THREE ESSAYS IN CORPORATE AND ENTREPRENEURIAL FINANCE

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My dissertation consists of three chapters. In the first chapter, I study the impact of a place-based tax credit policy, the Opportunity Zone program created under the Tax Cuts and Jobs Act of 2017, on local private investments and entrepreneurship. Using a difference-in-differences approach and comparing census tracts designated as Opportunity Zones and other eligible but non-designated tracts, I find that the policy has drawn significantly more private investments to economically distressed areas. Surprisingly, however, these private investments have led to decreases in local new business registration. The decrease in entrepreneurship was mainly in the non-tradable sector, which is more sensitive to local conditions than the tradable or construction sector. Further robustness tests suggest that the above results are causal. I provide one explanation for the above findings that more private investments went to existing and older firms in Opportunity Zones, discouraging potential entrepreneurs from competing with the better-financed firms locally.

In the second chapter, I examine 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.

In the third chapter, which is co-authored with Thomas Chemmanur and Harshit Rajaiya, we address three important research questions by using a large sample of angel and venture capital (VC) financing data from the Crunchbase and VentureXpert databases and private firm data from the NETS database. First, we analyze the relative extent of value addition by angels versus VCs to startup firms. We show that startups

financed by angels rather than VCs are associated with a lower likelihood of successful exit (IPO or acquisition), lower sales and employment growth, lower quantity and quality of innovation, and lower net inflow of high-quality inventors. We disentangle selection and monitoring effects using instrumental variable (IV) and switching regression analyses and show that our baseline results are causal. Second, we investigate the complementarity versus substitution relationship between angel and VC financing. We find that a firm that received a larger fraction of VC or angel financing in the first financing round is likely to receive a larger fraction of the same type of financing in a subsequent round; however, when we include other non-VC financing sources such as accelerators and government grants into the analysis, a firm that received angel (rather than other non-VC) financing in the first round is also more likely to receive VC financing in a subsequent round. Third, we analyze how the financing sequence (order of investments by angels and VCs across rounds) of startup firms is related to their successful exit probability. We find that firms that received primarily VC financing in the first round and continued to receive VC financing in subsequent rounds (VC-VC) or those that received primarily angel financing in the first round and received VC financing in subsequent rounds (Angel-VC) have a higher chance of successful exit compared to those with other financing sequences (VC-Angel or Angel-Angel).

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Chapter 1

The Effect of Tax Incentives on Local Private Investments and Entrepreneurship: Evidence from the Tax Cuts and Jobs Act of 2017

1.1 Introduction

Entrepreneurship is crucial for economic growth and job creation (Decker, Haltiwanger, Jarmin, & Miranda, 2014). Over the past three decades, however, there has been a decline in entrepreneurship in the U.S., especially in economically distressed communities (Pugsley & Șahin, 2019).[1] Lack of access to capital is often cited as one of the largest handicaps for starting businesses in economically distressed communities.[2] Policymakers have undertaken many efforts to revitalize the economies of these towns and expand possibilities for the people who live there. Among them are the place-based programs that target neighborhoods instead of focusing on a group of people or a type of firm. Although place-based policies have attracted much debate and usually cost billions of dollars, little is known about their effects on local private investments and entrepreneurial activity, despite mixed findings from academic literature on local economic growth and employment. This paper studies the impact of a recent place-based tax policy in the U.S., the

[1]According to the Economic Innovation Group, the number of business establishments and employment fell by 8.3% and 6.7% for average distressed zip codes from 2010 to 2013, respectively, while average prosperous zip codes observed increases of 8.8% and 17.4% in business establishments and employment, respectively. The full report is available at `https://eig.org/wp-content/uploads/2016/02/2016-Distressed-Communities-Index-Report.pdf`.

[2]"One significant handicap for these communities has been the lack of access to loans, grants, and venture capital needed to start or expand a small business." (The U.S. Senate Republican Policy Committee, 2019).

Opportunity Zone program of 2017, on local private investments and entrepreneurship.

The Opportunity Zone program was introduced under the Tax Cuts and Jobs Act of 2017 (TCJA). The aim of the program is to draw long-term private investments to neighborhoods with high poverty and sluggish business growth by providing investors with tax incentives on the invested capital gains they earned from elsewhere. 8,762 out of 42,160 eligible census tracts, primarily low-income communities, were designated as Opportunity Zones. Investors who reinvest their capital gains from out-of-zone businesses into in-zone businesses for a qualified period can enjoy certain levels of tax delays and tax benefits, depending on the length of the investment (from five years to ten years). As the first attempt at place-based policy by the U.S. government in the past ten years, the Opportunity Zone is different from other government programs that target either specific types of firms or groups of people. In addition, the Opportunity Zone program involves much less government effort compared to other previous place-based policies as the government is only responsible for the selection of zones and offer tax incentives while in most of the previous practices, the government also actively participated in selecting businesses to locate inside the zones, monitoring in-zone companies, and providing infrastructure.[3] The importance of understanding the effects of the Opportunity Zone program goes beyond evaluating the policy itself as it also sheds light on how the government can better design and launch place-based policies.

In this paper, I use a difference-in-differences (DiD) approach and compare census tracts designated as Opportunity Zones with other eligible but not-designated tracts before and after the implementation of the Opportunity Zones policy. I find that the Opportunity Zone policy effectively introduced private investments to economically distressed areas. Compared to the non-designated tracts, Opportunity Zones experienced

[3] The Opportunity program is estimated to have an annual cost of $1.6 billion between 2018 and 2027. Some criticize that the expensive program would only drive up local real estate development and housing prices, crowding out low-income residents. Others worry that the program would only benefit the rich and increase inequality, given that only 7 percent of Americans report taxable capital gains on their tax returns, according to a report by the New York Times (see `https://www.nytimes.com/2019/08/31/business/tax-opportunity-zones.html`).

a 1 percent larger increase in the number of private investment deals and a 15 percent larger increase in the amount of private investments. When examining specific sectors, I observe significant increases in private investments to both real-estate-related firms and other businesses except financial firms. Several additional tests show support of causality. The estimation of the coefficient dynamics provides supportive evidence for the parallel trend assumption required by the DiD approach. I also perform a propensity score matching based on census tracts' observable characteristics, and the findings from the baseline regressions are robust using the matched sample.

I then examine the impact of the Opportunity Zone policy on local entrepreneurship. Inflows of private investments are likely to alleviate the capital constraints of potential entrepreneurs and boost local entrepreneurship (Cagetti & De Nardi, 2006; Evans & Jovanovic, 1989). Surprisingly, I find that the policy had a significantly negative impact on local business formation, analyzing the large dataset that contained more than 8 million business registration records during the sample period and collected from states' business registers. The results show that Opportunity Zones, on average, experienced a 2.3 percent greater decrease in the total number of new businesses incorporated (1.9 percent for for-profit firms and 1.8 percent for non-profit firms). To show that the decrease in new business formation was not trivial, I break down the decrease by the ex-post survival period and by the legal type of new firms. I show that the decrease in business formation was greater in magnitude with larger statistical significance for firms that could survive for a longer time ex-post (at least 1 year). When looking at the business structure of firms incorporated, I observe that the decrease was more significant and greater for firms that registered as a "corporation" than a "limited liability company" or other types such as partnerships or sole propriety.

Why the Opportunity Zone has successfully drawn more private investments to economically distressed census tracts but led to decreases in local entrepreneurship? One explanation is that existing firms received more private investments and used the additional financial resources to build up their competitive advantages such as pricing, advertisement, hiring, which discouraged potential entrepreneurs from starting up their businesses and competing with the existing firms locally, leading to a decrease in local entrepreneurship. I support this explanation by showing that the increases in private investments are four to five times larger for firms that have registered for at least one year than those yet-to-be-formed or just-incorporated companies. The tax-saving incentives provided by the Opportunity Zone policy, combined with the limited time for investors to select target companies, have shaped investors' preference toward less-risky, existing firms rather than newly-formed firms with little information, short operating history, uncertain future. Additionally, I show that the small business lending subsidized by the government agency Small Business Administration (SBA) did not help alleviate the shift in equity investing toward existing and older firms in Opportunity Zones. The SBA lending to firms more than one-year-old did not experience significant decreases. In contrast, the lending to less than one-year-old firms significantly decreased.

To examine which type of entrepreneurship was more affected by the policy, I decompose local business formation into four sectors as in Mian and Sufi (2014).[4] Then, I perform the previous DiD analysis and find that the Opportunity Zone had a significantly negative impact on local entrepreneurship in the non-tradable sector, which is more sensitive to local conditions. However, there was no significant impact on the formation of businesses in the tradable sector or the construction sector. The above decomposition of the decrease in local entrepreneurship provides further support for the explanation that the private investments introduced by the Opportunity Zone policy helped the local incumbents to maintain their market position while discouraging new business formation.

Further, I examine the real effects of the Opportunity Zone policy. I first show that

[4] Even though the business registration dataset does not provide the industry classification code, I use the machine learning approach and train an algorithm using company names and geographical locations to predict the sector of local companies. The trained model based on Logistic Regression achieves a prediction accuracy of 83%.

there was no significant change in local housing price in Opportunity Zones after the policy implementation, compared to other eligible but non-designated census tracts. I do not observe a significant increase in the number of mortgage applications in Opportunity Zones. I show that the number of people (by various education or poverty levels) moving into the Opportunity Zones did not change significantly compared to other eligible but non-designated census tracts. Additionally, the total number of employment in counties with more population living in census tracts designated as Opportunity Zones did show significant changes compared to other counties. The above results together picture that despite costing billions of taxpayers' money, the policy had limited economic impact in terms of geographical mobility and employment. One reason for the limited real effect is closely related to the decrease in the formation of new businesses in Opportunity Zones: Previous literature has documented that younger firms create more jobs than existing and older firms (Adelino, Ma, & Robinson, 2017).

This paper provides important policy implications. Future policymakers need to take consideration of the potential distributional effects across businesses with different levels of riskiness when offering market-based tax incentives. One potential better way is to provide higher tax incentives riskier firms such as new businesses or non-real-estate firms.

The rest of the paper is organized as follows. Section 1.2 discusses the contribution of this study to related literature. Section 1.3 introduces the institutional background of the Opportunity Zone policy. Section 1.4 describes the data sources and variable construction. Section 1.5 shows the empirical strategy and results of the impact on local private investments and entrepreneurship. Section 1.6 provides the explanation of the above findings. Section 1.7 tests whether the Opportunity Zone program had real effects on the local economy. Section 1.8 concludes the paper and discusses the policy implications.

1.2 Literature Review

This paper contributes to several strands of literature. First of all, this paper builds on literature that studies government's role in motivating entrepreneurial activity (Bayar, Chemmanur, & Liu, 2019). Many types of government programs have been created to address market failures associated with entrepreneurial finance (Hall, 2002). One type includes government awards and grants. Lerner (2000), Audretsch, Link, and Scott (2002), and Howell (2017) show that the awards provided by the U.S. Small Business Innovation Research (SBIR) positively impact firms' R&D investment, commercialization, subsequent firm growth, the probability of receiving subsequent VC financing, and innovation output. Da Rin, Nicodano, and Sembenelli (2006), however, find no evidence that public R&D spending has a positive effect on innovation using European data while Babina, He, Howell, Perlman, and Staudt (2020) show that industry grants, compared to government grants, lead to greater appropriation of intellectual property. There are also government-sponsored venture capitalists (GVCs) who invest equity in entrepreneurial firms, as shown by Brander, Du, and Hellmann (2015) that GVCs help firms obtain more funding than private VCs. Another type of government intervention in the entrepreneurial finance market is providing loans to small business such as the Small Business Administration in the U.S.[5] This paper adds to this literature by focusing on another type of policy, place-based tax incentives, on local entrepreneurial activity.

Second, this paper contributes to the literature that examines the impact of tax policies on the local economy. Previous papers have studied the impact of corporate tax rates on economic growth (Romer & Romer, 2010), employment (Suárez Serrato & Zidar, 2016), innovation (Mukherjee, Singh, & Žaldokas, 2017), and reallocation of establishments and employment within companies (Giroud & Mueller, 2019). Others

[5]Brown and Earle (2017) show that SBA loans have a positive impact on the employment by small businesses and the taxpayer cost per job created is over $21,000. Denes, Duchin, and Hackney (2019) employ the changes in industry size standards and show positive impact of government subsidies, including loans, on the growths of employment and wages of small businesses.

have look at the effectiveness of tax credits on encouraging corporate R&D spending (Bloom, Griffith, & Van Reenen, 2002; Z. Chen, Liu, Suárez Serrato, & Xu, in press; Wilson, 2009). In terms of the effect of promoting entrepreneurship, the results of previous studies depend on the type of policy and whether the policy targets on a group of people, a neighborhood, or a type of firms. Curtis and Decker (2018) show positive effects of lowering corporate taxes on new business formation. On the other hand, Denes, Howell, Mezzanotti, Wang, and Xu (2020) show that investor tax credits increase angel financing, but do not have a significant effect in boosting high-growth entrepreneurship. The place-based tax policies, targeting specific communities instead of targeting firms or people, have not been well studied of its impact on local entrepreneurship and my paper provides important policy evaluation and implications by studying the Opportunity Zone program.

Third, this paper contributes to the literature on the effects of place-based policies. Most previous studies examine the past place-based policies which have involved much more government efforts(such as certifying and monitoring) than the Opportunity Zones, with mixed findings of the impact on local economic growth and employment (see Neumark and Simpson (2015) and Austin, Glaeser, and Summers (2018) for a summary of the studies).[6] After the introduction of the Opportunity Zone, there have been studies looking at its impact on local housing prices (J. Chen, Glaeser, & Wessel, 2019), commercial property prices (Sage, Langen, & Van de Minne, 2019), employment and wage growth (Arefeva, Davis, Ghent, & Park, 2020; Atkins, Hernandez-Lagos, Jara-Figueroa, & Seamans, 2020; Freedman, Khanna, & Neumark, 2021). My paper differentiates from the above studies by examining the impact of the policy on private investments and entrepreneurship and suggests potentially negative distributional effects of the policy on entrepreneurs and small business owners within affected regions, who were part of the group that the policy aimed to help.

[6]Tian and Xu (in press) show that the national high-tech zones in China, a place-based policy, positively affected local innovation and entrepreneurship.

1.3 Institutional Background

The Opportunity Zone policy was introduced under the Tax Cuts and Jobs Act (TCJA) and signed into law on December 22, 2017. The main aim of this policy was to provide tax incentives to potential investors to re-invest capital gains to economically distressed communities and boost local economic development in these communities. More than 8700 census tracts were designated in the U.S. Figure 1.1 shows the geographical distribution of the Opportunity Zones.

The Opportunity Zone policy is the first place-based policy introduced in the U.S. in the past decade. Previous place-based policies usually involve a lot of government efforts such as selecting firms for grants or tax benefits and monitoring their uses. Despite costing about $60 billion annually (Bartik, 2020), studies have shown mixed findings of the impact of place-based policies on local investment, employment, and economic growth (Busso, Gregory, & Kline, 2013; Freedman, 2012; Neumark & Kolko, 2010).[7] The Opportunity Zone program differentiates from most previous place-based policies by taking a more "market-based" approach as they "have no cap on participation and require no government approval" (Council of Economic Advisers, 2021).

The Opportunity Zone concept was first proposed by the Economic Innovation Group in 2015. In April 2016, the bill to create Opportunity Zones was first introduced in the Senate and House and reintroduced in February 2017 but did not get much attention. The introduction of the TCJA at the end of 2017 finally created Opportunity Zones. After the introduction, the U.S. Department of Treasury first identified 42,160 eligible census tracts out of 74,134 census tracts in the U.S. For a census tract to be eligible for the designation, it has to be a "low-income community" (LIC) that has a

[7] The most comparable precedents are the federal Empowerment Zones (EZ) and the New Markets Tax Credit (NMTC), which offer certain tax credits to companies located in a specific area. Compared to the Opportunity Zone program, the tax incentives offered in the EZ program target not only investments but also employment and property development. The size of the tax incentives is much smaller than the Opportunity Zone program. NMTC also targets economically distressed communities but the tax credits provided are capped and offered to companies on a competitive base.

poverty rate higher than 20% or a median household income of less than 80% of the local median statewide household income, or the tract is contiguous to a LIC tract and has a relatively low household income. Governors could nominate up to 25% of a state's LIC census tracts for Opportunity Zone designation by March 21, 2018 (was later extended to April 20, 2018). Up to 5% of the total nominated tracts can be non-LIC but contiguous to a nominated LIC tract. In the end, 8,762 census tracts received the designation of Opportunity Zone, of which 8,534 were LICs, by the U.S. Treasury Department as Opportunity Zones in June 2018.

The benefits of investing capital gains received from out-of-zone businesses in Opportunity Zones are as following. First of all, taxes on the initial capital gain will be deferred until 2026 or when the asset is sold. In addition, upon investing the capital gain for at least seven years (five years), Opportunity Zone investors can receive a reduction of 15% (10%) on the amount of prior capital gains tax. Finally, for investment held for more than 10 years, investors will receive an increase in the tax basis that equals to the fair market value upon sale, effectively making the taxes due from new capital gains eliminated. To receive the tax benefits, investors need to invest capital gains into either Qualified Opportunity Zone (QOZ) businesses or Qualified Opportunity Funds (QOF). QOZ businesses need to have at least 50% of their gross income earning from trade, business, or service conducted in an Opportunity Zone and QOFs need to invest at least 90% of their assets into QOZ businesses. There is no other requirement for receiving the tax benefits except the investment period and the geographical location. 2018 is the first year for investments to qualify for participating the Opportunity Zone program.

Ideally, anyone with capital gains may invest in Opportunity Zones. In practice, however, most QOFs have filed for an exemption with the SEC under Regulation D, Rule (b) and Rule (c) limiting their offerings to mostly accredited investors. In the sense that they obtaining their funding from mostly accredited investors, QOFs are similar as other financial intermediaries with a focus on the private market such as angel groups, venture capital funds, and private equity funds. On the other hand, there are several differences between QOFs and the above financial intermediaries. The first is the restriction on the geographical location of the invested firms to be mainly in economically distressed areas designated as Opportunity Zones while other funds can freely invest in companies all over the U.S. The second difference is that investors need to invest their capital gains into a QOF within 180 days after their capital gains are triggered and QOFs are subject to the same restriction to invest their money into QOZ businesses within 180 days.[8] For other private funds such as venture capital (VC) funds, they do not have a specific deadline of finishing investment choices and Sorenson and Stuart (2001) shows that VC firms begin investing one year after closing a fund and invest 80% of their committed capital within the first three years. As later discussed in the paper, these differences between QOFs and other financial intermediaries can be the explanations of why the Opportunity Zone program and the associated QOFs have a negative effect on local entrepreneurship while the previous literature has in general found positive impact of venture capital, private equity, and angle investors on entrepreneurial activity (Ewens & Farre-Mensa, 2020; Samila & Sorenson, 2011; J. Xu, 2019).

The Opportunity Zone has attracted much attention from investors and the dollar amount involved has been sizable. As of July 2021, more than 300 QOFs, with more than $64 billion investment capacity, have been created since the passage of the law.[9] Congress' Joint Committee on Taxation estimated that the loss of federal revenue over ten years created by the Opportunity Zone program to be at least $1.6 billion.[10]

[8]The IRS relaxed the 180-day rules after the Covid outbreak in 2020, which is beyond this study's time scope and does not affect the results of this study.

[9]There are 303 QOFs as of July 6, 2021, according to the OpportunityDB website (`https://opportunitydb.com/funds/`).

[10]Joint Committee on Taxation, "Estimated Budget Effects of the Conference Agreement for H.R. 1, the 'Tax Cuts and Jobs Act,'" JCX-67-17, `https://www.jct.gov/publications.html?func=startdown&id=5053`.

1.4 Data

1.4.1 Data Sources and Variable Construction

Data on Private Investments

I collect data on private investments from Form D filings. Historically, information on private investment has been hard to observe, and very recently, some papers have started to use Form D filings to analyze private investments (Denes et al., 2020; Ewens & Malenko, 2020; J. Xu, 2019). The federal securities laws require that firms who raise capital by conducting private placements file a Form D, a notice of an exemption for security offerings, with the SEC. Form D filings track information such as the name, location, industry, incorporation year of filing firms, and the date and total offering amount of each filing. Firms are required to file Form D within 15 days after the first sale of securities in the offering. Failure of filing a Form D may incur consequences such as being prohibited from future private investments and constituting a felony.[11] Appendix 4 shows a sample Form D.

Using the information from Form D filings, I construct two variables measuring local private investments occurred in a census tract and a given year, the natural logarithm of one plus the number of private investment deals (*Ln(Num_Inv+1)*) and the natural logarithm of one plus the dollor amount of private investment deals (*Ln(Amount_Inv+1)*). To further break down the impact of the policy on local private investments in across sectors, I category all the investment deals into three sectors (business, real estate, and finance) based on the industry information provided in Form D.[12]

[11] See more details from the guidance issued by SEC on the Regulation D Rule 507 and the case of *Hamby v. Clearwater Consulting Concepts*.

[12] Item 4 in Form D provides the industry group of an issuer. I label firms in the "Banking and Financial Services" industry group as the "Finance" sector, "Real Estate" as a sector, and all the other industry groups as "Business" sector.

Data on Business Registration

To examine how the Opportunity Zone policy impacts local entrepreneurship, I use local business registration data provided by *OpenCorporates*. *OpenCorporates* is an open database of companies all over the world and it collects information from business registers (or other regulatory sources). The database contains the company name, address, type, dates of incorporation and dissolution (if applicable) and other company information. Some recent studies have used data from *OpenCorporates* to obtain information such as the incorporation date and active status for private firms (Bogdani, Causholli, & Knechel, 2021; Ewens & Farre-Mensa, 2020).

I first geocode company addresses using the Census Geocoder API to obtain the census tract code.[13] I then aggregate the number of companies incorporated for each census tract by their incorporation year. Specifically, I define *Ln(Num_NewFirm+1)* as the natural logarithm of one plus the number of new firms incorporated in a census tract and a given year. To further look into the types of firms incorporated, I aggregate the number of new firms incorporated by whether they are for-profit or non-profit and construct *Ln(Num_NewForProfit+1)* and *Ln(Num_NewNonProfit+1)*, the natrual logarithm of one plus the number of for-profit and non-profit firms incorporated in a census tract and a given year, respectively. To measure the quality of firms formed, I looked their survival period. I group firms into those that have maintain an active status for at least one year and two years after registration of business and those have not. Based on the legal structure of companies incorporated, I group new firms into those incorporated as a corporation, limited liability company, or other types such as a partnership for a given tract in a year. *OpenCorporates* does not provide business registration records in Delaware, Illinois, and Puerto Rico due to limited accessibility of the state government

[13]When more than one address is provided for a company, I only use the business address instead of the mailing address. I excluded a firm from the sample when it only has one address and the address is associated with a P.O. box because the opportunity zone policy requires real business operation physically happens in a census tract.

websites, therefore, the census tracts for these states are excluded in the analysis on local business formation.

Data on Small Business Lending

To examine the financing aspect of local entrepreneurial activity, I use data on lending under the Small Business Administration (SBA)'s 7(a) and 504 programs. Under the Freedom of Information Act, I am able to collect information for each borrower firm's name, address, loan approval date, loan amount, and other information. Using firm name and location, I match the business registration data with the SBA lending data. Then, I categorize the SBA loans by firm age: borrower firms registered for more than one year (or half year) and those not. I examine both the number of SBA loans and amount of SBA loans (*Num_SBALoan* and *Amount_SBALoan*) for each types of borrowers. The SBA loan observations are then aggregated to the census tract level by year.

Using Machine Learning to Assign Business Sector

One data challenge to perform cross-sectional analysis for this study is that Form D and *OpenCorporates* data do not have industry codes such as NAICS or SIC. I tackle this issue by taking a machine learning approach.[14] I first use the NETS database, which contains NAICS code, to train the model and then use the trained model to predict the sector of the firm.

To conduct machine learning, I first prepare the independent and dependent variables for both the training data set and prediction data set. I mainly use company names together with geographical information to predict the sector of a firm following Cuffe et al. (2019).[15] I use data from the NETS to form the training data. I first stan-

[14]Many recent studies in economics and finance have used machine learning, see Mullainathan and Spiess (2017) for a survey.

[15]Cuffe et al. (2019) use company name as well as information web-scraped from Google Reviews to predict the industry code of firms. They first use the *word2vec* approach to analyze and vectorize the text information. They then adopt a *RandomForest* model to predict the industry code. Using this approach, they achieved a 59% accuracy in assigning correct NAICS sectors.

dardize the text data by lowering the case, removing punctuations, special characters, and stopwords. There are 2,420,466,463 unique words in the training data set before standardization and 224,911,471 unique words after. I then vectorize the words in company name using the Count Vectorization approach. Combining with the geographical information (zip code), I obtain a vector of information to predict a firm's sector.

Mian and Sufi (2014) categorize industries, based on the connectedness with local supply and demand, into four sectors: tradable, non-tradable, construction, and other. As the NETS database provides NAICS code, I then match the list of industry codes in the Appendix of Mian and Sufi (2014) with the four-digit NAICS code provided in NETS for each firm. To evaluate the model performance and select the model that has the best prediction accuracy, I split the NETS data sets into two data sets: 80% as the training set and 20% as the validation set. I train the model on the training set and then predict the sector in the validation set. I focus on three types of algorithms, Logistic Regression, XGBOOST, and Support Vector Machine. The prediction accuracy on the validation set is 82.7%, 80.5%, and 72.6%, respectively. Therefore, I choose the model trained with Logistic Regression and predict firms' sector.[16]

Data on Housing Prices and Mortgages

To examine whether the change in local housing prices serves as a mechanism of how policy impact local private investments and entrepreneurship, I look at data on housing prices as well as the mortgage applications. Federal House Finance Agency (FHFA) provides an annual housing price index (HPI) for each census tract. I collect the mortgage application information from the Home Mortgage Disclosure Act (HMDA) database. Given that the distribution is right-skewed for the raw HPI and the number of mortgage applications, I take a log transformation for these two variables.

[16] I have perform the same training process on the SBA loan datasets, which also provides the name, industry code, and geographical location of firms. The prediction accuracy are similar.

Control Variables and Data on Geographical Mobility

To control for changes in local demographic and economic conditions, I include the population, median income, the percentage of white people alone, poverty rate, and unemployment rate for each census tract in a given year. The data for the control variables are from the annual American Community Survey (ACS), which is conducted among three million U.S. residents each year.

To examine whether changes in the geographical mobility is a mechanism of the policy to have an effect, I use census-tract-level data from ACS and look at the total number of people moved into a census tracts in a year. I also group the in-migrants to a census tract by their education level and poverty status.

1.4.2 Summary Statistics

Summary statistics are reported in Table 1.1. In the sample of this study, there are 42,171 census tracts which were eligible for designation of Opportunity Zones, among which 31,859 are low-income-communities (LIC) while the rest are non-LIC contiguous tracts. There are 8,761 census tracts in the sample that were designated as Opportunity Zones and 8,531 tracts are LICs. For most analyses in the paper, I only include the LIC tracts to make the treated and control groups more comparable. The main findings are all robust if including the non-LIC contiguous tracts. The sample period is from 2015 to 2019. All the tract-level amount variables are winsorized at the 1st and 99th percentiles to avoid data errors involving extreme values to drive the results.[17]

As shown in Table 1.1, 24.7% of the tracts are Opportunity Zones while the rest tracts are eligible but non-designated. On average, a census tract receive 0.119 private investments per year with an average amount of $2.732 million and has around 10 new firms incorporated. An average census tract in the sample has 4,046 people with a median income of $39,156, a poverty rate of 22%, 61% of its people are white, and an

[17] The results are similar when not winsorizing these variables.

unemployment rate of 11%.

1.5 Empirical Analysis and Results

1.5.1 Main Specification

The Opportunity Zone policy can serve as a natural experiment to local private investment and entrepreneurship. I use a difference-in-differences (DiD) approach to identify the impact of the Opportunity Zone policy on local private investments and entrepreneurial activity. In the baseline regressions, I estimate the following equation:

$$Y_{i,t} = \alpha + \beta OZ_i * Post_t + Controls_{i,t-1} + \delta_t + \eta_i + \epsilon_{i,t}. \tag{1.1}$$

where i is a census tract and t represents a year. OZ is an indicator that takes a value of one if the tract was designated as an Opportunity Zone (OZ) and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterwards. To control for local demographic and economic characteristics, I include the natural logarithm of the population, the natural logarithm of the median income, the natural logarithm of the median age, poverty rate, percentage of white or black people, percentage of population without a high-school degree, and unemployment rate of census tract i in year *t-1* as control variables. To account for unobservable location-specific characteristics and time-specific trends, the DiD model further includes census tract fixed effects and year fixed effects. I cluster standard errors by tract.[18]

1.5.2 Identification Assumptions and Challenges

The difference-in-differences approach compares outcome variables before and after the policy between designated and eligible but non-designated census tracts. This identifications strategy relies on two main assumptions. First, I assume that changes in private

[18]The main findings are robust when I use the alternative clustering options (see results in Table A1 in the Appendix).

investments would have been the same across census tracts with and without the designation as an Opportunity Zone, absent of the TCJA policy change (i.e., the parallel trend assumption). Second, I assume that the Opportunity Zone policy was not determined based on the level of local private investments and new business formation prior to the policy.

I take several steps to substantiate these assumptions and address potential concerns. One concern is that the selection of Opportunity Zones was not random and outcome variables such as private investments may have evolved differently between designated and non-designated tracts absent of the policy. I address this concern by plotting the annual coefficient estimates around the policy was introduced in 2017. As shown in Figures 1.2 and 1.3, local private investments and entrepreneurship did not diverge prior to the policy was introduced as the 95% confidence intervals all covers zero for 2015 and 2016. The differences between the treated tracts and control tracts started to enlarge significantly only after 2017. These two figures provide support for the parallel trend assumption required by the difference-in-differences approach.

Another concern is related to the differences in characteristics between the designated and non-designated tracts. Even though the difference-in-differences strategy requires only the pre-trends to be similar instead of the levels of other characteristics, some may still worry that non-balanced covariates may threaten the parallel trend assumption. To alleviate this concern, I first add tract-level control variables including population, median income, median age, percentage of white or black population, poverty rate, and unemployment rate. I also performed a propensity score matching (PSM) on pre-treatment characteristics for tracts designated as Opportunity Zones and keep the non-designated tracts with the highest propensity score within the same county as its matched control. Column (1) of Table A2 shows the logit regression for producing the propensity score and Column (2) shows the logit regression running on the matched sample. One can observe that the independent variables lost significance in the matched sample and the pseudo R2 decreased from 0.055 to 0.0012, suggesting the pre-shock characteristics are comparable in the matched sample and not likely to explain the selection of the Opportunity Zone the matched sample. Table A3 shows the DiD results with a sub-sample with matched pairs from PSM and suggests that the main findings are robust. To further ensure that the comparability of designated and non-designated census tracts, I conduct the analyses in the paper mainly using a sample that only contains tracts that are considered low-income communities (i.e., not include non-low-income but contiguous tracts), even though most results stay robust using all eligible census tracts (see Table A4).

1.5.3 Impact on Local Private Investments

I first study the impact of the Opportunity Zone policy on local private investments. As the main aim of the policy is to draw investments to economically distressed areas that otherwise would not be invested, it is important to first check whether the policy indeed has any effect on local private investments.

Table 1.2 shows the results. In Columns (1) and (2), the dependent variable is the natural logarithm of one plus the number of private investment deals in census tract i and year t, *Ln(Num_Inv+1)*. In Columns (3) and (4), the dependent variable is replaced with the the natural logarithm of one plus the dollar amount of private investments, *Ln(Amount_Inv+1)*. I show the results when only county and year fixed effects are included with no control variables in columns (1) and (3) and with control variables in Columns (2) and (4). The coefficient estimates on $OZ * Post$ in Table 1.2 are all positive and significant at the 1% level. The magnitude of the coefficient estimates suggests that the effects are also economical sizable: The number and amount of private investments flowed into treated tracts (the Opportunity Zones) increased 1% and 14.2%, respectively, more than those eligible but not designated tracts after the policy shock.

Next, I examine the impact of the Opportunity Zone policy on local private investments by sector. In Table 1.3, I group all the private investments into three categories based on the "industry" information provided in the Form D filings: Business, real estate, and finance. The empirical specification is similar as in Table 1.2 and dependent variables are the number of deals (*Ln(Num_Inv+1)*) and the dollar amount of deals (*Ln(Amount_Inv+1)*). We observe that the coefficient estimates on $OZ * Post$ for investments in the business sector both statistically significant at 1% significance level, indicating that Opportunity Zones experienced a 0.9% larger increase in the number of investments and a 13.1% larger increase in the amount of investments, compared to other eligible but non-designated census tracts. I also observe that the Opportunity Zone policy had a positive and statistically significant impact on private investments in the real estate sector even though the size of the impact is smaller than on the business sector: Opportunity Zones experienced a 0.1% larger increase in the number of investments and a 2.1% larger increase in the amount of investments, compared to other eligible but non-designated census tracts. On the other hand, the policy does not have a statistically significant impact on the financial sector.

1.5.4 Impact on Local Entrepreneurship

The previous results show that Opportunity Zone policy has drawn significantly more private investments into treated census tracts compared to other non-designated zones after its implementation. How will the increased local investments affect local business formation? Some may argue that increased investments could help entrepreneurs relax their liquidity constraints and, therefore, foster local business formation (Evans & Jovanovic, 1989; Kihlstrom & Laffont, 1979; Knight, 1921). On the other hand, investors may have certain preferences toward existing older firms than newly-formed firms due to information frictions between investors and entrepreneurs about the quality of the firms. When existing local firms are equipped with more financial resources to build up their competitive advantages, potential entrepreneurs could potential be discouraged to start up businesses in the place. In this section, I empirically examine how the Opportunity Zone policy affect local business formation.

Table 1.4 shows the impact of the Opportunity Zone policy on local entrepreneurship by running the same DiD regression illustrated by Equation (1.1). In Column (1), the dependent variable is the total number of new businesses registered in census tract i and year t (*Ln(Num_NewFirm+1)*). The coefficient estimate on $OZ * Post$ is negative and statistically significant at the 1% significance level. This suggests that census tracts that were designated as Opportunity Zones, on average, experienced a 2.7% larger decrease in local entrepreneurship compared to the other eligible but non-designated tracts after the introduction of the policy. In Columns (2) and (3), the dependent variables are replaced with the number of for-profit firms registered (*Ln(Num_NewForProfit+1)*) and the number of non-profit firms (*Ln(Num_NewNonProfit+1)*) registered in census tract i and year t. We observe negative and statistically significant coefficient estimates on $OZ*Post$, suggesting that both the establishment of for-profit firms and non-profit firms experienced significantly greater decreases after the introduction of the Opportunity Zone policy for the designated census tracts, compared to eligible but non-designated tracts.

I also examine the policy's impact by the heterogeneity of firms incorporated. I begin by showing that the decrease in local entrepreneurship appear in all types of businesses registered: corporation, limited liability company (LLC), or other types such as partnership. Table 1.6 shows that there was a decrease for all three types of firms registered with firms registered as a corporation had the largest decrease (2.5%), and LLC the second largest decrease (2.1%), and other types the least (0.5%).

Next, I examine the heterogeneity of impact on firms with different survival length. In Table 1.5, I group all the new firms formed by whether or not they could survive for more than one or two years.[19] I find that the decreases are mainly in the firms that

[19]The information is also collected from *OpenCorporate*, which contains the date when a firm was incorporated and the date when it was dissolved.

can survive for a longer time. If the survival time can be considered as one measure of firm quality, this evidence indicate that the decrease in local entrepreneurship brought by the policy is a serious concern.

To alleviate the concerns regarding the quality of the business registration data collected from *OpenCorporates*, I obtain data from the ZIP Codes Business Patterns (ZBP) provided by the Census Bureau and validate the results. Instead of counting the number of firms registered, the ZBP provides the statistics on the total number of establishments in a zip code. I calculate the changes in the number of establishments to as a proxy for local net creation of businesses.[20] As shown in Column (1) in Table A5 in the Appendix, the Opportunity Zone policy had a negative effect on local net establishment creation, which is another proxy for local entrepreneurship, confirming the previous finding using the *OpenCorporates* data. The ZBP data also provides the employment size of establishments and I show that the decrease in local net creation of establishments was mainly concentrated in small ones (with less than 10 employees or between 10 and 50 employess) but not in large ones (with more than 50 employees).

1.6 Mechanisms

The previous findings show that the Opportunity Zone policy has generated positive impact on local private investments but negative impact on local entrepreneurship. In this section, I test the potential explanations for the above seemingly surprising findings. I first show that existing (older) firms receive significantly more financing resources (both equity and debt) than newly-formed firms in Opportunity Zones compared to other eligible census tracts, suggesting an unintended "crowding-out effect" of the policy. Anticipating that older firms would have better access to financial resources and competitive advantage with newly-formed firms, potential entrepreneurs chose not to open businesses in the Opportunity Zones. I corroborate this hypothesis by decom-

[20]Many studies in the economics and finance literature have used this data set to measure local entrepreneurship (see Adelino, Schoar, and Severino (2015) as an example).

posing the local entrepreneurship into non-tradable, tradable, construction and other sectors following Mian and Sufi (2014). I also show that other hypotheses, related to housing price changes and geographical mobility, are unlikely to explain the previous findings on local private investments and entrepreneurship.

1.6.1 Older Firms Increased Access to Finance Than Newly-Formed Firms

The aim of the Opportunity Zone policy is to invite private investments to economically distressed communities that otherwise would not happen. The policy, however, does not have any restriction or requirement on which type of firm investors should to invest in. Which type of firms would be more attractive to investors for them to receive tax benefits? This question can be tested empirically. To link this question with the finding of the decrease in the number of new businesses formed after the introduction of the Opportunity Zone policy, I am particularly interested in the age of the firms that receive private investments.

In Table 1.7, I group firms by whether or not they have been established for at least one year and examine the impact of the Opportunity Zone policy on local private investments received by the two groups of firms. The dependent variables are the number of investment deals (*Ln(Num+1)*) and the amount of deals (*Ln(Amount+1)*) like in Table 1.2. The other empirical specification is similar as in Equation (1.1). The first two columns in Table 1.7 suggest that the Opportunity Zone policy has invited 1% more private investment deals for firms that have been incorporated for at least one year (statistically significant at 1 percentage level) and 0.2% more for firms that are less than one-year old (statistically significant at 5 percentage level). The last two columns in Table 1.7 show that the Opportunity Zone policy has invited 14% greater amount of private investments for more-than-one-year-old firms (statistically significant at 1 percentage level) compared to 3.5% more for less-than-one-year-old firms (statistically significant at 5 percentage level).

In Table 1.8, I find similar crowding-out effect in loans offered under the Small Business Administration (SBA) program, another common financing source for entrepreneurs. I observe that the number of SBA loans and the total amount of SBA loans offered to firms that are less than one year old decreased significantly after the introduction of the Opportunity Zone policy for the treated census tracts compared to control tracts. On the other hand, the number and amount of SBA loans offered to firms that are at least one year old did not experience significant decreases.

The above findings show that the Opportunity Zone policy while bringing in private investments into economically distressed communities, have (unintended) distributional effects of financing sources among older and newly-formed firms. Potential entrepreneurs observe or anticipate this crowding-out effect against new firms would be deterred to start businesses in the areas that were designated as Opportunity Zones.

1.6.2 Declined Entrepreneurship in Sectors Sensitive to Local Competition

As investors prefer existing and older firms located in Opportunity Zones to put their money in, these firms can use the money to build their competitive advantage over the newly-formed firms. A rational potential entrepreneur should therefore not start up business in the Opportunity Zones to compete with the existing firms. If the above hypothesis holds, I should observe that the decline in local entrepreneurship mostly in sectors that are more sensitive to local competition.

Following the definition in Mian and Sufi (2014), I categorize firms into non-tradable, tradable, construction, and other sectors. Table 1.9 shows the results of testing the impact of the Opportunity Zone on local entrepreneurship by sector. The coefficient estimate on $OZ * Post$ is negative and statistically significant for the non-tradable and other sectors while insignificant for the tradable and construction sectors. The results suggest that the decline in local entrepreneurship was mainly concentrated in sectors that are more sensitive to local demand and supply. Existing firms can use the additional financial resources brought by the Opportunity Zone to gain more competitive advantages locally such as lowering prices, making advertisement, and hiring labor. Sage et al. (2019) indeed find significant increases in local commercial property prices after the introduction of the policy in Opportunity Zones. Potential entrepreneurs, especially the ones in non-tradable sector, anticipate (or observe) that they would not have the same financial resources to compete with existing firms in the neighborhood, therefore, choose not to start the business.

1.7 Real Effects of the Opportunity Zone Program?

Even though the Opportunity Zone had a negative impact on the formation of new businesses, some may question if this is a cost worth considering if the policy had other positive impact on the local economy such as increasing the employment opportunities. In this section, I test whether there were any significant changes in local housing price and the number of mortgage applications in Opportunity Zones. I also examine if the number of people moving into the Opportunity Zones changed significantly compared to other eligible but non-designated census tracts. Finally, I compare the total number of employment in counties with more population living in census tracts designated as Opportunity Zones to other counties.

Table 1.10 shows the impact of Opportunity Zone policy on local housing prices and applications for housing mortgages. I collect the annual housing price index (HPI) for each census tracts provided by the Federal House Finance Agency (FHFA). The mortgage application information is from the Home Mortgage Disclosure Act (HMDA) database. The dependent variable is the natural logarithm of HPI (*LnHPI*) in Column (1) and the natural logarithm of one plus the number of total mortgage applications (*Ln(Num_mortgage+1)*). The rest of the empirical specification is the same as in Equation (1.1). One can observe that the coefficient estimates on $OZ * Post$ are statistically insignificant in either columns. The results indicate that the Opportunity Zone did not have a significant impact on the local housing market.[21]

Table 1.11 shows the policy's impact on the total number of people moved into a census tract each year and the decomposition of these migrants using data from the American Community Survey. Column (1) shows the results when the dependent variable is the natural logarithm of one plus the total number of in-migrants to a census tract in a year. I do not observe a significant coefficient estimates on $OZ * Post$, suggesting there is no significant changes in the number of people who moved into Opportunity Zones after the introduction of the policy compared to other eligible but non-designated tracts. Columns (2) to (4) show the results when examining the number of in-migrants by the level of education (less than high-school, high-school degree, and bachelor's degree and above). Columns (5) to (7) show the effects on in-migrants decomposed by their poverty status (less than 100 percent, between 100 to 149 percent, and at or above 150 percent of poverty level). Again, I do not observe significant effects of the Opportunity Zone policy on the inflows of a specific group of people by education or by poverty status.

I then examine whether the policy had any impact on local employment. I switch the observation unit to county level from census tract level due to data limitation. I construct the treatment variable at the county level, $OZ\%$, the percentage of people reside in Opportunity Zones in a county. The other empirical specification is similar as in Equation (1.1) with the census tract fixed effect changed to county fixed effect and the clustering of standard errors set at the county level instead. Table 1.12 presents the results: The policy had no significant impact on the total number of employment in various counties differentially exposed to the Opportunity Zone policy with only increases in the employment in the construction and other sectors but not in the non-tradable and tradable sectors.

[21] J. Chen et al. (2019) also find no significant effects of the Opportunity Zone on local housing prices.

The above results together suggest that the Opportunity Zone policy has generated limited economic impact on geographical mobility and local employment. As discussed in the previous literature, younger firms create more jobs than the existing and older firms (Adelino et al., 2017). The limited real effect could be due to the decrease in the formation of new businesses.

1.8 Conclusion and Policy Implications

This paper studies the impact of a new place-based tax credit policy, the Opportunity Zone program of 2017, on local private investments and new business registration. Using a difference-in-differences approach and comparing census tracts assigned as Opportunity Zones and other eligible but non-designated tracts, I find that the policy had significantly positive effects on local private investments (both the number and amount of investment deals). However, these private investments led to decreases in local new business registration and loss of entrepreneurship was not trivial in terms of firm quality. The decrease in entrepreneurship was mainly significant in the non-tradable sector, which is more sensitive to local competition, but not in the tradable or construction sector. I show that the results are robust under a few additional tests, suggesting that the above relationships between the Opportunity Zone policy and local private investments and entrepreneurship are causal.

I provide one explanation for why the Opportunity Zone policy had positive impact on local private investments but negatively affected local entrepreneurship. I show that the increase in local private investments brought by the Opportunity Zone policy is much more considerable for existing firms than newly-formed firms. The results suggest that the tax incentives provided by the Opportunity Zone and its associated investment timing requirement have shifted investors' preference toward existing firms with a longer operating history and more information. In addition, I find that the government-sponsored small business loans did not alleviate the financial challenges of potential entrepreneurs: The SBA loans lent to new businesses decreased significantly in Opportunity Zones but did not change significantly for existing firms.

Further, I show that the Opportunity Zone policy did not seem to have positive real effects in the local economy. I show that local housing prices and the number of mortgage applications did not change. The number of people who moved into the Opportunity Zone did not experience significant increases either. County-level employment also did not change significantly. The results suggest that despite billions of taxpayers' money and hurting local business dynamism, the policy did not generate positive impact on local employment, which was partly due to the decrease in local entrepreneurship.

This paper provides important policy implications. One lesson from the Opportunity Zone policy is that policymakers need to be aware of the potential distributional effects when offering market-based tax incentives. The tax-saving incentives combined with the limited time in selecting investment targets might lead investors to avoid new firms with little information and prefer existing and older firms. Therefore, with good intentions to assist entrepreneurs and small businesses to have better access to financing resources, some policies might discriminate against and discourage potential entrepreneurs from starting new businesses, which are the primary force of creating jobs and boosting local economic growth (Haltiwanger, Jarmin, & Miranda, 2013). This paper sheds new light on the discussion about the optimal design of place-based programs and the role of governments in promoting equal opportunities for households to accumulate wealth through business ownership (Cagetti & De Nardi, 2006).

Future policies need to have a more precise target: Instead of setting the goal at drawing private investments into economically distressed areas, the policy could be more effective if it aims at either increasing local employment or promoting the births of new local firms. Policymakers also need to be aware of investors' preference for mature businesses and real-estate firms, which usually have fewer job creations than the newborn businesses. The government could also offer higher tax credits to investors who put money in young firms in economically distressed areas. Overall, to address the inequality between the rich and poor in access to capital, the government needs to put more oversight on the destination of investments when providing tax credits to wealthy investors.

1.9 Figures and Tables

Figure 1.1. Geographical Distribution of Opportunity Zones

This figure shows the geographical locations of the designated census tracts as Opportunity Zones created under the Tax Cuts and Jobs Act (TCJA) and signed into law on December 22, 2017. Areas marked in "red" are the census tracts designated as Opportunity Zones. Areas marked in "yellow" are the census tracts that are eligible for designation but not selected as Opportunity Zones.

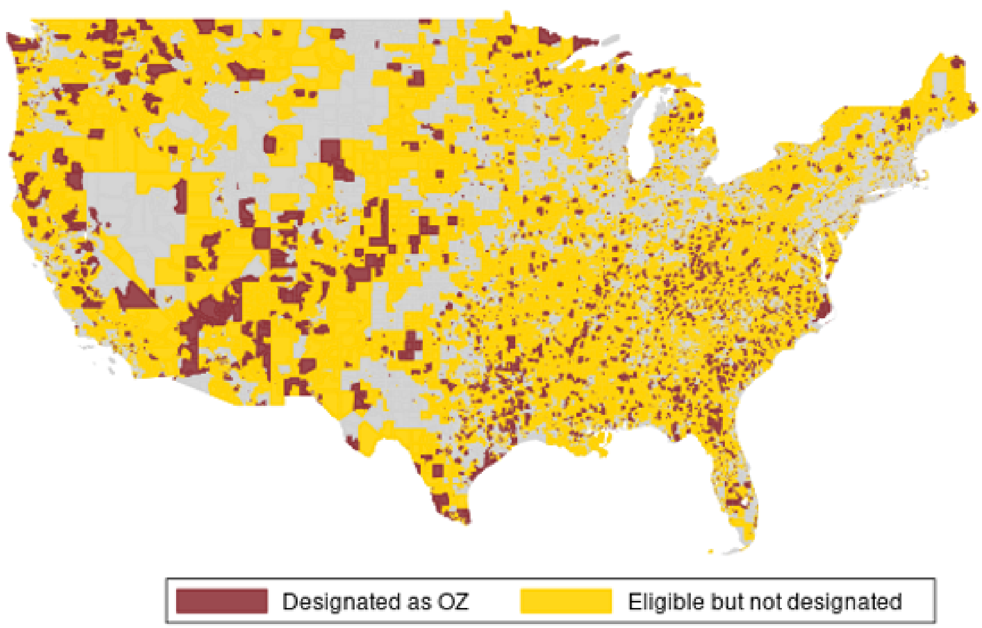
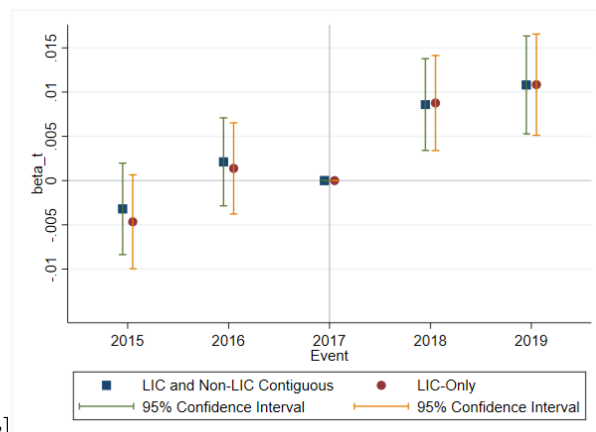


Figure 1.2. Coefficient Estimates on Private Investments

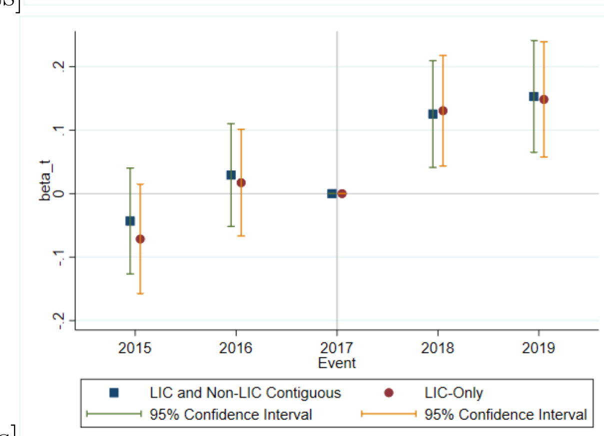
The figures show the coefficients plot around the Opportunity Zone policy by estimating the following model:

$$Y_{i,t} = \alpha + \beta OZ_i * Year_t + OZ_i + Controls_{i,t-1} + \delta_t + \eta_i + \epsilon_{i,t}$$

where $Year_t$ is a set of year indicator variables that equals one in year t. The benchmark group comprises of observations from 2017, when the Opportunity Zone policy was signed into law. OZ_i is a dummy that equals one if census tract i was designated as an Opportunity Zone and equals zero if the tract was eligible but not selected. Panel (a) shows the plot of coefficient estimates of β_t when the outcome variable is the natural logarithm of one plus the number of private 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 private investments. The center points show the point estimates of β_t and the vertical lines denote the 95% confidence intervals of β_t estimates. The blue square dots represent the coefficient estimates for a sample with both low-income-community (LIC) tracts and non-LIC but contiguous to LIC tracts. The red round dots represent the coefficient estimates for a sample with LIC tracts only.



[Number of Private Investments]



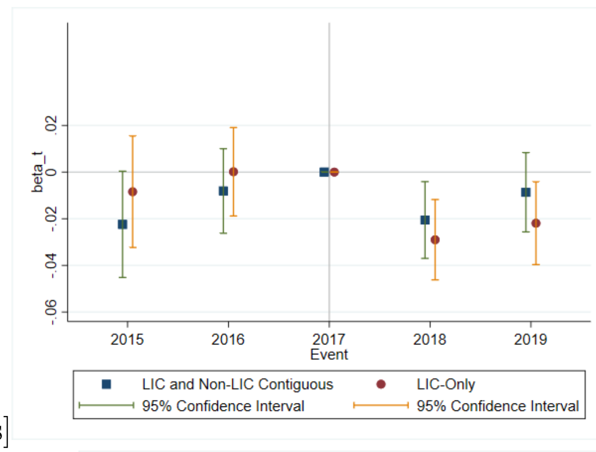
[Amount of Private Investments]

Figure 1.3. Coefficient Estimates on New Business Formation

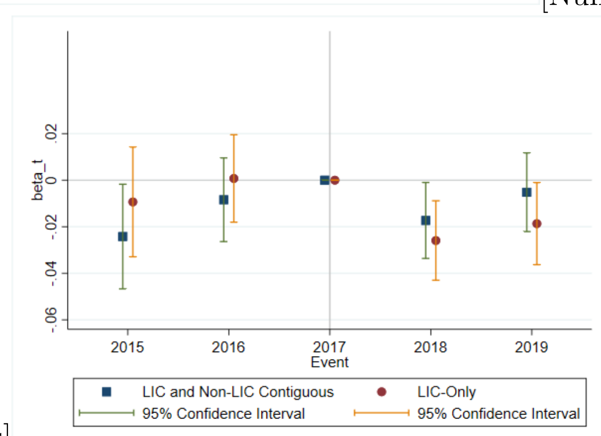
The figures show the coefficients plot around the Opportunity Zone policy by estimating the following model:

$$Y_{i,t} = \alpha + \beta OZ_i * Year_t + OZ_i + Controls_{i,t-1} + \delta_t + \eta_i + \epsilon_{i,t}$$

where $Year_t$ is a set of year indicator variables that equals one in year t. The benchmark group comprises of observations from 2017, when the Opportunity Zone policy was signed into law. OZ_i is a dummy that equals one if census tract i was designated as an Opportunity Zone and equals zero if the tract was eligible but not selected. Panel (a), (b) and (c) shows the plot of coefficient estimates of β_t when the outcome variable is the natural logarithm of one plus the number of total new firms formed, the natural logarithm of one plus the number of new for-profit firms formed, and the natural logarithm of one plus the number of new non-profit firms formed, respectively. The center points show the point estimates of β_t and the vertical lines denote the 95% confidence intervals of β_t estimates. The blue square dots represent the coefficient estimates for a sample with both low-income-community (LIC) tracts and non-LIC but contiguous to LIC tracts. The red round dots represent the coefficient estimates for a sample with LIC tracts only.



[Number of New Firms]



[Number of New For-Profit Firms]

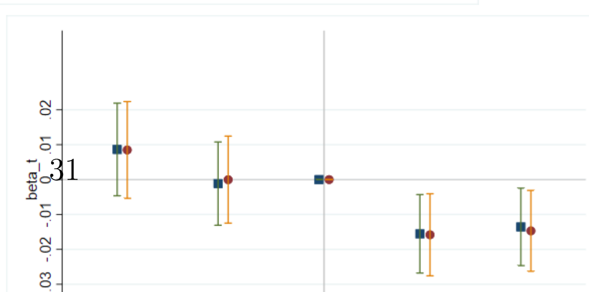


Table 1.1. Summary Statistics

This table displays the summary statistics for the data used in this study. The observation unit is a census-tract-year. Variable construction and data sources are introduced in Section 1.4.

	N	Mean	SD	Min	Median	Max
OZ	154,566	0.247	0.431	0.000	0.000	1.000
Num_Inv	154,566	0.118	2.257	0.000	0.000	641.000
Amount_Inv ($milions)	154,566	2.732	92.635	0.000	0.000	15,422.180
Num_NewFirm	154,566	10.343	47.016	0.000	3.000	4,739.000
Num_NewForProfit	154,566	9.804	45.735	0.000	2.000	4,628.000
Num_NewNonProfit	154,566	0.540	1.954	0.000	0.000	202.000
Population (thousands)	154,509	4.046	1.892	0.011	3.801	40.616
Median Income ($thousands)	154,509	39.156	12.987	2.499	38.141	181.125
Median Age	154,509	35.860	7.501	21	35.1	80.4
Poverty Rate (%)	154,509	22.181	9.976	3.356	20.619	51.164
White Alone (%)	154,509	61.206	28.727	1.061	67.339	99.339
Black Alone (%)	154,509	23.000	27.929	0.000	10.426	100.000
Unemployment Rate (%)	154,509	10.827	6.116	1.190	9.603	30.999
%NoHighSchool	154,509	20.867	11.883	0.000	18.630	100.000

Table 1.2. Impact of Opportunity Zones on Private Investment

This table shows the impact of the Opportunity Zone policy on local private investments. Specifically, I shows the results of the DiD analysis by estimating the following model:

$$Y_{i,t} = \alpha + \beta OZ_i * Post_t + Controls_{i,t-1} + \delta_t + \eta_i + \epsilon_{i,t}.$$

where i is a census tract, t represents a year. The dependent variables are the natural logarithm of the one plus the number of private investment deals invested in census i and year t (*Ln(Num_Inv+1)*) and the amount of private investment deals invested (*Ln(Amount_Inv+1)*). *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone (OZ) and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterwards. Control variables include the population, median income, median age, poverty rate, percentage of white or black people, unemployment rate, and percentage of population without a high-school degree of a census tract in a given year. I also control for year and tract fixed effects. Standard errors are clustered at the census tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) *Ln(Num_Inv+1)*	(2) *Ln(Num_Inv+1)*	(3) *Ln(Amount_Inv+1)*	(4) *Ln(Amount_Inv+1)*
*OZ*Post*	0.011***	0.011***	0.158***	0.158***
	(0.002)	(0.002)	(0.032)	(0.032)
Population		0.018**		0.270**
		(0.008)		(0.122)
Median_Income		0.010		0.112
		(0.006)		(0.087)
Median_Age		0.003		0.105
		(0.008)		(0.120)
%White		0.000*		0.005***
		(0.000)		(0.002)
%Black		-0.000		0.001
		(0.000)		(0.003)
Poverty_Rate		0.000*		0.005**
		(0.000)		(0.002)
Unemp_Rate		-0.000		-0.001
		(0.000)		(0.002)
%NoHighSchool		0.000		0.001
		(0.000)		(0.002)
Constant	0.042***	-0.230**	0.603***	-3.612**
	(0.000)	(0.099)	(0.003)	(1.479)
Observations	154,563	154,490	154,563	154,490
R-squared	0.733	0.733	0.579	0.579
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Table 1.3. Impact of Opportunity Zones on Private Investment by Sector

This table shows the impact of the Opportunity Zone policy on local private investments by sector. In Columns (1)-(3), the dependent variable is the natural logarithm of one plus the number of private investment deals in a specific sector invested in census i and year t (*Ln(Num_Inv+1)*). In Columns (4)-(6), the dependent variable is the natural logarithm of one plus the amount of private investment deals in a specific sector invested (*Ln(Amount_Inv+1)*). *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone (OZ) and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterwards. Control variables include the population, median income, median age, poverty rate, percentage of white or black people, unemployment rate, and percentage of population without a high-school degree of a census tract in a given year. I also control for year and tract fixed effects. Standard errors are clustered at the census tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(Num_Inv+1)			*Ln(Amount_Inv+1)*		
	Business	*Real Estate*	*Finance*	*Business*	*Real Estate*	*Finance*
*OZ*Post*	0.010***	0.001*	-0.000	0.147***	0.021**	-0.008
	(0.002)	(0.001)	(0.000)	(0.031)	(0.010)	(0.007)
Population	0.021***	-0.000	-0.000	0.308**	0.050	0.002
	(0.008)	(0.003)	(0.001)	(0.122)	(0.047)	(0.024)
Median_Income	0.008	0.002	0.001	0.095	0.039	0.010
	(0.006)	(0.002)	(0.001)	(0.086)	(0.025)	(0.017)
Median_Age	0.004	-0.001	0.002*	0.106	0.005	0.037
	(0.007)	(0.002)	(0.001)	(0.116)	(0.035)	(0.025)
%White	0.000*	-0.000	0.000	0.005**	0.001	0.000
	(0.000)	(0.000)	(0.000)	(0.002)	(0.001)	(0.000)
%Black	-0.000	-0.000***	0.000	0.001	-0.002*	0.000
	(0.000)	(0.000)	(0.000)	(0.003)	(0.001)	(0.000)
Poverty_Rate	0.000*	-0.000	0.000	0.005**	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.002)	(0.001)	(0.000)
Unemp_Rate	-0.000	-0.000	-0.000	-0.001	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.002)	(0.001)	(0.001)
%NoHighSchool	0.000	-0.000	0.000	0.002	-0.001*	0.000
	(0.000)	(0.000)	(0.000)	(0.002)	(0.001)	(0.000)
Constant	-0.249**	-0.013	-0.014	-3.771**	-0.774	-0.259
	(0.097)	(0.033)	(0.017)	(1.466)	(0.544)	(0.318)
Observations	154,490	154,490	154,490	154,490	154,490	154,490
R-squared	0.731	0.514	0.500	0.578	0.446	0.420
Tract FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

Table 1.4. Impact of Opportunity Zones on New Business Formation

This table shows the impact of the Opportunity Zone policy on local new business formation. The dependent variables are the natural logarithm of the one plus the number of total new firms registered in census i and year t (*Ln(Num_NewFirm+1)*), the number of for-profit businesses registered (*Ln(Num_NewForProfit+1)*), and the number of non-profit businesses registered (*Ln(Num_NewNonProfit+1)*). *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone (OZ) and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterwards.This analysis does not include census tracts in Delaware, Illinois, and Puerto Rico due to data coverage. Control variables include the population, median income, median age, poverty rate, percentage of white or black people, unemployment rate, and percentage of population without a high-school degree of a census tract in a given year. I also control for year and tract fixed effects. Standard errors are clustered at the census tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) *Ln(Num_NewFirm+1)*	(2) *Ln(Num_NewForProfit+1)*	(3) *Ln(Num_NewNonProfit+1)*
*OZ*Post*	-0.023**	-0.019**	-0.018***
	(0.010)	(0.010)	(0.004)
Population	-0.049	-0.040	-0.042**
	(0.040)	(0.039)	(0.018)
Median_Income	0.088***	0.085***	0.019
	(0.026)	(0.025)	(0.012)
Median_Age	0.152***	0.149***	0.004
	(0.044)	(0.043)	(0.020)
%White	-0.001*	-0.001*	0.000
	(0.001)	(0.001)	(0.000)
%Black	-0.005***	-0.005***	-0.001**
	(0.001)	(0.001)	(0.000)
Poverty_Rate	0.003***	0.003***	0.001**
	(0.001)	(0.001)	(0.000)
Unemp_Rate	0.001*	0.001	0.001***
	(0.001)	(0.001)	(0.000)
%NoHighSchool	-0.004***	-0.004***	-0.001***
	(0.001)	(0.001)	(0.000)
Constant	0.594	0.534	0.398*
	(0.467)	(0.462)	(0.216)
Observations	147,565	147,565	147,565
R-squared	0.851	0.849	0.619
Tract FE	YES	YES	YES
Year FE	YES	YES	YES

Table 1.5. Heterogeneous Impact on Local Entrepreneurship By Survival Period

This table shows the impact of the Opportunity Zone policy on local new business formation by the length of time a firm can survive. The dependent variables are the natural logarithm of the one plus the number of total new firms that survived for more than one (or two) years and those that did not. *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone (OZ) and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterwards. This analysis does not include census tracts in Delaware, Illinois, and Puerto Rico due to data coverage. Control variables include the population, median income, median age, poverty rate, percentage of white or black people, unemployment rate, and percentage of population without a high-school degree of a census tract in a given year. I also control for year and tract fixed effects. Standard errors are clustered at the census tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
	Survive >1 Years	*Survive ≤1 Year*
*OZ*Post*	-0.023**	-0.002
	(0.010)	(0.002)
Population	-0.050	0.019**
	(0.040)	(0.008)
Median_Income	0.090***	-0.001
	(0.026)	(0.005)
Median_Age	0.150***	0.021**
	(0.044)	(0.008)
%White	-0.001*	-0.000
	(0.001)	(0.000)
%Black	-0.005***	-0.000*
	(0.001)	(0.000)
Poverty_Rate	0.003***	0.000
	(0.001)	(0.000)
Unemp_Rate	0.001*	0.000
	(0.001)	(0.000)
%NoHighSchool	-0.004***	0.000
	(0.001)	(0.000)
Constant	0.593	-0.168*
	(0.466)	(0.096)
Observations	147,565	147,565
R-squared	0.851	0.441
Tract FE	YES	YES
Year FE	YES	YES

Table 1.6. Heterogeneous Impact on Local Entrepreneurship By Business Structure

	(1) *Corporation*	(2) *Limited Liability Company*	(3) *Other*
*OZ*Post*	-0.023***	-0.016*	-0.010***
	(0.006)	(0.010)	(0.003)
Population	-0.037	-0.033	-0.016
	(0.025)	(0.039)	(0.014)
Median_Income	0.051***	0.079***	0.001
	(0.018)	(0.025)	(0.010)
Median_Age	0.095***	0.134***	0.001
	(0.029)	(0.043)	(0.015)
%White	-0.001**	-0.001	0.000
	(0.000)	(0.001)	(0.000)
%Black	-0.004***	-0.004***	0.000
	(0.001)	(0.001)	(0.000)
Poverty_Rate	0.002***	0.003***	-0.000
	(0.000)	(0.001)	(0.000)
Unemp_Rate	0.001**	0.001	-0.000*
	(0.001)	(0.001)	(0.000)
%NoHighSchool	-0.002***	-0.003***	-0.000
	(0.001)	(0.001)	(0.000)
Constant	0.315	0.358	0.327*
	(0.307)	(0.463)	(0.168)
Observations	147,565	147,565	147,565
R-squared	0.809	0.828	0.772
Tract FE	YES	YES	YES
Year FE	YES	YES	YES

Table 1.7. Impact on Private Investments by Firm Age

This table shows the impact of the Opportunity Zone policy on local private investments grouped by the age of firms that received the investments. The dependent variable in Columns (1) and (2) is the natural logarithm of one plus the total number of private investments (*Ln(Num_Inv+1)*) received by firms that are at least one-year old or less than one-year old census tract i and year t, respectively. The dependent variable in Columns (3) and (4) is the natural logarithm of one plus the total amount of private investments (*Ln(Amount_Inv+1)*) received by firms that are at least one-year old or less than one-year old census tract i and year t, respectively. *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone (OZ) and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterwards. Control variables include the population, median income, median age, poverty rate, percentage of white or black people, unemployment rate, and percentage of population without a high-school degree of a census tract in a given year. I also control for year and tract fixed effects. Standard errors are clustered at the census tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively..

	(1)	(2)	(3)	(4)
	Ln(Num_Inv+1)		*Ln(Amount_Inv+1)*	
	One Year and Above	*Less Than 1 Year*	*One Year and Above*	*Less Than 1 Year*
*OZ*Post*	0.010***	0.002**	0.140***	0.035**
	(0.002)	(0.001)	(0.031)	(0.016)
Population	0.020**	0.004	0.278**	0.063
	(0.008)	(0.004)	(0.118)	(0.069)
Median_Income	0.009	0.004	0.121	0.037
	(0.006)	(0.003)	(0.086)	(0.044)
Median_Age	0.004	0.000	0.109	0.028
	(0.007)	(0.003)	(0.115)	(0.057)
%White	0.000*	-0.000	0.005**	0.000
	(0.000)	(0.000)	(0.002)	(0.001)
%Black	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.003)	(0.001)
Poverty_Rate	0.000	0.000**	0.003	0.002*
	(0.000)	(0.000)	(0.002)	(0.001)
Unemp_Rate	-0.000	0.000	-0.002	-0.000
	(0.000)	(0.000)	(0.002)	(0.001)
%NoHighSchool	0.000	-0.000	-0.000	0.000
	(0.000)	(0.000)	(0.002)	(0.001)
Constant	-0.248***	-0.063	-3.722***	-0.943
	(0.094)	(0.050)	(1.426)	(0.799)
Observations	154,490	154,490	154,490	154,490
R-squared	0.727	0.579	0.576	0.450
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Table 1.8. Impact on SBA Lending by Firm Age

This table shows the impact of the Opportunity Zone policy on local Small Business Administration (SBA) lending grouped by the age of firms that received the investments. The dependent variable in Columns (1) and (2) is the natural logarithm of one plus the total number of SBA loans (*Ln(Num_SBALoan+1)*) received by firms that are at least one-year old or less than one-year old census tract i and year t, respectively. The dependent variable in Columns (3) and (4) is the natural logarithm of one plus the total amount of SBA loans (*Ln(Amount_SBALoan+1)*) received by firms that are at least one-year old or less than one-year old census tract i and year t, respectively. *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone (OZ) and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterwards. Control variables include the population, median income, median age, poverty rate, percentage of white or black people, unemployment rate, and percentage of population without a high-school degree of a census tract in a given year. I also control for year and tract fixed effects. Standard errors are clustered at the census tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Ln(Num_SBALoan+1)		*Ln(Amount_SBALoan+1)*	
	One Year and Above	*Less Than 1 Year*	*One Year and Above*	*Less Than 1 Year*
*OZ*Post*	-0.005	-0.005***	-0.010	-0.077**
	(0.005)	(0.002)	(0.059)	(0.031)
Population	0.028	-0.020***	0.613***	-0.251**
	(0.018)	(0.008)	(0.235)	(0.116)
Median_Income	-0.021*	-0.004	-0.220	-0.055
	(0.012)	(0.005)	(0.156)	(0.079)
Median_Age	-0.000	-0.010	0.091	-0.058
	(0.020)	(0.008)	(0.264)	(0.125)
%White	-0.000	-0.000	-0.006	-0.003
	(0.000)	(0.000)	(0.004)	(0.002)
%Black	-0.001*	-0.000	-0.011**	-0.003
	(0.000)	(0.000)	(0.006)	(0.003)
Poverty_Rate	0.001*	0.000	0.010**	0.004
	(0.000)	(0.000)	(0.004)	(0.002)
Unemp_Rate	-0.001**	-0.000	-0.008*	-0.004
	(0.000)	(0.000)	(0.005)	(0.002)
%NoHighSchool	-0.000	0.000	-0.003	0.001
	(0.000)	(0.000)	(0.005)	(0.002)
Constant	0.281	0.292***	0.957	3.617**
	(0.213)	(0.091)	(2.796)	(1.404)
Observations	154,490	154,490	154,490	154,490
R-squared	0.516	0.368	0.435	0.347
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Table 1.9. Heterogeneous Impact on Local Entrepreneurship By Sector

This table shows the impact of the Opportunity Zone policy on local new business formation by categorizing firms into sectors following Mian and Sufi (2014). The dependent variables in Columns (1), (2), and (3) are the natural logarithm of the one plus the number of total new firms in the non-tradable, tradable, construction, and other sector registered in census i and year t, respectively. *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone (OZ) and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterwards. This analysis does not include census tracts in Delaware, Illinois, and Puerto Rico due to data coverage. Control variables include the population, median income, median age, poverty rate, percentage of white or black people, unemployment rate, and percentage of population without a high-school degree of a census tract in a given year. I also control for year and tract fixed effects. Standard errors are clustered at the census tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) *Non-tradable*	(2) *Tradable*	(3) *Construction*	(4) *Other*
*OZ*Post*	-0.016***	-0.003	-0.005	-0.021**
	(0.005)	(0.002)	(0.006)	(0.009)
Population	0.014	-0.007	-0.021	-0.053
	(0.018)	(0.007)	(0.023)	(0.038)
Median_Income	-0.001	0.008	0.025	0.084***
	(0.012)	(0.005)	(0.016)	(0.025)
Median_Age	-0.000	0.004	0.068***	0.140***
	(0.020)	(0.008)	(0.026)	(0.042)
%White	-0.000	-0.000	-0.000	-0.001*
	(0.000)	(0.000)	(0.000)	(0.001)
%Black	-0.001**	-0.000	-0.002***	-0.005***
	(0.000)	(0.000)	(0.001)	(0.001)
Poverty_Rate	0.000	0.000*	0.000	0.003***
	(0.000)	(0.000)	(0.000)	(0.001)
Unemp_Rate	0.000	-0.000	-0.000	0.002**
	(0.000)	(0.000)	(0.000)	(0.001)
%NoHighSchool	-0.001	0.000	-0.002***	-0.003***
	(0.000)	(0.000)	(0.000)	(0.001)
Constant	0.184	0.000	0.252	0.575
	(0.216)	(0.090)	(0.282)	(0.452)
Observations	147,565	147,565	147,565	147,565
R-squared	0.568	0.388	0.722	0.847
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Table 1.10. Impact of Opportunity Zones on Housing Price and Mortgage Applications

This table shows the impact of the Opportunity Zone policy on local housing price and mortgage applications. The dependent variables are the natural logarithm of the housing price index provided by the Federal House Finance Agency (FHFA) and the natural logarithm of the one plus the number of total housing mortgages in census i and year t (*Ln(Num_mortgage+1)*). *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone (OZ) and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterwards. Control variables include the population, median income, poverty rate, percentage of white people, and unemployment rate of a census tract in a given year. Control variables include the population, median income, median age, poverty rate, percentage of white or black people, unemployment rate, and percentage of population without a high-school degree of a census tract in a given year. I also control for year and tract fixed effects. Standard errors are clustered at the census tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) *Ln(HPI)*	(2) *Ln(Num_mortgage+1)*
*OZ*Post*	-0.001	0.005
	(0.002)	(0.005)
Population	0.173***	0.122***
	(0.008)	(0.021)
Median_Income	-0.000	-0.016
	(0.005)	(0.014)
Median_Age	0.054***	0.007
	(0.008)	(0.024)
%White	-0.000***	0.000
	(0.000)	(0.000)
%Black	-0.001***	0.000
	(0.000)	(0.001)
Poverty_Rate	-0.001***	-0.000
	(0.000)	(0.000)
Unemp_Rate	-0.004***	-0.001***
	(0.000)	(0.000)
%NoHighSchool	-0.000	0.000
	(0.000)	(0.000)
Constant	3.735***	-0.314
	(0.094)	(0.253)
Observations	89,325	154,490
R-squared	0.981	0.509
Tract FE	YES	YES
Year FE	YES	YES

Table 1.11. Impact of Opportunity Zones on Geographical Mobility

This table shows the impact of the Opportunity Zone policy on the geographical mobility for current residents who moved in within last one year. The dependent variable in Column (1) is the natural logarithm of one plus the total number of movers into census tract i and year t within the last one year (*Ln(Num_migrants+1)*). In Columns (2) to (4), the dependent variables are the number of move-ins by education: the natural logarithm of one plus the number of movers that do not have a high-school degree, have a high-school degree, and have a bachelor's degree or above, respectively. In Columns (5) to (7), the dependent variables are the number of move-ins by their poverty status: the natural logarithm of one plus the number of movers that are below 100 percent of the poverty level, 100 to 149 percent of the poverty level, at or above 150 percent of the poverty level, respectively. *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone (OZ) and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterwards. Control variables include the population, median income, median age, poverty rate, percentage of white or black people, unemployment rate, and percentage of population without a high-school degree of a census tract in a given year. I also control for year and tract fixed effects. Standard errors are clustered at the census tract level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Ln(Num_migrants+1)						
		Education			Relative to Poverty Level		
	All Move-ins	Less than High School	High School	Bachelor's or Above	< 100%	[100%, 149%]	≥ 150%
*OZ*Post*	-0.007	0.001	0.004	0.009	0.009	0.005	0.009
	(0.004)	(0.012)	(0.009)	(0.012)	(0.010)	(0.015)	(0.007)
Population	0.575***	0.549***	0.620***	0.456***	0.496***	0.575***	0.545***
	(0.028)	(0.053)	(0.041)	(0.052)	(0.047)	(0.064)	(0.041)
Median_Income	0.023	0.024	-0.032	0.117***	-0.017	-0.440***	0.245***
	(0.015)	(0.035)	(0.026)	(0.034)	(0.028)	(0.043)	(0.024)
Median_Age	-0.188***	0.171***	0.124***	0.071	-0.328***	-0.474***	-0.077**
	(0.024)	(0.055)	(0.040)	(0.055)	(0.046)	(0.070)	(0.035)
%White	-0.001***	-0.002*	-0.000	-0.002**	-0.001	-0.001	-0.001
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
%Black	-0.001**	-0.000	0.001	-0.005***	-0.001	0.001	-0.002**
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Poverty_Rate	0.002***	0.003***	0.001	-0.002**	0.023***	-0.017***	-0.004***
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Unemp_Rate	0.003***	0.002**	0.005***	0.001	0.005***	0.006***	0.001
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
%NoHighSchool	0.000	0.031***	-0.005***	-0.006***	0.000	0.005***	-0.001*
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Constant	1.976***	-2.377***	-0.890*	-1.292**	1.622***	5.568***	-1.197***
	(0.295)	(0.628)	(0.472)	(0.599)	(0.532)	(0.753)	(0.452)
Observations	154,484	154,484	154,484	154,484	154,484	154,484	154,484
R-squared	0.923	0.785	0.811	0.845	0.814	0.735	0.885
Tract FE	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES

Table 1.12. The Impact of Opportunity Zones on County Employment (Total and By Sector)

This table shows the impact of the Opportunity Zone policy on the number of employment. I collect the total number of employment from the County Business Patterns from the Census Bureau. The dependent variables are the natural logarithm of the number of employment in county i and year t by sector. *OZ%* is a continuous variable that equals the percentage of population that resides in Opportunity Zones of the total population that resides in the eligible census tracts in the same county (the results of using the percentage of all the county population is shown in the Appendix). *Post* is a dummy that equals zero prior to 2018 and one afterwards. Control variables include the population, median income, poverty rate, percentage of white people, and unemployment rate of a census tract in a given year. I also control for year and county fixed effects. Standard errors are clustered at the county level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	Ln(Employment+1)				
	Total	*Non-Tradable*	*Tradable*	*Construction*	*Other*
*%OZ*Post*	-0.014	-0.015	0.039	0.123**	0.053
	(0.012)	(0.034)	(0.116)	(0.063)	(0.037)
Population	0.928***	0.453**	0.348	-0.052	-0.116
	(0.131)	(0.181)	(0.590)	(0.281)	(0.265)
Median_Income	-0.013	0.287**	0.618*	-0.045	0.006
	(0.036)	(0.129)	(0.327)	(0.172)	(0.166)
Median_Age	0.193	0.874**	-0.096	0.582	0.540
	(0.124)	(0.359)	(0.540)	(0.388)	(0.456)
%White	0.004	0.005	0.006	-0.001	-0.002
	(0.005)	(0.003)	(0.009)	(0.005)	(0.004)
%Black	-0.000	0.005	-0.021	-0.010	-0.013*
	(0.002)	(0.008)	(0.022)	(0.012)	(0.007)
Poverty_Rate	0.001	0.007*	-0.005	-0.007	0.002
	(0.002)	(0.004)	(0.010)	(0.005)	(0.004)
Unemp_Rate	-0.008***	-0.002	-0.003	-0.009*	0.000
	(0.002)	(0.003)	(0.010)	(0.005)	(0.005)
%NoHighSchool	0.000	-0.001	-0.006	-0.006	0.006
	(0.002)	(0.004)	(0.010)	(0.005)	(0.008)
Constant	-1.557	-4.423	-5.979	5.152	6.804**
	(1.191)	(2.956)	(6.996)	(3.665)	(3.038)
Observations	15,707	15,707	15,707	15,707	15,707
R-squared	0.996	0.989	0.941	0.979	0.991
County FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

Chapter 2

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

2.1 Introduction

Small businesses, which account for two-thirds of new jobs created in the U.S., are the basis for innovation and crucial for economic growth.[1] Raising capital for small businesses is important but not easy in a market with large 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 protecting investors, especially for small investors who may lose a significant amount of money by investing in entrepreneurial firms that turn out to be unsuccessful.

Recently, the debate on this trade-off has escalated when the accredited investor standard was amended by the SEC on August 26, 2020: In addition to the existing tests for income or net worth, the amendment allows investors to qualify when they have certain professional knowledge, experience or certifications. Immediately afterwards,

[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 a large strand of literature discussing these frictions, see examples in Leland and Pyle (1977), Grinblatt and Hwang (1989), and Conti, Thursby, and Rothaermel (2013).

[3] In an 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."

two SEC Commissioners issued a joint statement publicly criticizing that the Commission majority failed to protect vulnerable investors and the update was issued without "sufficient data or analysis."[4] This recent debate indicates that timely research on the aforementioned trade-off is critical, which will not only expand our academic knowledge of the capital market, but also provide useful evidence to regulators for policy making and evaluation. In this paper, I exploit a 2011 SEC regulation change to empirically analyze this trade-off in the context of angel financing.

Angel financing presents a good setting to study the above trade-off. Angel investors drive a large portion of the financing for entrepreneurial firms (Denes et al., 2020; W. R. Kerr, Lerner, & Schoar, 2014; Shane, 2008). Many firms were backed by angel investors at their early stage, with some famous examples including Google, Amazon, Facebook, Paypal, Costco, and The Home Depot. Yet, angel investors are individual investors, as distinguished from institutional investors like venture capital (VC) and private equity (PE) firms. They may be more vulnerable to investing in frauds and scams, having less risk-bearing ability, and more likely to make irrational investment decisions compared to institutional investors (Collewaert & Fassin, 2013; Drover et al., 2017). The concerns about protecting individual investors increased rapidly after the 2008 financial crisis, in which many 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

[4]Specifically, the statement (Lee & Crenshaw, 2020) wrote, "With its actions today, the Commission continues a steady expansion of the private market, affording issuers of unregistered securities access to more and more investors without due regard for the risks they face, and without sufficient data or analysis to ensure that our policy choices are grounded in fact rather than supposition." The SEC press release on updating the accredited investor definition is available here: `https://www.sec.gov/news/press-release/2020-191`.

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).

The 2011 SEC regulation change provides an appropriate context to study how changes in investor protection affect entrepreneurial activity. First, it directly changed the investor protection environment in the private offering market by restricting the definition of accredited investors, which is considered as the "most important investor protection in the private market" (Lee & Crenshaw, 2020). Second, the implementation of this regulation change was not driven by local entrepreneurial activity, ruling out reverse causality: The SEC regulation change was enacted under the requirement of the 2010 Dodd-Frank Act with the main goal of preventing unsophisticated investors from personal bankruptcies and loss of residency by investing in unsuccessful firms. In addition, I am able to use the heterogeneity of home value to net worth across U.S. cities as a proxy for variation in the investor protection environment, which has traditionally been hard to observe and measure in the private market.

Using this SEC regulation change across U.S. cities as a quasi-natural experiment, I apply a difference-in-difference (DiD) approach with a continuous treatment variable to examine its impact. To reflect the average extent of a city being affected by the regulation change, I construct a variable, home-value-to-net-worth (HV/NW henceforth), by dividing the average home value by the average net worth in a city at the end of 2011. The results of the DiD analysis show that the 2011 SEC regulation change had a significantly negative impact on local angel financing. Cities with a higher HV/NW ratio, experienced significantly larger decreases in both the number and amount of angel financing after the regulation change. Specifically, a one standard deviation increase in

[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`.

the HV/NW ratio prior to the regulation change, on average, led to a 11.3% larger decrease in the amount and a 1.3% larger decrease in the number of angel investments after the regulation change. Translating the estimates into dollar amount, there would be a $2.75 billion larger decrease per year in angel financing across the U.S. if the HV/NW ratios increased one standard deviation in all sample cities.

To justify the causal interpretation of the results, I conduct several additional tests to substantiate the identifying assumptions and address other concerns. First, the DiD approach requires that there is no significantly different trend in local angel financing prior to the policy shock across different sample cities and I show dynamics of the coefficient estimates to support this assumption. Second, I substantiate the assumption that most angel investors invest locally by first showing that more than 60% of angel-firm pairs have a distance of less than 100 miles. I also show that the previous results are robust controlling spillover effects from nearby regions. Third, the causal interpretation relies on the assumption that the treatment variable (HV/NW ratio) is indicative of the extent of a city being affected by the SEC regulation change instead of reflecting other factors. I support this assumption by first showing that alternative explanations such as housing price growth are unable to explain the previous findings. The results are also not driven by outlier cities with bottom and/or top deciles 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). Fourth, the findings are robust when using an alternative measure of the treatment, the ratio of top-tier home value to the net worth of top-class income group in a city (HV_top/NW_top). Finally, 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.

To show that the decrease in local angel financing had non-negligible impact on the financing of some high-quality firms, I examine the impact of the 2011 SEC regulation change 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 one-standard-deviation higher *HV/NW* ratio prior to the regulation change, on average, led to a 0.75% larger decrease in the number of angel-backed firms that later receive next-round financing and a 0.40% larger decrease in the number of angel-backed firms that later receive VC financing 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 angel-backed firms receiving subsequent financing did not change significantly and the rate of having successful exits declined after the SEC regulation change. The results confirm that distance-related frictions could hinder the matching between investors and firms in the early-stage financing market (A. Agrawal, Catalini, & Goldfarb, 2015): Some marginal angel investors who had local information to better select or monitor local firms lost the eligibility to participate in angel investing due to the regulation change, and some high-quality firms lost access to capital because of the restricted pool of local investors.

I also 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 one-standard-deviation higher *HV/NW* ratio prior to the regulation change, it experienced a 0.99% larger decrease in the number of patents, 0.05% larger decrease in the number of patent citations generated by angel-backed firms in the city, on average. The same city also experienced a 11.24% greater decrease in sales generated and a 3.23% greater decrease in the number of jobs supported by angel-backed firms after the above regulation change.

To validate the above findings and 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 entrepreneurs' financial constraints, the increased use of these two alternative financing sources may also generate concerns related to the efficient usage of government funding (Babina et al., 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 - investor protection through loss avoidance. The costs are measured by the present value of reduced sales generated by entrepreneurial firms that did not receive angel financing (i.e., the present value of lost sales). Specifically, assuming the discount rate is 30% and growth rate is 25% (when early investors require a high return and young firms have high sales growth) and the impact of the regulation change lasts for five years, the present value of total net benefits of the regulation change is negative 6.32 billion dollars at the end of 2011. I also show that the costs of reduced patents and employment generated by these firms are non-negligible. The results of the above analysis provide suggestive evidence that the costs of the 2011 SEC regulation change seem to outweigh its benefits.

The rest of the paper is organized as follows. Section 2.2 discusses the contribution of this paper to the related literature. Section 2.3 introduces the institutional background of angel investors and private placements in the U.S. Section 2.4 describes data sources and variable construction in this study. Sections 2.5 to 2.7 explain the empirical strategy and show how the SEC regulation change impacted local angel financing, entrepreneurial activity, and the local economy. Section 2.8 analyzes the substitution effects reduced angel financing on alternative financing sources. Section 2.9 presents a cost-benefit analysis of the regulation change. Section 2.10 discusses the policy implications from this study. I conclude the paper in Section 2.11.

2.2 Related Literature and Contribution

This paper contributes to several strands of 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 (T. 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, 2021a). Venugopal and Yerramilli (2017) examine how seed-round successes of angel investors impact the evolution of investor network. Bernstein, Korteweg, and Laws (2017) study which firm characteristics are more important to attract early-stage investors. There is a contemporaneous and independent paper by Lindsey and Stein (2019), which uses the same policy shock (the regulation change on the accreditation standard of angel investors) but differs in execution and findings. First, they focus mainly on the impact of the regulation change on aggregated small business employment. In contrast, the focus of my paper analyze the trade-off between investor protection regulations and the promotion of entrepreneurial activity by angel investors. Whereas they study the state-level aggregated business formation and employment for small firms (but not necessarily on angel-backed firms), I use different and more micro-level data sets to examine how the regulation change directly affected local angel financing, how it reduced the innovation, sales, and employment generated by angel-backed firms, and these firms' subsequent financing and successful exits.[6,7] Their paper suggests that angel financing is complementary to alternative financing sources. I find that the decreased angel financing has significant substitution effects on other financings such as small business loans and second-lien mortgages, even though these sources may not serve as perfect substitutes for angel financing.[8]

Second, my paper contributes to the literature on the impact of investor protection regulations on firm performance and financial 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). A. K. Agrawal (2013) shows that investor protection has a causal impact on

[6]I obtain firm-level angel financing data from SEC Form D filings, Crunchbase, and VentureXpert, patent data from the USPTO, annual sales and employment from the NETS, and their 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 (2019), 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 Appendix A.

[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).

[8]Potential explanations for the different findings on alternative financing sources between the two papers are as following. First, the two papers measure alternative financing sources differently: I use the number of applications and approvals of small business loans and home equity loans, while they use bank asset volumes and housing price growth. Second, the methodology is different in the two papers: I show substitution effects by directly testing changes in the usage of the small business loans and home equity loans, while they infer complementarity indirectly from a sub-sample test where the outcome variable is firm entry and divide states based on past housing price appreciation and bank asset volumes. Finally, the difference could also be due to the different geographic units used in the two papers (city in mine vs. state in theirs).

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 offering market. To my knowledge, this is the first paper in the literature that empirically analyzes the impact of investor protection regulation in the private market on local entrepreneurial activity and on the local economy.

Third, my paper is related to the literature on the role of government in promoting entrepreneurship and innovation. Lerner (2000) and Audretsch et al. (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 financing and has a positive impact on firms' innovation output and revenue growth. Da Rin et al. (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 (in press) 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 the effects of the JOBS Act on the funding of small businesses and entrepreneurship in the U.S. 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.

2.3 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 the first outside equity raised by entrepreneurial firms ("first-money-in" after friends and family).[9] 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.[10] 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.

Unlike VC investors, the geographical 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

[9]This statistic is from Marianne Hudson, Executive Director, Angel Capital Association, Presentation to SEC Advisory Committee on Small and Emerging Companies, Washington, DC (December 17, 2014) and The 2017 Halo Report, available at `https://angelresourceinstitute.org/reports/halo-report-full-version-ye-2017.pdf`.

[10]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/`.

to be distance sensitive (A. Agrawal et al., 2015; Michelacci & Silva, 2007; Stuart & Sorenson, 2005), most angel investors invest locally. As illustrated in Figure 2.1, 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 register with the SEC to make sure that all investors have enough information about what they are buying. Private placements, which are governed by SEC registration 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.[11] 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. 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. Even though Rule 506 permits up to 35 non-accredited investors to participate, these investors need to receive "an extensive disclosure document with almost as much detail as is required for an initial public offering."[12] These additional disclosure requirements mean high accounting and legal costs for early-stage firms. Therefore, start-up firms rarely include non-accredited investors in early private offerings (especially for angel financing when the total amount is relatively small compared to later rounds of financing). In fact, more than 90% of private placements were sold only to accredited investors (Ivanov & Bauguess, 2013),

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

[12]Matthew W. Bower, "Reasons to Include Only Accredited Investors in Your Rule 506(b) Private Offering," `https://www.varnumlaw.com/newsroom-publications-reasons-to-include-only-accredited-investors-in-your-rule-506b-private-offering`.

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 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, which are used to determine whether an individual may invest in certain unregistered securities offerings. The amendment became effective on February 27, 2012.[13] 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).[14]

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 economic costs and

[13]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.

[14]In unreported analysis, I observe that there was no significant increase in the usage of placement agents in angel financing after the regulation change. However, the definition of accredited investors can still be binding due to the search friction between entrepreneurs and investors. Rubinstein and Wolinsky (1987) suggests that the value of a buy-side middleman will decrease when the ratio of the number of sellers to the number of buyers becomes smaller. In the context of angel financing, the network of placement agents mainly help entrepreneurs reach out to marginal angel investors who were hard to be contacted by entrepreneurs themselves. Once the number of marginal investors decreased due to the regulation change, entrepreneurs' necessity to use placement agents to find angel investors may not change or even decrease.

benefits of the above regulation change.

2.4 Data

2.4.1 Data Sources

I compile data from various sources. Among them, angel investments are the most difficult to observe and previous studies had to rely mainly on estimations from surveys (Shane, 2008). Following Denes et al. (2020), I combine data from SEC Form D filings, Crunchbase, and Thomson Reuters VentureXpert to overcome this 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.[15] 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 each 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 only allow one filing per day for one firm to avoid duplicate filings.

The Form D observations are then supplemented with angel investments from Crunchbase and VentureXpert.[16] 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

[15]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.

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

the round type and investor identity.[17] 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. Non-matched observations are then added with the first-time Form D filings to form a comprehensive angel-investment database. As part of the matching procedure, I exclude first-time Form D filings if they are regarded as VC/PE rounds using information from Crunchbase and VentureXpert. Finally, I aggregate the angel investments at the city level semiannually.[18]

To measure the extent of a city being affected by the SEC regulation change, I construct the mean home-value-to-net-worth ratio. Higher ratios indicate greater potential impact. Home value data are from Zillow.[19] The 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).

To examine the impact on local entrepreneurial activity, I look at the subsequent financing and successful exits (i.e., IPO or acquisition) of angel-backed firms. Data on firms' subsequent financing, investor identity, and successful exits are collected from SEC

[17]I include round types specified as "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 VC" as angel investments.

[18]I set the unit of the analysis to be a city instead of other geographic 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 Downtown 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`. Regarding choosing semester as the main time unit, there are mainly two reasons. First, choosing semesters over years would increase the number of units in the analysis, which enables me to show more specific dynamics of the coefficient estimates (providing evidence for the parallel trend assumption) and perform the placebo test using more specific pseudo-event times. Second, I did not use quarterly time units because much more cities would have zero filings and zero firms having successful exits and subsequent financings in a quarter. The main results are robust under different time units (see Table B10 in the Appendix).

[19]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 (2020); Mian, Sufi, and Trebbi (2015)).

Form D filings, Crunchbase and VentureXpert. I match firms in these databases 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 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.[20]

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) and data on second-lien mortgages collected under the Home Mortgage Disclosure Act (HMDA).

The unit of analysis in my study is at the city level. I match all the variables using city names and manually check for matching accuracy.[21] 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.

[20]29,808 out of 43,123 angel-backed sample firms have matched information in the NETS database. The matching rate of 69% is similar as in Denes et al. (2020). I observe 13,459 sample firms that have at least one patent during the sample period and treat the unmatched firms as having zero patents. The matching between the angel-backed firms and other datasets is separated from the aggregation of the angel financing records to city level, and therefore, does not affect the main result of how the regulation change impacted local angel financing.

[21]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 (available at `https://simplemaps.com/data/us-cities`) to make sure that the territory a city name refers to, remains the same during the sample period.

2.4.2 Variable Construction

Construction of Outcome Variables

The first set of outcome variables in the analysis are related to local angel financing. I construct two variables, 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 received angel-backing in city i and time t and later receive next-round financing (*ln(Num_next_financing+1)*) and the number of angel-backed firms that later receive VC financing (*ln(Num_later_VC+1)*) as the outcome variables for subsequent financing (t 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 are acquired later (*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 examine the innovation, employment, and sales generated by 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 who received their investments in city i and time t in the next year (*ln(Employment+1)*) and the natural logarithm of one plus the amount of sales generated by these 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 (i is the city where borrower firms locate in and t is the loan application time). To examine the impact on home equity loans, I use the number and the amount of second-lien mortgages (*ln(2ndlien_num+1)* and *ln(2ndlien_amnt+1)*) in city i annually (i is the city where mortgage borrows locate in and t is the mortgage application time).

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. The key explaining 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 construction of the weighted average net worth in city i is estimated through the following steps: 1) the total net worth and the net worth of five categories of assets of an average household in a state in 2011 are collected using data from the SIPP; 2) using data from the IRS, state-level net-worth-to-income ratios, $(\frac{NW}{Income})_{state,category}$, are calculated by dividing the average net worth of each asset category to the average household gross income of that category in 2011; 3) multiplying the net-worth-to-income ratio at the state-level by the income from each asset category using the ZIP-code level income ($Income_{zip,category}$) data from the IRS, I obtain the household net worth of each asset category at the ZIP-code level ($NW_{zip,category} = \left(\frac{NW}{Income}\right)_{state,category} * Income_{zip,category}$) and add them up to get the household total net worth at the ZIP-code level;[22] 4) the city-level household net worth is estimated by taking the weighted average of the net worth of all ZIP codes in the city using ZIP code-level population as the weights.[23] I discuss more on the details of constructing the net worth 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.[24]

[22]Net worth statistics are not available for geographic units lower than the state level. To conduct this research at a finer geographic level and employ more variation in the treatment variable across the U.S., an assumption is made in the estimation that the net-worth-income ratio is constant within a state.

[23]Note that the estimated net worth does not include home value even though it may include the net worth of real estate assets.

[24]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.

2.4.3 Summary Statistics

Summary statistics are reported in Table 2.1. To alleviate the concern that the results may be driven by outliers, I winsorize all city-level aggregated variables at the 1st and 99th percentiles in the regressions.[25]

As shown in Panel A Table 2.1, the median of 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 2.2 shows the geographic variance of the *HV/NW* ratio across the U.S. in 2011.[26] 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 2.2 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. Furthermore, the impact of the regulation change does not seem to be merely a metropolitan phenomenon.[27]

Panel B of Table 2.1 reports the summary statistics on the outcome variables related to local angel financing. On average, a sample city has 1.2 (=0.616*2) angel investments per year totaling $6.2 (=3.110*2) million. Panel C of Table 2.1 reports

[25]The statistics reported in Table 2.1 are not winsorized. The main results are similar if using non-winsorized variables in the regressions.

[26]Note that the figure is used to illustrate the geographic 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 2.4.1, 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.

[27]In Figure B1, I show the *HV/NW* ratio of cities that are within top-30 metropolitan statistical areas (MSA). 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*.

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 2.1 shows statistics related to small business loans and second-lien mortgages. As reported in Panel F of Table 2.1, 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.

Panel G of Table 2.1 presents the summary statistics of the sample firms. There are 43,123 firms that received angel financing in the sample (i.e., 43,123 angel financing deals). On average, the amount raised is about $2.4 million per deal with $750,000 as the sample median.[28] Among these firms, 21.4% of them received the next round of financing within 5 years and 6% of them received VC financing later, 0.4% of the sample firms have gone public, and 1.6% have been acquired. In Table B1 in the Internet Appendix, I report more details on the age and geographical distributions of the sample firms.

2.5 Impact on Local Angel Financing

2.5.1 Main Specification and Baseline Results

To examine whether the 2011 regulation change of removing primary residence from net wealth in the qualification standard for accredited investors has generated any impact on local angel financing, I use a DiD approach with a continuous treatment.

The 2011 regulation change appears to be a good candidate to generate exogenous variation in investor protection strength given that the heterogeneity in housing values and net worths could lead to differences in the fraction of accredited investors being affected across U.S cities. The reverse causality concern is low given that the regulation

[28]These statistics are comparable to other datasets. For example, Pitchbook reports that "the median deal size for angel rounds is $600,000 compared to $2.1 million for seed rounds." The Pitchbook report is available at `https://pitchbook.com/news/reports/3q-2019-2019-venture-capital-outlook-1h-follow-up`.

change was mainly enacted to prevent unsophisticated investors from loss of primary residency and personal bankruptcies, and not in anticipation of future entrepreneurial activity. This exogenous variation in investor protection strength is captured by the treatment variable $ln(HV/NW)_i$ for each city i at the end of 2011.[29] I more fully discuss on the causal interpretation and the validity of the treatment variable after showing the results on local angel financing.

The DiD analysis is performed by estimating the following equation:

$$Y_{i,t} = \alpha + \beta ln(HV/NW)_i * Post_t + Controls_{i,t} + \delta_t + \eta_i + \epsilon_{i,t}. \tag{2.1}$$

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. $Post_t$ is a dummy that equals one if period t is after 2011 and equals zero otherwise.[30] $Controls_{i,t}$ 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 (and therefore, the $ln(HV/NW)$ and $Post$ are omitted in the regressions). In all regressions, I cluster

[29] To account for the right skewness of the variables and to facilitate the interpretation of the estimation magnitude, I take log transformation for both the treatment variable and dependent variables.

[30] As discussed in footnote 13, 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 all the provisions (most of which are not relevant for individuals but on regulating banking and financial institutions), 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.

standard errors both at the city level and at the time level.[31]

Table 2.2 shows the results. In columns (1) and (3), the dependent variable is the quantity variable of angel financing, *ln(Num+1)*. In columns (2) and (4), the dependent variable is replaced with the amount variable of angel financing, *ln(Amount+1)*. Columns (1) and (2) show the results when controlling for city fixed effects and time fixed effects. Columns (3) and (4) show results with additional demographic control variables. The coefficient estimates on $ln(HV/NW) * Post$ in Table 2.2 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 increases 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 put it in another way, when the HV/NW ratio increases one standard deviation (49.7% = 0.574/1.154) for all the cities in the sample, there would be a $2.75 billion-larger decrease in the amount of angel financing per year.[32]

2.5.2 Identification Assumptions and Challenges: Additional Tests

The causal interpretation of the results relies on three main identifying assumptions. I take several steps to provide supporting evidence for these assumptions.

First, to 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 by replacing the time dummy ($Post_t$) in equation (2.1) with a set of dummies that represent each semiannual period ($Period_t$). The dummy for the event period (i.e., the second-half year

[31] 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.

[32] From the coefficient estimate, an average city would experience a 11.33% (=.228%*0.497) larger decrease in the amount, i.e., $11.33\% * \$3,110,000 = \$352,363$ per semester. Hence, all the sample cities across the U.S. would experience a $\$352,363 * 2 * 3,896 = \$2.75$ billion larger decrease per year if all sample cities had a one-standard-deviation increase in the HV/NW ratio in 2011.

of 2011) is dropped to avoid the multicollinearity problem. I control for the same set of variables as in equation (2.1) with city fixed effects and time fixed effects included.

Figure 2.3 plots the coefficient estimates of β_t in equation (2.3). The dependent variable in the regression is *ln(Num+1)* in the left panel of Figure 2.3 and *ln(Amount+1)* in the right panel. Both panels in Figure 2.3 show 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 change indeed had a negative effect on angel financing. Figure 2.3 provides supporting evidence for the parallel trend assumption not being violated.

Second, my empirical approach will be most effective when angel investments (especially from those marginal investors) are local. Previous research has shown that entrepreneurial investments tend to be distance-sensitive (A. Agrawal et al., 2015; Michelacci & Silva, 2007; Stuart & Sorenson, 2005), which is consistent with the assumption. Furthermore, Figure 2.1 shows that around 60% of the angel-firm pairs in the Crunchbase database have a distance of less than 100 miles, suggesting that most angel investors in the U.S. invest locally. Next, to address the concern that the previous results might disappear when considering spillover effects, I run the baseline regressions controlling for these effects from nearby cities in regressions. The results are shown in Panel A of Table 2.3. In addition to $ln(HV/NW) * Post$, 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, $ln(HV/NW)_{25(50,100)Miles} * Post$. The results suggest that the SEC regulation change had negative spillover effects on the angel financing in nearby regions and that after controlling for the spillover effects, the main effect of the regulation change on local angel financing is still significant.[33]

[33] For the succinctness of the paper, I report results of the additional identification tests in Table 2.3 when the dependent variable is the number of angel investments, *ln(Num+1)* as the number of firms receiving angel financing is more relevant than the total dollar amount of financing received in a city. The results and conclusions are similar when using the amount variable, *ln(Amount+1)*.

Third, readers may worry that the treatment variable, the *HV/NW* ratio, may not reflect the extent of a city being affected by the SEC regulation change, but indicate other contemporaneous factors. One specific concern is that the 2011 SEC regulation change was implemented during the recovery of housing market after the Great Recession. Regions hit the most during the recession may experience a greater recovery afterwards, and therefore, the decline in angel financing in these regions may not be driven by the 2011 regulation change but by potential entrepreneurs switching from angel financing to mortgaging housing equity to relax their financial constraints (Corradin & Popov, 2015; S. P. Kerr, Kerr, & Nanda, 2015; Schmalz, Sraer, & Thesmar, 2017). In Panel B of Table 2.3, I control for short-term housing price changes (past six-month or one year) in addition to the level of housing price (*Home_value*) and find that the previous findings stay robust. In Table B2 in Appendix B, I split all the sample cities into two groups based on their housing market growths from the end of 2008 to the end of 2011 and run a sub-sample test. If the alternative explanation was true, the baseline results should be stronger in cities with a higher housing price growth because entrepreneurs could borrow more against their housing equity. The results are contrary to the explanation of housing market recovery .

One may also question whether the SEC regulation change, which in theory should only affect marginal angel investors, would have impact on local angel financing when the large angel clusters are excluded from the sample.[34] Panel C of Table 2.3 suggests that the negative impact on local angel investments was particular strong in regions that are not within the radius of San Francisco, New York, and Boston, confirming that marginal angel investors were the ones that drove the results.

I perform two placebo tests to further substantiate the causal interpretation of the results. In the first test shown in Panel D of Table 2.3, I show that the SEC regulation

[34] Angel investments are prevalent 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). In fact, Huang et al. (2017) show that 63% of angel investors reside outside the three cities, San Francisco, New York, and Boston (hereafter, "the three" cities) where most VCs located in.

change had no significant impact on non-angel or later-stage investments (the 2011 SEC regulation change which mainly affected marginal angel investors should not have significant impact on these investments). Pane E of Table 2.3 shows the results of the second test where I replace the actual event time with different pseudo-event times to address the concern that other contemporaneous events may contaminate the previous findings. I do not observe significant results using pseudo-event times.

I conduct several other tests to show the robustness of the main results in the Internet Appendix B. In Table B3 and Figure B2, I show that the results are similar when using a classic DiD approach, where the continuous treatment variable ($ln(HV/NW)$) is replaced by a dummy variable, which equals one if city i's HV/NW ratio is larger than the sample median of the HV/NW ratio in 2011. Tables B4 and B5 show that the results are robust when excluding cities with the top and/or bottom deciles of net worths or housing values. Table B6 shows that the results are similar to those in the baseline regressions both statistically and economically when using an alternative treatment variable, the ratio of top-tier home value to the average net worth of individuals with top-bracket income in a city (the HV_top/NW_top ratio).[35] Table B7 shows that the negative impact of the regulation change on angel financing appeared across firms of all age groups.

2.6 Impact on Local Entrepreneurial Activity

Even though the last section shows that the SEC regulation change limited the participation of marginal angel investors and reduced local angel financing, it is unclear if it would affect the financing for high-quality firms and have real impact on the local economy. In a perfect market where marginal investors match with marginal firms, reduced supply of capital by restricting the participating of investors with marginal wealth should not affect the fund raising of high-quality firms, such as those who would have

[35]The HV/NW ratio and the HV_top/NW_top ratio are highly correlated (a correlation coefficient of 0.8).

an IPO or receiving next-round financing. However, if there are some frictions in the market that could hinder the matching between investors and firms, then high-quality firms would also face challenges in raising angel capital when the pool of local investors shrank. In this section, I examine the impact of the 2011 SEC regulation change on local entrepreneurial activity measured by the number of angel-backed firms receiving subsequent financing or successful exits (i.e., IPO or Acquisition). I then examine the rate of the above entrepreneurial activity of angel-backed firms. I use the same empirical specification as illustrated by equation (2.1).

Table 2.4 examines whether the SEC regulation impacted 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 negative at the 5% significance level and at the 10% significance level, respectively. The magnitude of the above coefficient estimates suggests that one standard deviation increase (i.e., a 49.7% increase) in the HV/NW ratio is associated with a 0.75% greater decrease in the number of angel-backed firms that received next-round financing and 0.40% greater decrease in the number of angel-backed firms that later received VC financing.

Table 2.5 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 a one standard deviation increase in the HV/NW ratio, led to a 0.30% greater decrease in the number of angel-backed firms that have an acquisition, a 0.25% greater decrease in the number of angel-backed firms have an IPO, and a 0.40% greater decrease in the number of angel-backed firms having an acquisition or an IPO after the regulation change.

In addition to the aggregated variables, I also examine how the SEC regulation change has affected the rate of receiving subsequent financing and the rate of having a successful exit conditional on firms having received angel financing. Table B8 in Internet Appendix B shows the results. 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.[36]

The above results show that the regulation change of restricting the definition of accredited investors had negative impact on local entrepreneurial activity generated by angel-backed firms. The results also suggest that due to certain frictions in the angel

[36]There can be two explanations for the above findings on the non-positive impact on the aggregated number and the rate of entrepreneurial activity. One is due to the distance-related frictions (A. Agrawal et al., 2015) in the angel financing market that some marginal investors who had better local information to select or monitor firms were not able to invest after the regulation change, which also led to some high-quality firms lost the access to angel capital. Another explanation is that some high-quality firms switched from angel financing to debt financing as Section 2.8 shows that the aggregated small business lending and mortgage lending increased.

financing market, the regulation change affected the funding raising even for some high-quality firms.

2.7 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.

Table 2.6 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)*.[37] 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 a one standard deviation increase from the mean of a city's HV/NW ratio, on average, led to a 0.99% greater decrease in the total number of patents, a 0.05% greater decrease in the total number of patent citations, and a 0.02% greater decrease in the number of citations per patent by firms that received angel financing after the 2011 regulation change than those received angel financing prior to the regulation change.

Table 2.7 presents the results of examining whether the SEC regulation change has affected the total employment supported and total sales generated by local angel-

[37] All variables related to patents have been adjusted for truncation bias following Hall et al. (2001), as discussed in Section 2.4.

backed firms. The dependent variable in column (1) is *ln(Employment+1)*, the natural logarithm of one plus the number of jobs supported in the next year by firms that received angel financing in city i and time t. The coefficient estimate in column (1) is significantly negative at the 1% significance level. The magnitude of the estimate in column (1) suggests that a one standard deviation increase in a city's HV/NW ratio, led to a 3.23% 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 *ln(Sales+1)*, 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. 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 a one standard deviation increase in a city's HV/NW ratio, led to a 11.24% greater decrease in the amount of sales generated in the next year by local angel-backed firms after the regulation change.

The above 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.

2.8 Impact on Demands for Alternative Financing Sources

After showing that the 2011 regulation change has indeed generated 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 could validate the prediction based on the previous findings that entrepreneurs would search for alternatives when the availability of angel financing declined; second, it may show potential unintended consequences of the regulation change on the other sectors of the economy through these alternative financing channels.

2.8.1 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 any 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 observations at the city-semiannual level using the application date. I use the same empirical specification as illustrated by equation (2.1).

Table 2.8 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 a one standard deviation increase in a city's HV/NW ratio prior to the SEC regulation change, would lead to a 26.67% increase in the number of small business loans 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% significance 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.

2.8.2 Second-Lien Mortgages

Previous literature has shown the importance of housing mortgages as a funding source for entrepreneurship (Adelino et al., 2015; Corradin & Popov, 2015; S. P. Kerr et al., 2015; Schmalz et al., 2017). 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. In this section, I examine whether the 2011 regulation change, which reduced local angel financing, had any impact 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.[38] Specifically, I construct two variables using 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.

Results are reported in Table 2.9. 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 a one standard deviation increase in the HV/NW ratio of a city, led to a 9.15% 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 a one standard deviation increase in the HV/NW ratio, led to a 13.13% increase in the amount of second-lien mortgage applications after the SEC regulation change.

[38] HMDA only reports the year of the mortgage application during my sample period and therefore, I had to switch from aggregating semiannually to annually for this specific test.

2.8.3 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. These results, however, need to be carefully interpreted mainly for the two reasons discussed below.

First, given the differences between debt and equity financing, borrowing either from government-sponsored loans or home equity loans is not 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.[39] 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. Additionally, 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, 2002a; W. R. Kerr et al., 2014). Hence, more than just providing funds to a firm, angel investors can also influence the growth and 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 entrepreneurs 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. Even these firms succeed, their successes are subsidized by taxpayers' money: Brown and Earle (2017) estimate that the taxpayer cost per job created from small business loans is at least $21,000. In addition, shifting from equity financing to debt financing may incur underinvestment among risk-averse entrepreneurs

[39] 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

(Myers, 1977).

2.9 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 led to a reduction in angel financing and entrepreneurial activity, which, in turn, imposed real costs on the economy. In this section, I evaluate the trade-off between investor protection and the promotion of entrepreneurial activity. Specifically, I estimate the benefits of the above regulation change in terms of avoiding losses of angel investors through investment in unsuccessful entrepreneurial firms. I estimate the costs of the above SEC regulation change in terms of lost sales, innovation, and employment generated by entrepreneurial firms that did not receive angel financing. I then perform a cost-benefit analysis under different assumptions and discuss the results.

2.9.1 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 average failure rate of angel-backed firms in the city as following:

$$Benefit_{i,t} = \Delta(Amount)_{i,t} * Failure\_rate_{i,t} \tag{2.2}$$

The average 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 next five years by the total number of angel investments in city i and time t.[40] The reduced amount of angel investment of city i in time t, $\Delta(Amount)_{i,t}$,

[40] 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.

is the difference between the estimated amount of angel investments if there was no regulation change and the actual amount of angel investments with the above regulation change. Specifically, the reduced amount of angel financing is estimated as below:

$$\Delta(Amount)_{i,t} = exp\left[|\hat{\beta}| * \frac{HV}{NW}_i + ln(Amount+1)_{i,t}\right] - exp\left[ln(Amount+1)_{i,t}\right], \tag{2.3}$$

and $\hat{\beta}$ in equation (2.3) is obtained from the estimation of the following equation:[41]

$$ln(Amount+1)_{i,t} = \alpha + \beta \frac{HV}{NW}_i * Post_t + Controls_{i,t} + \delta_t + \eta_i + \epsilon_{i,t}, \tag{2.4}$$

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 defined in Section 2.4. The estimated β in equation (2.4) is shown in Table B9 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 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

[41] I use the HV/NW ratio instead of the natural logarithm of the ratio ($ln(HV/NW)$) as in equation (2.1) simply for illustration purpose: When the HV/NW ratio is less than one, $ln(HV/NW)$ is negative and hard to interpret in equation (2.3). 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.

due to the regulation is likely to be lower: According to Table B8, 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.

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 2.10 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).

2.9.2 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 (2.3) and equation (2.4) with replacements of the variable $Amount_{i,t}$ with $Sales_{i,t}$.[42] The estimated reduced sales due to the SEC regulation change are \$0.73 billion for angel-backed firms in 2012 and \$1.05 billion in 2013.[43]

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

[42]The results of the estimation of equation (2.4) are shown in Table B9 column (2) in Internet Appendix B.

[43]\$1.05 billions are the amount of the reduced sales that would have been generated by affected firms that did not receive angel financing in 2013, but not include the sales generated by firms 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).

5% per year, the present value of forgone future sales is $4.36 billion in 2012.[44] 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 Section 2.9.1: The quality of firms received angel financing after the regulation change is assumed to be the same as the quality before the change in the estimation, while the quality of firms actually declined according to Table B8. Therefore, the actual foregone sales, innovation, and employment of firms that did not receive angel financing could be larger than what above estimation suggests.

Similar as 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 2.10 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).

Using the above strategy with a replacement of the variable $Amount_{i,t}$ with $Num\_patents_{i,t}$ and $Employment_{i,t}$, I also estimate the reduced innovation output and employment generated by angel-backed firms.[45] The estimation suggests that the SEC regulation change reduced 292 patents generated by angel-backed firms in 2012 and 289 patents in 2013, 3,770 jobs supported by angel-backed firms in 2012 and 4,392 jobs in 2013. These reduced patents and employment are the additional costs brought by the 2011 regulation change.

[44] The $4.36 billion is calculated from the formula: $\frac{P}{r-g} * \left(1 - \frac{(1+g)^n}{(1+r)^n}\right) = \frac{1.05}{0.15-0.05} * \left(1 - \frac{(1+0.05)^{10}}{(1+0.15)^{10}}\right)$.

[45] The estimates of β in equation (2.4) are shown in Table B9 column (3) and column (4) in Internet Appendix B.

2.9.3 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 2.10 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 (10 years, 5 years, and 3 years). One can observe from Table 2.10 Panel C that the estimated net benefits of the SEC regulation change are negative in 58 out of 63 scenarios. Among all the scenarios, the closest case to the real world is where the discount rate is 30% and the growth rate is 25% (when early investors require a high return and young firms enjoy high sales growth).[46] Under these two assumptions and assuming that the impact of the regulation change lasts for five years, the present value of total net benefits of the regulation change is negative 6.32 billion dollars at the end of 2011.

As mentioned in the previous two subsections, the estimated benefits of the 2011 SEC regulation change are likely to be the upper bound of the actual benefits while the estimated costs tend to be the lower bound of the actual costs. Therefore, the costs of the SEC regulation change are likely to exceed its benefits in most cases from a pecuniary viewpoint, not to mention the costs in terms of the reduced innovation output and employment generated by entrepreneurial firms that would have received angel financing without the regulation change. It is important, however, for readers to notice two major

[46]One study, sponsored by the Ewing Marion Kauffman Foundation (Wiltbank & Boeker, 2007) looking at 3,097 angel investments, shows that the average IRR of these investments is 27%. Other studies provide estimates of returns of angel investors around this number (for a summary, see `http://www.rightsidecapital.com/assets/documents/HistoricalAngelReturn.pdf`). Regarding the growth rate, Kabbage Small Business Revenue Index shows that the median revenue growth of all small businesses across the U.S. is 16% in 2019, while angel-backed firms usually enjoy a higher growth rate than the median small business.

limitations of the above analysis and carefully interpret its results. 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 spillovers 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.[47]

2.10 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.

[47]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 underinvestment problem generated from the shift from equity financing to debt financing when angel investment decreases. Due to the risk aversion of entrepreneurs, they may choose to invest in less risky projects under debt financing even though these projects may bring lower growth to the firm.

2.11 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 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 investing in unsuccessful firms and an estimation of the costs in terms of the reduced sales, patents, and employment generated by angel-backed firms. The cost-benefit analysis suggests that at least 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 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.

2.12 Figures and Tables

Figure 2.1. 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.

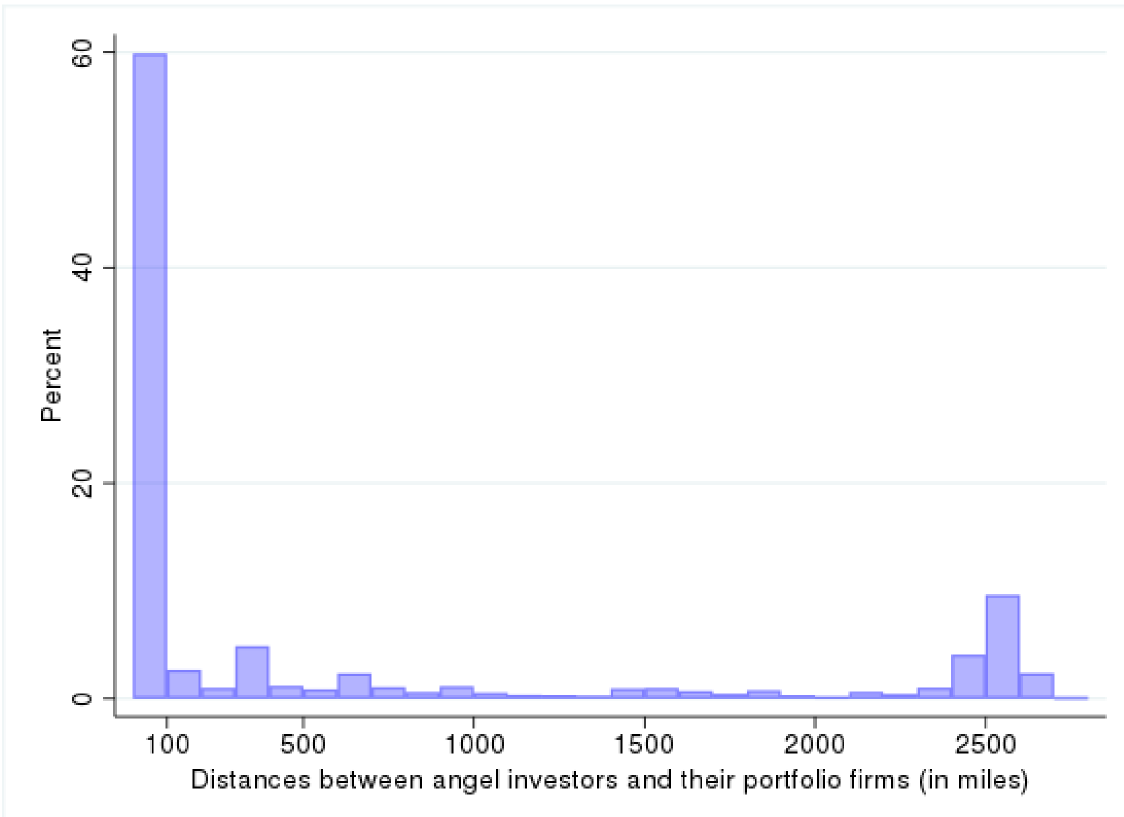


Figure 2.2. Geographical Variation of the Home-Value-To-Net-Worth Ratio in 2011

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.

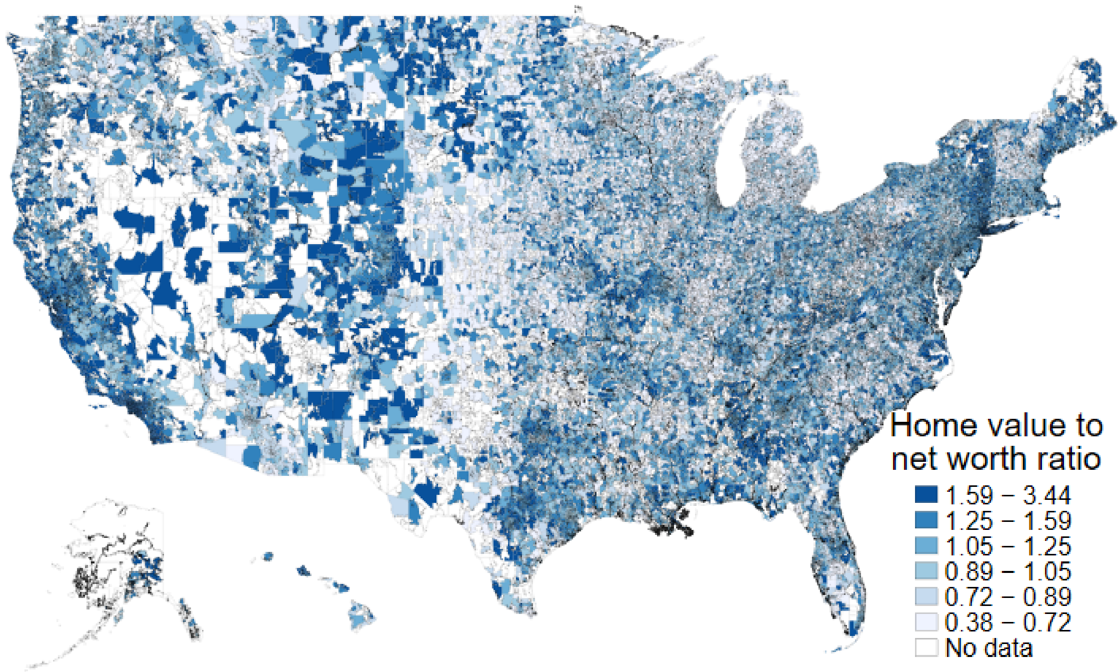
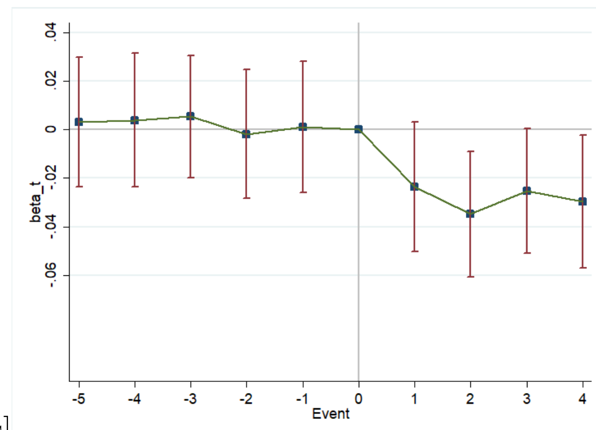


Figure 2.3. Plot of Coefficients Around the Event Time

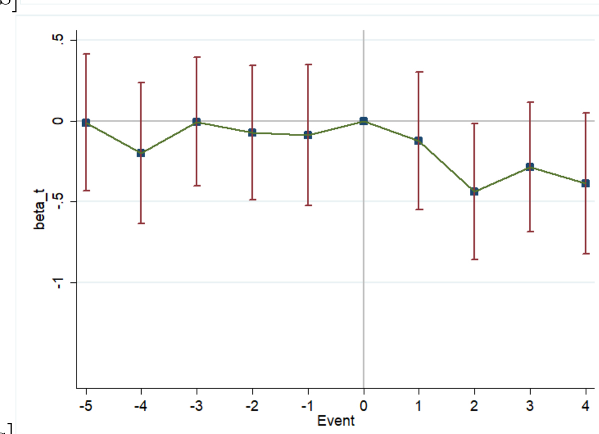
The figure shows the coefficients plot around the SEC regulation change in 2011 by estimating the following model:

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

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.



[Number of Angel Investments]



[Amount of Angel Investments]

Table 2.1. Summary Statistics

	N	Mean	Std. Dev.	Min	Median	Max
Panel A: Treatment variable						
HV/NW	38,960	1.154	0.574	0.119	1.029	4.514
Panel B: Angel investments						
Num	38,960	0.616	2.125	0.000	0.000	20.000
Amount ($million)	38,960	3.110	14.699	0.000	0.000	130.000
Panel C: Entrepreneurial activity (subsequent financing and successful exits)						
Num_next_financing	38,960	0.196	0.776	0.000	0.000	7.000
Num_later_VC	38,960	0.050	0.299	0.000	0.000	3.000
Num_IPO	38,960	0.005	0.090	0.000	0.000	5.000
Num_Acq	38,960	0.018	0.219	0.000	0.000	13.000
Num_Acq_or_IPO	38,960	0.023	0.267	0.000	0.000	13.000
Panel D: Economic activity (innovation, employment, and sales)						
Num_patents	38,960	0.088	0.559	0.000	0.000	5.894
Num_total_cites	38,960	0.002	0.017	0.000	0.000	0.202
Num_cites_per_patent	38,960	0.001	0.007	0.000	0.000	0.068
Employment	38,960	6.907	50.408	0.000	0.000	3,306.044
Sales ($million)	38,960	0.662	6.067	0.000	0.000	494.742

Continued on next page

Table 2.1 – continued from previous page

	N	Mean	Std. Dev.	Min	Median	Max
Panel E: Small business loans and second-lien mortgages						
Num_SBL (million)	38,960	0.076	0.178	0.000	0.019	1.630
Amount_SBL ($million)	38,960	1.554	3.828	0.000	0.135	32.764
Guaranteed_Amount_SBL ($million)	38,960	0.870	2.231	0.000	0.032	18.944
2ndlien_num (000s)	19,375	0.063	0.170	0.000	0.023	5.660
2ndlien_amnt ($million)	19,375	3.499	10.545	0.000	1.217	447.397
Panel F: Control variables						
Population (million)	38,214	0.050	0.127	0.000	0.022	2.923
Income_per_person ($million)	38,214	0.038	0.031	0.009	0.030	0.786
Home_value ($million)	38,960	0.251	0.206	0.022	0.189	3.106
Panel G: Firm-level statistics						
Amount ($million)	43,123	2.414	3.355	0.004	0.750	12.000
1(Next_financing)	43,123	0.214	0.410	0.000	0.000	1.000
1(Later VC)	43,123	0.060	0.238	0.000	0.000	1.000
1(IPO)	43,123	0.004	0.066	0.000	0.000	1.000
1(Acq)	43,123	0.016	0.126	0.000	0.000	1.000
1(Exit)	43,123	0.020	0.139	0.000	0.000	1.000
Patents	43,123	0.094	0.495	0.000	0.000	5.433
Total_cites	43,123	0.005	0.068	0.000	0.000	5.218
Sales ($million)	43,123	0.216	0.541	0.000	0.002	2.138
Employment	43,123	5.141	12.675	0.000	1.060	89.000

Table 2.2. 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 2.4.2. I also control for time and city fixed effects. Standard errors are double-clustered 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)*
$ln(HV/NW) * Post$	-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 2.3. Additional Identification Tests

This table presents the results of additional identification tests. Panel A shows the results of the robustness test by controlling for spillover effects from nearby regions. $ln(HV/NW)_{25(50,100)Miles}$ is the natural logarithm of the average home-value-to-net-worth ratio in cities within 25 (50, 100) miles to city i. Panel B shows the results of the robustness test by controlling for short-term housing price changes. *Home_value_growth_6M* is the change in the natural logarithm of the housing price in a city in the last six months, and the *Home_value_growth_12M* is the change in the natural logarithm of the housing price in a city in the last year. Panel C shows the results of the robustness test by excluding entrepreneurship cluster cities. In Column (1), I exclude San Francisco, New York, and Boston ("the three" cities) in the analysis; In Column (2), I exclude "the three" cities and cities within 100 miles in the analysis. Panel D shows the impact of the SEC regulation change on non-angel investments. The dependent variable in Column (1), $ln(Num\_VC + 1)$, is the natural logarithm of one plus the number of investments made by venture capitalists or private equity firms in city i and time t. In Column (2), the dependent variable is $ln(Num\_later + 1)$, the natural logarithm of one plus the number of non-first-time SEC Form D filings in city i and time t. Panel E presents the results of the placebo test using pseudo event time prior to the actual event time (i.e., the second half of 2011). *Post_09H2* (*Post_10H2*) is a dummy that equals one if period t is after the second half year of 2009 (2010) and equals zero otherwise. Similarly, *Post_10H1* (*Post_11H1*) is a dummy that equals one if period t is after the first half year of 2010 (2011) and equals zero otherwise. The dependent variable in Panels A, B, C, and E 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. In all the panels, I include control variables, *Population*, *Income_per_person*, and *Home_value*. 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.

Panel A. Controlling for Spillover Effects from Nearby Regions

	(1) *ln(Num+1)*	(2) *ln(Num+1)*	(3) *ln(Num+1)*
$ln(HV/NW) * Post$	-0.015*	-0.016*	-0.014*
	(0.007)	(0.008)	(0.007)
$ln(HV/NW)_{25} * Post$	-0.023*		
	(0.011)		
$ln(HV/NW)_{50} * Post$		-0.027**	
		(0.012)	
$ln(HV/NW)_{100} * Post$			-0.041**
			(0.015)
Observations	38,064	38,194	38,204
R-squared	0.669	0.668	0.669
Controls	YES	YES	YES
City FE	YES	YES	YES
Semi-annual FE	YES	YES	YES

Panel B. Controlling for Short-Term Housing Price Changes

	(1) *ln(Num+1)*	(2) *ln(Num+1)*	(3) *ln(Num+1)*	(4) *ln(Num+1)*
$ln(HV/NW) * Post$	-0.027***	-0.027***	-0.027***	-0.027***
	(0.006)	(0.006)	(0.006)	(0.006)
Home_value_growth_6M	0.064	0.071		
	(0.064)	(0.061)		
Home_value_growth_12M			0.026	0.032
			(0.034)	(0.031)
Observations	38,214	38,214	38,214	38,214
R-squared	0.668	0.668	0.668	0.668
Controls	YES	YES	YES	YES
City FE	YES	YES	YES	YES
Semi-annual FE	YES	YES	YES	YES

Panel C. Excluding Top-Three Entrepreneurship Cities and Cities Nearby

	(1) *ln(Num+1)*	(2) *ln(Num+1)*
$ln(HV/NW) * Post$	-0.026***	-0.024***
	(0.006)	(0.006)
Observations	38,184	37,174
R-squared	0.663	0.658
Exclude Cities	"the three"	<100 miles "the three"
Controls	YES	YES
City FE	YES	YES
Semi-annual FE	YES	YES

Panel D. Placebo Test: Impact on Non-Angel Investments

	(1) *ln(Num_VC+1)*	(2) *ln(Num_later+1)*	(3)
$ln(HV/NW) * Post$	-0.002	0.001	
	(0.002)	(0.012)	
Observations	38,214	38,214	
R-squared	0.638	0.636	
Controls	YES	YES	
City FE	YES	YES	
Semi-annual FE	YES	YES	

Panel E. Placebo Test: Using Pseudo Event Time

	(1) *ln(Num+1)*	(2) *ln(Num+1)*	(3) *ln(Num+1)*	(4) *ln(Num+1)*
*ln(HV/NW)*Post_09H2*	-0.007			
	(0.006)			
*ln(HV/NW)*Post_10H1*		-0.006		
		(0.006)		
*ln(HV/NW)*Post_10H2*			-0.009	
			(0.006)	
*ln(HV/NW)*Post_11H1*				-0.005
				(0.005)
Observations	38,214	38,214	38,214	38,214
R-squared	0.499	0.499	0.499	0.385
Pseudo Event-Time	2009H2	2010H1	2010H2	2011H1
Controls	YES	YES	YES	YES
City FE	YES	YES	YES	YES
Semi-annual FE	YES	YES	YES	YES

Table 2.4. 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 2.2. 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 2.4.2. I also control for time and city fixed effects. Standard errors are double-clustered 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)*
$ln(HV/NW) * Post$	-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 2.5. 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 2.2. 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 2.4.2. I also control for time and city fixed effects. Standard errors are double-clustered 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)*
$ln(HV/NW) * Post$	-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
Semi-annual FE	YES	YES	YES

Table 2.6. 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 2.4.2. I also control for time and city fixed effects. Standard errors are double-clustered 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)*
$ln(HV/NW) * Post$	-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 2.7. 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 2.4.2. I also control for time and city fixed effects. Standard errors are double-clustered 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)*
$ln(HV/NW) * Post$	-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 2.8. 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 2.4.2. I also control for time and city fixed effects. Standard errors are double-clustered 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 2.9. 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 2.4.2. I also control for year and city fixed effects. Standard errors are double-clustered 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)*
$ln(HV/NW) * Post$	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 2.10. 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 (n years) and on the discount rate (r). Panel B of the table shows the estimated 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 (g), the discount rate (r), and the length of the policy impact will last (n). For example, given that the estimated benefit is \$3.19 billion for 2012 and \$3.08 billion in 2013 (according to Section 2.9.1) and assuming the estimated benefit after 2013 is the same as in 2013, the present value of the total benefits at the end of 2011 can be calculated as $\frac{\$3.19}{1+r} + \sum_{t=2}^{n-1} \frac{\$3.08}{(1+r)^t}$. When $r = 15\%$ and $n = 10$, the present value of benefit is \$15.55 billion. Section 2.9.2 shows that the estimated reduced amount of annual sales of affected firms is \$0.73 billion in 2012 and \$1.05 billion in 2013, when assuming that firms operate for 10 years, $g = 5\%$ and $r = 15\%$, then the discounted value of reduced sales for affected firms in 2012 is \$4.36 billion ($\$4.36 = \frac{\$0.73}{0.15-0.05} * \left(1 - \frac{(1+0.05)^{10}}{(1+0.15)^{10}}\right)$) and \$6.27 billion in 2013. We can obtain the present value of the total costs in terms of reduced sales at the end of 2011 by calculating $\frac{\$4.36}{1+r} + \sum_{t=2}^{n-1} \frac{\$6.27}{(1+r)^t} = \$29.81$ billion. Panel C of the table shows the estimated net benefits (*i.e.,* benefits minus costs) under different assumptions. The details of the estimation is described in the Section 2.9.

Panel A. Estimation of Benefits

[6mm] **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

	g=r=	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
	g=r=	**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
	g=r=	**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

Chapter 3

Angels and Venture Capitalists: Complementarity versus Substitution, Financing Sequence, and Relative Value Addition to Entrepreneurial Firms

3.1 Introduction

Angel Investors (angels) and venture capital (VC) investors are two of the most important types of financiers investing in entrepreneurial firms not only in the U.S., but also around the world. A 2011 report by the OECD mentions that in 2009 the total amount of capital investment made by angel investors (angels) in the U.S. was $17.7 billion, which is similar to the $18.7 billion investment made by VCs.[1] Practitioners, particularly VCs, often believe that, while angels are important in providing seed capital to firms, they lack in "due-diligence" ability compared to VCs. Further, they also assume that angels are less capable than VCs in adding value to start-up firms.[2] However, there is an alternative view that VCs and angels do not differ in terms of adding value to start-ups. For example, an article by AngelList mentions that the presence of top VCs in a seed funding round of a start-up does not affect the probability of receiving a Series A funding for the start-up.[3] It is therefore important to empirically analyze and compare the value added to start-ups by angels and VCs. However, the existing

[1]Please refer to the following report for greater detail: `http://www.oecd.org/sti/financinghigh-growthfirmstheroleofangelinvestors.htm`

[2]Please refer to an article in the Wall Street Journal, titled, "AngelList And Beyond: What VCs Really Think Of Crowdfunding," which includes comments from VCs who mentioned that angels have a lower ability to add value compared to VCs.

[3]Please refer the article here: `https://www.angellist.com/blog/top-vc-seed-performance`.

finance literature has not yet empirically analyzed and compared the value-addition by VCs versus angels.[4] The objective of this paper is to fill this gap in the literature.

In this paper, we use several private firm data sets to address three important research questions. First, we compare the extent of value addition by angels versus VCs. We use several important measures to capture value additions: the probability of successful exits (IPO or acquisitions), the quantity and quality of innovation output; sales growth; employment growth; and the net inflow of high-quality inventors to start-ups. Second, we analyze whether VCs and angel financing are complements or substitutes.[5] Third, we examine the effect of financing sequence or the order of investment by VCs and angels into a start-up firm on the likelihood of its successful exit.

We compile our private firm data from various sources. We collect round by round financing information on U.S. start-ups from CrunchBase and VentureXpert. While Crunchbase provides information on aggregate funding per investment round at start-ups, VentureXpert provides information on the investment made by an individual VC in each investment round. We obtain the fraction of VC investment in an investment round using the above two datasets. The successful exits of start-ups in terms of initial public offerings (IPO) or acquisitions are also collected from CrunchBase, which also provides information on the founding years of start-ups. We use the National Establishment Time Series (NETS) dataset to obtain information on the sales and employment levels of private firms. Our patent and inventor data is obtained from the United States Patent and Trademark Office (USPTO) dataset shared on PatentsView. Using the above datasets, we construct our main outcome variables for start-ups: the probability of a successful exit (IPO versus acquisition), the annual sales and employment growth of private firms, the quantity and quality of patents granted to start-up firms (standard measures of in-

[4]The existing literature has examined the impact of VC-backing on the performances of start-up firms (Bernstein, Giroud, and Townsend (2016); Chemmanur, Krishnan, and Nandy (2011); Hellmann and Puri (2000); Hellmann and Puri (2002b) among others) and the impact of angel investors on start-ups (Denes et al. (2020); W. R. Kerr et al. (2014); Lerner et al. (2018), among others), separately.

[5]Hellmann, Schure, and Vo (2021b) have empirically analyzed the "complementarity" versus "substitutability" of angels and VC using data from British Columbia, Canada.

novation output), and the net inflow of inventors and high-quality inventors. In most of our analysis, we focus on firms that have received only angel or VC-financing or both in the first round of investment at firms, i.e., we exclude firms that have received financing from other categories of investors such as accelerators or government grants. This allows us to use the fraction of angel financing received by a firm as the main independent variable.[6] Our main sample covers 5,586 U.S. start-up firms financed between 1990 to 2015.

We now discuss the results of our empirical analysis. We start with baseline analyses to compare the effects of VC versus angel financing on start-ups' performance. Our main independent variable is the fraction of angel investment in the first round of financing for a start-up. In our analyses, we focus on investor composition at the first investment round itself, since the types of investors who participate in later investment rounds are likely to be affected by the types of early-stage investors. This approach enables us to distinguish between the value added by angels versus that by VCs given that they invest at the same stage of a start-up's life cycle.[7]

First, we show that firms with a higher fraction of angel investment in their first financing round are associated with a smaller likelihood of successful exit either through an IPO or an acquisition. Our results are statistically and economically significant. A one standard deviation increase in the fraction of angel investment (relative to VC investment) in the first financing round is associated with a 0.6 percentage point decrease in the probability of a firm conducting an IPO in the future. This is equivalent to a decrease of 11.1 percentage in the average probability of an IPO. Further, a one standard deviation increase in the fraction of angel investment in the first financing round is associated with a decrease of 17.7 percentage in the average probability of a successful

[6]Note that in the sample of start-up firms with only angel or VC financing, the fraction of VC financing is the complement of the fraction of angel financing.

[7]Ewens, Nanda, and Rhodes-Kropf (2018) show that technological shocks changed the investment strategies of VCs, leading to VCs investing smaller amounts across a larger pool of startups ("spray and pray" investment strategy). They also mention that, following the technological shocks, there is an increase in the participation of VCs in early-stage financing rounds of startup firms.

exit.

Second, we show that firms with a higher fraction of angel investment in their first financing round are associated with smaller sales growth. A one standard deviation increase in the fraction of angel investment in the first financing round is associated with a 9.3 percentage point smaller growth rate of sales in one year after receiving the first round of investment. This is equivalent to a decrease of 25.6 percentage in the average sales growth in the next year after the first round of investment. We find similar results for sales growth for the second and the third year after the first financing round. Lastly, we show that firms with a higher fraction of angel investment in their first financing round are associated with smaller employment growth. A one standard deviation increase in the fraction of angel investment in the first financing round is associated with an 8.8 percentage point smaller annual employment growth rate one year after the first round of investment. We find similar results for employment growth for the second and the third year after the first financing round.

Third, we show that firms with a higher fraction of angel investment in their first financing round are associated with a smaller quantity and quality of innovation output. Our results are statistically and economically significant. A one standard deviation increase in the fraction of angel investment in entrepreneurial firms' first financing round is associated with a 13.2 percentage point decrease in the number of patents applied (and eventually granted) within the next three years after receiving investment, which is equivalent to a decrease of 24.8 percentage in the average number of patents applied (and eventually granted) within the next three years. Similarly, a one standard deviation increase in the fraction of angel investment in entrepreneurial firms' first financing round is associated with a decrease of 30.5 percentage in the average of citations on patents applied (and eventually granted) within the next three years. We find similar results for innovation output for the second and the third year after the first financing round.

Fourth, we show that firms with a higher fraction of angel investment in their first financing round are associated with a smaller net inflow of inventors and a smaller net inflow of top-quality inventors. Again, our results are statistically and economically significant. A one standard deviation increase in the fraction of angel investment in entrepreneurial firms' first round of financing is associated with a decrease of 1.3 percentage point in the net inflow of top inventors within the next three years, which is equivalent to a decrease of 33.9 percentage in the average net inflow of top inventors within the next three years.

Our baseline analyses may suffer from a common endogeneity concern in the entrepreneurial finance literature: "selection" versus "value-addition" (or monitoring). In other words, do VCs have a better ability to select start-ups or do they have better monitoring abilities compared to angel investors or do both factors play important roles? To disentangle the selection versus value-addition effect, we use two methodologies: instrumental variable (IV) analyses and switching regression analyses.

First, for our IV analyses, we construct two IVs for our key variable of interest, the fraction of angel investment in the first round of financing for entrepreneurial firms. Our first IV is a dummy variable for the angel tax credit following Denes et al. (2020), which equals one if a firm is located in a state that has an active angel tax credit program. The angel tax credit will affect the supply of angel funding, without affecting the supply of VC funding. Our second IV is constructed using the portfolio returns of limited partners (LPs) of VCs following Samila and Sorenson (2011). Given that it has been documented that LPs have a home bias in their investment strategies and that they allocate a fixed ratio of funds to VCs, past returns of LPs in a state will affect the supply of VC funding to start-ups in the state (see, e.g., Samila and Sorenson (2011)). Our IV analyses using the above two IVs show that angels add less value to start-ups than VCs. In other words, angels have lower monitoring ability compared to VCs. First, we show that a higher fraction of angel investment in the first round causally leads to a smaller probability of successful exit through an IPO or an acquisition. Second, we show that a higher fraction of angel investment in the first round causally leads to smaller sales and employment growth. Third, we show that a higher fraction of angel investment in the first round causally leads to a smaller quantity and quality of innovation output. Lastly, we show that a higher fraction of angel investment in the first round causally leads to a smaller net inflow of inventors and top-quality inventors to start-ups.

The second methodology we employ is the "switching regression with endogenous switching" approach, which accounts for unobservable factors that may affect both the probability of receiving angel or VC financing for a start-up firm as well as the start-up's future performance in terms of successful exit, sales growth, and employment growth, innovation output, and the net inflow of top-quality inventors. The results from this analysis can provide answers to the following "what-if" questions: what would be the future outcome for start-ups that are initially VC-backed if they had not received any VC financing, or in other words, received financing only from angel investors? Similarly, what would be the future outcome for start-ups that are initially only angel-financed if they had received financing from VCs? The difference between the actual outcome and the counterfactual outcome of entrepreneurial firms generated from the above analyses represents the gap caused by differences in the monitoring (value-adding) abilities of angels and VCs. Specifically, we find that VC-backed firms have a higher likelihood of having a successful exit, higher sales growth, and higher employment growth, greater quality and quantity of innovation output, and a larger net inflow of inventors compared to the counterfactual (hypothetical) scenario if they had received financing only from angels. Similarly, we find that firms financed by angels alone could have enjoyed a significant increase in the likelihood of having a successful exit, greater sales growth and employment growth, greater innovation output, and a greater net inflow of inventors had they received VC investment (counterfactual). In summary, our IV analyses and switching regression analyses disentangle the selection effect from the value addition effect and suggest that angels have a lower ability to add value to start-ups compared to VCs.

We also conduct additional robustness tests to address some potential concerns with our findings. One may argue that our results showing that angel investors add less value to start-ups than VCs are driven by unsophisticated angel investors, who provide funds to their friends and families. To address this concern, we restrict our sample to first-round angel investments that include at least one sophisticated angel investor, e.g., angel groups or "serial" angel investors. We show that all our main IV analyses hold in the above subsample. Further, we also repeat our IV analyses on subsamples of first investment rounds comprising only VCs or angel groups. We find that VCs add more value than angel groups, thereby addressing the above concern. Another potential concern is that VCs and angels invest in separate industries because of their different specializations. In order to test whether angels add less value than VCs only in certain industries, we conducted an additional subsample analysis based on industry categories. We identify high technology (HiTech), manufacturing, and healthcare as three industry categories where VCs may dominate in providing financing. We use the Fama-French 10 classification to identify the above industries. We find that angels add less value than VCs in both VC-dominated and other industries, thereby addressing the above concern.

Next, we study the relationship between angels and VCs: we test whether angels and VCs are "complements" or "substitutes." In other words, does a start-up receiving a larger fraction of angel financing in its first round make it more or less likely to receive VC financing in a subsequent round? Further, does receiving a larger fraction of VC financing in the first round of financing make the start-up more likely to receive a larger fraction of VC financing in its later round? For this analysis, we include firms financed by syndicates consisting of not only VCs and angels but also by other types of investors such as accelerators and governments.[8] We find that having a greater fraction of VC financing in the first round makes a firm more likely to have a larger fraction of VC

[8]Given that we include intermediaries other than angels and VCs, the fraction of angel financing is no longer the complement of VC financing in the empirical analysis of this research question.

financing in its next round of financing. However, having an angel investor present in the first round of financing also makes a firm more likely to receive a higher fraction of VC investment in the next round. Our result stands in contrast to the findings of Hellmann et al. (2021b) using British Columbia (Canada) data on start-ups; they find that angels and VCs are substitutes and invest in different industries. Our findings support the prediction of the theoretical model of Chemmanur and Chen (2014), who suggest that angels and VCs are complements and that angels prepare start-ups for future VC investments. The above findings are consistent with angel financing serving as a way to make a start-up viable and "VC-ready" if it did not get VC financing in the first round. However, we also find that a greater fraction of VC investment in the first round is associated with a smaller likelihood of participation of angels in the next round, while the presence of angels rather than VCs in the first round is associated with a higher likelihood of participation of angels in the next round of financing. Overall, the above analyses suggest that angels and VCs cannot be classified solely as complements or substitutes in the financing of entrepreneurial firms. Further, they suggest that the relationship of angel investors and VCs is complex: angels and VCs may act as either complements or substitutes.

We also examine the relationship between start-ups' financing sequence (the order of investments made by angels and VCs in various rounds) and their probability of subsequent successful exits (IPOs or acquisitions). In this analysis, we only include firms that either received only angel or VC investments (or both) in their first two rounds of financing. Thus, we are able to define a dominant financier based on the fraction of investment in a funding round, i.e., we define VCs as a dominant financier if the fraction of aggregate VC investment in a round is greater than 50 percent, and similarly for angels. We categorize four financing sequences based on the first two rounds of investment: from angel-dominated to VC-dominated (angel-to-VC), from VC-dominated to angel-dominated (VC-to-angel), from VC-dominated to VC-dominated

(VC-to-VC), and from angel-dominated to angel-dominated (angel-to-angel). We find that firms with VC-to-VC or angel-to-VC financing sequences have a greater likelihood of successful exit compared to angel-to-angel and VC-to-angel financing sequences. The above results are consistent with the theoretical predictions of Chemmanur and Chen (2014), who argue that venture capital investments in early rounds are positive signals of start-up firms' quality resulting in a higher chance of successful exit, while venture exits in later rounds convey negative signals about firm quality, leading to a smaller probability of successful exit for such firms.

The rest of the paper is organized as follows. Section 3.2 discusses how our paper contributes to the related literature. Section 3.3 discusses the underlying theory that we use to develop our testable hypotheses. Section 3.4 describes our data sources and the construction of variables. Section 3.5 describes our baseline analysis, where we compare the effect of VCs versus angels on start-ups' performance. Section 3.6 presents our results using IV and switching regression analyses to disentangle the effects of screening and monitoring ability of angels and VCs on start-ups' future performance. Section 3.7 discusses our robustness tests. Section 3.8 presents the results of our analysis on whether angels and VCs are complements or substitutes. Section 3.9 presents our analysis of the impact of the financing sequence of investors at start-ups on the likelihood of start-up firms' successful exits. We conclude the paper in Section 3.10.

3.2 Related Literature and Contribution

Our paper contributes to several strands in the literature. First, we contribute to the recent growing literature on the impact of angel investors on the future performance of start-ups. W. R. Kerr et al. (2014) and Lerner et al. (2018) show that professional angel groups have significant positive impact on the performance of their portfolio firms. 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. Lindsey and

Stein (2019) have shown the impact of a regulatory change in the accreditation standard of angel investors on the aggregated employment, while J. Xu (2019) studies the impact of changes in the above accreditation standard of angel investors on the local economy in terms of entrepreneurial firms' innovation, sales, successful exits and the costs and benefits of above regulatory changes on the local economy. In contrast, ours is the first paper in the literature to show that VCs provide greater value addition to start-ups compared to angels, i.e., VCs are causally related to a higher likelihood of successful exit, higher innovation output, higher sales and employment growth for start-ups compared to angels.

Second, our paper also contributes to the large literature on the impact of VC investors in various dimensions of firm performance. Prior literature has shown that VCs improve the efficiency of private firms (Chemmanur et al. (2011)), enhance the professionalization of start-up firms (Hellmann and Puri (2002b)), and VCs' monitoring and tolerance of failure leads to an increase in innovation and the likelihood of going public (Bernstein et al. (2016); Tian and Wang (2014)). Chemmanur et al. (2014) have compared the effect of independent versus corporate VCs on firm innovation. In contrast, our paper shows that VCs add greater value than angels and also provides evidence that the financing sequence of an entrepreneurial firm is associated with its successful exit. Thus, our paper connects the two strands of the finance literature on angel and VC financing by comparing the value added by the above two types of investors and analyzing the impact of different possible financing sequences involving these two investors.[9]

Third, we contribute to the literature studying the relationship between angels and

[9]Existing studies comparing the efficacy of angels versus VC investments in start-ups are based mostly on surveys: see, e.g., Dutta and Folta (2016). In a cross-country study, Cumming and Zhang (2019) show that, compared to private equity (PE) or VC-funded firms, angel-funded firms are associated with a lower propensity for successful exit. However, they do not demonstrate causality in their analysis. In contrast, our paper provides causal evidence that angels add less value to startups than VCs. We do so by analyzing the relative effect of angel versus VC investments on startups' propensity for successful exit, their innovation output, their sales and employment growth, and the net inflow of inventors to these startups.

VCs. Prior studies have posited contrasting predictions on the relationship between angels and VCs. While the theoretical analysis of Hellmann and Thiele (2015) predicts that angels and VCs act as substitutes, Chemmanur and Chen (2014) argue theoretically that angels provide early round of financing to start-ups followed by VC investments in later rounds, suggesting a complementary relationship between VCs and angels. Hellmann et al. (2021b) empirically examine the above question using data on start-ups located in British Columbia, Canada, and find that angels and VCs are substitutes. While Hellmann et al. (2021b) conduct their analysis on a sample restricted to British Columbia-based firms, we conduct our study, in contrast, using the entire universe of start-ups in the U.S. We find that angels and VCs have a complex relationship in making entrepreneurial firms successful: in other words, this relationship cannot be classified strictly as being either a complementary or a substitution relationship.

3.3 Theory and Hypotheses

In this section, we discuss the relevant theoretical literature and develop testable hypotheses.

3.3.1 Angels versus VCs and the Future Success and Performance of Entrepreneurial Firms

In this subsection, we develop our hypotheses on the impact of angels versus VCs on the future success of entrepreneurial firms. On the one hand, the existing theoretical literature has argued that VCs provide various value-adding services to firms that increase their probability of future success (e.g., Chemmanur et al. (2011), Ueda (2004), and others). On the other hand, W. R. Kerr et al. (2014) and Lerner et al. (2018) argue that angel investor groups also contribute to the future success of private firms. However, there is general belief among academics and practitioners that VCs are more capable of identifying and investing in better quality firms (selection) and are more capable in monitoring entrepreneurs and providing other value-adding services. Assuming that VCs are better than angels in selecting entrepreneurial firms and in providing value-adding services, we expect a negative relation between the fraction of angel investment in a start-up and the probability of future successful exit (IPO or acquisition) of the start-up (**H1**). Following the above arguments, we also expect a negative relation between the fraction of angel investment in a start-up and the future growth of the start-up firm as measured by sales growth and employment growth (**H2**).

Prior literature has argued that both angels and VCs contribute to improving the innovation output of investee firms. We expect that VCs are more likely to improve their investee firms' innovation output compared to angels. There are multiple reasons for that. We expect that VCs are better than angels in selecting higher quality firms, which, in turn, are more likely to be innovative, compared to angel-backed firms. We also expect that VCs are better equipped than angels in attracting higher quality talent to entrepreneurial firms, which, in turn, drives the innovation output of investee firms. We also expect that VCs (who act on behalf of limited partners) have greater tolerance for failure compared to angels (who invest their own money). Thus, we expect a negative relation between the fraction of angel investment in a start-up and the future innovation output of the start-up firms (**H3**). Assuming that VCs have a greater network and are more resourceful in attracting high-quality talent to start-ups, we also expect a negative relation between the fraction of angel investment in a start-up and the net inflow of high quality inventors to the start-ups (**H4**).

3.3.2 Angel and VC Financing: Complements or Substitutes?

In this subsection, we develop our hypotheses on the potential relationship between angels and VCs. There are two opposing sets of view on the relationship between angels and VCs in the investment life-cycle of start-ups. Using a theoretical model, Chemmanur and Chen (2014) show that VCs and angels act as complements and that angels prepare the start-ups to receive VC investment in the future. In other words, their model shows that angels are the early-stage investors and that VCs are the late-stage investor in start-ups, leading to a complementarity between the two kinds of investors. This effect is driven by the scarcity of VC funding. Thus, following the above argument, we expect VCs and angels to act as complements (**H5a**). However, Hellmann and Thiele (2015) and Hellmann et al. (2021b) argue that VCs and angels act as substitutes. They argue that VCs and angels cater to different sets of companies and that companies that receive financing from one type of investors in a round are more likely to stick to that same type in future rounds of investment. Based on the later set of argument, we expect angels and VCs to act as substitutes (**H5b**).

3.3.3 Financing Sequence of Angel and VC Financing across Rounds and Probability of Successful Exit

In this subsection, we develop our hypothesis on the relationship between the financing sequence across investment rounds in firms and the likelihood of their future successful exit. In the setting of the multi-period theoretical model of Chemmanur and Chen (2014), VCs are able to add greater value to entrepreneurial firms, but VC financing is scarce (while angel financing is plentiful). Further, while initially (in earlier rounds) entrepreneurs have private information relative to the external financiers (VCs or angels), this information asymmetry disappears after the first financing round as the outside financier learns more about the firm during the interaction with the entrepreneur in earlier rounds (the entrepreneur's private information is about the nature of the firm and the ability of VCs or angels to add significant value to it). Finally, it is more efficient for the VC to start financing the firm in early rounds from a value-addition point of view (since the contracting between the entrepreneur and the VC is more efficient in the second and subsequent rounds in this case). Chemmanur and Chen (2014) predict the relationship between the financing sequences of start-ups and the probability of their future successful exit. First, firms that received VC funding in early stages followed by more VC funding in later stages (VC-VC) are of highest quality and are most likely to have a successful exit. Second, firms that received angel funding in early stages followed by VC funding in later stages (angel-VC) are of lower quality and are less likely to have a successful exit. Finally, firms that received angel investment in early rounds and continue to be angel financed in subsequent rounds (angel-angel) or firms that received VC investment in early rounds followed by angel investment in later rounds are of the lowest quality and are least likely to have a successful exit (**H6**). This is because in an environment with information asymmetry between the entrepreneur and outside investors regarding the quality of the start-up, early-stage VC investment in the start-up acts as a signal of the start-up's quality since VCs may have better abilities than angels in selecting higher quality firms to invest in. Similarly, if VCs continue to invest in a firm in subsequent rounds, this is an even better signal of a firm's quality. However, an exit of an early-stage VC investor from a start-up is a negative signal since the early-stage VC investor is likely to have negative information about the start-up firm.

3.4 Data

3.4.1 Data Sources and Sample Selection

We collate information on start-ups from multiple sources. The primary data source for our paper is Crunchbase, a leading open-source database collecting profiles of start-ups and information on their financing.[10] Specifically, we obtain the name, location, founding date, and the status of IPO or acquisition of firms and the names and types of investors as well as the total amount of investment for each round of transaction from CrunchBase. We supplement investor composition information from CrunchBase with data from VentureXpert, which provides information on the investment made by a given

[10]Many studies have used data from CrunchBase, some examples include Denes et al. (2020), Wang (2018), J. Xu (2019), and Yu (2020).

VC in an investment round at a firm. By merging our data on the total aggregate investment amount per round from CrunchBase with the investment made by an individual VC per round from VentureXpert, we calculate the percentage of the amount raised in a financing round from VC investors.

To measure the innovation output of entrepreneurial firms after receiving investment, we collect patent data from the United States Patent and Trademark Office (USPTO) dataset hosted on the website, PatentsView. The USPTO data on PatentsView provide detailed information on the application date, the technology classes, and citations of a patent as well as the name, unique identification number, and the location of assignees or firms filing the patents. The USPTO data also provides patent inventor information with a unique identifier. We obtain data on employment and sales for entrepreneurial firms from the National Establishment Time-Series (NETS), which is a longitudinal database provided by Dun & Bradstreet and is widely used in research on private firms.[11] We match firms in CrunchBase, VentureXpert, the USPTO database, and the NETS database based on firm name and location. Our final sample covers start-ups from 1991 to 2015.[12]

For our analysis comparing the value added to entrepreneurial firms by angel investors versus VCs and examining the impact of financing sequences on successful exits, we restrict our sample to firms that receive investments from either only VCs or only angel investors or both in their first investment round. For our analysis on complementarity versus substitutability of VCs and angels, we include firms financed by all categories of investors, which not only include VCs and angels, but also include other kinds of investors such as accelerators and government grants.

[11]See Neumark, Wall, and Zhang (2010) for a more detailed description of the NETS data set.

[12]We restrict our sample to first-round investment to 2015 so that we have around five years to observe their future potential IPO or acquisition. However, our results on successful exits (IPOs or acquisitions) are robust to restricting our sample to 2010 so that we have more time to observe future exits of start-ups.

3.4.2 Variable Construction

The primary independent variable in our paper is the investor composition, or the percentage of investment made in the first investment round in a start-up by VCs. After merging data on start-ups from CrunchBase with the startup-data on VentureXpert, we compute the fraction of VC investment of the total investment received by a firm in its first financing round (*1st-round_VC%*).[13] Since we restrict our sample to include firms either receiving only angel or only VC investment or both in most of our analysis, the fraction of angel investment would naturally be one minus the fraction of VC investment and we denote it by *1st-round_angel%*. Using data from CrunchBase, we also construct a dummy, *1st-round_has_angel*, which is equal to one if there is at least one angel investor investing in the round and is equal to zero otherwise. We also show trends of VC and angel investments in the first investment round of start-ups. We identify VC- and angel-dominated investment rounds based on the fraction of investment by the two categories of investors in a round. If round receives at least fifty percentage investment from angel investors, it is classified as an angel-dominated round, otherwise, it is classified as a VC-dominated round. We show in figure 3.1 the trends in angel-dominated first investment rounds compared to VC-dominated rounds.[14]

Following the existing literature, we construct standard measures of successful exits (IPO or acquisition) for entrepreneurial firms. Using data from CrunchBase, we construct three dummy variables, *IPO*, *Acq*, and *Exit*. *IPO* equals one if a firm has conducted an IPO after its first financing round and zero otherwise. *Acq* is equal to

[13] We focus on the first investment round since financing and value-addition at initial stages of start-ups are important for their future growth. This approach also enables us to distinguish between value added by angels versus that by VCs when they invest at the same investment round in a start-up. Further, the participation of different categories of investors in later rounds of investments may be driven by the original investors who participated in the first round of investment or fundraising for a start-up.

[14] Given that we restrict our sample to first round of investments where only VCs or angel investors are involved, our number of observations are smaller in the early 1990s. While plotting the trends on angel- and VC-dominated first rounds of investments, we restrict our sample to years where at least five start-ups received their first rounds of investments from only VCs or angel investors.

one if a firm has been acquired after the first financing round and zero otherwise. *Exit* takes a value of one if a firm either has been acquired or has gone public after the first round of financing and zero otherwise. We show trends of successful exits, IPOs, and acquisition for VC- and angel-dominated rounds in figures 3.1 and 3.2. We find that start-ups whose first investment rounds are dominated by VCs are associated with greater fraction of successful exits, IPOs, and acquisitions, compared to start-ups that have angel-dominated first investment rounds.

To evaluate how angels and VCs have different effects on the future sales and employment of entrepreneurial firms, we construct growth rates of sales and employment using our NETS data set. Specifically, we calculate the annual growth rate of sales in the first year after the first financing round (*Sales growth (Year 0 to 1)*), the growth rate of sales in the second year after the first round of investment (*Sales growth (Year 1 to 2)*), and the growth rate of sales in the third year after the first financing round (*Sales growth (Year 2 to 3)*). Similarly, we construct *Employment growth (Year 0 to 1)*, *Employment growth (Year 1 to 2)*, and *Employment growth (Year 2 to 3)* as the annual growth rate of employment in the first, second, and third year after the first financing round, respectively.

To compare angels and VCs on their impact on firms' innovation, we construct standard measures of the quantity and quality of patents generated in the years after the first round of financing. To measure the quantity of innovation, we construct the natural logarithm of one plus the total number of patents applied (and eventually granted) by a firm within one year after its first round of financing and denote the variable as *Patents (1 year)*. Similarly, we construct the natural logarithm of one plus the total number of patents applied (and eventually granted) within two and three years after its first round of financing as *Patents (2 years)* and *Patents (3 years)*, respectively. To measure the quality of innovation, we calculate the natural logarithm of one plus the total number of forward citations of the patents which were applied by a firm within one year after its first round of financing (*Citations (1 year)*), within two years after the first round of financing (*Citations (2 years)*), and in three years after the first round of financing (*Citations (3 years)*). Patents data are subject to truncation biases. First, there is a lag between when a patent is applied and when it is granted. Second, patents granted in earlier years are likely to have higher citations than patents granted in later years, on average. Following Seru (2014), we address this problem by dividing each patent of a firm in a filing year by the mean number of patents for all firms for that year having the same 3-digit technology class as the patent. We address truncation bias in citations by scaling the citations of a given patent by the total number of citations received by all patents filed in that year in the same 3-digit technology class as the patent (Seru (2014)).

We construct our inventor mobility measures following Chemmanur, Kong, Krishnan, and Yu (2019) and Marx, Strumsky, and Fleming (2009). For a given firm, an inventor's move-in year is the year when she filed her first patent in this firm (or when she files her first patent at the firm after moving our from a different firm); her move-out year is the year when she filed her first patent in a different firm. In case of the last patent filed by the inventor, we assume that she remains in the firm till the end of our sample period.[15] Once we identify each mobile inventor's move-in and move-out year, we aggregate the number of mobile inventors that move in and move out at the firm-year level to obtain the total inflows and outflows of mobile inventors for a given firm in a year. We then construct a set of variables (*Net Inflow of Inventors (1 Year)*, *Net Inflow of Inventors (2 Years)*, and *Net Inflow of Inventors (3 Years)*), defined as the difference between the natural logarithm of one plus the inflow and the natural logarithm of one plus the outflow of inventors within the subsequent one, two, and three years, respectively, after an entrepreneurial firm received its first round of financing. To further examine the innovative ability of inventors, we look at a specific set of top-quality inven-

[15]Inventors that have only filed one patent are excluded from our sample as we can only identify the inventor flow based on at least two patent filings.

tors who filed patents with a higher number of citations. We define top-quality inventors as those with average citations per patent for all the patents he has filed prior to the current year above the sample's top quartile (top 25 %) of citations in the year. Similarly, we construct the net inflows of the top-quality inventors for each entrepreneurial firm within one, two, and three years after they received their first-round of financing (*Net Inflow of Top 25% Inventors (1 Year)*, *Net Inflow of Top 25% Inventors (2 Years)*, and *Net Inflow of Top 25% Inventors (3 Years)*).

3.4.3 Summary Statistics

Table 3.1 shows the summary statistics of our sample. To alleviate the concern that outliers may drive our results, we winsorize all variables at the 1st and 99th percentiles in the regressions.

Our final sample contains 5,583 firms with their first round of investor composition information and future firm performances. We show in Table 3.1 that for our sample of firms receiving only angel or VC financing or both, the average fraction of angel investment is 30 percent of the total investment in the first round of financing (or 70 percent for VC investment). On average, 26 percent of the sample start-up firms have at least one angel investor participating in the first financing round. The IPO rate of firms in our sample is 6 percent and the rate of being acquired by other firms in our sample is 38 percent.

3.5 Angel versus VC and Entrepreneurial Firms' Future Performances: Baseline Analyses

In this section, we examine how investor composition for entrepreneurial firms in terms of angel versus VC is associated with their future performances, using ordinary least squares (OLS) analyses for our baseline analyses. Specifically, we analyze the impact of the fraction of angel investment on successful exits (i.e., IPO or acquisition), sales growth, and employment growth, the quantity and quality of innovation output, and the net inflow of inventors estimating the following the model:

$$Y_{i,t+X} = \alpha + \beta 1st - round\_angel\%_{i,t} + Controls_{i,t} + Year_t + Industry_i + \epsilon_{i,t+X}, \quad (3.1)$$

where i represents a firm and t is the year of the first round of financing. $Y_{i,t+X}$ is a set of dependent variables related to the future performance of entrepreneurial firms after receiving their first financing round, which are described above. The key variable of interest is *1st-round_angel%*, which equals the fraction of angel investment in the total amount received in the financing round. A financing round is fully financed by angel investors if *1st-round_angel%* takes the value of one and is fully financed by VCs if *1st-round_angel%* equals zero. We control for the natural logarithm of one plus the age of the firm when receiving the investment (*lnage*) and the natural logarithm of one plus the amount of sales in the year (*lnsales*). We include a set of dummies each representing a two-digit SIC code ($Industry_i$) to account for unobservable industry-specific characteristics. We add investment year fixed effects ($Year_t$) to control for time-specific shocks that may affect our analysis. In all regressions, we cluster standard errors at the two-digit SIC code level.

3.5.1 Successful Exits

We first examine how the composition of angels and VCs affects successful exits of entrepreneurial firms. A successful exit for investors is defined as either having an IPO or being acquired by other firms.

Table 3.2 reports the results. In Column (1), the dependent variable is *IPO*, which takes the value of one if a firm becomes public after the first financing round and zero otherwise. The coefficient estimate on *1st-round_angel%* is negative and statistically significant at the 5 percent significance level. The magnitude suggests that a one standard deviation increase in the fraction of angel investment (relative to VC investment) in the first financing round is associated with a 0.6 percentage point decrease in the probability of a firm conducting an IPO in the future. This is equivalent to a decrease of 11.1 percentage in the average probability of an IPO. In Column (2), we replace the dependent variable with *Acq*, which equals one if a firm has been acquired after the first financing round and zero otherwise. The coefficient estimate on *1st-round_angel%* in Column (2) is also negative and statistically significant at the 1 percent level. The magnitude suggests that a one standard deviation increase in the fraction of angel investment (relative to VC investment) in the first financing round is associated with a 7.2 percentage point decrease in the probability of a firm getting acquired in the future, which is equivalent to a decrease of 18.8 percentage in the average probability of getting acquired. In Column (3), we use *Exit* as the dependent variable, which equals one if a firm has either been acquired or has conducted an IPO after the first financing round, and zero otherwise. The coefficient estimate on *1st-round_angel%* is both negative and statistically significant at the 1 percent level. A one standard deviation increase in the fraction of angel investment (relative to VC investment) in the first financing round is associated with a decrease of 17.7 percentage in the average probability of a successful exit.

The above results suggest that firms receiving more angel investment relative to VC investment in their first financing round are associated with a smaller probability of successful exits in the future, which supports our hypothesis **H1**.

3.5.2 Sales Growth and Employment Growth

Next, we examine how the composition of angels and VCs affects the sales growth and employment growth of entrepreneurial firms. We calculate sales growth and employment growth in years after the first round of financing using data from the NETS database.

Table 3.3 presents the results. In Column (1), we use *Sales Growth (Year 0 to 1)* as the dependent variable, which is defined as the growth rate of sales for a firm one year after the investment. We find that the coefficient estimate on 1st-round_angel% is negative and significant at the 1 percent level. Further, a one standard deviation increase in the fraction of angel investment in the first financing round is associated with a 9.3 percentage point lower growth rate of sales in the next year after receiving investment. This is equivalent to a decrease of 25.6 percentage in the average sales growth in the next year after the first round of investment. We replace the dependent variable with the growth rate of sales in the second year after the investment (*Sales Growth (Year 1 to 2)*) and the growth rate of sales in the third year after the investment (*Sales Growth (Year 2 to 3)*) in Columns (2) and (3), respectively. The coefficient estimates on *1st-round_angel%* in these two columns are both negative and significant at the 1 percent level. The above coefficients are also economically significant. The dependent variable in Column (4) is the growth rate of employment one year after receiving the first round of investment (*Employment Growth (Year 0 to 1)*). The coefficient estimate in Column (4) is negative and significant at the 1 percent significance level. The magnitude of the coefficient estimate shows that a one standard deviation increase in the fraction of angel investment in the first financing round is associated with an 8.8 percentage point smaller annual growth rate of employment one year after the investment, which is equivalent to a decrease of 29.2 percentage in the average employment growth. In Column (5) and (6), the dependent variables are replaced with the annual employment growth rates in the second year and the third year after the investment (*Employment Growth (Year 1 to 2)* and *Employment Growth (Year 2 to 3*), respectively. The coefficient estimates are both negative and statistically significant at 1%.

The results shown above suggest that firms receiving a greater fraction of angel investment compared to VC investment in their first round of financing are associated with a lower growth rate of sales and employment, which supports our hypotheses **H2**.

3.5.3 Innovation and Human Capital

Next, we evaluate how the composition of angel investors and VCs in entrepreneurial firms' first financing round affects their future innovation activity and talent inflows. We use the number of patents applied (and eventually granted) after the financing and the number of citations on these patents to measure the quantity and quality of innovation output. We use the number of net inflows of inventors to measure the high-quality talent.

We show the results on the quantity of innovation output (i.e., the number of patents) in Table 3.4. In Columns (1)-(3), the dependent variables are defined as the number of patents applied (and eventually granted) within the next one, two, and three years after receiving the first financing round (*Patents (1 year)*, *Patents (2 years)*, and *Patents (3 years)*), respectively. The number of patents has been adjusted for truncation bias due to the lag between patent application and patent grant following Seru (2014). The coefficient estimates on *1st-round_angel%* are all negative and statistically significant at the 5 percent or the 1 percent levels in the above three columns. The magnitude of estimates indicates that the effect is also economically significant: a one standard deviation increase in the fraction of angel investment in entrepreneurial firms' first financing round is associated with a 13.2 percentage point decrease in the number of patents applied (and eventually granted) within the next three years after receiving investment, which is equivalent to a decrease of 24.8 percentage in the average of patents applied (and eventually granted) within the next three years. We also report the results on the quality of innovation output (i.e., the number of patent citations). In Column (4), (5), and (6), the dependent variables are *Citations (1 year)*, *Citations (2 years)*, and *Citations (3 years)*, respectively, which represent the number of total citations received by patents applied (and eventually granted) within the next one year, two years, and three years, respectively, after a firm receives its first financing round. The coefficient estimate on *1st-round_angel%* are negative and statistically significant at the 1 percent significance level.[16] The magnitude of the coefficient estimate suggests that a one standard deviation increase in the fraction of angel investment in entrepreneurial firms' first round of financing is associated with a decrease of 0.3 percentage point in the number of citations on patents applied (and eventually granted) within three years after receiving the investment, which is equivalent to a decrease of 30.5 percentage in the average of citations on patents applied (and eventually granted) within the next three years. The number of citations is also adjusted for potential truncation bias, since it takes years to receive citations after the patent application and grant.

We test our hypothesis related to attracting talents to entrepreneurial firms in Table 3.5. The outcome variables we test in Columns (1)-(3) are the net inflows of inventors in one, two, and three years after a start-up's first round of financing (*Net Inflow of Inventors (1 Year)*, *Net Inflow of Inventors (2 Years)*, and *Net Inflow of Inventors (3 Years)*), respectively. We observe that the coefficient estimates on *1st-round_angel%* are all negative and statistically significant at the 1 percent level in three columns, suggesting a higher fraction of angel investment (instead of VC investment) in entrepreneurial firms' first round of financing is associated with a smaller net inflow of inventors in the subsequent years. The results are also economically significant. A one standard deviation increase in the fraction of angel investment in entrepreneurial firms' first round of financing is associated with a decrease of 4.5 percentage points in the net inflow of inventors within the next three years, which is equivalent to a decrease of 25 percentage points in the average net inflow of inventors within the next three years. In Columns (4)-(6), we look at the net inflows of top-quality inventors with the top-quartile number of citations per patent within one, two, and three years after a start-up's first round of financing (*Net Inflow of Top 25% Inventors (1 Year)*, *Net Inflow of Top 25% Inventors (2 Years)*, and *Net Inflow of Top 25% Inventors (3 Years)*). The coefficient estimates on *1st-round_angel%* are all negative and statistically

[16] Our results are also robust to using Poisson regressions with the count of class-adjusted patents and citations as our dependent variables.

significant at the 1 percent level. A one standard deviation increase in the fraction of angel investment in entrepreneurial firms' first round of financing is associated with a decrease of 1.3 percentage point in the net inflow of top inventors within the next three years, which is equivalent to a decrease of 33.9 percentage in the average net inflow of top inventors within the next three years. The results show that start-ups with more angel investment than VC investment are less likely to attract inventors (top-quality inventors).

The above findings suggest that firms receiving relatively more angel investment than VC investment in their first round of financing are associated with smaller quantity and quality of innovation and fewer talent inflows, which supports our hypotheses **H3** and **H4**.

3.6 Are the Differences between Angels and VCs Driven by Screening or Monitoring?

3.6.1 IV Analysis

Our baseline analyses may suffer from common endogeneity concerns in entrepreneurial finance literature: selection versus value-addition. In other words, do VCs have a better ability to select start-ups or better monitoring abilities compared to angel investors, or do both factors play important roles? To disentangle the selection versus value-addition effect, we use the instrumental variable (IV) approach and construct two IVs for our key variable of interest, the fraction of angel investment in the first round of financing for entrepreneurial firms (*1st-round_angel%*). The first IV we construct is ATC to represent the shock affecting the regional supply of angel investor capital. ATC is a dummy that equals one if a firm is located in a state that has an active angel tax credit program. Denes et al. (2020) find that the staggered provision of angel investor tax credits in 31 U.S. states significantly increased the number of angel investments and average investment size, which suggests that the IV is relevant. Of course, our first stage results in our two stage least squares (2SLS) regressions provide direct evidence of the relevance of our instrument. In Denes et al. (2020), they also show that state-level economic, political, fiscal, and entrepreneurial activity factors do not predict the implementation of angel investor tax credits. Therefore, the provision of angel tax credits across states may affect firms' performances only through the following channel: an increase in the likelihood of receiving greater amount of angel investment. Thus, the above IV is likely to satisfy the exclusion restriction.

The second IV we construct is LPR, which represents the portfolio returns of VC limited partners as suggested by Samila and Sorenson (2011). The rationale behind using this IV is as follows. First, the returns of the limited partners will only affect the supply of VC funds, but will not affect the supply of funding from angel investors. This is based on the stylized fact that limited partners of VCs are typically institutional investors who adopt an investment strategy allocating a fixed ratio of funds into alternative assets (such as VCs and PEs). When the limited partners earn higher returns in their portfolios, they must invest more assets to venture capital to maintain their asset allocations. Angel investors, on the other hand, are wealthy individual investors who do not receive money from institutional investors and thus, would not be affected by changes in the returns of the limited partners. Our first stage results in our two stage least squares (2SLS) regressions provide direct evidence of the relevance of our instrument. Second, the intuition behind this IV also relies on another stylized fact that limited partners have a home bias to invest in locally headquartered VC funds, while VCs too have a tendency to invest in start-ups located closer to their headquarters (Chemmanur, Krishnan, and Yu (2016); Samila and Sorenson (2011)). Collectively, the above stylized facts suggest that higher portfolio returns earned by limited partners are likely to lead to greater VC investments in nearby start-ups in the subsequent years. The returns of the limited partners are not likely to be driven by local entrepreneurial activity and are only correlated to the supply of VC funds. Thus, our second instrument is also likely to satisfy the exclusion restriction. The construction of the IV is as follows,

$$LPR_{it} = \sum_{j} \sum_{s=t-1}^{t-3} \frac{ER_s lnLP_{js}}{1 + dist_{ij}}, \tag{3.2}$$

where i is the state of the start-up located in and t is a year. ER_s is the average return across all college endowments in year s, obtained from the study of the National Association Of College and University Business Officers. LP_{js} is equal to one plus the number of limited partners in a state j that had invested in venture capital at least ten years before year s. $dist_{ij}$ is the distance in miles between the centroid of state i and the centroid of state j. We use the returns weighted by the distances to account for the home bias of limited partners that they intend to invest in VC funds that locate near them.

We instrument the fraction of angel investment in the first round of financing *1st-round_angel%* with the provision of angel tax credits and the average past returns of the limited partners. Thus, we can distinguish the effect driven by the differences between angels and VCs in their respective monitoring abilities from the effect driven by their differences in selection ability or their ability to select firms. Specifically, we run the following first stage regression:

$$\mathit{1st\text{-}round\_angel\%}_{i,t} = \alpha + \beta_1 LPR_{s,t} + \beta_2 ATC_{s,t} + \gamma_1 lnage_{i,t} + \gamma_2 lnsales_{i,t} + Year_t + Industry_i + \epsilon_{i,t}, \tag{3.3}$$

and the second-stage as

$$Y_{i,t+X} = \alpha + \beta \, \widehat{\mathit{1st\text{-}round\_angel\%}}_{i,t} + \gamma_1 lnage_{i,t} + \gamma_2 lnsales_{i,t} + Year_t + Industry_i + \delta_{i,t+X}, \tag{3.4}$$

where i represents a firm, s is the state that a firm's headquarter is located in, and t is the year that the firm receives its first round of financing. Other variables are defined in the same manner as in our baseline regressions.

IV Analysis: Successful Exit

First, we show the results of our IV analysis of the impact of investor composition on the successful exits of entrepreneurial firms.

Table 3.6 shows the result. In the first stage of the analysis, we instrument the fraction of angel investment in the first round of financing (*1st-round_angel%*) using the provision of angel tax credits (ATC) and the weighted returns of limited partners (LPR). In the first stage, we find that, as expected, the coefficient on the past return of limited partners in firm-headquarter state is negative and significant (1 percent level), which is consistent with Samila and Sorenson (2011). The coefficient on the dummy variable for the state angel tax credit program is positive and significant (1 percent level), suggesting that when angel investment is encouraged by the government, a start-up is more likely to receive angel financing. The Kleibergen-Paap rk Wald statistic (Kleibergen and Paap (2006)), which tests directly whether the IV predicts a sufficient amount of the variance in the endogenous variables to identify our equations, has a value of 27.297 and is far beyond the critical value required by Stock and Yogo (2005) for the IV estimates to have no more than 10% of the bias of the OLS estimates. Thus, our instruments satisfy the relevance condition. In the second stage of the analysis, we regress the variables that represent successful exits on the predicted value of *1st-round_angel%* from the first stage. Column (1) shows the first-stage results. Columns (2) to (4) report the second-stage results of the IV analysis. The coefficient estimates are both negative and statistically significant at 5 percent or the 1 percent levels, suggesting a causal impact of having more angel investment relative to VC investment on the successful exits of entrepreneurial firms. In other words, the difference in value-adding abilities between angel and VC investors causally affects the probability of their portfolio firms' likelihood of getting a successful exit. Thus, the above results suggest that firms greater level of angel investment compared to VC investment causally leads to a smaller likelihood of successful exit in the future, which supports our hypothesis **H1**.

IV Analysis: Sales and Employment Growth

Second, we show the results of our IV analysis of the impact of investor composition on the sales and employment growth at start-ups.

We show our results in Table 3.7. Again, the first-stage regression exhibits a significantly positive estimate on ATC and a significantly negative coefficient estimate on LPR with the Kleibergen-Paap rk Wald statistic of 9.064. In the second-stage analysis, we observe from Column (4) that the coefficient estimate is negative and statistically significant at the 1 percent level, suggesting that having more angel investment relative to VC investment in the first round of financing is causally related with a lower sales growth rate in the third year after receiving the investment. We observe similar second-stage results for employment growth, indicating that having more angel investment relative to VC investment in the first round of financing is causally associated with smaller employment growth. We observe from Column (7) that the coefficient estimate is negative and statistically significant at the 1 percent level.[17]

The above IV analyses suggest that more angel investment (rather than VC investment) in entrepreneurial firms is causally associated with a lower level of sales and employment growth in the subsequent years, which supports our hypothesis **H2**.

IV Analysis: Innovation Output

Third, we show the results of our IV analysis of the impact of investor composition on the innovation output, measured using the quantity and quality of patents of entrepreneurial firms.

Table 3.8 reports the results. Similar to Table 3.6, we report the first-stage results

[17] We do not observe the significance of the second-stage coefficient estimates on *1st-round_angel%* for the sales growth rate and employment growth rate of start-ups in the first year and the second year after receiving the financing. The potential explanation for the above results is that it takes time for investors to engage in the business of start-ups and turn their monitoring and expertise into real economic improvements of these firms. For example, it is possible that investors' efforts to promote innovation in start-ups and in attracting better talents to start-ups may lead to real economic benefits after a few years.

in Column (1) and observe a significantly positive coefficient estimate on ATC and a significantly negative estimate on LPR with the Kleibergen-Paap rk Wald statistic of 27.297. In Columns (2) to (4), we show the second-stage results of the effects of *1st-round_angel%* on the number of patents applied, which were eventually granted, in one, two, and three years after a firm receives the first round of financing (*Patents (1 year)*, *Patents (2 years)*, and *Patents (3 years)*). We observe that all of the coefficient estimates on the predicted value of *1st-round_angel%* are negative and statistically significant at the 5 percent significance level or at the 1 percent significance level. In Columns (5) to (7), we use the number of citations on patents applied (and eventually granted) in one, two, and three years after receiving the first round of financing (*Citations (1 year)*, *Citations (2 years)*, and *Citations (3 years)*) as our dependent variable. All of the coefficient estimates on *1st-round_angel%* are negative in these three columns and statistically significant at the 5 percent significance level or at the 1 percent significance level. Our results in Table 3.8 suggest a causal impact of the composition of angel and VC investors on entrepreneurial firms' innovation output.

The above IV analyses suggest that more angel investment (rather than VC investment) in entrepreneurial firms is causally associated with a lower level of innovation output (quantity and quality) in the subsequent years, which supports our hypothesis **H3**.

IV Analysis: Attracting Talents

Fourth, we show the results of our IV analysis of the impact of investor composition on attracting talents, measured using net inflows of inventors to start-up firms.

Table 3.9 reports the results. In Column (1), we report the first-stage results and observe a significantly positive coefficient estimate on ATC and a significantly negative estimate on LPR with the Kleibergen-Paap rk Wald statistic of 27.297. In Columns (2) to (4), we show the second-stage results of the effects of *1st-round_angel%* on the net inflows of inventors in one, two, and three years after a firm receives the first round of financing (*Net Inflow of Inventors (1 Year)*, *Net Inflow of Inventors (2 Years)*, and *Net Inflow of Inventors (3 Years)*). We observe that all of the coefficient estimates on the predicted value of *1st-round_angel%* are negative and statistically significant at the 1 percent significance level. In Columns (5) to (7), we focus on the net inflows of inventors with the top-quartile number of citations per patent in one, two, and three years after receiving the first round of financing (*Net Inflow of Top 25% Inventors (1 Year)*, *Net Inflow of Top 25% Inventors (2 Years)*, and *Net Inflow of Top 25% Inventors (3 Years)*) as our dependent variable. All of the coefficient estimates on *1st-round_angel%* are negative. In Column (4) the coefficient is statistically significant at the 10 percent level. The findings from Table 3.9 confirms our hypothesis **H4** that there is a causal impact of the composition of angel and VC investors on attracting talents for entrepreneurial firms

Overall, all of the above IV results suggest that the difference in the ability of angel and VC investors to add value to start-ups (due to differences in their respective ability to monitor or to ensure better resources for start-ups) at least partially drives the variation in their portfolio firms' performances.

3.6.2 Switching Regressions

In this section, we provide further empirical evidence to show the differences in terms of value added by angels versus VCs on entrepreneurial firms. To isolate the effect driven by the differences in the value-adding ability, we employ the following "what-if" analysis framework: what would be the outcome of start-ups that are initially angel-financed if they had not received any angel financing, or in other words, received financing only from VC funds (*VC-only*). Similarly, we test the outcome of firms that are initially financed only by VC funds if they had received financing from angel investors instead.

We run switching regressions with endogenous switching methodology (as discussed in detail in Heckman (1979) and Maddala (1983)) to disentangle the different impact of selection versus value-addition by angels and VCs on successful exits, sales, and employment growth of start-ups, innovation output, and on the net inflow of inventors. The above method, which is a generalized version of the traditional Heckman model, accounts for the impact of unobservables (which determines the selection effect) by using inverse Mills ratios. The inverse Mills ratios for angel-backed and non-angel-backed firms are obtained by running the first-stage regression to predict the probabilities of receiving angel funding. Next, we regress successful exits, innovation output, net inflow of inventors, and sales and employment growth on the inverse Mills ratios and control variables in the second stage of the estimation, separately for the sample of VC-only and angel-backed firms. Finally, the predicted values of the outcome variables from the second-stage estimates are used to conduct the above-mentioned counterfactual (i.e., "what-if") analysis. This method has been used in many finance studies, e.g., Fang (2005) uses switching regression to analyze the relationship between investment bank reputation and bond underwriting, while Chemmanur et al. (2011) study the impact of venture capitalists on the efficiency of private firms using switching regressions.

We control for firm-level characteristics that may affect investor-firm matching, such as the natural logarithm of firm age (*lnage*), sales of the firm (*lnsales*), and the 2-digit SIC industry dummy. We also control for the year of the first financing round for the firm. In addition, we also include the two instruments, which are described in Section 3.6.1, the past return of limited partners in the state where the firm's headquarter is located (LPR), and the dummy variable that represents whether the state has an active angel tax credit program or not (ATC). We include these instruments since they provide us with exogenous variation regarding the supply of VC funding and angel investment that affects investors' selection of firms, but does not directly affect firm outcomes.

In the following tables, we report the results of the switching regression analysis. In the first stage, the dependent variable is a dummy which equals one if a firm has angel backing in the first round of financing, otherwise it is equal to zero. The results are reported in Table C1 in the Internet Appendix. We find that the age and sales of a firm are both negative and statistically significant at the 1 percent level, suggesting that younger and smaller firms are more likely to receive angel financing (instead of VC funding). In terms of the instruments, we find that the coefficient on the past return of limited partners in firm-headquarter state is negative and significant (5 percent level), which is consistent with Samila and Sorenson (2011). The coefficient on the dummy variable for the state angel tax credit program is positive and significant (1 percent level), suggesting that when angel investment is encouraged by the government, a firm is more likely to receive angel financing. Next, we use the inverse Mills ratio calculated from the first stage to augment the second-stage regressions for our samples of VC-only firms and angel-backed firms to account for endogenous selection based on unobservable factors.

Tables 3.10 report the results when the outcome variables are related to successful exits. Panel A of Table 3.10 presents the results of the second-stage regressions. The inverse Mills ratio is statistically significant for VC-only firms at the 1 percent level in all three columns (while it is only marginally significant for angel-backed firms in Column (2) and insignificant in Columns (4) and (6)), suggesting that venture capitalists may have used more unobservable factors when they select which firms to invest relative to angel investors, and these unobservable factors have bigger impact on future successful exits through selection. Panel B of Table 3.10 shows the results of our counterfactual analysis of VC-only versus angel-backed firms. We obtain the counterfactual values for VC-only firms as the predicted values of the angel-backed regression and the corresponding inverse Mills ratio using data from VC-only firms, and vice versa. In the first part of the Panel B, we observe that angel-backed firms could have achieved a hypothetical improvement in the rate of IPO, acquisition, and successful exit by 0.9, 9.7, and 10.3 percentage points, respectively, compared to the hypothetical case had the same firms received only VC-financing. In the second part of the Panel B, we show that VC-only firms face smaller probability of either an IPO, or an acquisition, or a successful exit (IPO or acquisition) by 1.6, 14.1, and 14 percentage points, respectively, had they received angel financing. The estimates of these changes in the rate of successful exit are all statistically significant. Thus, our switching regression results show that VC-backing rather than angel-backing leads to greater likelihood of a successful exit for start-ups, supporting our hypothesis **H1**.

Tables 3.11 report the results when the outcome variables are related to growths of sales and employment in years after the financing. Panels A and B show the results of second-stage of switching regressions when the dependent variables are growths of sales and employment in the future years, respectively, after the first round of financing. Most of the coefficient estimates of the Inverse Mills ratio are statistically insignificant for both VC-only and angel-backed firms. Most of the coefficient estimates of firm sales are significantly negative, suggesting that larger firms have smaller future growths in both the samples of VC-only and angel-backed firms. In Panel C, we show the results of the counterfactual analysis for firms' sales and employment growth. The first part of Panel C shows that angel-backed firms could have achieved a higher value of both sales and employment growth in the first, second, and third year after the first round of financing compared to the hypothetical case had the same firms received only VC-financing. Our results are economically significant. For example, angel-backed firms experience higher sales growth by 24.7 percentage points compared to the hypothetical scenario of backing only from angel investors for one year after receiving the first round of financing. In the second part of the Panel C, we show that VC-only firms would experience a hypothetical drop in both sales and employment growth in the first, second, and third year after the first round of financing, had they received angel financing. Most estimates of these changes in the rate of sales and employment growth are statistically significant. Thus, our switching regression results show that VC-backing rather than angel-backing leads to a greater level of sales and employment growth for start-ups, supporting our hypothesis **H2**.

Tables 3.12 report the results when the outcome variables are related to innovation. Panels A and B show the results of second-stage switching regressions when the dependent variables are the number of patents and the number of patent citations in the future years, respectively, after the first round of financing. The coefficient of inverse Mills ratio is statistically significant for VC-only firms but insignificant for VC-backed firms, again suggesting that VCs may use some unobservable factors when selecting firms to invest in, and these factors may affect the quantity and quality of innovation output of entrepreneurial firms positively. In Panel C, we show the results of counterfactual analysis for firms' innovation output. The first part of Panel C shows that angel-backed firms could have experienced an increase in patents filed (and eventually granted) in the first, second, and third year after the first round of financing by 10.6, 29.9, and 34.8 percentage points, respectively, compared to the hypothetical case had the same firms received only VC-financing. We find similar results for the quality of patents filed by firms in our sample. In the second part of Panel C, we show that VC-only firms experience a hypothetical decrease in the rate of filing patents in the first, second, and third year after the first round of financing by 3.1, 16.9, and 12.5 percentage points, respectively, had they received angel financing. We find similar results for the quality of patents filed by firms in our sample. The estimates of these changes in the rate of filing quantity and quality of patents are all statistically significant. Thus, our switching regression results show that VC-backing rather than angel-backing leads to a greater quantity and quality of innovation output for start-ups, supporting our hypothesis **H3**.

Tables 3.13 report the results when we examine talent inflows. Panels A and B show the results of second-stage switching regressions when the dependent variables are the net inflows of inventors (all inventors and the top-quality inventors) in the future years after the first round of financing. In Panel C, we show the results of counterfactual analysis for firms' inventor inflows. The first part of Panel C shows that angel-backed firms could have experienced more net inflows of inventors in the subsequent years compared to the hypothetical case had the same firms received only VC-financing. We find similar results for top-quality inventors in our sample. In the second part of Panel C, we show that VC-only firms experience a hypothetical decrease in net inflows of inventors, had they received angel financing. We find similar results for top-quality inventors in our sample. The estimates of these changes in net inflows of inventors are all statistically and economically significant. Thus, our switching regression results show that VC-backing rather than angel-backing leads to significantly more inflows of talents to start-ups, supporting our hypothesis **H4**.

Overall, the above results show the impact of differences in the value-adding abilities (i.e., monitoring) of angel investors and VCs for entrepreneurial firms in our sample. Accounting for the endogenous selection of investors using switching regressions, we find that angel investors add less value, in terms of the rate of successful exits, the growths of sales and employment, the quantity and quality of innovation, and the inflows of talents, to their portfolio firms compared to VCs.

3.7 Robustness Tests

3.7.1 VCs vs Sophisticated Angel Investors

In this section, we discuss some robustness tests which support our main analysis comparing the impact of VC and angel investment on future outcomes of start-ups firms. One may argue that our results showing that angel investors add less value to start-ups than VCs are driven by unsophisticated angel investors, who provide funds to their friends and families irrespective of the quality of the underlying start-up and who may not have decent ability to monitor these start-ups. To address this concern, we restrict our sample to first-round angel investments that include at least one angel group or one "serial" angel investor. We define a serial angel investor in a firm-investment round as an angel investor that has invested in at least one different firm in the past, i.e., the angel investor has prior experience of investing. It is likely that serial angel investors and angel groups are sophisticated investors and contribute to the growth and success of start-ups. Indeed, prior literature has argued that angel groups add positive value to portfolio start-ups (W. R. Kerr et al. (2014) and Lerner et al. (2018)). Thus, our subsample enables us to compare VC investments with angel investments where at least one angel group or one serial investor is involved.

Tables C2, C3, C4, and C5 in the Internet Appendix report the results on successful exit, growth, innovation, and inventor inflow for the above subsample. We show that, in line with our main results, sophisticated angel investors are less likely to add value to start-ups compared to VCs. Further, we conduct additional analysis restricting our sample to firm-investment rounds involving either VCs or angel groups only. This subsample enables us to directly compare the impact of VCs vs angel groups on the future success of start-ups. We also show in table C6 in the Internet Appendix that greater fraction of angel group investment in the first investment round of a start-up is associated with smaller likelihood of the future successful exit of the start-up. Thus, our results suggest that angel groups are also inferior to VCs in ensuring successful exits of start-ups.[18]

3.7.2 Industry-wise Subsample Analysis

In this section, we discuss subsample analysis based on different industry categories. One potential concern is that VCs and angels invest in separate industries because of their different specializations. Prior literature has argued that VCs tend to invest in high-tech and biotechnology industry sectors (Graham, Merges, Samuelson, and Sichelman (2009)). In order to test whether angels add less value than VCs only in certain industries, we conducted an additional subsample analysis based on industry categories.

[18]In untabulated analyses, we conducted additional tests directly comparing the impact of angel groups vs VCs on sales and employment growth, innovation, and inventor inflow. We find similar results compared to the above robustness tests. Thus, our analyses using different outcome variables show that even angel groups add less value than VCs to start-ups.

We identified high technology (HiTech), manufacturing, and healthcare as three industries categories where VCs may dominate compared to angel investors. We define the above industry categories based on Fama-French 10 industry classification. One subsample consists of firms in the above industry categories. The other subsample consists of firms in remaining industry categories. We find that angel investors add less value than VCs in both subsamples. In table C7 in the Internet Appendix, we show that greater fraction of angel investment leads to smaller likelihood of successful exits for start-ups irrespective of their industry categories.[19] Thus, we show that angel investors add less value than VCs in general across industries.

3.8 Angels and VC Financing: Complements or Substitutes?

Next, we examine the question of whether angel investors and VCs are complements (**H5a**) or substitutes (**H5b**). Specifically, we look at how the participation of angel and VC investors in the first round of financing affects the participation of VC and angel investors in the second round. To perform this analysis, we use a sample that also includes other types of investors (accelerator and government grants) to analyze the complementarity and substitutability between angel investors and VCs. In addition, we restrict our sample to firms that have experienced at least two rounds of investment. We run regressions based on the following model,

$$\begin{aligned} \textit{2nd-round\_VC\%(has\_angel)}_i = \alpha + \beta_1 \textit{1st-round\_VC\%}_i + \beta_2 \textit{1st-round\_has\_angel}_i + \\ \gamma_1 lnage_i + \gamma_2 lnsales_i + Year_i + Industry_i + \delta_i, \quad (3.5) \end{aligned}$$

where the dependent variable is either the second-round fraction of VC investment (*2nd-round_VC%*) or a dummy variable indicating presence of angel investor (*2nd-round_has_angel*) in the second round or not. The key variables of interests are the

[19] We also conducted our above subsample analyses for cases where outcomes were sales and employment growth, innovation, and inventor inflow. In untabulated analyses, we find that angel investors add less value than VCs in term of the above parameters across different industries.

first-round fraction of VC investment (*1st-round_VC%*) or the dummy variable for presence of angels (*1st-round_has_angel*). We use the fraction of VC investment and the dummy variable for presence of angels in this analysis, since we have included other types of investors (besides angels or VCs), and we only have the information on the amount of VC investment per round and the total amount of investment per round. We control for firm age (*lnage*) and firm sales (*lns*) in the year of receiving the first financing round. We also control for year fixed effects and industry fixed effects.

Table 3.14 reports the results. The dependent variable in Column (1) is *2nd-round_VC%*. The coefficient estimate on *1st-round_VC%* is positive and significant at the 1 percent level, which suggests that the fraction of VC investment in the first round is highly correlated with the fraction of VC investment in the second round. The coefficient estimate on the dummy *2nd-round_has_angel*, which equals one if there is at least one angel investor participating in the first round and zero otherwise, is also positive and significant at the 1 percent level. This result suggests that the presence of angel investors in the first round is associated with a larger percentage of VC financing in the second round. This result is different from Hellmann et al. (2021b), who use data on firms in British Columbia, Canada, and find that angels and VCs are substitutes. In contrast, the results from our analysis are consistent with the theoretical paper by Chemmanur and Chen (2014), who argue that angel investors are complements of VCs (**H5a**). Next, in Column (2), we replace the dependent variable with the dummy variable representing the presence of angel investors in the second round of financing, *2nd-round_has_angel*. The coefficient estimate on *1st-round_VC%* is negative and significant at the 1 percent level, which suggests that having more VC investment in the first round is associated with a lower probability of having an angel investor the second round. The coefficient estimate on *1st-round_has_angel*, on the other hand, is positive and statistically significant at the 1 percent level, indicating that the presence of angel investors in the first round is associated with a higher probability of the presence of angel investors in the second round. This particular result suggests that angels and VCs may act as substitutes (**H5b**).

Overall, the above analyses suggest that angels and VCs cannot be classified either as complements or substitutes in the financing of entrepreneurial firms. The relationship between angel investors and VCs is complex: they may act as either complements or substitutes. The presence of angel investors in the first round is associated with greater VC-financing in the second round. However, greater VC-financing in the first round is associated with a smaller likelihood of the presence of an angel investor in the second round.

3.9 Financing Sequence of Angel and VC Financing across Rounds and Probability of Successful Exit

Next, we examine the relationship between the financing sequence of investors at start-ups in terms of the order of financing by angels and VCs, and start-up firms' subsequent successful exits either via IPOs or acquisitions (**H6**), which is one of the most important success parameters for start-ups.

In this analysis, we only include firms that either have VC investors or angel investors or both in their first two rounds of financing. Hence, we can define the dominance of an investor type (angel or VC) in a financing round by measuring whether the percentage of VC investment in a round is greater than 50 percent or not. The firms in our sample can, therefore, be categorized into four subgroups based on their financing sequence in the first two rounds: from angel-dominated to VC-dominated (*FinPath=Angel to VC*), from VC-dominated to angel-dominated (*FinPath=VC to Angel*), from VC-dominated to VC-dominated (*FinPath=VC to VC*), and from angel-dominated to angel-dominated (*FinPath=Angel to Angel*). Specifically, we run regressions based on the following model,

$$Exit_{i,T} = \alpha + \beta_1 FinPath\ (Angel\ to\ VC) + \beta_2 FinPath\ (VC\ to\ Angel) + \beta_3 FinPath\ (VC\ to\ VC) \\ + \gamma_1 lnage_{i,t} + \gamma_2 lnsales_{i,t} + Year_t + Industry_i + \delta_{i,T}, \quad (3.6)$$

where we include the three dummy variables each representing one type of financing sequence, and we use firms with angel-dominated first round and angel-dominated second round of financing (*FinPath=Angel to Angel*) as the comparison group. As in our previous analyses, we control for firm age ($lnage$) and firm sales ($lnsales$) in the year of the first financing round. We also include year fixed effects and industry fixed effects in our regressions.

The results are reported in Table 3.15. In Column (1), the dependent variable is IPO. We observe that coefficient estimate on *FinPath (Angel to VC)* is positive and statistically significant at the 5 percent level, suggesting that firms with a financing sequence from angel dominated to VC dominated (i.e., angel-to-VC) in the first two rounds have a 12.3 percentage point higher probability of going public in the subsequent years than firms have an angel-to-angel financing sequence. We replace the dependent variable with *Acq* in Column (2). The coefficient estimates on *FinPath (Angel to VC)* and *FinPath (VC to VC)* are both positive and statistically significant (1 percent level), suggesting that compared to firms with an angel-to-angel financing sequence, firms with an angel-to-VC or a VC-to-VC financing sequence, on average, enjoy a 18.5 percentage point and 23.9 percentage point higher probability of being acquired in the subsequent years, respectively. In Column (3), the dependent variable is $Exit$, which equals one if a firm has an IPO or is acquired in the following years and zero otherwise. Similar to the results in Column (2), firms with an angel-to-VC or VC-to-VC financing sequence have a significantly (1 percent level) higher (by 28.8 percentage point and 23.4 percentage point, respectively) probability of having a successful exit, compared to firms with an angel-to-angel financing sequence. The coefficient estimates on *FinPath (VC to Angel)* in all the three columns are not significant, indicating that firms with a VC-to-angel financing sequence do not exhibit a significant difference in the rate of having a successful exit compared to firms with an angel-to-angel financing sequence. In their theoretical paper, Chemmanur and Chen (2014) describe the intuition behind the above results. They posit that if firms obtain venture financing in their early rounds of investments, it will convey favorable information to outside private equity investors, who will revise their estimates of firms' valuation upwards. Further, later rounds of VC investments will also act as favorable signals to outside private equity investors. Lastly, exit by VCs from firms initially backed by them will convey negative signals to outside investors. Thus, firms which experience angel-to-VC or VC-to-VC financing sequence are likely to be of higher quality compared to firms that experience VC-to-angel or angel-to-angel financing sequence.

Thus, our results show that the financing sequence of entrepreneurial firms are associated with their future successful exits: firms experiencing VC-to-VC or angel-to-VC investment sequences have a better likelihood of successful exits compared to firms experiencing VC-to-angel or angel-to-angel investment sequences, which supports our hypothesis **H6**.

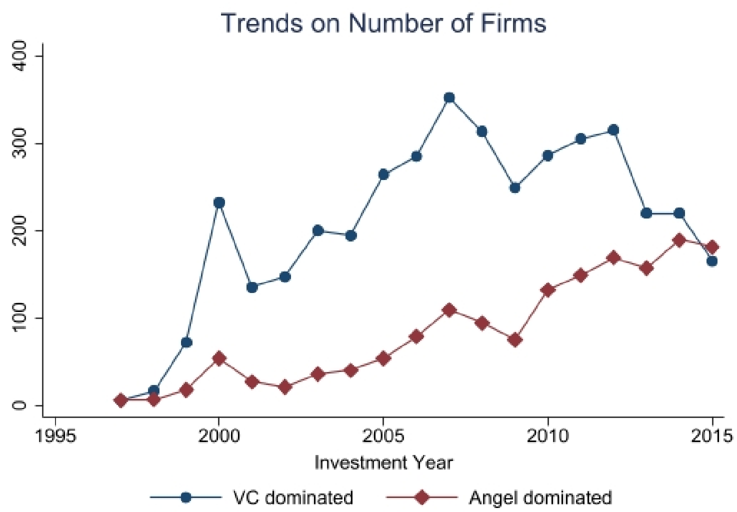
3.10 Conclusion

In this paper, using a large sample of angel and venture capital (VC) financing data from the Crunchbase and VentureXpert databases and private firm data from the NETS database, we addressed three important research questions. First, we analyzed the relative extent of value addition by angels versus VCs to start-up firms. We showed that start-ups financed by angels rather than VCs are associated with a smaller likelihood of successful exit (IPO or acquisition), smaller sales and employment growth, smaller quantity and quality of innovation, and a smaller net inflow of top-quality inventors. We disentangled selection and monitoring effects using instrumental variable (IV) and switching regression analyses and show that our baseline results are causal. Second, we investigated the complementarity versus substitution relationship between angel and VC financing. We found that a firm that received a larger fraction of VC or angel financing in the first financing round is likely to receive a larger fraction of the same type of financing in a subsequent round; however, when we include other non-VC financing sources such as accelerators and government grants into the analysis, a firm that received angel (rather than other non-VC) financing in the first round is also more likely to receive VC financing in a subsequent round. Third, we analyzed how the financing sequence (order of investments by angels and VCs across rounds) of start-up firms is related to their successful exit probability. We found that firms that received primarily VC financing in the first round and continued to receive VC financing in subsequent rounds or those that received primarily angel financing in the first round and received VC financing in subsequent rounds have the highest chance of successful exit compared to those with other financing sequences (VC-angel or angel-angel).

3.11 Figures and Tables

Figure 3.1. Trends in the Number of Angel- and VC-dominated First Round Investments in Startups and their Eventual Successful Exit

This figure below shows the trends in first-round investment for our sample startups and trends in percentage of their future successful exit. We only include years that consist of at least five startup first-investment rounds in our sample. The blue line with circle markers indicates startups that have received more than fifty percentage of funding in the first round from VC investors (*VC-dominated*). The red line with diamond markers indicates startups that have received equal to or more than fifty percentage of funding in the first round from angel investors (*Angel-dominated*).



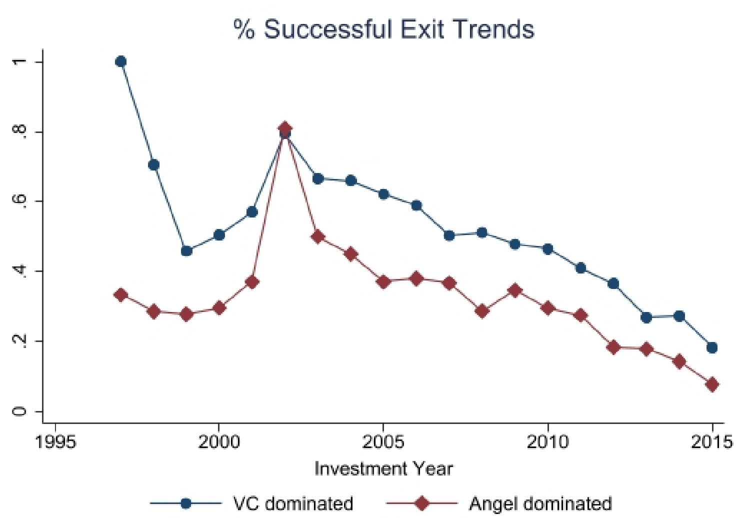
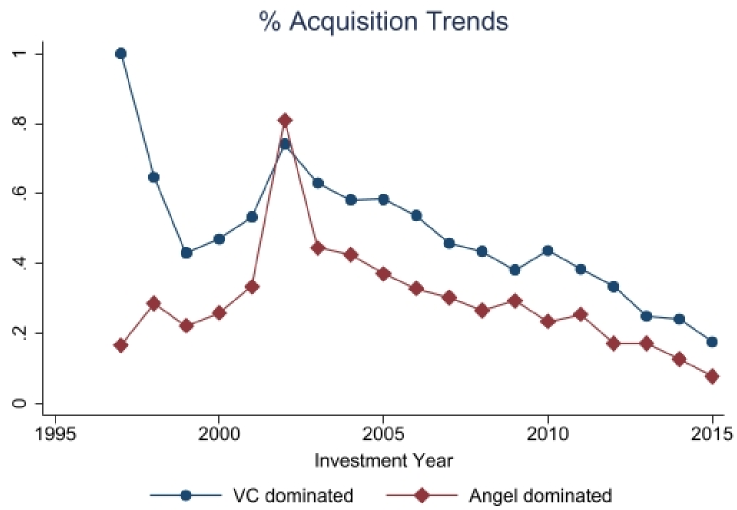


Figure 3.2. Trends in Acquisitions and IPOs of Angel- and VC-dominated First Round Investments

This figure below shows the trends in percentage of future acquisition and IPOs for angel-dominated and VC-dominated firms in our sample. We only include years that consist of at least five startup first-investment rounds in our sample. The blue line with circle markers indicates startups that have received more than fifty percentage of funding in the first round from VC investors (*VC-dominated*). The red line with diamond markers indicates startups that have received equal to or more than fifty percentage of funding in the first round from angel investors (*Angel-dominated*).



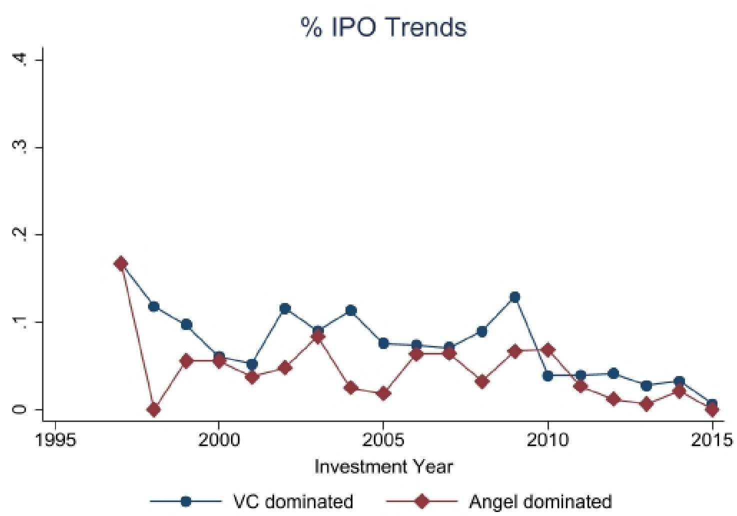


Table 3.1. Summary Statistics

This table displays summary statistics for the main variables in the analysis. This sample include all startup firms whose first financing rounds involved only angel and VC investors. *1st-round_angel%* is the fraction of angel investment in the first round of financing, which is measured as the amount of angel-investment scaled by total investment in the round. *Angel Investment (1st round)* is a dummy variable which equals one if there is at least one angel investor participating in the first round of financing. *IPO* is a dummy variable which equals to one if a firm has gone public in the future after its first-round of financing and zero otherwise. *Acq* is a dummy variable which equals to one if a firm has been acquired and zero otherwise. *Exit* is a dummy variable which equals one if a firm has either been acquired or gone public in the future after its first-round of financing. *Patents (3 years)* is the natural logarithm of one plus the number of patents applied (and eventually granted) to a startup within three years after its first-round of financing adjusted for the truncation bias, respectively. *Citations (3 years)* is the natural logarithm of one plus the number of citations on patents applied (and eventually granted) by startups within three years after its first-round of financing adjusted for the truncation bias, respectively. *Sales growth (Year 0 to 1)* is defined as the annual growth rate of sales in the first after the first-round of financing for a firm. *Employment growth (Year 0 to 1)* is defined as the annual growth rate of employment in the first year after a firm's first-round of financing. *Net Inflow of Inventors (3 Years)* is defined as the difference between the natural logarithm of one plus the inflow and the natural logarithm of one plus the outflow of inventors in the subsequent three years after an entrepreneurial firm received its first-round of financing. *Net Inflow of Top 25% Inventors (3 Years)* is defined as the difference between the natural logarithm of one plus the inflow and the natural logarithm of one plus the outflow of inventors, who are in the top quartile on the basis of prior citations on their patents, in the subsequent three years after the first investment round. *lnage* and *lnsales* are the natural logarithms of firm age and firm sales in the year of the first investment round.

Variable	**N**	**Mean**	**SD**	**Min**	**Median**	**Max**
1st_round_angel%	5583	0.30	0.37	0.00	0.09	1.00
Angel Investment (1st round)	5583	0.26	0.44	0.00	0.00	1.00
IPO	5583	0.06	0.23	0.00	0.00	1.00
Acq	5583	0.38	0.49	0.00	0.00	1.00
Exit	5583	0.42	0.49	0.00	0.00	1.00
Patents (3 years)	5583	0.53	2.38	0.00	0.00	14.61
Citations (3 years)	5583	0.01	0.05	0.00	0.00	0.31
Sales Growth (Year 0 to 1)	3951	0.36	1.24	-1.00	0.00	6.50
Employment Growth (Year 0 to 1)	3952	0.30	0.96	-1.00	0.00	4.50
Net Inflow of Inventors (3 years)	5583	0.18	0.53	-1.10	0.00	2.08
Net Inflow of Top 25% Inventors (3 years)	5583	0.04	0.25	-0.69	0.00	1.10
lnage	5583	1.24	0.78	0.00	1.10	2.71
lnsales	5583	10.78	6.40	0.00	13.55	21.53

Table 3.2. Investor Composition and Successful Exits

This table shows the results of examining how investor composition for entrepreneurial firms in the first round of financing is associated with these firms' successful exits in the subsequent years. This sample include all startup firms whose first financing rounds involved only angel and VC investors. The dependent variables are dummy variables representing whether a firm has gone public (IPO), has been acquired (Acq), or has either been acquired or gone public ($Exit$) in the years after its first-round of financing. *1st-round_angel%* is the fraction of angel investment in the first round of financing, which is measured as the amount of angel-investment scaled by total investment in the round. We control for the natural logarithms of firm age ($lnage$) and firm sales ($lnsales$) in the year of the first round. We control for the year that firms receive their first-round of financing and the two-digit SIC code of firms' primary industry. Constants are suppressed. The standard errors are clustered at the two-digit SIC code level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Variables	(1) *IPO*	(2) *Acq*	(3) *Exit*
1st_round_angel%	-0.017**	-0.192***	-0.199***
	(0.007)	(0.015)	(0.013)
lnage	0.001	0.014	0.016
	(0.006)	(0.010)	(0.011)
lnsales	0.002***	0.001	0.002
	(0.001)	(0.001)	(0.001)
Observations	5,583	5,583	5,583
R-squared	.074	.126	.138
Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes

Table 3.3. Investor Composition and Sales Growth and Employment Growth

This table reports the results of examining how investor composition for entrepreneurial firms in the first round of financing is associated with firms' subsequent sales growth and employment growth. This sample include all startup firms whose first financing rounds involved only angel and VC investors. The dependent variables are the annual growth rates of sales in the first, second, and third year after its first-round of financing (*Sales_growth (Year 0 to 1)*, *Sales_growth (Year 1 to 2)*, and *Sales_growth (Year 2 to 3)*), respectively. In Columns (4)-(6), the dependent variables are the annual growth rates of employment in the first, second, and third year after its first-round of financing (*Employment growth (Year 0 to 1)*, *Employment growth (Year 1 to 2)*, and *Employment growth (Year 2 to 3)*), respectively. *1st-round_angel%* is the fraction of angel investment in the first round of financing, which is measured as the amount of angel-investment scaled by total investment in the round. We control for the natural logarithms of firm age (*lnage*) and firm sales (*lnsales*) in the year of the first round. We control for the year that firms receive their first-round of financing and the two-digit SIC code of firms' primary industry. Constants are suppressed. The standard errors are clustered at the two-digit SIC code level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Sales Growth			*Employment Growth*		
Variables	Year 0 to 1	Year 1 to 2	Year 2 to 3	Year 0 to 1	Year 1 to 2	Year 2 to 3
1st_round_angel%	-0.248***	-0.206***	-0.140***	-0.236***	-0.193***	-0.101***
	(0.050)	(0.025)	(0.037)	(0.034)	(0.022)	(0.030)
lnage	0.067**	0.009	-0.044	0.017	-0.030**	-0.064***
	(0.026)	(0.017)	(0.028)	(0.017)	(0.014)	(0.016)
lnsales	-0.150***	-0.007*	-0.006	-0.093***	-0.002	-0.004
	(0.018)	(0.004)	(0.004)	(0.012)	(0.003)	(0.003)
Observations	3,951	4,239	3,973	3,952	4,244	3,977
R-squared	.075	.031	.03	.081	.047	.044
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 3.4. Investor Composition and Patent Quantity and Quality

This table shows the results of examining how investor composition for entrepreneurial firms in the first round of financing is associated with patent applications and citations in the subsequent years. This sample include all startup firms whose first financing rounds involved only angel and VC investors. The dependent variables in columns (1) to (3) are the natural logarithm of one plus the number of patents applied (and eventually granted) in the next one, two, and three years after its first-round of financing adjusted for the truncation bias (*Patents (1 year)*, *Patents (2 years)*, and *Patents (3 years)*), respectively. The dependent variables in columns (4) to (6) are the natural logarithm of one plus the number of citations on patents applied (and eventually granted) in the next one, two, and three years after its first-round of financing adjusted for the truncation bias (*Citations (1 year)*, *Citations (2 years)*, and *Citations (3 years)*). *1st-round_angel%* is the fraction of angel investment in the first round of financing, which is measured as the amount of angel-investment scaled by total investment in the round. We control for the natural logarithms of firm age (*lnage*) and firm sales (*lnsales*) in the year of the first round. We control for the year that firms receive their first-round of financing and the two-digit SIC code of firms' primary industry. Constants are suppressed. The standard errors are clustered at the two-digit SIC code level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
		Patents			*Citations*	
Variables	1 Year	2 Years	3 Years	1 Year	2 Years	3 Years
1st_round_angel%	-0.101**	-0.294***	-0.352***	-0.002***	-0.007***	-0.008***
	(0.038)	(0.081)	(0.079)	(0.001)	(0.002)	(0.002)
lnage	-0.110***	-0.313***	-0.379***	-0.002***	-0.007***	-0.009***
	(0.023)	(0.048)	(0.053)	(0.000)	(0.001)	(0.001)
lnsales	-0.004***	-0.011***	-0.009	-0.000***	-0.000**	-0.000*
	(0.001)	(0.004)	(0.005)	(0.000)	(0.000)	(0.000)
Observations	5,583	5,583	5,583	5,583	5,583	5,583
R-squared	.048	.078	.076	.048	.072	.072
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 3.5. Investor Composition and Inventor Inflows

This table shows the results of examining how investor composition for entrepreneurial firms in the first round of financing is associated with inventor net inflows in the subsequent years. This sample include all startup firms whose first financing rounds involved only angel and VC investors. The dependent variables in columns (1) to (3) are the net inflow of inventors in one, two, and three years after its first-round of financing (*Net Inflow of Inventors (1 Year)*, *Net Inflow of Inventors (2 Years)*, and *Net Inflow of Inventors (3 Years)*), respectively, defined as the difference between the natural logarithm of one plus the inflow and the natural logarithm of one plus the outflow of inventors in the subsequent one, two, and three years after an entrepreneurial firm received its first-round of financing. The dependent variables in columns (4) to (6) are the net inflow of the inventors with the top-quartile number of citations in one, two, and three years after its first-round of financing (*Net Inflow of Top 25% Inventors (1 Year)*, *Net Inflow of Top 25% Inventors (2 Years)*, and *Net Inflow of Top 25% Inventors (3 Years)*), respectively. *1st-round_angel%* is the fraction of angel investment in the first round of financing, which is measured as the amount of angel-investment scaled by total investment in the round. We control for the natural logarithms of firm age (*lnage*) and firm sales (*lnsales*) in the year of the first round. We control for the year that firms receive their first-round of financing and the two-digit SIC code of firms' primary industry. Constants are suppressed. The standard errors are clustered at the two-digit SIC code level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Net Inflow of Inventors			*Net Inflow (Top 25% Inventors)*		
Variables	1 Year	2 Years	3 Years	1 Year	2 Years	3 Years
1st_round_angel%	-0.065***	-0.110***	-0.119***	-0.021***	-0.032***	-0.035***
	(0.011)	(0.015)	(0.016)	(0.005)	(0.005)	(0.007)
lnage	-0.054***	-0.085***	-0.103***	-0.016***	-0.026***	-0.031***
	(0.008)	(0.011)	(0.012)	(0.003)	(0.004)	(0.007)
lnsales	0.002	0.001	0.002	-0.000	-0.000	-0.001
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)
Observations	5,583	5,583	5,583	5,583	5,583	5,583
R-squared	.045	.061	.07	.029	.038	.043
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 3.6. IV Analysis: Successful Exits

This table shows the results of the IV analysis of the impact of investor composition on entrepreneurial firms' successful exits in the subsequent years. This sample include all startup firms whose first financing rounds involved only angel and VC investors. *ATC* is the IV which is a dummy variable that equals one if the state where a firm is located in has an active angel tax credits program and zero otherwise. *LPR* is the IV which proxies for the returns of the limited partners in the past three years. Column (1) shows the first-stage of the IV analysis. In Column (2)-(4), the dependent variables are dummy variables representing whether a firm has gone public (*IPO*), has been acquired (*Acq*), or has either been acquired or gone public (*Exit*) in the years after its first-round of financing, respectively. *1st-round_angel%* equals the fraction of angel investment in the first round of financing. We control for the natural logarithms of firm age (*lnage*) and firm sales (*lnsales*) in the year of the first round. We also include the year that firms receive their first-round of financing and the two-digit SIC code of firms' primary industry. Constants are suppressed. We also report Kleibergen-Paap rk Wald F statistic. The standard errors are clustered at the two-digit SIC code level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Variables	(1) *1st-stage*	(2) *IPO*	(3) *Acq*	(4) *Exit*
LPR	-0.081***			
	(0.015)			
ATC	0.041***			
	(0.009)			
1st_round_angel%		-0.308**	-0.610***	-0.872***
		(0.146)	(0.233)	(0.292)
lnage	-0.074***	-0.019**	-0.015	-0.031**
	(0.006)	(0.008)	(0.012)	(0.015)
lnsales	-0.005***	0.001	-0.001	-0.002
	(0.001)	(0.001)	(0.002)	(0.003)
Observations	5,583	5,583	5,583	5,583
R-squared	.132	-	-	-
F-stat	27.297	-	-	-
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes

Table 3.7. IV Analysis: Sales Growth and Employment Growth

This table shows the results of the IV analysis of the impact of investor composition on the sales growth of entrepreneurial firms in the subsequent years. This sample include all startup firms whose first financing rounds involved only angel and VC investors. *ATC* is the IV which is a dummy variable that equals one if the state where a firm is located in has an active angel tax credits program and zero otherwise. *LPR* is the IV which proxies for the returns of the limited partners in the past three years. Column (1) shows the first-stage of the IV analysis. In Column (2)-(4), the dependent variables are the annual growth rates of sales in the first, second, and third year after its first-round of financing (*Sales Growth (Year 0 to 1)*, *Sales Growth (Year 1 to 2)*, and *Sales Growth (Year 2 to 3)*), respectively. In Column (5)-(7), the dependent variables are the annual growth rates of employment in the first, second, and third year after its first-round of financing (*Employment growth (Year 0 to 1)*, *Employment growth (Year 1 to 2)*, and *Employment growth (Year 2 to 3)*), respectively. *1st-round_angel%* equals the fraction of angel investment in the first round of financing. We control for the natural logarithms of firm age (*lnage*) and firm sales (*lnsales*) in the year of the first round. We also include the year that firms receive their first-round of financing, the state that firms locate in, and the two-digit SIC code of firms' primary industry. Constants are suppressed. We also report Kleibergen-Paap rk Wald F statistic. The standard errors are clustered at the two-digit SIC code level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Sales Growth			*Employment Growth*	
Variables	1st-stage	Year 0 to 1	Year 1 to 2	Year 2 to 3	Year 0 to 1	Year 1 to 2	Year 2 to 3
LPR	-0.064***						
	(0.017)						
ATC	0.040***						
	(0.012)						
1st_round_angel%		-0.294	0.960	-1.949***	-0.670	-0.013	-0.930***
		(0.626)	(0.974)	(0.551)	(0.429)	(0.559)	(0.332)
lnage	-0.057***	0.064**	0.061	-0.108***	-0.007	-0.022	-0.094***
	(0.006)	(0.031)	(0.042)	(0.034)	(0.027)	(0.024)	(0.019)
lnsales	-0.027***	-0.151***	-0.003	-0.013***	-0.104***	-0.002	-0.008**
	(0.007)	(0.028)	(0.003)	(0.005)	(0.017)	(0.002)	(0.003)
Observations	3,951	3,951	4,239	3,973	3,952	4,244	3,977
R-squared	.119	-	-	-	-	-	-
F-stat	9.064	-	-	-	-	-	-
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 3.8. IV Analysis: Patent Quantity and Quality

This table shows the results of the IV analysis of the impact of investor composition on entrepreneurial firms' innovation quantity and quality in the subsequent years. This sample include all startup firms whose first financing rounds involved only angel and VC investors. *ATC* is the IV which is a dummy variable that equals one if the state where a firm is located in has an active angel tax credits program and zero otherwise. *LPR* is the IV which proxies for the returns of the limited partners in the past three years. In Column (2)-(4), the dependent variables are the natural logarithm of one plus the number of patents applied (and eventually granted) in one, two, and three years after its first-round of financing adjusted for the truncation bias (*Patents (1 year)*, *Patents (2 years)*, and *Patents (3 years)*), respectively. In Column (5)-(7), the dependent variables are the natural logarithm of one plus the number of citations on patents applied (and eventually granted) in one, two, and three years after its first-round of financing adjusted for the truncation bias (*Citations (1 year)*, *Citations (2 years)*, and *Citations (3 years)*), respectively. *1st-round_angel%* equals the fraction of angel investment in the first round of financing. We control for the natural logarithms of firm age (*lnage*) and firm sales (*lnsales*) in the year of the first round. We also include the year that firms receive their first-round of financing and the two-digit SIC code of firms' primary industry. Constants are suppressed. We also report Kleibergen-Paap rk Wald F statistic. The standard errors are clustered at the two-digit SIC code level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Patents			*Citations*	
Variables	1st-stage	1 Year	2 Years	3 Years	1 Year	2 Years	3 Years
LPR	-0.081***						
	(0.015)						
ATC	0.041***						
	(0.009)						
1st_round_angel%		-1.139***	-2.944**	-3.214**	-0.016***	-0.044**	-0.053**
		(0.336)	(1.311)	(1.568)	(0.004)	(0.017)	(0.025)
lnage	-0.074***	-0.184***	-0.500***	-0.582***	-0.003***	-0.009***	-0.012***
	(0.006)	(0.028)	(0.109)	(0.132)	(0.000)	(0.001)	(0.002)
lnsales	-0.005***	-0.010***	-0.026***	-0.024***	-0.000***	-0.000***	-0.001***
	(0.001)	(0.002)	(0.006)	(0.006)	(0.000)	(0.000)	(0.000)
Observations	5,583	5,583	5,583	5,583	5,583	5,583	5,583
R-squared	.132	-	-	-	-	-	-
F-stat	27.297	-	-	-	-	-	-
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 3.9. IV Analysis: Inventor Inflows

This table shows the results of the IV analysis of the impact of investor composition on entrepreneurial firms' inventor inflows in the subsequent years. This sample include all startup firms whose first financing rounds involved only angel and VC investors. *ATC* is the IV which is a dummy variable that equals one if the state where a firm is located in has an active angel tax credits program and zero otherwise. *LPR* is the IV which proxies for the returns of the limited partners in the past three years. In Column (2)-(4), the dependent variables are the net inflow of inventors in one, two, and three years after its first-round of financing (*Net Inflow of Inventors (1 Year)*, *Net Inflow of Inventors (2 Years)*, and *Net Inflow of Inventors (3 Years)*), respectively. The dependent variables in columns (5) to (7) are the net inflow of the inventors with the top-quartile number of citations in one, two, and three years after its first-round of financing (*Net Inflow of Top 25% Inventors (1 Year)*, *Net Inflow of Top 25% Inventors (2 Years)*, and *Net Inflow of Top 25% Inventors (3 Years)*), respectively. *1st-round_angel%* equals the fraction of angel investment in the first round of financing. We control for the natural logarithms of firm age (*lnage*) and firm sales (*lnsales*) in the year of the first round. We also include the year that firms receive their first-round of financing and the two-digit SIC code of firms' primary industry. Constants are suppressed. We also report Kleibergen-Paap rk Wald F statistic. The standard errors are clustered at the two-digit SIC code level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Net Inflow of Inventors			*Net Inflow (Top 25% inventors)*		
Variables	1st-stage	1 Year	2 Years	3 Years	1 Year	2 Years	3 Years
LPR	-0.081***						
	(0.015)						
ATC	0.041***						
	(0.009)						
1st_round_angel%		-0.455***	-0.910***	-0.713***	-0.125*	-0.182	-0.110
		(0.156)	(0.233)	(0.171)	(0.071)	(0.126)	(0.104)
lnage	-0.074***	-0.082***	-0.142***	-0.145***	-0.023***	-0.036***	-0.036***
	(0.006)	(0.011)	(0.021)	(0.014)	(0.007)	(0.012)	(0.012)
lnsales	-0.005***	-0.000	-0.003**	-0.002	-0.001**	-0.001**	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
Observations	5,583	5,583	5,583	5,583	5,583	5,583	5,583
R-squared	.132	-	-	-	-	-	-
F-stat	27.297	-	-	-	-	-	-
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 3.10. Switching Regressions: Successful Exits

This table reports the results from an endogenous switching regression model, examining the impact of investor composition on firm's successful exits. Panel A reports the results of the second-stage regressions where dependent variables are related to successful exits and independent variables are the *Inverse Mills Ratio* reported from the first stage and all the other independent variables the same as in the first stage (results reported in the Table C1 Internet Appendix). In the second stage of regressions, standard errors are bootstrapped and are clustered at the two-digit SIC code level. Panel B shows the "what-if" analysis based on the results of the switching regression model. Panel B first displays the counterfactual analysis for firms which received angel financing in their first round of financing and then shows the counterfactual analysis for firms which only received VC financing in their first round of financing. The actual outcome, the hypothetical value predicted from the switching regression model, the difference between actual value and the hypothetical value, and the t-statistics of the difference are shown in each panel. This means that for the sample of angel-backed, firms hypothetical scenario represents the case where the angel-backed firms did not receive any angel financing (but only VC investment), while for the sample of VC-only firms, hypothetical scenario represents the case where such firms received angel financing. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Switching Regressions with Endogenous Switching: Successful Exits

	(1)	(2)	(3)	(4)	(5)	(6)
Variable:	*IPO*		*Acq*		*Exit*	
Sub-sample:	*VC Only*	*Angel-backed*	*VC Only*	*Angel-backed*	*VC Only*	*Angel-backed*
Inverse Mills Ratio	0.176***	0.090*	0.316***	0.039	0.448***	0.157
	(0.052)	(0.048)	(0.101)	(0.117)	(0.112)	(0.107)
lnage	-0.045***	-0.020	-0.058**	0.001	-0.090***	-0.027
	(0.013)	(0.015)	(0.023)	(0.027)	(0.025)	(0.026)
lnsales	-0.000	0.000	-0.005**	0.002	-0.006***	0.002
	(0.001)	(0.001)	(0.002)	(0.003)	(0.002)	(0.003)
Observations	4,126	1,443	4,126	1,443	4,126	1,443
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

(Continued)

Panel B. Counterfactual Analysis on Successful Exits

	Actual	Hypothetical	Diff	t-statistics
Comparisons for Angel-Backed Firms				
IPO	0.028	0.038	-0.009	-2.101
Acq	0.229	0.326	-0.097	-8.854
Exit	0.252	0.355	-0.103	-9.159
Comparisons for VC-Only Firms				
IPO	0.068	0.052	0.016	4.105
Acq	0.436	0.296	0.141	18.661
Exit	0.481	0.341	0.140	18.595

Table 3.11. Switching Regressions: Sales Growth and Employment Growth

This table reports the results from an endogenous switching regression model, examining the impact of investor composition on firm's growth of sales and employment. Panel A reports the results of the second-stage regressions where dependent variables are related to sales growth and independent variables in the second stage of the regressions are the *Inverse Mills Ratio* reported from the first stage and all the other independent variables the same as in the first stage (results reported in the Table C1 Internet Appendix). In the second stage of regressions, standard errors are bootstrapped and are clustered at the two-digit SIC code level. Panel B reports the results of the second-stage regressions where dependent variables are related to employment growth. Panel C shows the "what-if" analysis based on the results of the switching regression model for sales growth and employment growth. Panel C first displays the counterfactual analysis for firms which received angel financing in their first round of financing and then shows the counterfactual analysis for firms which only received VC financing in their first round of financing. The actual outcome, the hypothetical value predicted from the switching regression model, the difference between actual value and the hypothetical value, and the t-statistics of the difference are shown in each panel. This means that for the sample of angel-backed firms, hypothetical scenario represents the case where the angel-backed firms did not receive any angel financing (but only VC investment), while for the sample of VC-only firms, hypothetical scenario represents the case where such firms received angel financing. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Switching Regressions with Endogenous Switching: Sales Growth

	(1)	(2)	(3)	(4)	(5)	(6)
Variables	*Sales Growth*					
	1 Year		2 Years		3 Years	
Sub-sample:	*VC Only*	*Angel-backed*	*VC Only*	*Angel-backed*	*VC Only*	*Angel-backed*
Inverse Mills Ratio	-0.115	-0.509	-0.509	-0.164	0.354	0.527**
	(0.265)	(0.346)	(0.311)	(0.313)	(0.228)	(0.255)
lnage	0.098	0.185**	0.122	0.069	-0.140**	-0.134*
	(0.066)	(0.083)	(0.079)	(0.088)	(0.068)	(0.070)
lnsales	-0.163***	-0.087***	0.001	-0.003	-0.014***	-0.004
	(0.019)	(0.030)	(0.005)	(0.005)	(0.004)	(0.005)
Observations	3,105	838	3,368	864	3,214	752
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

(Continued)

Panel B. Switching Regressions with Endogenous Switching: Employment Growth

Variables	(1)	(2)	(3)	(4)	(5)	(6)
	Employment Growth					
	1 Year		2 Years		3 Years	
Sub-sample:	*VC Only*	*Angel-backed*	*VC Only*	*Angel-backed*	*VC Only*	*Angel-backed*
Inverse Mills Ratio	-0.033	-0.093	-0.050	0.192	0.070	0.366
	(0.184)	(0.319)	(0.187)	(0.240)	(0.110)	(0.239)
lnage	0.027	0.043	-0.036	-0.044	-0.087***	-0.129**
	(0.047)	(0.074)	(0.048)	(0.065)	(0.033)	(0.065)
lnsales	-0.103***	-0.040*	-0.001	-0.004	-0.007**	-0.006**
	(0.013)	(0.024)	(0.003)	(0.004)	(0.003)	(0.003)
Observations	3,106	838	3,373	864	3,218	752
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel C. Counterfactual Analysis on Sales Growth and Employment Growth

	Actual	Hypothetical	Diff	t-statistics
Comparisons for Angel-Backed Firms				
Sales Growth (Yr 0 to 1)	0.221	0.468	-0.247	-6.887
Sales Growth (Yr 1 to 2)	0.129	0.421	-0.292	-10.396
Sales Growth (Yr 2 to 3)	0.204	0.296	-0.092	-2.479
Employment Growth (Yr 0 to 1)	0.171	0.380	-0.209	-8.021
Employment Growth (Yr 1 to 2)	0.097	0.334	-0.238	-11.294
Employment Growth (Yr 2 to 3)	0.151	0.225	-0.075	-2.532
Comparisons for VC-Only Firms				
Sales Growth (Yr 0 to 1)	0.399	0.225	0.174	7.485
Sales Growth (Yr 1 to 2)	0.408	0.178	0.230	10.115
Sales Growth (Yr 2 to 3)	0.297	0.275	0.021	0.994
Employment Growth (Yr 0 to 1)	0.339	0.197	0.142	7.921
Employment Growth (Yr 1 to 2)	0.345	0.145	0.200	11.333
Employment Growth (Yr 2 to 3)	0.208	0.185	0.023	1.513

Table 3.12. Switching Regressions: Patent Quantity and Quality

This table reports the results from an endogenous switching regression model, examining the impact of investor composition on firm's patent quantity and quality. Panel A reports the results of the second-stage regressions where dependent variables are related to patent quantity and independent variables in the second stage of the regressions are the *Inverse Mills Ratio* reported from the first stage and all the other independent variables the same as in the first stage (results reported in the Table C1 Internet Appendix). In the second stage of regressions, standard errors are bootstrapped and are clustered at the two-digit SIC code level. Panel B reports the results of the second-stage regressions where dependent variables are related to patent quality. Panel C shows the "what-if" analysis based on the results of the switching regression model for patent quantity and quality. Panel C first displays the counterfactual analysis for firms which received angel financing in their first round of financing and then shows the counterfactual analysis for firms which only received VC financing in their first round of financing. The actual outcome, the hypothetical value predicted from the switching regression model, the difference between actual value and the hypothetical value, and the t-statistics of the difference are shown in each panel. This means that for the sample of angel-backed firms, hypothetical scenario represents the case where the angel-backed firms did not receive any angel financing (but only VC investment), while for the sample of VC-only firms, hypothetical scenario represents the case where such firms received angel financing. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Switching Regressions with Endogenous Switching: Patent Quantity

	(1)	(2)	(3)	(4)	(5)	(6)
	Patents					
	1 Year		2 Years		3 Years	
Variables Sub-sample:	*VC Only*	*Angel-backed*	*VC Only*	*Angel-backed*	*VC Only*	*Angel-backed*
Inverse Mills Ratio	0.348**	0.404	1.026***	0.424	1.182**	0.686
	(0.142)	(0.270)	(0.380)	(0.561)	(0.499)	(0.614)
lnage	-0.229***	-0.117*	-0.650***	-0.227	-0.776***	-0.314*
	(0.033)	(0.067)	(0.119)	(0.152)	(0.157)	(0.170)
lnsales	-0.011***	-0.007	-0.031***	-0.008	-0.032***	-0.008
	(0.003)	(0.004)	(0.007)	(0.009)	(0.008)	(0.010)
Observations	4,126	1,443	4,126	1,443	4,126	1,443
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

(Continued)

Panel B. Switching Regressions with Endogenous Switching: Patent Quality

Variables	(1)	(2)	(3)	(4)	(5)	(6)
	Citations					
	1 Year		2 Years		3 Years	
Sub-sample:	*VC Only*	*Angel-backed*	*VC Only*	*Angel-backed*	*VC Only*	*Angel-backed*
Inverse Mills Ratio	0.003	0.007	0.007	0.008	0.009	0.014
	(0.002)	(0.004)	(0.006)	(0.010)	(0.009)	(0.013)
lnage	-0.003***	-0.002*	-0.010***	-0.004	-0.013***	-0.006*
	(0.000)	(0.001)	(0.002)	(0.003)	(0.002)	(0.004)
lnsales	-0.000***	-0.000*	-0.000***	-0.000	-0.000***	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	4,126	1,443	4,126	1,443	4,126	1,443
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel C. Counterfactual Analysis on Patent Quantity and Quality

	Actual	Hypothetical	Diff	t-statistics
Comparisons for Angel-Backed Firms				
Patents in the next 1 yr	0.079	0.185	-0.106	-6.680
Patents in the next 2 yrs	0.198	0.497	-0.299	-8.299
Patents in the next 3 yrs	0.255	0.603	-0.348	-7.864
Citations of patents in the next 1 yr	0.001	0.003	-0.002	-6.627
Citations of patents in the next 2 yrs	0.003	0.010	-0.007	-9.421
Citations of patents in the next 3 yrs	0.005	0.013	-0.008	-9.169
Comparisons for VC-Only Firms				
Patents in the next 1 yr	0.187	0.156	0.031	2.231
Patents in the next 2 yrs	0.511	0.342	0.169	5.020
Patents in the next 3 yrs	0.629	0.504	0.125	3.116
Citations of patents in the next 1 yr	0.003	0.002	0.001	2.161
Citations of patents in the next 2 yrs	0.009	0.006	0.003	4.889
Citations of patents in the next 3 yrs	0.012	0.009	0.003	3.188

Table 3.13. Switching Regressions: Inventor Inflows

This table reports the results from an endogenous switching regression model, examining the impact of investor composition on firm's inventor net inflows. Panel A reports the results of the second-stage regressions where dependent variables are related to all inventor net flows and independent variables in the second stage of the regressions are the *Inverse Mills Ratio* reported from the first stage and all the other independent variables the same as in the first stage (results reported in the Table C1 Internet Appendix). In the second stage of regressions, standard errors are bootstrapped and are clustered at the two-digit SIC code level. Panel B reports the results of the second-stage regressions where dependent variables are related to net inflows of inventors with the top-quartile of number of citations. Panel C shows the "what-if" analysis based on the results of the switching regression model for inventor net inflows. Panel C first displays the counterfactual analysis for firms which received angel financing in their first round of financing and then shows the counterfactual analysis for firms which only received VC financing in their first round of financing. The actual outcome, the hypothetical value predicted from the switching regression model, the difference between actual value and the hypothetical value, and the t-statistics of the difference are shown in each panel. This means that for the sample of angel-backed firms, hypothetical scenario represents the case where the angel-backed firms did not receive any angel financing (but only VC investment), while for the sample of VC-only firms, hypothetical scenario represents the case where such firms received angel financing. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Switching Regressions with Endogenous Switching: All Inventor Net Inflows

	(1)	(2)	(3)	(4)	(5)	(6)
	Net Inflow of Inventors					
	1 Year		2 Years		3 Years	
Variables Sub-sample:	*VC Only*	*Angel-backed*	*VC Only*	*Angel-backed*	*VC Only*	*Angel-backed*
Inverse Mills Ratio	0.145**	0.205**	0.361***	0.236*	0.212***	0.289**
	(0.068)	(0.088)	(0.077)	(0.130)	(0.070)	(0.116)
lnage	-0.111***	-0.058**	-0.203***	-0.084***	-0.194***	-0.086***
	(0.015)	(0.024)	(0.025)	(0.032)	(0.019)	(0.026)
lnsales	-0.000	-0.001	-0.005***	-0.001	-0.001	-0.002
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Observations	4,126	1,443	4,126	1,443	4,126	1,443
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

(Continued)

Panel B. Switching Regressions with Endogenous Switching: Top Inventor Net Inflows

Variables	(1)	(2)	(3)	(4)	(5)	(6)
	Net Inflow of Inventors (Top 25% Inventors)					
	1 Year		2 Years		3 Years	
Sub-sample:	*VC Only*	*Angel-backed*	*VC Only*	*Angel-backed*	*VC Only*	*Angel-backed*
Inverse Mills Ratio	0.026	0.111***	0.036	0.103*	-0.033	0.168**
	(0.018)	(0.037)	(0.037)	(0.059)	(0.039)	(0.067)
lnage	-0.028***	-0.030***	-0.042***	-0.035**	-0.035***	-0.042**
	(0.006)	(0.009)	(0.012)	(0.015)	(0.011)	(0.017)
lnsales	-0.000	-0.002***	-0.001	-0.002*	0.000	-0.003**
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	4,126	1,443	4,126	1,443	4,126	1,443
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel C. Counterfactual Analysis on Inventor Net Inflows

	Actual	Hypothetical	Diff	t-statistics
Comparisons for Angel-Backed Firms				
All inventor net inflows in the next 1 yr	0.052	0.129	-0.077	-10.279
All inventor net inflows in the next 2 yrs	0.005	0.026	-0.021	-7.784
All inventor net inflows in the next 3 yrs	0.082	0.205	-0.123	-12.237
Top inventor net inflows in the next 1 yr	0.016	0.051	-0.035	-8.552
Top inventor net inflows in the next 2 yrs	0.090	0.240	-0.150	-14.100
Top inventor net inflows in the next 3 yrs	0.022	0.070	-0.048	-10.296
Comparisons for VC-Only Firms				
All inventor net inflows in the next 1 yr	0.115	0.086	0.029	4.472
All inventor net inflows in the next 2 yrs	0.016	0.011	0.004	1.725
All inventor net inflows in the next 3 yrs	0.188	0.103	0.084	10.074
Top inventor net inflows in the next 1 yr	0.037	0.018	0.019	5.021
Top inventor net inflows in the next 2 yrs	0.210	0.124	0.086	9.712
Top inventor net inflows in the next 3 yrs	0.045	0.032	0.013	3.052

Table 3.14. Angels and VC Financing: Complements or Substitutes?

This table reports the results of a test examining how the initial investor composition between VC and angel investors affect the investor composition in the next round, irrespective of the type of investors involved. This sample includes all startups that have received at least two rounds of investment. The dependent variable in Column (1) is the fraction of VC investment in the second round of financing (*2nd-round_VC%*). The dependent variable in Column (2) is a dummy variable of whether or not a firms receives angel investment in the second round (*2nd-round_has_angel*). *1st-round_VC%* equals the fraction of VC investment in the total amount invested in the first round. *1st-round_has_angel* is a dummy which equals one if there is at least one angel invests in the first round and zero otherwise. We control for the natural logarithms of firm age (*lnage*) and firm sales (*lnsales*) in the year of the first round. We control for the year that firms receive their first-round of financing and the two-digit SIC code of firms' primary industry. Constants are suppressed. Standard errors are clustered at the two-digit SIC code level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Variables	(1) *2nd round VC%*	(2) *2nd round has Angel*
1st-round VC%	0.700***	-0.163***
	(0.014)	(0.030)
1st-round has Angel	0.042***	0.282***
	(0.011)	(0.025)
lnage	0.003	-0.030***
	(0.007)	(0.006)
lnsales	0.002***	0.000
	(0.001)	(0.001)
Observations	5,392	5,392
R-squared	0.575	0.237
Year FE	Yes	Yes
Industry FE	Yes	Yes

Table 3.15. Financing Sequence of Angel and VC Financing across Rounds and Probability of Successful Exit

This table reports the results of a test examining how the initial investor composition between VC and angel investors affect the investor composition in the next round. This sample includes all the startups that have received at least two rounds of investment involving either VCs or angels without the involvement of any other category of investors. We categorize sample firms into four subgroups based on their financing path in the first two rounds, from angel-dominated to VC-dominated (*FinPath=Angel to VC*), from VC-dominated to angel-dominated (*FinPath=VC to Angel*), from VC-dominated to VC-dominated (*FinPath=VC to VC*), and from angel-dominated to angel-dominated (*FinPath=Angel to Angel*). The dominance of a financing round is defined by looking at whether the percentage of VC investment in a round is larger than 50% or not. We use firms that have both angel-dominated first round and second round of financing as the control group and thus drop the variable *FinPath=Angel to Angel* in the regressions. We control for the natural logarithms of firm age (*lnage*) and firm sales (*lnsales*) in the year of the first round. We control for the year that firms receive their first-round of financing and the two-digit SIC code of firms' primary industry. Constants are suppressed. Standard errors are clustered at the two-digit SIC code level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Variables	(1) *IPO*	(2) *Acq*	(3) *Exit*
FinPath=(Angel to VC)	0.123**	0.185***	0.288***
	(0.050)	(0.066)	(0.074)
FinPath=(VC to Angel)	-0.040	0.087	0.086
	(0.052)	(0.085)	(0.096)
FinPath=(VC to VC)	0.009	0.239***	0.234***
	(0.019)	(0.028)	(0.022)
lnage	0.049***	-0.001	0.045*
	(0.014)	(0.024)	(0.025)
lnsales	0.005***	0.001	0.004
	(0.001)	(0.003)	(0.004)
Observations	2,294	2,294	2,294
R-squared	0.112	0.173	0.149
Last-round Year FE	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes

Chapter 4

Appendix for Chapter 1

A Additional Tests

Table A1. Impact of Opportunity Zones on Local Private Investment: Alternative Options of Clustering the Standard Errors

This table shows the impact of the Opportunity Zone policy on local private investments under alternative options of clustering the standard errors. The dependent variables are the natural logarithm of the one plus the number of private investment deals invested in census i and year t (*Ln(Num_Inv+1)*) and the amount of private investment deals invested (*Ln(Amount_Inv+1)*). *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone (OZ) and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterwards. Control variables include the population, median income, median age, poverty rate, percentage of white or black people, unemployment rate, and percentage of population without a high-school degree of a census tract in a given year. I also control for year and tract fixed effects. Standard errors are clustered at the county level, county and year level, or state and year level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
		Ln(Num_Inv+1)			*Ln(Amount_Inv+1)*	
*OZ*Post*	0.011***	0.011***	0.011**	0.158***	0.158***	0.158***
	(0.002)	(0.002)	(0.002)	(0.029)	(0.030)	(0.029)
Population	0.018*	0.018	0.018	0.270*	0.270	0.270*
	(0.010)	(0.010)	(0.009)	(0.150)	(0.138)	(0.122)
Median_Income	0.010*	0.010	0.010	0.112	0.112	0.112
	(0.006)	(0.006)	(0.006)	(0.093)	(0.123)	(0.114)
Median_Age	0.003	0.003	0.003	0.105	0.105	0.105
	(0.007)	(0.010)	(0.010)	(0.104)	(0.148)	(0.150)
%White	0.000*	0.000	0.000	0.005***	0.005**	0.005
	(0.000)	(0.000)	(0.000)	(0.002)	(0.002)	(0.003)
%Black	-0.000	-0.000	-0.000	0.001	0.001	0.001
	(0.000)	(0.000)	(0.000)	(0.003)	(0.002)	(0.003)
Poverty_Rate	0.000**	0.000	0.000	0.005**	0.005	0.005
	(0.000)	(0.000)	(0.000)	(0.002)	(0.003)	(0.003)
Unemp_Rate	-0.000	-0.000	-0.000	-0.001	-0.001	-0.001
	(0.000)	(0.000)	(0.000)	(0.002)	(0.003)	(0.003)
%NoHighSchool	0.000	0.000	0.000	0.001	0.001	0.001
	(0.000)	(0.000)	(0.000)	(0.002)	(0.004)	(0.005)
Constant	-0.230**	-0.230	-0.230	-3.612**	-3.612	-3.612
	(0.096)	(0.141)	(0.137)	(1.554)	(2.324)	(2.051)
Observations	154,490	154,490	154,490	154,490	154,490	154,490
R-squared	0.733	0.733	0.733	0.579	0.579	0.579
Tract FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Cluster of SE	County	County*Year	State*Year	County	County*Year	State*Year

Table A2. Logit Regressions for Propensity Score Matching

This table shows the logit regressions when the dependent variable is the indicator variable for Opportunity Zones (OZ) before and after the propensity score matching procedure. Independent variables include the population, median income, poverty rate, percentage of white people, and unemployment rate, percentage of population without high-school degree of a census tract at the end of 2017. I also include the level and the past two-year growth of private investments and new firm registrations. I include a set of dummy variables for each state. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) Pre-PSM *OZ*	(2) Post-PSM *OZ*
Population	0.507**	-0.441
	(0.256)	(0.299)
Median_Income	-11.420***	-1.134
	(0.716)	(0.833)
%White	0.007***	0.001
	(0.001)	(0.001)
%Black	0.007***	0.001
	(0.001)	(0.001)
Poverty_Rate	0.010***	0.001
	(0.002)	(0.003)
Unemp_Rate	0.030***	0.004
	(0.003)	(0.003)
%NoHighSchool	0.006***	0.002
	(0.002)	(0.002)
Num_Inv	-0.120	0.019
	(0.120)	(0.137)
Amnt_Inv	0.040***	-0.000
	(0.009)	(0.010)
New_Firm	0.271***	0.025
	(0.022)	(0.025)
Num_Inv_Growth	-0.122*	0.005
	(0.067)	(0.080)
Amnt_Inv_Growth	-0.000	0.007
	(0.000)	(0.005)
New_Firm_Growth	-0.010	-0.001
	(0.007)	(0.007)
Constant	22.926***	3.334
	(1.762)	(2.047)
Observations	30,904	15,210
Pseudo R2	0.0550	0.0012
State Dummies	YES	YES

Table A3. Impact of Opportunity Zones on Local Private Investment: Propensity-Score-Matched Sample

This table shows the impact of the Opportunity Zone policy on local private investments using a propensity-score-matched sample. The dependent variables are the natural logarithm of the one plus the number of private investment deals invested in census i and year t (*Ln(Num_Inv+1)*) and the amount of private investment deals invested (*Ln(Amount_Inv+1)*). *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone (OZ) and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterwards. Control variables include the population, median income, poverty rate, percentage of white people, and unemployment rate of a census tract in a given year. I also control for year and county fixed effects. Standard errors are clustered at the county level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Ln(Num_Inv+1)		*Ln(Amount_Inv+1)*	
*OZ*Post*	0.013***	0.013***	0.210***	0.210***
	(0.003)	(0.003)	(0.039)	(0.039)
Population		0.019		0.348*
		(0.013)		(0.201)
Median_Income		0.014		0.244*
		(0.010)		(0.138)
Median_Age		-0.001		0.139
		(0.012)		(0.179)
%White		0.000		0.005*
		(0.000)		(0.003)
%Black		-0.000		0.001
		(0.000)		(0.004)
Poverty_Rate		0.000		0.008**
		(0.000)		(0.003)
Unemp_Rate		-0.000		-0.001
		(0.000)		(0.003)
%NoHighSchool		0.000		0.006*
		(0.000)		(0.004)
Constant	0.052***	-0.265*	0.751***	-5.780**
	(0.001)	(0.161)	(0.008)	(2.375)
Observations	76,043	76,030	76,043	76,030
R-squared	0.737	0.737	0.593	0.594
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
PSM	YES	YES	YES	YES
# of Tracts	15210	15210	15210	15210

Table A4. Impact of Opportunity Zones on Private Investment: Include All Eligible Tracts

This table shows the impact of the Opportunity Zone policy on local private investments with all the eligible census tracts included (both low-income communities (LIC) and non-LIC but contiguous tracts). The dependent variables are the natural logarithm of the one plus the number of private investment deals invested in census i and year t (*Ln(Num_Inv+1)*) and the amount of private investment deals invested (*Ln(Amount_Inv+1)*). *OZ* is an indicator that takes a value of one if the tract was designated as an Opportunity Zone (OZ) and zero if it was eligible but not designated. *Post* is a dummy that equals zero prior to 2018 and one afterwards. Control variables include the population, median income, poverty rate, percentage of white people, and unemployment rate of a census tract in a given year. I also control for year and county fixed effects. Standard errors are clustered at the county level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) *Ln(Num_Inv+1)*	(2) *Ln(Num_Inv+1)*	(3) *Ln(Amount_Inv+1)*	(4) *Ln(Amount_Inv+1)*
*OZ*Post*	0.010***	0.010***	0.143***	0.144***
	(0.002)	(0.002)	(0.031)	(0.031)
Population		0.020***		0.257**
		(0.008)		(0.113)
Median_Income		0.011**		0.134*
		(0.005)		(0.080)
Median_Age		-0.001		0.019
		(0.007)		(0.111)
%White		0.000**		0.005***
		(0.000)		(0.002)
%Black		-0.000		0.000
		(0.000)		(0.003)
Poverty_Rate		0.000		0.003*
		(0.000)		(0.002)
Unemp_Rate		-0.000		-0.000
		(0.000)		(0.002)
%NoHighSchool		0.000		0.002
		(0.000)		(0.002)
Constant	0.043***	-0.253***	0.626***	-3.426**
	(0.000)	(0.092)	(0.002)	(1.366)
Observations	205,876	205,800	205,876	205,800
R-squared	0.739	0.739	0.580	0.580
Tract FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

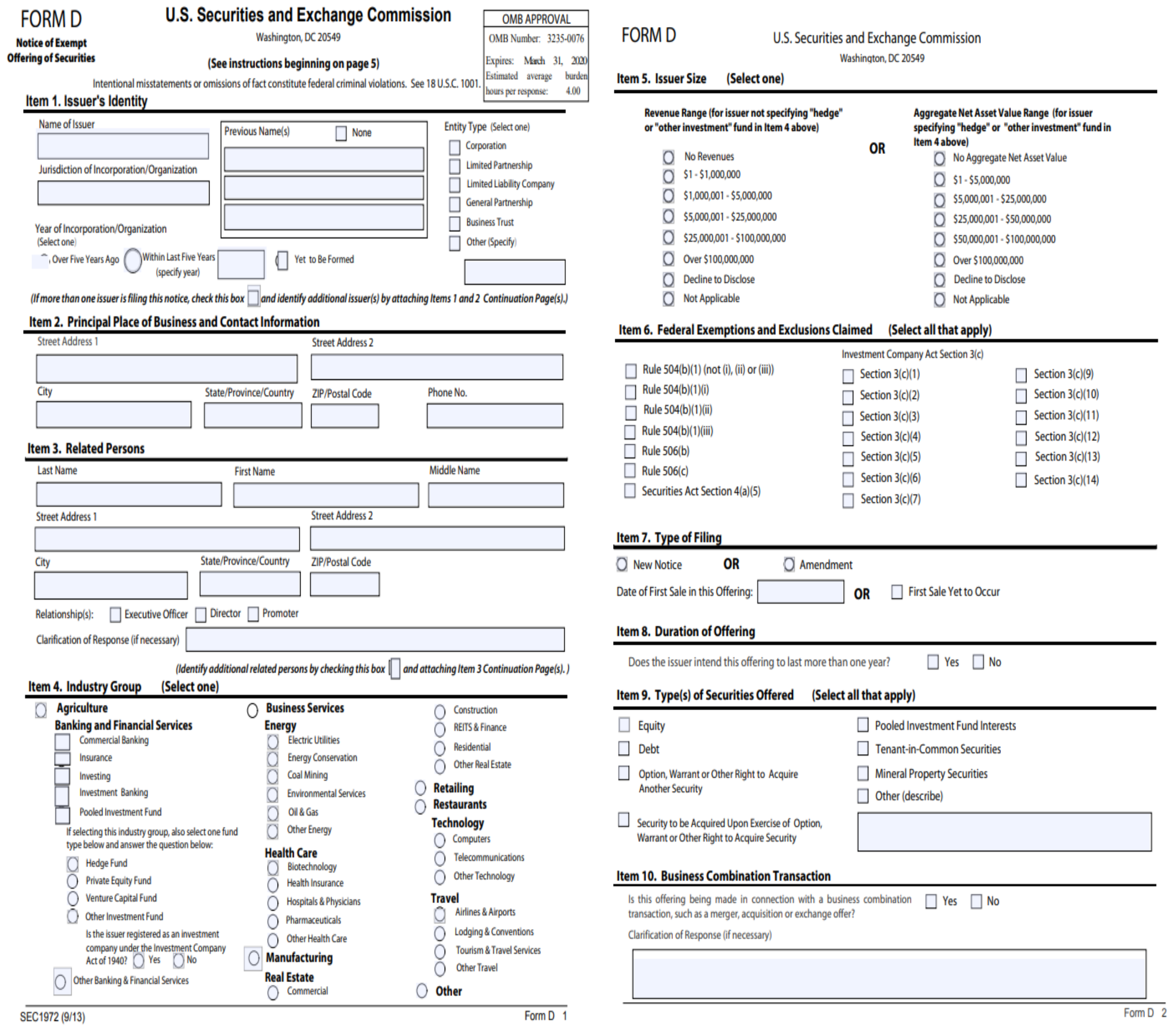
Table A5. Changes in Local Establishments by Employment Size

This table shows the impact of the Opportunity Zone policy on changes in the number of establishments by zip code. I collect the number of establishments by zip codes from the Zip Codes Business Patterns from the Census Bureau. The dependent variables are the natural logarithm of the absolute value of the changes in the number of establishments in zip code i and year t ($sign(Chg\_Estab)*Ln(|Chg\_Estab.|)$ by the size of employment. The dependent variable in Column (1) is the number of establishments of all sizes while it is the number of establishments when the size of employment is less than 10, from 10 to 49, from 50 to 99, and equal or more than 100 in Columns (2) to (5), respectively. *OZ%* is a continuous variable that equals the percentage of population in a zip code that resides in Opportunity Zones. *Post* is a dummy that equals zero prior to 2018 and one afterwards. Control variables include the population, median income, poverty rate, percentage of white people, and unemployment rate of a census tract in a given year. I also control for year and census tract fixed effects. Standard errors are clustered at the county level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1) *All Emp. Sizes*	(2) *Emp.<10*	(3) *10≤Emp.<50*	(4) *50≤Emp.<100*	(5) *Emp.≥100*
*OZ%*Post*	-0.082***	-0.075***	-0.077***	0.006	0.008
	(0.023)	(0.023)	(0.018)	(0.010)	(0.010)
OZ%	0.034	0.019	0.047***	-0.038***	-0.018***
	(0.021)	(0.018)	(0.012)	(0.007)	(0.006)
Population	0.217***	0.161***	0.109***	-0.043***	-0.050***
	(0.014)	(0.013)	(0.005)	(0.002)	(0.002)
Med_Income	0.513***	0.430***	0.178***	0.072***	0.056***
	(0.059)	(0.055)	(0.025)	(0.016)	(0.012)
%White	-0.002**	-0.001*	-0.001*	-0.000	0.000
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
Poverty_Rate	0.002	0.002	0.003***	0.000	0.000
	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)
Unemp_Rate	-0.006***	-0.003	-0.005***	0.001	-0.000
	(0.002)	(0.002)	(0.001)	(0.000)	(0.001)
Constant	-7.443***	-6.140***	-3.006***	-0.473***	-0.208*
	(0.650)	(0.619)	(0.264)	(0.166)	(0.123)
Observations	122,989	122,989	122,989	122,989	122,989
R-squared	0.151	0.101	0.056	0.038	0.069
County FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

Figure A1. 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.[1] 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, 2020
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

Chapter 5

Appendix for Chapter 2

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 (NW_{state}) 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 ($NW_{state,category}$): (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 ($Income_{state}$) in 2011. I also calculate the average of the income generated from the five categories ($Income_{state,category}$) 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 ($(\frac{NW}{Income})_{state,category}$).

Step 3: Using the net-worth-to-income ratios obtained from Step 2 multiplied by the ZIP-code income generated from each type of assets ($Income_{zip,category}$), I get the household net worth for each type of assets at the ZIP-code level as illustrated below:

$$NW_{zip,category} = \left(\frac{NW}{Income}\right)_{state,category} * Income_{zip,category}$$

Adding up the net worth for the five types of assets, I obtain the mean value of net worth at the ZIP code level (NW_{zip}). 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.[1]

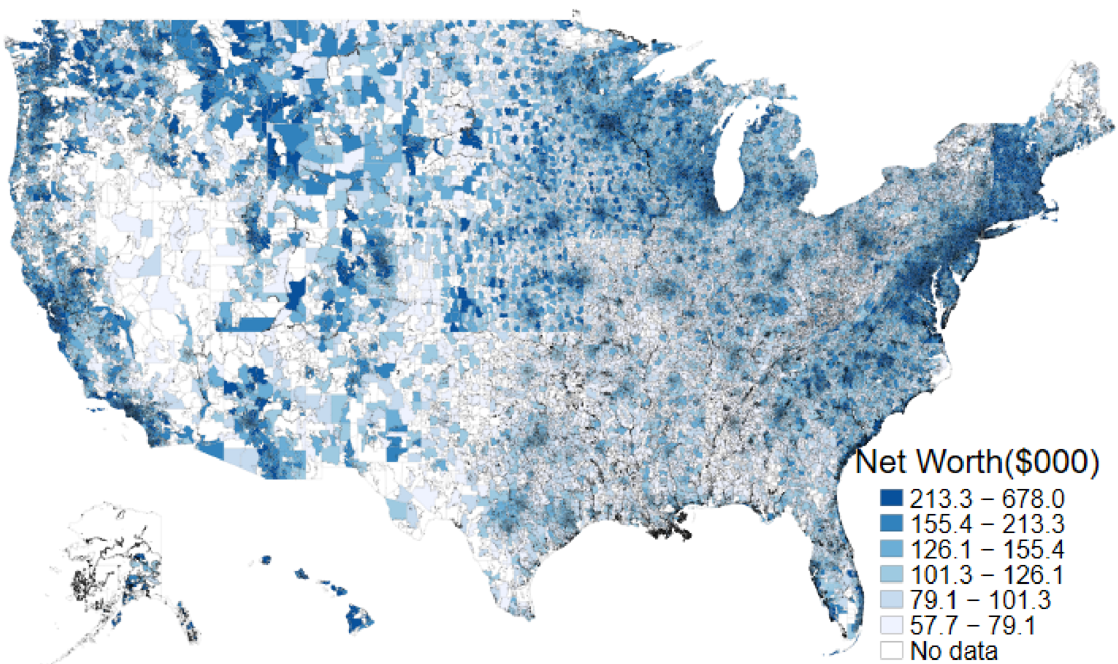
Figure B1 shows the geographic variance of the estimated average net worth across U.S. cities in 2011. The darker the color represents a higher net worth in a city. One can observe that the net worth in large cities along the east coast and west coast is relatively higher. Cities in Colorado and Illinois also enjoy high net worth.

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 (2019) 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. 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

[1]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.

Figure B1. Geographical Variation of the Net Worth in 2011

This figure 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.



these changes may due to other state-level or macro shocks. My approach, using the city-level mean HV/NW ratio, although may generate concerns discussed and addressed in Section 2.5.2, 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 future performances, therefore, provides more direct evidence of the impact of the regulation change compared to their paper.

B Additional Tests

Figure B1. 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.

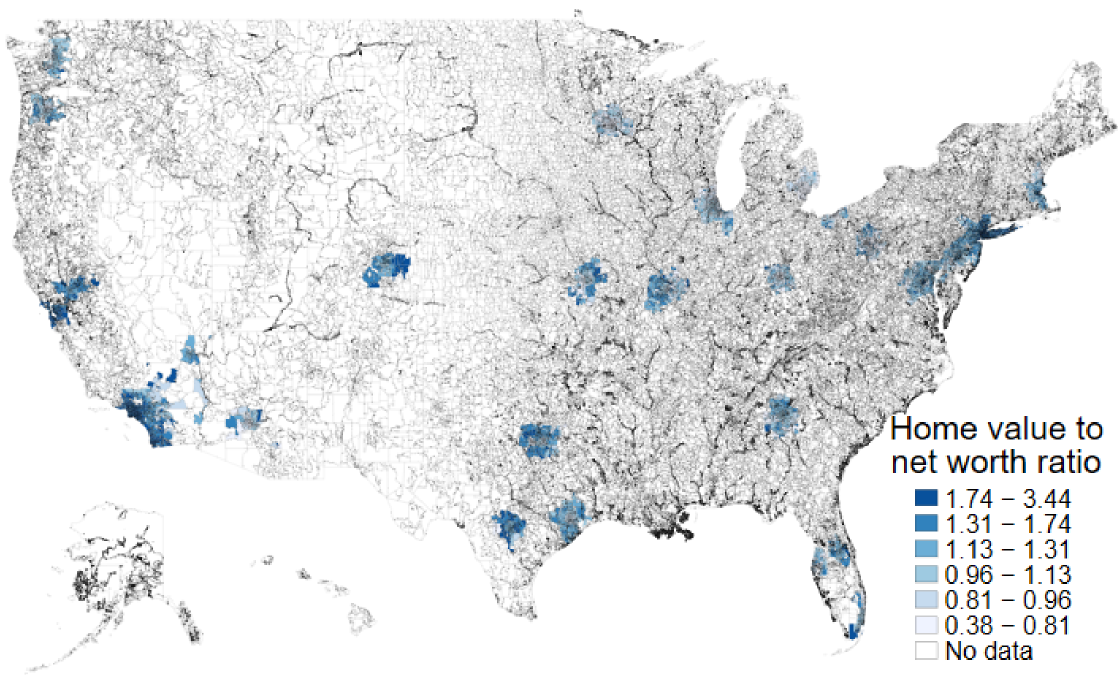
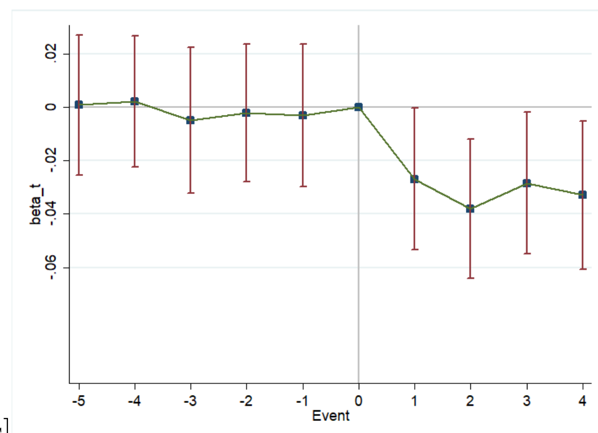


Figure B2. Plot of Coefficients Around the Event Time

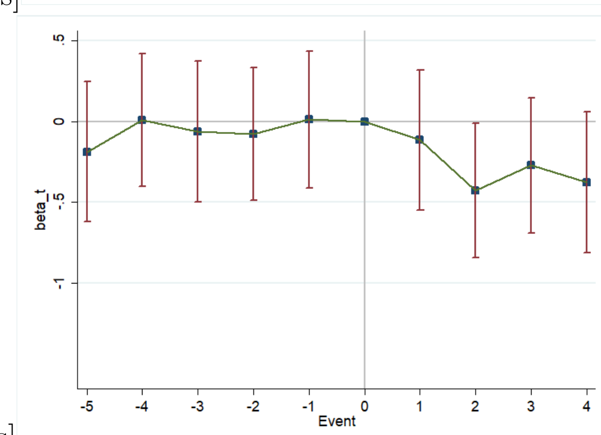
The figures show the coefficients plot around the regulation change by estimating the following model:

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

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.



[Number of Angel Investments]



[Amount of Angel Investments]

Table B1. Summary Statistics on the Distribution of Sample Firms by Age Group and by State

This table shows the distribution of the sample firms by age group and by state. Panel A shows the distribution of sample firms by age group: founded for less than 3 years, from 3 to 5 years, and above 5 years. Panel B displays the geographical distribution of the sample firms with states that have more than 1% of sample firms shown individually and the rest states shown jointly as "other states." The first column shows the age group or the state abbreviation. The second column shows the number of firms. The third and fourth columns show the percentage and cumulative percentage, respectively.

Panel A: Age distribution of sample firms			
Age group	Freq.	Percent	Cum. Percent
Less than 3	23,864	57.34	57.34
From 3 to 5	6,222	14.95	72.29
More than 5	11,531	27.71	100.00

Panel B: Geographical distribution of sample firms			
State	Freq.	Percent	Cum. Percent
CA	10,268	23.81	23.81
NY	3,855	8.94	32.75
TX	2,999	6.95	39.70
MA	2,562	5.94	45.64
WA	2,035	4.72	50.36
FL	1,976	4.58	54.94
CO	1,663	3.86	58.80
IL	1,279	2.97	61.77
PA	1,247	2.89	64.66
NC	1,041	2.41	67.07
GA	943	2.19	69.26
AZ	898	2.08	71.34
VA	855	1.98	73.32
MD	838	1.94	75.26
NJ	813	1.89	77.15
MN	785	1.82	78.97
OH	734	1.70	80.67
CT	719	1.67	82.34
UT	651	1.51	83.85
OR	643	1.49	85.34
TN	569	1.32	86.66
NV	527	1.22	87.88
MI	479	1.11	88.99
IN	443	1.03	90.02
Other states	4,301	9.98	100.00

Table B2. 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 $ln(Amount+1)$, 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 2.4.2. 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)*
$ln(HV/NW) * Post$	-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 B3. 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 2.4.2. 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 B4. 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 2.4.2. 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
$ln(HV/NW) * Post$	-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. 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 2.4.2. 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
$ln(HV/NW) * Post$	-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. 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. Specifically, 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. 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. 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 2.4.2. 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/NW_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. 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 2.2. 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 2.4.2. 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)*
$ln(HV/NW) * Post$	-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 B8. 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 2.2. 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 2.4.2. 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*
$ln(HV/NW) * Post$	-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 B9. Coefficient Estimates for the Cost-Benefit Analysis

This table shows the coefficient estimates for the cost-benefit analysis in section 2.9. I use the empirical specification as illustrated by equation (2.4). 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 2.4.2. 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

Table B10. Impact on Local Angel Financing Using Alternative Time Units

This table shows the estimation of the regulation change on local angel financing using alternative time units (year and quarter). *ln(Num+1)* (*ln(Amount+1)*) is the natural logarithm of one plus the number (amount) of angel investments in city i and period t. Columns (1) and (2) show the results when the time unit is set to be annual. Columns (3) and (4) show the results when the time unit is set to be quarterly. $ln(HV/NW)$ 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 2.4.2. I also control for time and city fixed effects. In all regressions. Standard errors are double-clustered at the city level and at the time level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
Time Unit:	Year		Quarter	
	ln(Num+1)	*ln(Amount+1)*	*ln(Num+1)*	*ln(Amount+1)*
*ln(HV/HW)*Post*	-0.042***	-0.362*	-0.019***	-0.205***
	-0.009	-0.156	-0.005	-0.061
Population	0.004	0.241	0.007	0.373
	-0.13	-2.041	-0.045	-0.701
Income_per_person	0.056	0.979	0.032	0.523
	-0.049	-0.865	-0.049	-0.499
Home_value	0.022	1.266	-0.040*	-0.364
	-0.046	-0.671	-0.021	-0.293
Constant	-0.473	-22.66	0.234	-2.411
	-1.517	-22.56	-1.068	-11.369
Observations	19,107	19,107	76,428	76,428
R-squared	0.772	0.478	0.647	0.406
City FE	YES	YES	YES	YES
Semi-annual FE	YES	YES	YES	YES
# of cities	3822	3822	3822	3822

Chapter 6

Appendix for Chapter 3

A Additional Tests

Table C1. First-Stage of Switching Regressions

This table shows the results of the first stage of the regressions. The dependent variable is whether or not a firm receives angel financing (*Angel Backing Dummy*) and the independent variables are the natural logarithm of firm age (*lnage*) and firm sales (*lnsales*), and our instruments: the dummy of whether or not a firm's headquarter is located in a state that has an active angel tax credit program (*ATC*), and the average past returns of limited partners in the firm-headquarter state (*LPR*). We also include dummies for the year of the first round of financing and the industry of the firm. The standard errors are clustered at the two-digit SIC code level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent Variable	*Angel Backing Dummy*
lnage	-0.347***
	(0.019)
lnsales	-0.021***
	(0.002)
LPR	-0.168**
	(0.069)
ATC	0.139***
	(0.040)
Constant	0.276***
	(0.099)
Observations	5,569
Year	Yes
Industry	Yes

Table C2. Robustness Tests: Successful Exits in case of 1st Investment Rounds having either VCs or Sophisticated Angel Investors (IV Analysis)

This table shows the results of the IV analysis of the impact of investor composition on entrepreneurial firms' successful exits in the subsequent years for a subsample of first-investment rounds containing either VCs or at least one sophisticated angel investor. We define angel groups and serial angel investors as sophisticated angel investors. A serial angel investor for a firm-investment round is an angel investor that has made at least one angel investment in a different firm in the past. ATC is the IV which is a dummy variable that equals one if the state where a firm is located in has an active angel tax credits program and zero otherwise. LPR is the IV which proxies for the returns of the limited partners in the past three years. Column (1) shows the first-stage of the IV analysis. In Column (2)-(4), the dependent variables are dummy variables representing whether a firm has gone public (IPO), has been acquired (Acq), or has either been acquired or gone public ($Exit$) in the years after its first-round of financing, respectively. *1st-round_angel%* equals the fraction of angel investment in the first round of financing. We control for the natural logarithms of firm age (*lnage*) and firm sales (*lnsales*) in the year of the first round. We also include the year that firms receive their first-round of financing and the two-digit SIC code of firms' primary industry. Constants are suppressed. We also report Kleibergen-Paap rk Wald F statistic. The standard errors are clustered at the two-digit SIC code level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Variables	(1) 1st-stage	(2) *IPO*	(3) *Acq*	(4) *Exit*
LPR	-0.090***			
	(0.019)			
ATC	0.045***			
	(0.016)			
1st_round_angel%		-0.175	-0.685**	-0.857**
		(0.133)	(0.343)	(0.404)
lnage	-0.068***	-0.014*	-0.033***	-0.046***
	(0.008)	(0.007)	(0.012)	(0.013)
lnsales	-0.005***	0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.004)	(0.005)
Observations	3,183	3,183	3,183	3,183
R-squared	.137	-	-	-
F-stat	15.727	-	-	-
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes

Table C3. Robustness Tests: Sales and Employment Growth in case of 1st Investment Rounds having either VCs or Sophisticated Angel Investors (IV Analysis)

This table shows the results of the IV analysis of the impact of investor composition on entrepreneurial firms' sales and employment in the subsequent years for a subsample of first-investment rounds containing either VCs or at least one sophisticated angel. We define angel groups and serial angel investors as sophisticated angel investors. A serial angel investor for a firm-investment round is an angel investor that has made at least one angel investment in a different firm in the past. *ATC* is the IV which is a dummy variable that equals one if the state where a firm is located in has an active angel tax credits program and zero otherwise. *LPR* is the IV which proxies for the returns of the limited partners in the past three years. Column (1) shows the first-stage of the IV analysis. In Column (2)-(4), the dependent variables are the annual growth rates of sales in the first, second, and third year after its first-round of financing (*Sales_growth (Year 0 to 1)*, *Sales_growth (Year 1 to 2)*, and *Sales_growth (Year 2 to 3)*), respectively. In Column (5)-(7), the dependent variables are the annual growth rates of employment in the first, second, and third year after its first-round of financing (*Employment growth (Year 0 to 1)*, *Employment growth (Year 1 to 2)*, and *Employment growth (Year 2 to 3)*), respectively. *1st-round_angel%* equals the fraction of angel investment in the first round of financing. We control for the natural logarithms of firm age (*lnage*) and firm sales (*lnsales*) in the year of the first round. We also include the year that firms receive their first-round of financing and the two-digit SIC code of firms' primary industry. Constants are suppressed. We also report Kleibergen-Paap rk Wald F statistic. The standard errors are clustered at the two-digit SIC code level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Sales Growth			*Employment Growth*	
Variables	1st-stage	Year 0 to 1	Year 1 to 2	Year 2 to 3	Year 0 to 1	Year 1 to 2	Year 2 to 3
LPR	-0.069**						
	(0.030)						
ATC	0.048***						
	(0.018)						
1st_round_angel%		-1.637**	1.250	-1.432	-1.128**	0.400	-1.112
		(0.678)	(1.004)	(1.175)	(0.569)	(0.555)	(0.893)
lnsales	-0.041***	-0.210***	-0.002	-0.015**	-0.137***	-0.002	-0.012**
	(0.008)	(0.036)	(0.005)	(0.007)	(0.025)	(0.003)	(0.005)
lnage	-0.047***	0.009	0.072*	-0.101**	-0.002	0.007	-0.094***
	(0.009)	(0.052)	(0.040)	(0.041)	(0.033)	(0.028)	(0.027)
Observations	2,243	2,243	2,382	2,215	2,244	2,385	2,218
R-squared	.152	-	-	-	-	-	-
F-stat	6.623	-	-	-	-	-	-
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table C4. Robustness Tests: Patent Quantity and Quality in case of 1st Investment Rounds having either VCs or Sophisticated Angel Investors (IV Analysis)

This table shows the results of the IV analysis of the impact of investor composition on entrepreneurial firms' patenting in the subsequent years for a subsample of first-investment rounds containing either VCs or at least one sophisticated angel. We define angel groups and serial angel investors as sophisticated angel investors. A serial angel investor for a firm-investment round is an angel investor that has made at least one angel investment in a different firm in the past. *ATC* is the IV which is a dummy variable that equals one if the state where a firm is located in has an active angel tax credits program and zero otherwise. *LPR* is the IV which proxies for the returns of the limited partners in the past three years. Column (1) shows the first-stage of the IV analysis. In Column (2)-(4), the dependent variables are the natural logarithm of one plus the number of patents applied in one, two, and three years after its first-round of financing adjusted for the truncation bias (*Patents (1 year)*, *Patents (2 years)*, and *Patents (3 years)*), respectively. In Column (5)-(7), the dependent variables are the natural logarithm of one plus the number of citations of patents applied in one, two, and three years after its first-round of financing adjusted for the truncation bias (*Citations (1 year)*, *Citations (2 years)*, and *Citations (3 years)*), respectively. *1st-round_angel%* equals the fraction of angel investment in the first round of financing. We control for the natural logarithms of firm age (*lnage*) and firm sales (*lnsales*) in the year of the first round. We also include the year that firms receive their first-round of financing and the two-digit SIC code of firms' primary industry. Constants are suppressed. We also report Kleibergen-Paap rk Wald F statistic. The standard errors are clustered at the two-digit SIC code level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Patents			*Citations*	
Variables	1st-stage	1 Year	2 Years	3 Years	1 Year	2 Years	3 Years
LPR	-0.090***						
	(0.019)						
ATC	0.045***						
	(0.016)						
1st_round_angel%		-0.623**	-1.317	-1.481	-0.008*	-0.022	-0.030
		(0.283)	(1.000)	(1.169)	(0.004)	(0.015)	(0.022)
lnage	-0.068***	-0.158***	-0.423***	-0.507***	-0.002***	-0.008***	-0.011***
	(0.008)	(0.034)	(0.108)	(0.123)	(0.000)	(0.002)	(0.002)
lnsales	-0.005***	-0.007***	-0.012***	-0.009	-0.000**	-0.000**	-0.000**
	(0.001)	(0.002)	(0.004)	(0.006)	(0.000)	(0.000)	(0.000)
Observations	3,183	3,183	3,183	3,183	3,183	3,183	3,183
R-squared	.137	-	-	-	-	-	-
F-stat	15.727	-	-	-	-	-	-
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table C5. Robustness Tests: Inventor Inflows in case of 1st Investment Rounds having either VCs or Sophisticated Angel Investors (IV Analysis)

This table shows the results of the IV analysis of the impact of investor composition on inventor inflows in entrepreneurial firm in the subsequent years for a subsample of first-investment rounds containing either VCs or at least one sophisticated angel. We define angel groups and serial angel investors as sophisticated angel investors. A serial angel investor for a firm-investment round is an angel investor that has made at least one angel investment in a different firm in the past. ATC is the IV which is a dummy variable that equals one if the state where a firm is located in has an active angel tax credits program and zero otherwise. LPR is the IV which proxies for the returns of the limited partners in the past three years. Column (1) shows the first-stage of the IV analysis. In Column (2)-(4), the dependent variables are the net inflow of inventors in one, two, and three years after its first-round of financing (*Net Inflow of Inventors (1 Year)*, *Net Inflow of Inventors (2 Years)*, and *Net Inflow of Inventors (3 Years)*), respectively. The dependent variables in columns (5) to (7) are the net inflow of the inventors with the top-quartile number of citations in one, two, and three years after its first-round of financing (*Net Inflow of Top 25% Inventors (1 Year)*, *Net Inflow of Top 25% Inventors (2 Years)*, and *Net Inflow of Top 25% Inventors (3 Years)*), respectively. *1st-round_angel%* equals the fraction of angel investment in the first round of financing. We control for the natural logarithms of firm age (*lnage*) and firm sales (*lnsales*) in the year of the first round. We also include the year that firms receive their first-round of financing and the two-digit SIC code of firms' primary industry. Constants are suppressed. We also report Kleibergen-Paap rk Wald F statistic. We also report Kleibergen-Paap rk Wald F statistic. The standard errors are clustered at the two-digit SIC code level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Net Inflow of Inventors			*Net Inflow (Top 25% inventors)*		
Variables	1st-stage	1 Year	2 Years	3 Years	1 Year	2 Years	3 Years
LPR	-0.090***						
	(0.019)						
ATC	0.045***						
	(0.016)						
1st_round_angel%		-0.490**	-0.669**	-0.491*	-0.113	-0.149*	-0.112
		(0.207)	(0.277)	(0.254)	(0.087)	(0.082)	(0.108)
lnage	-0.068***	-0.079***	-0.131***	-0.135***	-0.022***	-0.040***	-0.042***
	(0.008)	(0.018)	(0.018)	(0.014)	(0.008)	(0.010)	(0.011)
lnsales	-0.005***	-0.001	-0.002	-0.000	-0.000	-0.001	-0.000
	(0.001)	(0.002)	(0.003)	(0.002)	(0.000)	(0.000)	(0.001)
Observations	3,183	3,183	3,183	3,183	3,183	3,183	3,183
R-squared	.137				-	-	-
F-stat	15.727	-	-	-	-	-	-
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table C6. Robustness Tests: Successful Exits in case of 1st Investment Rounds having either VCs or Angel Groups only (IV Analysis)

This table shows the results of the IV analysis of the impact of investor composition on entrepreneurial firms' successful exits in the subsequent years for a subsample of first-investment rounds containing either VCs or angel groups only. APR is the IV which is a dummy variable that equals one if the state where a firm is located in has an active angel tax credits program and zero otherwise. LPR is the IV which proxies for the returns of the limited partners in the past three years. Column (1) shows the first-stage of the IV analysis. In Column (2)-(4), the dependent variables are dummy variables representing whether a firm has gone public (IPO), has been acquired (*Acq*), or has either been acquired or gone public (*Exit*) in the years after its first-round of financing, respectively. *1st-round_angel%* equals the fraction of angel investment in the first round of financing. We control for the natural logarithms of firm age (*lnage*) and firm sales (*lnsales*) in the year of the first round. We also include the year that firms receive their first-round of financing and the two-digit SIC code of firms' primary industry. Constants are suppressed. We also report Kleibergen-Paap rk Wald F statistic. The standard errors are clustered at the two-digit SIC code level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Variables	(1) *1st-stage*	(2) *IPO*	(3) *Acq*	(4) *Exit*
LPR	-0.117***			
	(0.017)			
ATC	0.038**			
	(0.016)			
1st_round_angel%		-0.131	-0.762**	-0.890**
		(0.127)	(0.302)	(0.361)
lnage	-0.010	-0.008	0.013	0.007
	(0.007)	(0.006)	(0.017)	(0.017)
lnsales	-0.005***	0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.003)	(0.004)
Observations	2,724	2,724	2,724	2,724
R-squared	.097	-	-	-
F-stat	30.371	-	-	-
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes

Table C7. Industry Subsample Analysis: Successful Exits (IV Analysis)

This table shows the results of the second stage of the IV analysis of the impact of investor composition on entrepreneurial firms' successful exits in the subsequent years for subsamples of firms that are in VC-specific industries and other industries. VCs tend to invest in Hitech, manufacturing, and healthcare industries. We classify the above industries using the Fama-French 10 industry classification. Hitech, manufacturing, and healthcare industries are classified as 5, 3, and 8, respectively in the Fama-French 10 industry categories. Column (1) shows the first-stage of the IV analysis. In Column (2)-(4), the dependent variables are dummy variables representing whether a firm has gone public (IPO), has been acquired (*Acq*), or has either been acquired or gone public ($Exit$) in the years after its first-round of financing, respectively. *1st-round_angel%* equals the fraction of angel investment in the total amount invested in the first round of financing. We control for the natural logarithms of firm age (*lnage*) and firm sales (*lnsales*) in the year of the first round. We also include the year that firms receive their first-round of financing and the two-digit SIC code of firms' primary industry. Constants are suppressed. The standard errors are clustered at the two-digit SIC code level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Other Industries			HiTech + Manufacturing + Healthcare		
Variables	*IPO*	*Acq*	*Exit*	*IPO*	*Acq*	*Exit*
1st_round_angel% (instrumented)	-0.080	-0.593**	-0.762***	-0.550**	-0.575*	-0.921**
	(0.141)	(0.258)	(0.285)	(0.261)	(0.294)	(0.405)
lnage	-0.004	-0.025	-0.040	-0.021	-0.005	-0.016
	(0.013)	(0.024)	(0.027)	(0.013)	(0.008)	(0.010)
lnsales	0.001	-0.003	-0.004	0.001	0.001	0.002
	(0.001)	(0.003)	(0.003)	(0.001)	(0.002)	(0.002)
Observations	2,885	2,885	2,885	2,698	2,698	2,698
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes

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