha + \sum_{t=-5, t\neq 0}^{4} \beta_t Treat_i * Period_t + Controls_{i,t} + \delta_t + \eta_i + \epsilon_{it}$$

where $Period_t$ is a set of dummy variables that equals one if a city-half-year observation is from the time unit t. For example, $Period_1$ equals one if observations are from the first-half year of 2012. The benchmark group comprises of observations that are in the event period (the second half of 2011, $t = 0$). $Treat_i$ is a dummy that equals one if city i's HV/NW ratio is larger than the median of the HV/NW ratio in the sample in 2011 and equals zero otherwise, $Post_t$ is a dummy that equals one if period t is after 2011 and equals zero otherwise. Panel (a) shows the plot of estimates of β_t when the outcome variable is the natural logarithm of one plus the number of angel investments. Panel (b) shows the plot of estimates of β_t when the outcome variable is the natural logarithm of one plus the amount of angel investments. The center points show the point estimates of β_t and the vertical lines denote the 90% confidence intervals of β_t estimates.

(a) Number of Angel Investments



(b) Amount of Angel Investments

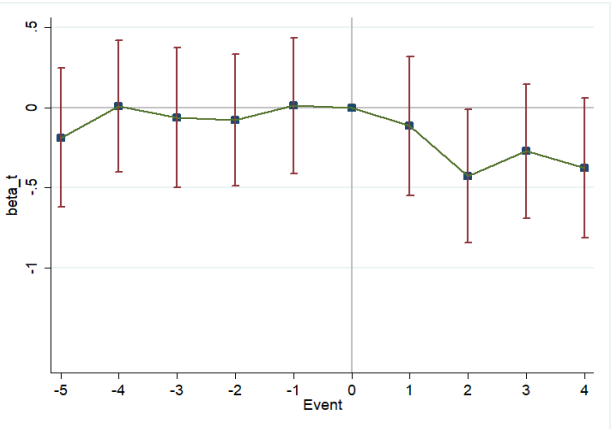


Figure B2. Geographical Variation of the Home-Value-To-Net-Worth Ratio in 2011 (Only Cities Within Top-30 Metropolitan Statistical Areas are Included)

This figure shows the geographical variance of the *HV/NW* ratio across among cities within top-30 metropolitan statistical areas (MSA) in 2011. Top-30 MSAs are chosen based on the total population in 2011. The darker the color represents a higher *HV/NW* ratio. The *HV/NW* ratio is calculated by diving the average home value in a city by the average household net worth in the city. The average home value in city i is calculated by averaging the Zillow home value index across all ZIP codes in city i. The average net worth in city i is estimated by combining data from SIPP and IRS following the procedure specified in Appendix A.

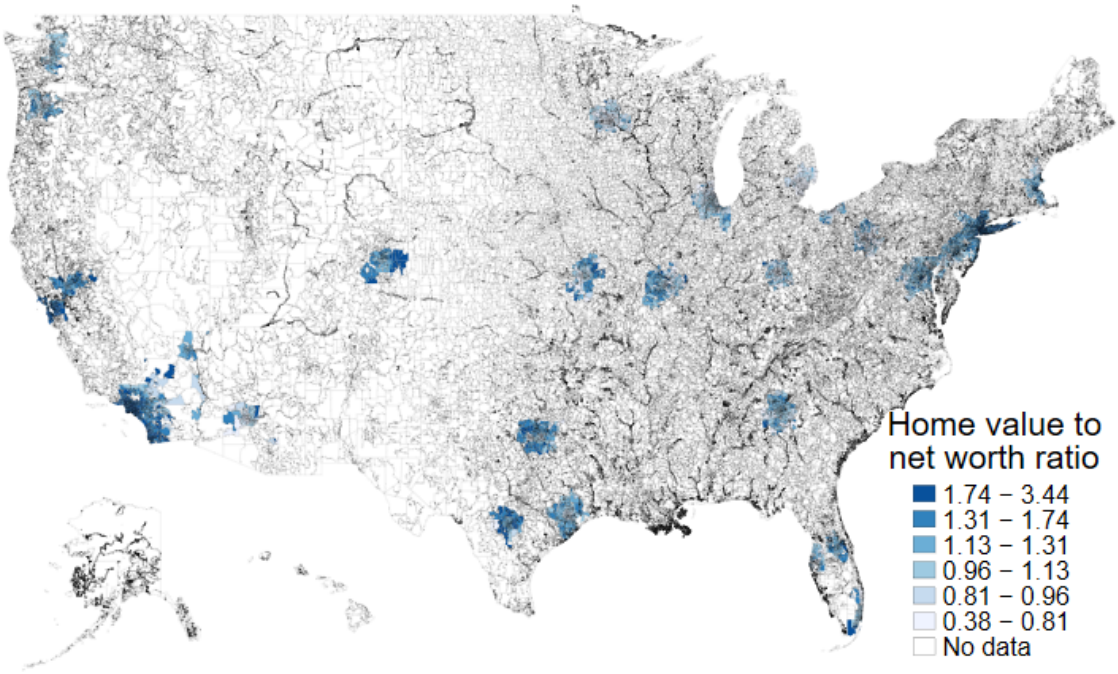


Table B1. Impact on Local Angel Financing

This table shows the results of the classic DiD analysis by estimating the following model:

$$Y_{i,t} = \alpha + \beta Treat_i * Post_t + Controls_{i,t} + \delta_t + \eta_i + \epsilon_{i,t}$$

where i represents a city and t represents a semi-annual time period. $Y_{i,t}$ are the two dependent variables that represent local angel financing: *ln(Num+1)*, the natural logarithm of one plus the number of angel investments, and *ln(Amount+1)*, the natural logarithm of one plus the amount of angel investments in city i and time t. $Treat_i$ is a dummy that equals one if city i's HV/NW ratio is larger than the median of the HV/NW ratio in the sample in 2011 and equals zero otherwise, $Post_t$ is a dummy that equals one if period t is after 2011 and equals zero otherwise. Control variables, *Population*, *Income_per_person*, and *Home_value*, are described in section 4.2. I also control for time and city fixed effects. In all regressions, I double-cluster standard errors at the city level and at the time level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1) *ln(Num+1)*	(2) *ln(Amount+1)*	(3) *ln(Num+1)*	(4) *ln(Amount+1)*
*Treat*Post*	-0.026***	-0.279**	-0.025***	-0.259*
	(0.007)	(0.110)	(0.007)	(0.118)
Population			0.011	0.325
			(0.059)	(0.999)
Income_per_person			0.038	0.607
			(0.060)	(0.835)
Home_value			-0.018	0.327
			(0.040)	(0.537)
Constant	0.247***	3.518***	-0.041	-9.983
	(0.001)	(0.019)	(1.206)	(17.494)
Observations	38,960	38,960	38,214	38,214
R-squared	0.667	0.432	0.668	0.433
City FE	YES	YES	YES	YES
Semi-annual FE	YES	YES	YES	YES
# of cities	3896	3896	3822	3822

Table B2. Addressing Concern I: Controlling for Short-Term Housing Price Changes

This table shows the results of the robustness test by controlling for short-term housing price changes. The dependent variable is $ln(Num+1)$, the natural logarithm of one plus the number of angel investments in city i and time t. The dependent variable is $ln(Amnt+1)$, the natural logarithm of one plus the amount of angel investments in city i and time t. $ln(HV/NW)$ is the natural logarithm of city i's home-value-to-net-worth ratio in 2011, *Post* is a dummy that equals one if period t is after 2011 and equals zero otherwise. Control variables, *Population*, *Income_per_person*, and *Home_value*, are described in section 4.2. The additional controls in this table are *Home_value_growth_6M*, the changes in the natural logarithm of the housing price in a city in the last six months, and the *Home_value_growth_12M*, the changes in the natural logarithm of the housing price in a city in the last year. I also control for time and city fixed effects. In all regressions, I double-cluster standard errors at the city level and at the time level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) *ln(Num+1)*	(2) *ln(Amnt+1)*	(3) *ln(Num+1)*	(4) *ln(Amnt+1)*	(5) *ln(Num+1)*	(6) *ln(Amnt+1)*	(7) *ln(Num+1)*	(8) *ln(Amnt+1)*
*ln(HV/HW)*Post*	-0.027***	-0.230**	-0.027***	-0.234**	-0.027***	-0.226**	-0.027***	-0.229**
	(0.006)	(0.096)	(0.006)	(0.100)	(0.006)	(0.097)	(0.006)	(0.101)
Population	0.002	0.304	0.006	0.259	0.003	0.322	0.007	0.275
	(0.060)	(1.042)	(0.058)	(0.984)	(0.060)	(1.042)	(0.058)	(0.982)
Income_per_person	0.035	0.660	0.039	0.619	0.035	0.663	0.039	0.616
	(0.060)	(0.834)	(0.060)	(0.837)	(0.060)	(0.833)	(0.060)	(0.842)
Home_value			-0.024	0.267			-0.026	0.302
			(0.039)	(0.540)			(0.038)	(0.568)
Home_value_growth_6M	0.064	0.646	0.071	0.569				
	(0.064)	(0.985)	(0.061)	(1.040)				
Home_value_growth_12M					0.026	0.169	0.032	0.091
					(0.034)	(0.560)	(0.031)	(0.615)
Constant	-0.140	-6.357	0.073	-8.756	-0.149	-6.561	0.084	-9.309
	(1.002)	(15.377)	(1.194)	(17.473)	(1.003)	(15.360)	(1.199)	(17.542)
Observations	38,214	38,214	38,214	38,214	38,214	38,214	38,214	38,214
R-squared	0.668	0.433	0.668	0.433	0.668	0.433	0.668	0.433
City FE	YES	YES	YES	YES	YES	YES	YES	YES
Semi-annual FE	YES	YES	YES	YES	YES	YES	YES	YES
# of cities	3822	3822	3822	3822	3822	3822	3822	3822

Table B3. Addressing Concern I: Sub-sample Test Based on Housing Price Growth Since the Crisis

This table shows the results of the robustness test by performing a sub-sample test sorting all cities into two groups based on the housing price growth from the end of 2008 to the end of 2011. The first two columns show the sub-sample where cities that had a housing price growth below the median are included. The last two columns show the sub-sample where cities that had a housing price growth above the median are included.The dependent variable is $ln(Num+1)$, the natural logarithm of one plus the number of angel investments in city i and time t. The dependent variable is $ln(Amnt+1)$, the natural logarithm of one plus the amount of angel investments in city i and time t. $ln(HV/NW)$ is the natural logarithm of city i's home-value-to-net-worth ratio in 2011, *Post* is a dummy that equals one if period t is after 2011 and equals zero otherwise. Control variables, *Population*, *Income_per_person*, and *Home_value*, are described in section 4.2. I also control for time and city fixed effects. In all regressions, I double-cluster standard errors at the city level and at the time level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) *ln(Num+1)*	(2) *ln(Amount+1)*	(3) *ln(Num+1)*	(4) *ln(Amount+1)*
*ln(HV/HW)*Post*	-0.029***	-0.212*	-0.024**	-0.247
	(0.007)	(0.106)	(0.009)	(0.172)
Population	0.074	1.126	-0.068	-0.724
	(0.109)	(1.975)	(0.075)	(1.483)
Income_per_person	0.028	-0.011	0.059	1.348
	(0.049)	(0.772)	(0.078)	(1.194)
Home_value	-0.005	0.037	0.011	0.859
	(0.048)	(0.685)	(0.069)	(1.105)
Constant	-0.759	-8.432	0.194	-13.567
	(1.599)	(27.276)	(1.284)	(23.903)
Observations	19,314	19,314	18,900	18,900
R-squared	0.642	0.398	0.686	0.459
Housing Price Growth (08'E to 11'E)	Low	Low	High	High
City FE	YES	YES	YES	YES
Semi-annual FE	YES	YES	YES	YES
# of cities	1932	1932	1890	1890

Table B4. Addressing Concern II: Excluding Outliers Based on Cities' Average Net Worth in 2011

This table shows the results of the robustness test by excluding sample outliers based on cities' average net worth in 2011. The dependent variable in column (1)-(3), $ln(Num+1)$, is the natural logarithm of one plus the number of angel investments in city i and time t. The dependent variable in column (4)-(6), $ln(Amount+1)$, is the natural logarithm of one plus the amount of angel investments in city i and time t. In column (1) and column (4), I exclude cities that have the largest 10% of net worth in the sample in 2011. In column (2) and column (5), I exclude cities that have the smallest 10% of net worth in the sample in 2011. In column (3) and column (6), I exclude cities that have the largest 10% of net worth or the smallest 10% of net worth in the sample in 2011. $ln(HV/NW)$ is the natural logarithm of city i's home-value-to-net-worth ratio in 2011, *Post* is a dummy that equals one if period t is after 2011 and equals zero otherwise. Control variables, *Population*, *Income_per_person*, and *Home_value*, are described in section 4.2. I also control for time and city fixed effects. In all regressions, I double-cluster standard errors at the city level and at the time level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Net worth in 2011					
	ln(Num+1)			*ln(Amount+1)*		
	Exclude largest	Exclude smallest	Exclude largest and smallest	Exclude largest	Exclude smallest	Exclude largest and smallest
*ln(HV/HW)*Post*	-0.027***	-0.035***	-0.038***	-0.231*	-0.349**	-0.377**
	(0.007)	(0.007)	(0.007)	(0.110)	(0.115)	(0.122)
Population	0.004	0.023	0.022	0.116	0.493	0.366
	(0.054)	(0.062)	(0.060)	(0.925)	(1.028)	(0.986)
Income_per_person	0.052	0.039	0.053	1.051	0.583	1.021
	(0.062)	(0.063)	(0.071)	(0.972)	(0.904)	(1.131)
Home_value	-0.023	-0.023	-0.033	0.050	0.321	-0.018
	(0.026)	(0.047)	(0.032)	(0.348)	(0.616)	(0.437)
Constant	-0.080	-0.094	-0.124	-9.318	-11.258	-10.561
	(1.119)	(1.333)	(1.308)	(18.364)	(18.737)	(20.782)
Observations	34,384	34,458	30,628	34,384	34,458	30,628
R-squared	0.661	0.674	0.668	0.418	0.441	0.428
City FE	YES	YES	YES	YES	YES	YES
Semi-annual FE	YES	YES	YES	YES	YES	YES
# of cities	3439	3446	3063	3439	3446	3063

Table B5. Addressing Concern II: Excluding Outliers Based on Cities' Average Home Value in 2011

This table shows the results of the robustness test by excluding sample outliers based on cities' average home value in 2011. The dependent variable in column (1)-(3), $ln(Num+1)$, is the natural logarithm of one plus the number of angel investments in city i and time t. The dependent variable in column (4)-(6), $ln(Amount+1)$, is the natural logarithm of one plus the amount of angel investments in city i and time t. In column (1) and column (4), I exclude cities that have the largest 10% of home value in the sample in 2011. In column (2) and column (5), I exclude cities that have the smallest 10% of home value in the sample in 2011. In column (3) and column (6), I exclude cities that have the largest 10% of home value or the smallest 10% of home value in the sample in 2011. $ln(HV/NW)$ is the natural logarithm of city i's home-value-to-net-worth ratio in 2011, *Post* is a dummy that equals one if period t is after 2011 and equals zero otherwise. Control variables, *Population*, *Income_per_person*, and *Home_value*, are described in section 4.2. I also control for time and city fixed effects. In all regressions, I double-cluster standard errors at the city level and at the time level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Home value in 2011					
	ln(Num+1)			*ln(Amount+1)*		
	Exclude largest	Exclude smallest	Exclude largest and smallest	Exclude largest	Exclude smallest	Exclude largest and smallest
*ln(HV/HW)*Post*	-0.025***	-0.030***	-0.029***	-0.222**	-0.290*	-0.294*
	(0.006)	(0.008)	(0.008)	(0.096)	(0.140)	(0.141)
Population	0.007	0.023	0.023	0.096	0.420	0.251
	(0.058)	(0.061)	(0.063)	(0.950)	(1.054)	(1.043)
Income_per_person	0.044	0.039	0.045	0.648	0.602	0.623
	(0.057)	(0.063)	(0.060)	(0.920)	(0.866)	(0.945)
Home_value	0.004	-0.010	0.014	0.398	0.516	0.610
	(0.029)	(0.046)	(0.032)	(0.412)	(0.624)	(0.522)
Constant	-0.359	-0.251	-0.641	-9.250	-13.127	-13.011
	(1.092)	(1.332)	(1.224)	(17.970)	(19.019)	(19.879)
Observations	34,474	34,376	30,636	34,474	34,376	30,636
R-squared	0.640	0.675	0.649	0.406	0.440	0.415
City FE	YES	YES	YES	YES	YES	YES
Semi-annual FE	YES	YES	YES	YES	YES	YES
# of cities	3448	3438	3064	3448	3438	3064

Table B6. Addressing Concern II: Using Top-Bracket HV/NW Ratio as an Alternative Treatment Measure

This table shows the results of the DiD analysis using an alternative measure of the treatment. Specifically, the mean home-value-to-net-worth ratio (HV/NW) is replaced with the top-home-value-to-top-net-worth ratio (HV_top/HW_top) for a city in 2011. The dependent variables are *ln(Num+1)*, the natural logarithm of one plus the number of angel investments, and *ln(Amount+1)*, the natural logarithm of one plus the amount of angel investments in city i and time t. *Post* is a dummy that equals one if period t is after 2011 and equals zero otherwise. Control variables, *Population*, *Income_per_person*, and *Home_value*, are described in section 4.2. I also control for time and city fixed effects. In all regressions, I double-cluster standard errors at the city level and at the time level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) *ln(Num+1)*	(2) *ln(Amount+1)*	(3) *ln(Num+1)*	(4) *ln(Amount+1)*
*ln(HV_top/HW_top)*Post*	-0.024***	-0.195*	-0.025***	-0.213*
	(0.007)	(0.098)	(0.007)	(0.111)
Population			0.085	1.543
			(0.082)	(1.266)
Income_per_person			0.038	-0.036
			(0.075)	(0.977)
Home_value			-0.062	0.126
			(0.071)	(1.058)
Constant	0.294***	4.058***	-0.201	-12.804
	(0.001)	(0.012)	(1.484)	(20.456)
Observations	24,760	24,760	24,580	24,580
R-squared	0.702	0.459	0.702	0.459
City FE	YES	YES	YES	YES
Semi-annual FE	YES	YES	YES	YES
# of cities	2476	2476	2458	2458

Table B7. Addressing Concern III: Controlling for Spillover Effects from Nearby Regions

This table shows the results of the robustness test by controlling for spillover effects from nearby regions. The dependent variable is $ln(Num+1)$, the natural logarithm of one plus the number of angel investments in city i and time t. $ln(HV/NW)$ is the natural logarithm of city i's home-value-to-net-worth ratio in 2011, *Post* is a dummy that equals one if period t is after 2011 and equals zero otherwise. $ln(HV/NW)_{25(50,100)Miles}$ is the natural logarithm of the average home-value-to-net-worth ratio in cities within 25 (50, 100) miles to city i. Control variables, *Population*, *Income_per_person*, and *Home_value*, are described in section 4.2. I also control for time and city fixed effects. In all regressions, I double-cluster standard errors at the city level and at the time level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) *ln(Num+1)*	(2) *ln(Num+1)*	(3) *ln(Num+1)*
*ln(HV/HW)*Post*	-0.015*	-0.016*	-0.014*
	(0.007)	(0.008)	(0.007)
delta_value25_event	-0.023*		
	(0.011)		
delta_value50_event		-0.027**	
		(0.012)	
delta_value100_event			-0.041**
			(0.015)
Population	0.006	0.009	0.009
	(0.059)	(0.057)	(0.057)
Income_per_person	0.036	0.039	0.038
	(0.060)	(0.059)	(0.059)
Home_value	-0.015	-0.015	-0.014
	(0.041)	(0.040)	(0.041)
Constant	-0.001	-0.060	-0.062
	(1.228)	(1.194)	(1.202)
Observations	38,064	38,194	38,204
R-squared	0.669	0.668	0.669
City FE	YES	YES	YES
Semi-annual FE	YES	YES	YES
# of cities	3807	3820	3821

Table B8. Addressing Concern IV: Excluding Top Three Entrepreneurship Cities and Cities Near Them

This table shows the results of the robustness test by excluding three cities where many angels live and the cities near them. The dependent variable in columns (1) and (3), $ln(Num+1)$, is the natural logarithm of one plus the number of angel investments in city i and time t. The dependent variable in columns (2) and (4), $ln(Amount+1)$, is the natural logarithm of one plus the amount of angel investments in city i and time t. In columns (1) and (2), I exclude San Francisco, New York, and Boston ("the three" cities) in the analysis. In columns (3) and (4), I exclude "the three" cities and the cities within 100 miles to "the three" cities in the analysis. $ln(HV/NW)$ is the natural logarithm of city i's home-value-to-net-worth ratio in 2011, *Post* is a dummy that equals one if period t is after 2011 and equals zero otherwise. Control variables, *Population*, *Income_per_person*, and *Home_value*, are described in section 4.2. I also control for time and city fixed effects. In all regressions, I double-cluster standard errors at the city level and at the time level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) *ln(Num+1)*	(2) *ln(Amount+1)*	(3) *ln(Num+1)*	(4) *ln(Amount+1)*
*ln(HV/HW)*Post*	-0.026***	-0.228**	-0.024***	-0.225*
	(0.006)	(0.099)	(0.006)	(0.100)
Population	0.009	0.280	0.008	0.302
	(0.057)	(0.967)	(0.057)	(1.012)
Income_per_person	0.038	0.610	0.034	0.446
	(0.060)	(0.837)	(0.061)	(0.858)
Home_value	-0.016	0.326	-0.010	0.324
	(0.040)	(0.534)	(0.040)	(0.501)
Constant	-0.037	-9.616	-0.069	-8.138
	(1.203)	(17.343)	(1.242)	(18.283)
Observations	38,184	38,184	37,174	37,174
R-squared	0.663	0.431	0.658	0.427
Exclude Cities	"the three"	"the three"	<100 miles "the three"	<100 miles "the three"
City FE	YES	YES	YES	YES
Semi-annual FE	YES	YES	YES	YES
# of cities	3819	3819	3718	3718

Table B9. Placebo Test: Impact on Non-Angel Investments

This table shows the impact of the SEC regulation change on non-angel investments. I use the same empirical specification (DiD with continuous treatment) as described in Table 2. The dependent variable in column (1), $ln(Num\_VC + 1)$, is the natural number of one plus the number of investments made by venture capitalists or private equity firms in city i and time t. The dependent variable in column (2), $ln(Num\_later + 1)$, is the natural logarithm of one plus the number of non-first-time SEC Form D filings in city i and time t. The dependent variable in column (3), $ln(Amount\_later + 1)$, is the natural logarithm of one plus the amount of non-first-time SEC Form D filings in city i and time t. $ln(HV/NW)$ is the natural logarithm of city i's home-value-to-net-worth ratio in 2011, *Post* is a dummy that equals one if period t is after 2011 and equals zero otherwise. Control variables, *Population*, *Income_per_person*, and *Home_value*, are described in section 4.2. I also control for time and city fixed effects. In all regressions, I double-cluster standard errors at the city level and at the time level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) *ln(Num_VC+1)*	(2) *ln(Num_later+1)*	(3) *ln(Amnt_later+1)*
*ln(HV/HW)*Post*	-0.002	0.001	-0.009
	(0.002)	(0.012)	(0.138)
Population	-0.000	0.141**	1.765**
	(0.015)	(0.054)	(0.689)
Income_per_person	0.026***	0.181**	2.201**
	(0.007)	(0.073)	(0.836)
Home_value	-0.014	0.100*	1.028
	(0.017)	(0.048)	(0.593)
Constant	-0.064	-4.394**	-51.235**
	(0.317)	(1.363)	(15.934)
Observations	38,214	38,214	38,214
R-squared	0.638	0.636	0.521
City FE	YES	YES	YES
Semi-annual FE	YES	YES	YES

Table B10. Placebo Test: Using Pseudo Event Time

This table shows the results of the placebo test using pseudo event time prior to the actual event time (the second half of 2011). The dependent variable in columns (1), (3), (5), and (7) is $ln(Num+1)$, natural logarithm of one plus the number of angel investments in city i and time t. The dependent variable in columns (2), (4), (6), and (8) is $ln(Amnt+1)$, natural logarithm of one plus the amount of angel investments in city i and time t. $ln(HV/NW)$ is the natural logarithm of city i's home-value-to-net-worth ratio in 2011. *Post_09H2* is a dummy that equals one if period t is after the second half year of 2009 and equals zero otherwise. *Post_10H1* is a dummy that equals one if period t is after the first half year of 2010 and equals zero otherwise. *Post_10H2* is a dummy that equals one if period t is after the second half of 2010 and equals zero otherwise. *Post_11H1* is a dummy that equals one if period t is after the first half year of 2011 and equals zero otherwise. Control variables, *Population*, *Income_per_person*, and *Home_value*, are described in section 4.2. I also control for time and city fixed effects. In all regressions, I double-cluster standard errors at the city level and at the time level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) *ln(Num+1)*	(2) *ln(Amnt+1)*	(3) *ln(Num+1)*	(4) *ln(Amnt+1)*	(5) *ln(Num+1)*	(6) *ln(Amnt+1)*	(7) *ln(Num+1)*	(8) *ln(Amnt+1)*
*ln(HV/NW)*Post_09H2*	-0.007	-0.058						
	(0.006)	(0.122)						
*ln(HV/NW)*Post_10H1*			-0.006	-0.056				
			(0.006)	(0.103)				
*ln(HV/NW)*Post_10H2*					-0.009	-0.119		
					(0.006)	(0.101)		
*ln(HV/NW)*Post_11H1*							-0.005	-0.130
							(0.005)	(0.100)
Population	0.007	0.225	0.007	0.229	0.009	0.258	0.015	0.260
	(0.048)	(0.944)	(0.049)	(0.947)	(0.049)	(0.956)	(0.048)	(0.944)
Income_per_person	0.037	0.666	0.037	0.666	0.036	0.658	0.036	0.654
	(0.046)	(0.808)	(0.046)	(0.811)	(0.046)	(0.812)	(0.038)	(0.789)
Home_value	0.016	0.348	0.016	0.350	0.017	0.368	0.022	0.418
	(0.033)	(0.503)	(0.033)	(0.507)	(0.033)	(0.510)	(0.027)	(0.522)
Constant	-0.443	-9.976	-0.448	-10.041	-0.472	-10.454	-0.626	-11.125
	(0.882)	(16.563)	(0.886)	(16.628)	(0.894)	(16.751)	(0.796)	(16.434)
Observations	38,214	38,214	38,214	38,214	38,214	38,214	38,214	38,214
R-squared	0.499	0.418	0.499	0.418	0.499	0.418	0.385	0.408
Pseudo Event-Time	2009H2	2009H2	2010H1	2010H1	2010H2	2010H2	2011H1	2011H1
City FE	YES	YES	YES	YES	YES	YES	YES	YES
Semi-annual FE	YES	YES	YES	YES	YES	YES	YES	YES

Table B11. Analysis of Impact on Angel Financing by Firm Age

This table shows the heterogeneous impact of the SEC regulation change by categorizing firms by the age when they received their angel investments. I use the same empirical specification (DiD with continuous treatment) as described in table 2. The dependent variable in column (1) is the natural logarithm of one plus the number of firms whose age are less than three years when they received the angel investments in city i and time t. The dependent variable in column (2) is the natural logarithm of one plus the number of firms whose age are three to five years when they received the angel investments in city i and time t. The dependent variable in column (3) is the natural logarithm of one plus the number of firms whose age are more than five years when they received the angel investments in city i and time t. $ln(HV/NW)$ is the natural logarithm of city i's home-value-to-net-worth ratio in 2011, *Post* is a dummy that equals one if period t is after 2011 and equals zero otherwise. Control variables, *Population*, *Income_per_person*, and *Home_value*, are described in section 4.2. I also control for time and city fixed effects. In all regressions, I double-cluster standard errors at the city level and at the time level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) Less than three years *ln(Num+1)*	(2) Three to five years *ln(Num+1)*	(3) Over five years *ln(Num+1)*
*ln(HV/HW)*Post*	-0.012*	-0.009**	-0.017***
	(0.006)	(0.004)	(0.005)
Population	0.045	-0.008	-0.037
	(0.055)	(0.012)	(0.032)
Income_per_person	0.065	-0.045	-0.036
	(0.050)	(0.027)	(0.029)
Home_value	0.042	-0.074***	-0.059*
	(0.046)	(0.019)	(0.028)
Constant	-1.466	1.497**	1.531**
	(0.989)	(0.468)	(0.653)
Observations	38,214	38,214	38,214
R-squared	0.634	0.439	0.481
City FE	YES	YES	YES
Semi-annual FE	YES	YES	YES

Table B12. Impact on Rates of Subsequent Financing and Successful Exits of Firms Received Angel Investments

This table shows the impact of the SEC regulation change on rates (instead of quantities) of local entrepreneurial activity for firms that received angel investments. I use the same empirical specification as described in Table 2. The dependent variable in column (1), *Rate_next_financing*, is the rate of receiving next-round financing in the future in firms that received angel investments in city i and time t. The dependent variable in column (2), *Rate_later_VC*, is the rate of receiving investments from venture capitals later in firms that firms that received angel investments in city i and time t. The dependent variable in column (3), *Rate_Acq*, is the rate of having an acquisition later in firms that received angel investments in city i and time t. The dependent variable in column (4), *Rate_IPO*, is the rate of having an IPO later in firms that received angel investments in city i and time t. The dependent variable in column (5), *Rate_Acq_or_IPO*, is the rate of having an acquisition or an IPO later in firms that received angel investments in city i and time t. $ln(HV/NW)$ is the natural logarithm of city i's home-value-to-net-worth ratio in 2011, *Post* is a dummy that equals one if period t is after 2011 and equals zero otherwise. Control variables, *Population*, *Income_per_person*, and *Home_value*, are described in section 4.2. I also control for time and city fixed effects. In all regressions, I double-cluster standard errors at the city level and at the time level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) *Rate_next_financing*	(2) *Rate_later_VC*	(3) *Rate_Acq*	(4) *Rate_IPO*	(5) *Rate_Acq_or_IPO*
*ln(HV/HW)*Post*	-0.007	-0.003	-0.000**	-0.001**	-0.001**
	(0.004)	(0.002)	(0.000)	(0.000)	(0.000)
Population	0.016	-0.003	-0.001	-0.005	-0.002
	(0.019)	(0.006)	(0.001)	(0.004)	(0.002)
Income_per_person	0.025	0.002	-0.002***	-0.000	-0.005**
	(0.022)	(0.008)	(0.001)	(0.002)	(0.002)
Home_value	-0.011	-0.016***	-0.003***	-0.004	-0.006***
	(0.012)	(0.005)	(0.001)	(0.003)	(0.001)
Constant	-0.222	0.215	0.066***	0.112	0.150***
	(0.301)	(0.148)	(0.015)	(0.070)	(0.039)
Observations	38,214	38,214	38,214	38,214	38,214
R-squared	0.255	0.250	0.318	0.117	0.293
City FE	YES	YES	YES	YES	YES
Semi-annual FE	YES	YES	YES	YES	YES

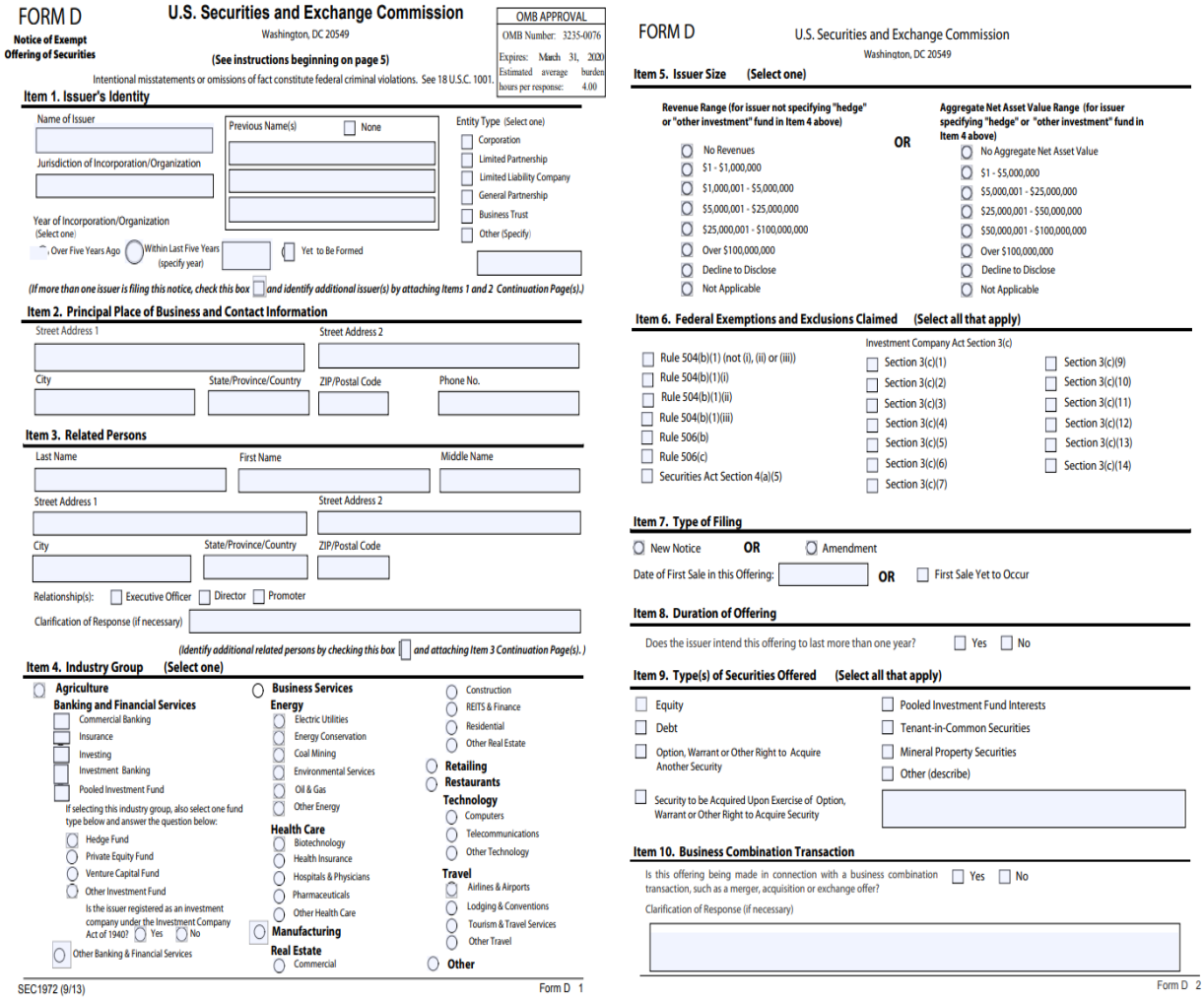
Table B13. Coefficient Estimates for the Cost-Benefit Analysis

This table shows the coefficient estimates for the cost-benefit analysis in section 9. I use the empirical specification as illustrated by equation (5). The dependent variable in column (1), *ln(Amount+1)*, is the natural logarithm of one plus the amount of angel investments in city i and time t. $ln(Sales+1)$ in column (2) is the natural logarithm of one plus the amount of sales generated in the next year by firms that received their angel investments in city i and time t. $ln(Employment+1)$ in column (3) is the natural logarithm of one plus the number of jobs supported in the next year by firms that received their angel investments in city i and time t. $ln(Num\_patents+1)$ in column(4), is the natural logarithm of one plus the number of patents generated by firms that received their angel investments in city i and time t. HV/NW is city i's home-value-to-net-worth ratio in 2011, *Post* is a dummy that equals one if period t is after 2011 and equals zero otherwise. Control variables, *Population*, *Income_per_person*, and *Home_value*, are described in section 4.2. I also control for time and city fixed effects. In all regressions, I double-cluster standard errors at the city level and at the time level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) *ln(Amount+1)*	(2) *ln(Sales+1)*	(3) *ln(Employment+1)*	(4) *ln(Num_patents+1)*
*HV/NW*Post*	-0.147*	-0.152**	-0.044***	-0.022***
	(0.087)	(0.067)	(0.012)	(0.006)
Population	0.240	0.144	-0.093	-0.064
	(1.073)	(0.805)	(0.162)	(0.035)
Income_per_person	0.615	0.382	-0.023	-0.105*
	(0.554)	(0.457)	(0.084)	(0.047)
Home_value	0.331	0.220	-0.102	-0.139***
	(0.515)	(0.365)	(0.060)	(0.042)
Constant	-9.248	-5.451	2.924	3.483***
	(12.536)	(12.422)	(2.408)	(0.877)
Observations	38,214	38,214	38,214	38,214
R-squared	0.433	0.454	0.554	0.447
City FE	YES	YES	YES	YES
Semi-annual FE	YES	YES	YES	YES

C Form D

Form D is used to file a notice of an exempt offering of securities with the SEC. The federal securities laws require the notice to be filed by companies that have sold securities without registration under the Securities Act of 1933 in an offering made under Rule 504 or 506 of Regulation D or Section 4(a)(5) of the Securities Act.[41] The figure below shows the first two pages of the Form D that firms file for exemption of registration to the SEC.



FORM D

Notice of Exempt Offering of Securities

U.S. Securities and Exchange Commission

Washington, DC 20549

(See instructions beginning on page 5)

Intentional misstatements or omissions of fact constitute federal criminal violations. See 18 U.S.C. 1001.

OMB APPROVAL

OMB Number: 3235-0076

Expires: March 31, 2009

Estimated average burden hours per response: 4.00

Item 1. Issuer's Identity

Name of Issuer

Previous Name(s) None

Entity Type (Select one)

Corporation

Limited Partnership

Limited Liability Company

General Partnership

Business Trust

Other (Specify)

Jurisdiction of Incorporation/Organization

Year of Incorporation/Organization (Select one)

Over Five Years Ago

Within Last Five Years (specify year)

Yet to Be Formed

(If more than one issuer is filing this notice, check this box and identify additional issuer(s) by attaching Items 1 and 2 Continuation Page(s).)

Item 2. Principal Place of Business and Contact Information

Street Address 1

Street Address 2

City

State/Province/Country

ZIP/Postal Code

Phone No.

Item 3. Related Persons

Last Name

First Name

Middle Name

Street Address 1

Street Address 2

City

State/Province/Country

ZIP/Postal Code

Relationship(s): Executive Officer Director Promoter

Clarification of Response (if necessary)

(Identify additional related persons by checking this box and attaching Item 3 Continuation Page(s).)

Item 4. Industry Group (Select one)

Agriculture

Banking and Financial Services

Commercial Banking

Insurance

Investing

Investment Banking

Pooled Investment Fund

If selecting this industry group, also select one fund type below and answer the question below:

Hedge Fund

Private Equity Fund

Venture Capital Fund

Other Investment Fund

Is the issuer registered as an investment company under the Investment Company Act of 1940? Yes No

Other Banking & Financial Services

Business Services

Energy

Electric Utilities

Energy Conservation

Coal Mining

Environmental Services

Oil & Gas

Other Energy

Health Care

Biotechnology

Health Insurance

Hospitals & Physicians

Pharmaceuticals

Other Health Care

Manufacturing

Real Estate

Commercial

Construction

REITS & Finance

Residential

Other Real Estate

Retailing

Restaurants

Technology

Computers

Telecommunications

Other Technology

Travel

Airlines & Airports

Lodging & Conventions

Tourism & Travel Services

Other Travel

Other

SEC1972 (9/13)

Form D 1

FORM D

U.S. Securities and Exchange Commission

Washington, DC 20549

Item 5. Issuer Size (Select one)

Revenue Range (for issuer not specifying "hedge" or "other investment" fund in Item 4 above)

No Revenues

$1 - $1,000,000

$1,000,001 - $5,000,000

$5,000,001 - $25,000,000

$25,000,001 - $100,000,000

Over $100,000,000

Decline to Disclose

Not Applicable

OR

Aggregate Net Asset Value Range (for issuer specifying "hedge" or "other investment" fund in Item 4 above)

No Aggregate Net Asset Value

$1 - $5,000,000

$5,000,001 - $25,000,000

$25,000,001 - $50,000,000

$50,000,001 - $100,000,000

Over $100,000,000

Decline to Disclose

Not Applicable

Item 6. Federal Exemptions and Exclusions Claimed (Select all that apply)

Rule 504(b)(1) (not (i), (ii) or (iii))

Rule 504(b)(1)(i)

Rule 504(b)(1)(ii)

Rule 504(b)(1)(iii)

Rule 506(b)

Rule 506(c)

Securities Act Section 4(a)(5)

Investment Company Act Section 3(c)

Section 3(c)(1)

Section 3(c)(2)

Section 3(c)(3)

Section 3(c)(4)

Section 3(c)(5)

Section 3(c)(6)

Section 3(c)(7)

Section 3(c)(9)

Section 3(c)(10)

Section 3(c)(11)

Section 3(c)(12)

Section 3(c)(13)

Section 3(c)(14)

Item 7. Type of Filing

New Notice OR Amendment

Date of First Sale in this Offering: OR First Sale Yet to Occur

Item 8. Duration of Offering

Does the issuer intend this offering to last more than one year? Yes No

Item 9. Type(s) of Securities Offered (Select all that apply)

Equity

Debt

Option, Warrant or Other Right to Acquire Another Security

Security to be Acquired Upon Exercise of Option, Warrant or Other Right to Acquire Security

Pooled Investment Fund Interests

Tenant-in-Common Securities

Mineral Property Securities

Other (describe)

Item 10. Business Combination Transaction

Is this offering being made in connection with a business combination transaction, such as a merger, acquisition or exchange offer? Yes No

Clarification of Response (if necessary)

Form D 2

[41] See more information on the website of the SEC: `https://www.sec.gov/smallbusiness/exemptofferings/formd`.