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Essays in Venture Capital and Banking

By

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Professor Nancy Wallace, Chair
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Abstract

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This thesis explores the roles of two significant financial intermediaries in the financial markets: venture capital firms, which facilitate equity financing, and banks, which provide debt financing. Initially, the thesis concentrates on the entrepreneurial financing market, examining the factors that influence venture capital firms' decision-making processes when selecting investments. Subsequently, as a complement to equity financing for startups, the thesis delves deeper into the role of banks in offering venture debt. Recognizing that deposits serve as the foundational basis for banks to issue debt, the final part of the thesis investigates competition in the deposit market and the methods banks employ to determine deposit rates.

Venture capital plays a critical role in entrepreneurial finance, with networks prominently featuring in venture capital (VC) markets. Chapter 1, **Networks in Venture Capital Markets**, explores the role and channels of networks in VC investments. The paper examines alumni networks between VCs and startups, using a new partner with new alumni networks joining a venture capital company as a plausibly exogenous shock to the VCs' alumni networks to identify the network effects on VC investments. New alumni ties lead to a significant increase in investments in startups with alumni founders. However, startups with new alumni ties perform worse, with higher failure rates and lower acquisition rates, IPO rates, and portfolio returns. This highlights VCs' overemphasis on alumni networks and potential inefficiencies in their investment strategies. To understand the underlying mechanism, the paper frames network effects through two channels: improving information (information channel) and inducing homophily-based favoritism (preference channel). Supplementary tests suggest that the preference channel outweighs the information channel, leading to VCs' capital misallocation.

As a complementary financing option to equity financing for startups, Chapter 2, **Signaling in the Venture Debt Financing**, sheds light on the interaction between startup financing and banks. Venture debt financing for startups has experienced steady growth in recent

years, prompting this paper to focus on understanding the rise of venture debt through a signaling channel. We model and document the role of venture debt as a positive signal in startup financing under asymmetric information, which increases the probability of a firm receiving future venture capital (VC) funding, thereby reducing the risk of venture debt and encouraging lending to startups. However, VCs' reliance on this signal induces over-investment in lower-quality startups, as they interpret venture debt as a positive signal and conduct less thorough due diligence. Data show that startups invested after obtaining venture debt perform worse than others. This paper offers a unique explanation for the rise of venture debt and highlights the efficiency loss induced by venture debt.

Chapter 3, **Banks' Rate Setting Behavior and Regional Distribution of Deposit Rates**, turns to the competition in the deposit market, documenting banks' differential deposit rate-setting behavior associated with bank size. Large banks set uniform deposit rates that ignore local market competition, while small banks set higher rates and respond to local market conditions. Despite large banks setting lower deposit rates, they hold the majority of deposits. We find that the differential rate setting behavior is due to customer segmentation between large and small banks. Large banks target more populated areas with higher-income populations who value complex financial services and are less sensitive to low deposit rates. In contrast, small banks serve rural regions where customers prioritize deposits and are more sensitive to deposit rates.

To my parents for their unconditional love and support.

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Chapter 1

Networks in Venture Capital Markets

1.1 Introduction

The venture capital (VC) market is a crucial financing source that incubates entrepreneurial ideas into successful companies, and VC companies serve as gatekeepers to winnow a pool of investment opportunities down to a small number of potential deals. Therefore, understanding how VC companies make decisions is crucial. An emerging strand of the literature answers this question by exploring the role of networks. Despite abundant empirical evidence on the importance of networks in the VC industry (Hochberg et al., 2007; Gompers et al., 2020; Nanda et al., 2020), little is known about whether VCs' reliance on networks is efficient. On the one hand, since the VC market features high risk and severe asymmetric information, networks serve as information sources for VCs to reduce asymmetric information and generate deal flows. On the other hand, networks can induce homophily-based biases or preferences that distort capital allocation.

In this paper, I empirically estimate the causal effect of networks between VC companies and startups on investment decisions and performance, and disentangle the information channel and preference channel effects. The empirical analysis is challenging in two aspects, commonly faced by the literature. First, the measure of networks is limited. Most of the literature studies VC syndication networks (Hochberg et al., 2007, 2010; Gompers et al., 2016) in which VCs are connected through co-investments. I supplement the literature by focusing on alumni networks between VCs and startups by constructing a novel dataset on individuals' working experience and educational backgrounds, and merging it with VC market data. There are two advantages to studying alumni networks. First, the measure of alumni networks is direct and less dependent on investment outcome variables. Second, VC companies treat alumni networks as an essential information source for deal sourcing and private information, with over 41% of deals in my data sample involving alumni networks. Additionally, universities actively manage and develop alumni networks and facilitate matching between investors and alumni founders. Understanding alumni networks' effects provides insight into the impact of networks in the VC market in general.

The second empirical challenge is that the formation of networks is endogenous to unobserved characteristics of investors and founders. To address this endogeneity, I take a novel approach and explore the shocks to alumni networks that arise after a new general partner joins a VC company. For example, when a new partner from university A enters a VC company consisting of partners from university B, startups with founders from university A form alumni ties with the VC company after the arrival of the new partner. By comparing the differences between alumni startups of university A and other startups before and after the new partner's arrival, I can estimate the causal effects of these networks on VC investments. The identification assumption is that conditional on control variables, new alumni firms (alumni firms of university A) and other firms share parallel trends, independent of alumni status.

I start my empirical analysis by testing the network effects on investment choice. Since data only observe actual investments, following Gompers et al. (2016) and Hegde and Tumlinson (2014), I construct counterfactual pairs that VCs could have considered investing in but did not, by matching startups based on location, industry, year, and funding stages. The difference-in-difference estimation shows that the investments in new alumni startups increase significantly by 8.21% after hiring new partners. Event-study plots depict no pre-trends, supporting the identification assumption of parallel trends. To address the concern of strategic hiring, meaning that VC companies may hire a new partner due to changes in future investment plans that correlate with alumni status, for robustness check, I only use the hiring events following the exit of current partners, where the purpose of hiring is for replacement. The results are robust to the main regressions. Additional placebo tests examine the effects of network changes due to non-partner hiring. Since junior employees are not decision-makers, hiring non-partners should not affect investment decisions. However, if strategic hiring dominates, VCs would tend to hire junior employees with similar educational backgrounds as the new partners, making junior hiring impact the investment choice. The placebo tests find no evidence that junior alumni affect the investments, alleviating the concern of strategic hiring.

Next, I explore the heterogeneous network effects on the market, university, VC characteristics, and founder demographics. First, alumni networks play a more influential role in early financing rounds which feature higher risks and more asymmetric information, increasing new alumni investments by 12.52%. Second, alumni of less prestigious universities benefit the most from the networks. As these founders have smaller market share and less information available in the market, VCs rely more on newly acquired networks. Third, younger and smaller VCs that have relatively limited networks increase their investments in new alumni startups the most. Fourth, female and black founders who are less represented in the market are more influenced by alumni networks. The heterogeneous effects indicate that network effects on investment are stronger under greater asymmetric information, implying that the information channel takes effect when VC companies screen potential deals.

Since the information and preference channels both increase the investments in connected firms, tests on investment performance are required to distinguish the relative importance of the two channels. Specifically, I compare the performance of new alumni startups funded before and after hiring the new partner. If the information channel dominates the network effects and helps VCs select startups of better quality, the performance of newly connected startups should be better. However, the treated startups funded after hiring experience a 7.7% increase in failure rates, a 6.8% decrease in acquisition rates, and a 21.46% decrease in IPO rates. The return of a portfolio consisting of newly connected startups also declines by 24.87%. These negative effects are stronger when alumni VCs are leading investors in the deals, where they are the major decision-maker. The performance results imply that the impact of the preference channel overwhelms the information channel, and VC companies allocate more funding to alumni firms than optimal. At the same time, they miss out on promising investments in non-alumni firms.

To disentangle and quantify the impact of information and preference channels, I turn to structural estimation of a model framing the network effects through better private information and favoritism to connected firms. Choosing investment probability and average investment returns as moments to match, I first estimate the information and preference parameters by distance minimization. The estimation indicates that VC companies do not have a strong preference for firms and receive noisy signals without alumni networks, but obtain high preference and better information for alumni firms. Next, I consider counterfactuals without information improvement or preference changes, finding that although the information and preference channels both take effect, shutting down the preference channel induces larger changes and substantial improvement in investment performance. I also compare the network effects of top and non-top universities. The results indicate that VC companies form a preference for startups from top universities even without alumni ties, resulting in a weaker impact from the preference channel.

Finally, I investigate VC hiring strategy for managing alumni networks. Is it better to hire a new partner with a similar educational background as incumbent partners to enhance existing alumni networks (*additional hiring*) or a partner from a different university to expand new alumni networks (*first hiring*)? Results show that *first hiring* increases investments in new alumni startups and worsens the investment performance, while *additional hiring* has a limited impact on investment probabilities but still has negative effects on the performance, indicating that hiring partners with diverse academic backgrounds appears to be a better choice, as VCs suffer from favoritism from both types of hiring but benefit from more information and deal flows from *first hiring*.

Literature Review. This paper contributes to three main strands of the literature. First, this paper contributes to the literature on networks in financial markets. Existing papers that study networks in the venture capital industry focus on VC syndication networks and social ties, and the majority find that better-networked VC firms experience better performance (Hochberg et al., 2007; Nahata, 2008; Hegde and Tumlinson, 2014; Gompers et al., 2020). In broader financial markets, the evidence of network effects is more controversial. On the one hand, some papers find that networks are associated with poor merger and acquisition performance (Ishii and Xuan, 2014), increased risk of corporate fraud (Khanna et al., 2015), weak firm governance, and reduced firm value (Fracassi and Tate, 2012). On the other hand, others also show that better networked sell-side analysts make better stock recommendations (Cohen et al., 2010), institutional investors with better connections to the brokers capture higher stock returns (Barbon et al., 2019), board connections induce greater value creation in M&A (Cai and Sevilir, 2012), and educational connections promote recruitment (Hacamo and Kleiner, 2022). In contemporaneous work to this paper (Garfinkel et al., 2021, most recently revised on Nov 21, 2022), the authors employ similar data to examine the alumni networks in the VC industry, focusing more on the network effects on investment choices. Due to differing setup in identification[1] and data construction[2], they find positive effects on both the probability of investment and performance.

Second, this paper adds to the literature on VCs' decision-making (Kaplan and Strömberg, 2004; Gompers et al., 2020). Rich work has documented the importance of the characteristics of founding teams in attracting VCs (Hellmann and Puri, 2002; Kaplan et al., 2009; Bernstein et al., 2017; Lyonnet and Stern, 2022). This paper documents the importance of founders' educational backgrounds in VC decision-making from the perspective of alumni networks, and measures the efficiency of alumni-based investments by disentangling the channels through which alumni networks take effect.

Third, this work closely aligns with literature on homophily-based biases and diversity in the VC industry. An extensive literature documents that VCs appear biased toward founders of the same gender (Raina, 2021; Balachandra et al., 2019; Ewens and Townsend, 2020; Gornall and Strebulaev, 2019; Hebert, 2018; Hu and Ma, 2020; Calder-Wang and Gompers, 2021), ethnic background (Gompers et al., 2016; Lyonnet and Stern, 2022), and geography (Chen et al., 2010). This paper provides supporting evidence on VC preference favoritism for alumni entrepreneurs and resource misallocation due to homophily bias.

Overview. The paper proceeds as follows. Section 1.2 introduces a model to frame the network effects through the information and preference channels. Section 1.3 describes the data and empirical strategy. Section 1.4 and 1.5 illustrates the alumni network effects on VC investment choice and performance. Section 1.6 presents the structural estimation of the model in section 1.2. Section 1.8 concludes.

1.2 Model

This section provides a stylized model to frame the network effects on investments.

Setup. This is a two-period $t = 0, 1$, static model with a risk-neutral investor v and two sets of continuum startups, one with connections to the investor, indicated by $g = 1$, and the other without, indicated by $g = 0$. Startups seek one unit of investment at $t = 0$, and have random payoffs $y_i \sim \mathcal{N}(\mu, \sigma^2)$ paying off at $t = 1$. The investor cannot observe the payoff. Instead, she receives a signal of the payoff, $s_i = y_i + \epsilon_i$, where $\epsilon_i \sim \mathcal{N}(0, (\kappa_g \sigma)^2)$. ϵ_i measures

[1]They implement partner turnover as a strategy for identification in investment analysis with a different setup, and this inditification strategy is not used in performance analysis.

[2]They do not merge the two datasets together, which results in different measures for both alumni and partner turnover.

the preciseness of the signal, and we assume that network connections improve the signal, i.e., $\kappa_{g=1} < \kappa_{g=0}$

At $t = 0$, the investor first decides on the amount of startups λ_g to search in each set with a searching cost $c(\lambda_g) = b\lambda_g^2$, and then receives noisy signals of searched startups i. She invests in startups whose signals exceed a threshold s_g, and the cost of funding is r. All payoffs realize at $t = 1$.

The ex-ante utility of investing in a startup with a signal s_i is

$$U_i = \overbrace{E\left[y_i|s_i\right]}^{\text{Expected payoff}} + \overbrace{\eta_g}^{\text{Preference}} - \overbrace{r}^{\text{Funding cost}}$$

where η_g denotes the extra utility from investing in the startups, and we assume $\eta_{g=1} > \eta_{g=0}$. In reality, η_g can be viewed as extra benefits from investing in connected startups, such as building alumni community and giving back to the university, or perceptual biases towards believing that connected firms perform better.

Objective. The investor's objective is the expected utility from total investments:

$$EU = \underbrace{\lambda_g}_{\text{Search size}} \times \underbrace{(\overbrace{E\left[y_i|s_i > s_g\right]}^{\text{Information}} + \overbrace{\eta_g}^{\text{Preference}} - r)}_{\text{Utility from investment}} \times \underbrace{Pr(s_i > s_g)}_{\text{Probability of investment}} - \underbrace{b\lambda_g^2}_{\text{Search costs}} \tag{1.2.1}$$

The investor chooses searching size λ_g and signal threshold s_g to maximize 1.2.1:

$$\begin{aligned}
\lambda_g^*, s_g^* &= \arg\max_{\lambda,s} \lambda(E\left[y_i|s_i > s\right] + \eta_g - r)Pr(s_i > s) - b\lambda^2 \\
\Longrightarrow s_g^* &= r - \eta_g + \kappa_g^2\left(r - \eta_g - \mu\right) \\
\lambda_g^* &= \frac{1}{2b}(E\left[y_i|s_i > s_g^*\right] + \eta_g - r)Pr(s_i > s_g^*) \\
&\equiv \frac{1}{2b}\pi_g^*
\end{aligned}$$

From the expression of s_g^*, it is straightforward to show that s_g^* increases in κ_g and decreases in η_g. Since the connected startup pool features lower signal noise ($\kappa_{g=1} < \kappa_{g=0}$) and higher preference ($\eta_{g=1} > \eta_{g=0}$), the signal threshold of connected startups is lower ($s_{g=1}^* < s_{g=0}^*$).

Given the optimal threshold s_g^*, the total investment is

$$\begin{aligned}
P_g &\equiv \lambda_g^* Pr(s_i > s_g^*) \\
&= \underbrace{\frac{1}{2b}(r - \eta_g - \mu)\left(\frac{\phi(x_g)}{x_g} - 1 + \Phi(x_g)\right)}_{\text{Search size}}\left(1 - \Phi\left(\underbrace{\frac{\sqrt{1+\kappa^2}}{\sigma}}_{\text{Information}}\underbrace{(r - \eta_g - \mu)}_{\text{Preference}}\right)\right)
\end{aligned}$$

where $x_g = \frac{\sqrt{1+\kappa_g^2}}{\sigma}(r - \eta_g - \mu)$, $\phi(x)$ and $\Phi(x)$ are the probability density function and cumulative distribution function of standard normal distribution, respectively.

Proposition 1 *The total investment P_g decreases in κ_g and increases in η_g. i.e. $\frac{\partial P_g}{\partial \kappa_g} < 0$, $\frac{\partial P_g}{\partial \eta_g} > 0$.*

Proposition 1 indicates that both the information channel and the preference channel induce more investments in connected startups, and they impact the results in two ways. First, conditional on searching size, better information and preference lower the screening criteria, resulting in a larger fraction of selected firms. Second, during the searching process, the investor will search until the marginal searching cost meets the expected investment utility π_g^*. Since a more precise signal and extra utility of investment both increase π_g^*, the investor will search for more connected startups, leading to disproportionately higher investments in connected firms.

Next, we explore how the effects on investment decisions vary with market volatility σ.

Proposition 2 *$\frac{\partial P_g}{\partial \kappa_g}$ decreases in σ and $\frac{\partial P_g}{\partial \eta_g}$ increases in σ. i.e. $\frac{\partial^2 P_g}{\partial \kappa_g \partial \sigma} < 0$, $\frac{\partial^2 P_g}{\partial \eta_g \partial \sigma} > 0$.*

Proposition 2 implies that when market volatility increases, the increase in investments with more precise signals of connected firms is steeper, as is the increase due to the preference for connected firms. Empirically, it predicts that the effects of both information and preference on the investment choice are stronger in markets with higher risk, such as startups in early stages or in less matured industries.

Finally, we investigate the average performance of invested startups. Specifically, the expected payoffs conditional on investment are

$$\Pi_g \equiv E\left[y_i | s_i > s_g^*\right] = \mu + \frac{\sigma}{\sqrt{1+\kappa_g^2}} \frac{\phi(x_g)}{1-\Phi(x_g)}$$

where $x_g = \frac{\sqrt{1+\kappa_g^2}}{\sigma}(r - \eta_g - \mu)$, $\phi(x)$ and $\Phi(x)$ are the probability density function and cumulative distribution function of standard normal distribution, respectively.

Proposition 3 *The performance of investments Π_g decreases in κ_g and η_g. i.e. $\frac{\partial \Pi_g}{\partial \kappa_g} < 0$ and $\frac{\partial \Pi_g}{\partial \eta_g} < 0$.*

Proposition 3 implies that information about and preference for connected startups influence performance in opposite directions. The information channel allows the investor to obtain an informational advantage and screen connected firms with higher quality, thus improving the average performance of selected connected firms. However, when the investor has a strong preference for connected firms, she will tolerate probabilistically lower quality and lower investment payoffs.

1.3 Data and Method

1.3.1 Data Source

The primary dataset is mainly composed of two parts. It starts with collecting comprehensive information on venture capital deals, including details on venture capital firms and startups involved in the deals. The second element consists of the working experience and educational background of venture capital partners and startup founders, which is central to identifying alumni networks.

The data on venture capital deals are provided by Pitchbook. Pitchbook is a data vendor owned by MorningStar, and has a growing prevalence in venture capital research studies as it has better data coverage of startup financing deals than other data sources (Ewens et al., 2022). Pitchbook collects deal terms and company information on VC-backed firms and essential information on VC companies involved in the deals. From Pitchbook, I collect data on startups founded between 2008 and 2015 with US headquarters and VC companies investing in these startups. I only focus on traditional VC companies and accelerators, excluding corporate VCs, non-profit VCs, and university VCs.

To augment Pitchbook data with individual information on startup founders and VC partners, I integrate individual-level data from CoreSignal. CoreSignal is a data provider that extracts information from websites and social media. It provides employee data consisting of over 500 million individuals, gathered by scraping LinkedIn and augmented with other data sources such as company websites. Each record reports an individual's full name, working history with company name, company website, working period, job title, and educational background with the university name, time, and degree. It also collects profile photo links. By utilizing DeepFace, a deep learning facial recognition system created by a research group at Facebook, I predict gender and race from profile photos.

I merge two datasets by matching company websites and company LinkedIn IDs. I first perform an exact match by company website URLs. For unmatched companies in Pitchbook, I search for the company name and industry on LinkedIn to obtain the unique LinkedIn ID for the company, and match them with CoreSignal by the ID. Based on merged companies, I select profiles working as a founding member in matched startups or as a general partner in merged VC companies. Table 1.1 indicates that 89.4% of the startups in the Pitchbook sample have merged with founder information, and the characteristics of the matched samples are comparable to the total Pitchbook sample. 63.2% of the VC companies have partner information in the CoreSignal dataset and matched VCs account for 82% of the deals. Matched VC companies tend to be more significant players in the market with more investment professionals and more deals. Deals made by matched VCs are representative of the whole sample and have similar average deal sizes and slightly less valuation.

I supplement the merged sample with university characteristics from the Department of Education's College Scorecard. The data reports features of US institutions of higher education, including enrollment, admission rate, average test score, tuition fees, and demographic components of cohorts. Scorecard data is merged with the core sample by the exact match of university website URLs and fuzzy name match if the URLs are missing. Since Scorecard only includes universities in the US, unmatched universities in the core sample with URLs of foreign countries are tagged as foreign universities, and the rest are tagged as unmatched.

1.3.2 Empirical Strategy

The empirical challenge of identifying the impact of alumni networks on investments is that the alumni networks are endogenous to VC investment decisions. Investors may value the characteristics of startups that correlate with alumni networks. For example, VCs may prefer investing in firms in the same location, while VC partners and entrepreneurs in the same location are more likely to be alumni. Thus a naive regression can result in an overestimation of the effect of alumni networks. To address the endogeneity problem, I explore the exogenous shocks in alumni networks when VC companies hire new partners. Specifically, when a new partner who graduated from university A arrives at a VC company controlled by alumni partners of university B, startups founded by alumni of university A obtain alumni ties with the VC company. However, the investment changes in the new alumni firms consist of the effect of alumni ties and the hiring of new partners (i.e., the change in investment style). To solely identify the alumni network effect, I implement a difference-in-difference strategy to compare the change in investments in firms with alumni ties with the new partners (new alumni group) to investments in other startups (control group). The identification assumption is that conditional on control variables, the trends of new alumni firms and other firms are comparable, independent of alumni status.

Since CoreSignal reports the year partners joined the VC companies, I can identify hiring events from the data and restrict the sample to VC companies with hiring events (hiring sample). 80% of matched VC companies hired new partners during the sample period, with a slightly larger investment team size and more investments. Deals made by VCs in the hiring sample are comparable to those of matched VCs in terms of deal size, industry, and valuation. Overall, the hiring sample is comparable to the matched VC sample.

Figure 1.1 shows the partner hiring frequency among investors. Partner hiring is infrequent, with 20% of VC companies not hiring any partners from 2010 to 2020, and 44% of investors hiring only one partner during the period. I choose the first hiring event as the event date.

1.3.3 Sample Construction

The Pitchbook deal data only observes VC-startup pairs in whitch VC companies made actual investments in the startups. However, to test the effect of alumni ties on investment decisions, the sample requires both actual deals and counterfactual pairs that VCs could have considered investing in but did not. To construct plausible pairs, based on methods in Gompers et al. (2016) and Hegde and Tumlinson (2014), for each year t, I identify a set of VC companies actively making investments in a 2-year window and a set of startups actively seeking funding. I assume startups spend at most two years attracting investments and go bankrupt if they fail (the results are robust when changing to 1-year window. Table A2). Thus, a startup is considered active if it has a successful deal or goes bankrupt within two years. After constructing the active pools, I match VC companies with startups depending on their preference for the state, industry, and startup development stages (seed rounds, early VC rounds, and late VC rounds), resulting in 6,185,555 VC-startup-year pairs.

Panel A in Table 1.2 reports the summary statistics of key variables of the final sample. Variable definitions are listed in Appendix A1. The average investment is 0.01, meaning that on average each real deal is matched with 100 counterfactual deals. The matching method produces a reasonable size of consideration sets, as Gompers et al. (2020) find that for each successful deal VC considers roughly 100 potential opportunities. 32.5% of pairs are categorized as new alumni firms to the VC, and 50% of pairs are founders from top-ranking universities. On average, female founders account for 14.4%, and black founders only account for 1.4%. Comparing new alumni pairs with the control pairs, new alumni firms receive more investments. Since the new partners are more from top-ranking universities, new alumni firms have a higher share of top university graduates than control pairs. New alumni firms also share more professional networks with the investors (namely, work in the same company as the partners). Therefore, it is important to add indicators for top universities and professional networks as control variables. There is no significant difference in demographics between new alumni pairs and other pairs.

To get an overview of the distribution of alumni in the VC market, Table 1.3 lists the universities with the highest alumni share in VC companies and startups. Harvard University and Stanford University have the highest alumni shares, with 17.68% and 15.82% of VC companies having partners graduating from the two universities, respectively. The distribution of alumni share is heavily skewed to the top-ranked ten universities, whose alumni dominate the VC companies. Similar to VC companies, the distribution of alumni shares in startups is highly skewed towards the same top university list, with minor changes in order. This table illustrates the high centralization and similarity of alumni networks in VC companies and startups.

1.3.4 Empirical Specification

I implement the difference-in-differences method to compare the changes in alumni startups of new partners with others before and after the arrival of the new partner. I first test the probability of a startup being invested in by a VC, using the full sample with both actual and counterfactual deals. The specification is

$$\begin{aligned}\text{Invest}_{f,v,t} = &\alpha_{t\times v} + \gamma_{t\times f} + \beta_1 \text{Alumni of New Partner}_{f,v} \\ &+ \boldsymbol{\beta_2} \text{After Hiring}_{f,v,t} \times \text{Alumni of New Partner}_{f,v} + \delta' X_{f,v,t} + \epsilon_{f,v,t}\end{aligned} \tag{1.3.1}$$

where $\text{Invest}_{f,v,t}$ is a dummy variable indicating whether the VC v invests in startup f in year t; $\alpha_{t\times v}$ are VC-by-year fixed effects; $\gamma_{t\times f}$ are startup-by-year fixed effects; $\text{After Hiring}_{f,v,t}$ is the event variable indicating whether year t is after the hiring events of VC company v;

Alumni of New Partner$_{f,v}$ equals one if any of the founders of startup f and the new partner of VC company v go to the same university; $X_{f,v,t}$ are control variables including alumni status of f with incumbent partners in VC v, professional networks, and VC-by-industry-by-year fixed effects controlling for the time-varying changes of VC's industry preference. The professional network variable is a dummy variable taking one if the founders of the startup ever worked in the same company as any of the partners. The standard error is clustered by VC investors. Since the chance of any random investor-startup combination being one is low, I multiply $Invest_{f,v,t}$ by 100, which can be read as the probability that a investor funds a startup.

Assumption

Under the difference-in-difference framework, the identifying assumption is that startups which do not share alumni ties with new partners (the control group), form a valid counterfactual group for alumni startups of the new partner, conditional on control variables. The assumption implies that the outcomes of the two groups share a parallel trend if there are no shocks to alumni networks. I implement an event study to examine whether the sample contains any pre-trends and plot the coefficients. The event study specification is

$$\begin{aligned}\text{Invest}_{f,v,t} = \alpha_{t\times v} + \gamma_{t\times f} + \beta_1 \text{Alumni of New Partner}_{f,v} \\ + \Sigma_{\tau=-5}^{5} \beta_\tau D^{\tau}_{f,v,t} \times \text{Alumni of New Partner}_{f,v} + \delta' X_{f,v,t} + \epsilon_{f,v,t} \qquad (1.3.2)\end{aligned}$$

where $D^{\tau}_{f,v,t}$ are event dummies and equal to one if t is τ year after the hiring event of VC v, and β_τs estimates the differences between two groups τ years after the hiring. τ ranges from -5 to 5, with τ below -5 or above 5 winsorized to -5 and 5, respectively.

Figure 1.2a plots the estimated β's with 95 percent confidence intervals. β's before the hiring events are not significantly different from zero, indicating that the new alumni group shares a similar trend as control startups, supporting the assumption of parallel trends. After the new partner joins the VC company, the new alumni group form new alumni ties with the VC company, and the probability of being invested by the focal VC significantly increases. The increase is salient in the first four years after the shock and gradually declines afterward. Figure 1.2b plots the estimation of the same equation, except restricting the sample to the early funding rounds. There is no strong evidence for pre-trends, and the positive effects on investments are stronger and more persistent.

A growing literature points out the problems in estimating the dynamic effects of treatment with variation in treatment timing (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021). For robustness check, I calculate the estimator proposed by Callaway and Sant'Anna (2021) and plot it in Figure A1.

1.4 Investment Decision

This section focuses on network effects on VC investment decisions. Table 1.4 reports the estimation of equation 1.3.1. All columns control for alumni status among startup founders and incumbent VC partners, and the standard errors are clustered by VC companies. Column 1 includes investor-year fixed effects and company-year fixed effects to control time-varying changes in VC companies and startups, and tests the full sample. Column 1 indicates that compared with the control group, the probability of a new alumni startup being invested in by the VC company increases significantly by 0.107% after the hiring event, or a 7.64% increase relative to the probability before hiring.

One identification concern is that the hiring events may correlate with VCs' changes in investment preference. For example, a VC company plans to expand investments in a particular industry and hires a new partner specializing in this field. When the industry has a concentration of alumni from the same school, the estimation in column 1 can be overestimated. Therefore, column 2 adds VC-year-industry fixed effects to eliminate the effects of changes in industry preference. The results show a 0.115% (8.21% relative to the baseline) increase, which is similar to that in column 1, implying a weak impact of industry preferences on the results, and the impact of networks is significantly positive.

Given that the majority of the new partners graduate from top universities, new alumni groups feature a higher share of top university alumni than control groups. To address the concern that founders from different tier universities may experience different shocks after the hiring events, column 3 only includes startups founded by top university alumni. The estimation is statistically significant and close to results in other columns, verifying that the results are not driven by selection.

Column 4 estimates the same regression as column 2 and includes VC companies that have not hired new partners in the sample to validate the estimation. The results do not vary significantly from other columns.

In sum, Table 1.4 indicates that forming alumni ties with VC companies significantly increases investment probability. The following regressions are based on the sample in column 2.

1.4.1 Heterogeneous Effects

To understand the channels and effects of alumni networks under different circumstances, I test the heterogeneous effects under different market characteristics, VC company types, founder demographics, and university characteristics.

Market Characteristics

Table 1.5 illustrates network effects under different market characteristics. The first three columns compare the effects of early founding and later founding rounds. Column 1 reports the results on firms in the early stage seeking seed or earlier round financing. The probability after forming alumni ties increases significantly by 0.169% (12.52% relative to the baseline). Column 2 shows the results for startups in later rounds, and the alumni effect increases by a smaller but still significant 0.101% (6.69% relative to the baseline). Column 3 estimates the difference using the entire sample. The network effects are significant in both stages of funding rounds, but the effects in early rounds are significantly stronger. The results indicate that networks play a more influential role in early financing rounds, which suffer more asymmetric information problems and higher uncertainty than later rounds. These differences imply the effectiveness of the information channel.

Columns 4 and 5 test how competition interacts with networks. I define submarkets by industry-location (state) and calculate Herfindahl concentration indexes (HHI) based on the market share of VC companies in deal counts in each submarket m.

For each year, I define markets whose HHI is below the median as low HHI markets and the rest as high HHI markets. Column 4 shows that startups benefit from forming an alumni tie with a VC company, and the increase in investment probability is higher in concentrated markets by 0.189%, implying that VC companies rely more on the networks in concentrated markets. Column 5 focuses on the sample of early round financing, and depicts a stronger networks effects and wider differences in highly competitive and less competitive markets. One explanation from the information channel is that high HHI markets tend to be thinner and less active, where information is less fluid in the market. Therefore, alumni ties provide information advantages to VC companies and impact investment decisions more.

Generally, the network effects are more profound in markets with more asymmetric information. Although the information channel and preference channel both explain the increase in investments in alumni startups and cannot easily be disentangled, the heterogenous effects in different markets imply that network effects through information acquisition exist.

University Characteristics

Table 1.6 explores the effects on alumni from different universities. Similarly, the odd columns present results for the entire sample, and the even columns focus on early rounds. Columns 1 and 2 test the differences between universities with different alumni shares. For each university in the data, I calculate the share of founders in the submarket (by industry state year) graduating from the focal university, and define the variable $HighAlumniShare = 1$ if the alumni share of the university the new partner graduated from is above the median. The results in columns 1 and 2 suggest that founders from less popular universities experience a more considerable impact from alumni networks, and the gap is larger in early financing rounds.

Columns 3 and 4 focus on the academic rankings of the universities. I categorize universities with top ten business programs according to US News ranking in 2020 as top universities and define the variable $Top = 1$ if the new partner graduated from one of the top universities. Since top universities overlap with higher share universities, the regression results are similar to columns 1 and 2, indicating that the alumni effects are less salient in top universities.

Next, I test the heterogenous effects on the US and foreign universities in columns 5 and 6. *Foreign* equals one if the university's main campus is located outside the US and zero otherwise. Alumni connections from both US universities and foreign universities impact the investment decision, and the alumni of US universities enjoy more benefits from networks.

Finally, columns 7 and 8 compare the network effects on founders' education degrees. The variables BA, MA, PhD, and MBA indicate if the founder attended the same universities as the new partner and what degree they received. The variable Undeclared equals one if the founder's degree is unknown. The results show that founders who obtain bachelor's or Ph.D. degrees experience the most significant investment increase after forming networks with investors. In the early financing stage, founders with Ph.D. degrees benefit the most from alumni networks, with an increase of 0.346% in investment probability, equivalent to a 22.6% increase compared to investment probability before connections. The results suggest that VC companies value Ph.D. and bachelor's degrees more than others.

VC Characteristics

How do different kinds of VC companies utilize alumni networks? Table 1.7 compares the network effects among VCs with various characteristics. The odd columns regress on the entire sample, and the even columns report the results on early rounds. The first two columns compare the effects on young VCs, defined as VC companies founded after 2010, with old VCs. The network effects mainly influence young VCs, with a 0.24% (16.9% relative to the baseline) increase in alumni investments, while older VCs do not experience a significant change. This difference is persistent when restricted to early rounds of financing. Compared with old VC companies, young VCs feature fewer information sources and networks, and thus rely more on network expansion.

Columns 3 and 4 compare the results on VCs with different amounts of assets under management (AUM). VC companies with AUM higher than the median are categorized as high AUM VC and the rest as low AUM VC. High AUM VCs do not significantly change investment behavior after the alumni status changes, while low AUM VCs increase their investments in new alumni startups by 0.154% (11.1% relative to the baseline). The difference is significant both in the whole sample and early rounds sample. Since VC companies with smaller AUM tend to have a smaller investment team and relatively limited networks, network changes can impact them more.

The last two columns consider the network effects regarding the geographic distances between VCs and startups. *Same State* equals one if the VC and the target startup are located in the same state and zero otherwise. Column 5 and 6 suggests that the networks play a more influential role when VC companies invest in startups in other states. Since VC companies may face higher costs in obtaining information about startups in other states, they rely more on alumni networks.

Founder Demographics

Table 1.8 compares how networks impact founders with different demographics. The odd columns report results for the entire sample, and the even columns are for those of early rounds. Column 1 and 2 compare male and female founders. *FemaleLed* takes one if the share of female founders in the startup is at least 0.5, which indicates whether the startup is female-dominated. Column 1 shows that male and female-dominated startups both have an increased probability of investment after being connected to VC companies, with female-led startups experiencing more substantial increases than males. Column 2 verifies that the difference is significant in early-round financing.

Column 3-8 test the ethnicity of founders. The ethnicity variable *Has Black Founder* (*Has Asian Founder*/*Has White Founder*) equals one if the startup has at least one black (Asian/white) founder and zero otherwise. Black founders experience the most considerable network effects compared to other ethnic groups. Asian and non-Asian founders do not have significant differences, and white founders are less affected by alumni connections. Since black founders (who make up only 1.5% of the founder population) and female founders (16%) are less represented in the VC market, VC companies may encounter barriers to acquiring information on minority founders and thus rely more on their alumni networks.

1.4.2 Robustness Checks

Although Figure 1.2 disproves the existence of pre-trends, the parallel trend assumption can be violated after the events. Specifically, there may be a chance that VC companies hire a new partner from a university due to an intention to invest in more startups with founders from the same university, which results in an overestimation of the alumni effects. To address this concern, this section reports additional robustness tests.

Partner Leaving

First, I only include hiring events following an incumbent general partner's exit. Since general partners are the most senior in the VC companies and usually invest personal capital into the fund, VC companies seldom dismiss a general partner. According to an informal survey I conducted with some partners, the exit of general partners is mainly due to personal career plan changes or location changes. Therefore, partners' leaving can be considered an exogenous shock to the VC companies, and the purpose of new hiring afterwards is most likely to fill the vacant position rather than to change investment strategies.

Table 1.9 restricts the sample to VC companies that hire a new partner within two years after a general partner leaves the company. Similar to Table 1.4, column 1 reports the results of regression 1.3.1 on qualified hiring events, implying a significant increase in investments of alumni startups of the new partners after the hiring. Column 2 includes VC-industry-year fixed effects to control for VCs' preference changes to the industry and yields similar coefficients to column 1. Column 3 includes never-treated VC companies, and the coefficient of the interaction term is statistically significant at the 90 percent level. Column 4 tests founders graduating from top universities, and column 5 focuses on early financing rounds. The network effects are salient in both subsamples.

I also estimate the LATE treatment effects by two-stage least square estimation in appendix Table A3, treating *After Leave* $\times$ *Alumni of New Partner* as an instrument for *After Hiring* $\times$ *Alumni of New Partner*. It shows a strong first-stage result, and the estimation in the second stage is very close to the main results in Table 1.4.

Placebo Test

In this subsection, I exploit the effects of hiring events on junior employees as a placebo test. Since junior employees have a limited impact on investment choices, obtaining alumni networks with junior analysts would have little impact on a startup's investment probability. However, as outlined above, one identification concern is that future investment plans might influence new hiring decisions. Specifically, if VC companies plan to invest in more startups from university A, they have a high propensity to hire a partner from university A. They might also hire more junior analysts from university A to facilitate research and due diligence. If such strategic hiring is significant, we anticipate that hiring junior employees would also have network effects on investments.

The regression specification is the same as Eq 1.3.1, except that *Alumni of New Employee* takes one if the startups are alumni of new junior employees. Table 1.10 reports the results of the placebo test and shows that the networks with non-partners have little influence on investment probability, alleviating the concern about strategic hiring.

1.5 Performance

As the model predicts, the network effects can increase investment probability through both information and preference channels. However, these channels impact investment performance in opposite directions, which provides opportunities to reveal which channel dominates the effects. In this section, I implement the same identification strategy to test the investment performance of alumni startups.

1.5.1 Exit Outcomes

The sample for the performance analysis consists of actual deals made by VC companies. Panel B in Table 1.2 presents the summary statistics of key variables. The regression is

$$\begin{aligned}\text{Outcome}_{v,t,f} = &\alpha_t + \gamma_v + \beta_1\text{After Hiring}_{f,v,t} + \beta_2\text{Alumni of New Partner}_{f,v}\\ &+ \boldsymbol{\beta_3}\text{After Hiring}_{f,v,t} \times \text{Alumni of New Partner}_{f,v} + \delta' X_{f,v,t} + \epsilon_{f,v,t} \quad (1.5.1)\end{aligned}$$

where $Outcome_{v,t,f}$ is the performance measures of firm f, including dummies indicating whether the startup fails, is acquired, or goes IPO; α_t and γ_v are time-fixed effects and VC fixed effects; and controls $X_{f,v,t}$ include startup's industry times year times state fixed effects, investor times industry fixed effects, alumni status with incumbent partners, professional connections, startup founding year, deal type, and the number of investors in the deal when VC v invested.

The regression tests how the quality of new alumni companies selected after the hiring events differs from those selected before. For example, suppose a VC hires a partner from university A at year t. In that case, startups of university A form new alumni ties after t. I test whether the startups of university A invested in by the VC after t perform better or worse than startups of the same university selected before t. I measure the performance by whether the startup goes bankrupt, gets acquired, or goes IPO.

Table 1.11 reports the impact of networks on performance. The odd-numbered columns test on the entire deal sample, and even-numbered columns restrict to deals where the focal VC company is a leading investor and the primary decision maker. The dependent variables in columns 1 and 2 is an indicator for bankruptcy. The results indicate that VC companies invest 0.013 more in new alumni startups that fail than before, equivalent to a 7.69% increase in unsuccessful investments. Column 2 suggests that the network effects are stronger when the VC is a leading investor. The new alumni startups invested in after forming alumni ties fail by 0.05 more, a 22.9% increase relative to the failure rate of startups invested in before alumni connections.

Columns 3 and 4 explore the probability of startups getting acquired, with the outcome variable indicating whether the invested startups are acquired. The alumni startups invested in after the network connection experience a decrease in acquisition probability by 0.021, equivalent to a decline of 6.80% relative to firms invested in before. Column 4 implies that the negative network effects on leading investments are more salient, with a 0.046 (17.5%) decline in acquisition.

The effects on IPO are shown in columns 5 and 6. The startups invested in after obtaining the alumni networks with the VC have 0.0065 less IPOs, a 21.5% decline compared to the IPO rates of startups of the same university invested in before the hiring. The decline is sharper for leading deals, with 0.02 fewer investments eventually going IPO.

Figure 1.3 plots the event study coefficients to examine the existence of pre-trends in failure rates, acquisition rates, and IPO rates. The graph focuses on the deals where the VC is the leading partner, and plot the 90% confidence interval. There are no obvious pre-trends in all outcome variables, and the graphs confirm the negative impact of alumni networks on investment performance.

The performance results imply that the impact of the preference channel overwhelms the information channel. As the model predicts, when investors gain better private information through networks, investors have the advantage of distinguishing high-quality startups and screening out unsuccessful ones. As a result, investments with alumni ties should outperform those without connections. However, Table 1.11 demonstrates that the performance of alumni startups is actually worse, implying the existence of the preference channel. When investors show favoritism for startups in their alumni networks, they can tolerate worse performances by alumni startups, resulting in a decline in investment quality and a distortion of asset allocation.

1.5.2 Portfolio Return

Another performance measure is the return on investment. One explanation for the previous results is that investors are willing to take more risks on the connected firm. In that case, they will invest in firms with higher failure rates, but the successful ones are more likely to grow into unicorn firms and be ultra-profitable. Therefore, I test how the investment return changes before and after forming alumni networks.

The investment return is constructed at the portfolio level. Specifically, for each VC company at year t, I bundle all new alumni companies invested in this year into a new-alumni portfolio and other invested companies into a control portfolio. Then I define the return as the sum of the valuation of portfolio startups in 2021 divided by the total amount invested in the portfolio. To consider the time value, I also calculate the portfolio's internal rate of return (IRR). The regression is

$$\begin{aligned}\text{Outcome}_{v,t,p} =& \alpha_t + \gamma_v + \beta_1 \text{After Hiring}_{p,v,t} + \beta_2 \text{Alumni of New Partner}_{p,v} \\ &+ \boldsymbol{\beta_3} \text{After Hiring}_{p,v,t} \times \text{Alumni of New Partner}_{p,v} + \epsilon_{p,v,t}\end{aligned} \tag{1.5.2}$$

The results are reported in Table 1.12. Columns 1 and 3 construct portfolios on all deals, while columns 2 and 4 only focus on leading deals. The dependent variable of columns 1 and 2 is the logarithm of the portfolio return. Column 1 shows that the return of new alumni portfolios constructed after the arrival of the new partner declines significantly by 0.291 (24.9%). Column 2 indicates that the return of leading deals of alumni startups drops by 0.305 (35.9% relative to the baseline), which is significant at the 90% confidence level, and the magnitude is slightly larger than all deals. Columns 3 and 4 regress on the logarithm of IRR. Similar to columns 1 and 2, IRR drops after new alumni connection, though the coefficients are less statistically significant. For robustness, the results on portfolio return constructed at industry level are presented in appendix Table A4.

1.5.3 Heterogeneous Effects on Performance

How do the network effects on performance differ among markets, investors, and founders? The following tables present the heterogeneous network effects on performance, with odd columns regressing on all deals and even columns on leading deals.

Table 1.13 tests how the performance of early-round financing, which features higher risks and asymmetric information, is affected by the networks. Similar to the entire deal sample, new alumni startups invested in after the new partner hiring experience more bankruptcy, less acquisition, and less IPO. The magnitude of the coefficients of interest is also similar to the baseline estimation, implying that the network effects does not vary much with market risks, unlike the results in section 1.4.1 on investment probability. The results of investment probability and performance together indicates that effects through both information and performance channels are stronger in riskier markets, resulting in similar negative network effects on performance.

Table 1.14 compares how young and mature VC companies rely on the preference channel. For old VC firms, the new alumni startups have slightly higher failure rates and lower acquisition and IPO rates, but the difference is not significantly different from zero. However, investments made by young VC firms are significantly affected by alumni networks, especially when they lead the deals. The failure rate of new alumni startups rises by 0.132, and the acquisition rate drops by 0.104, indicating that young VC firms rely more on networks and have a stronger preference for alumni firms.

In Table 1.15, I explore how the effects differ with university reputation. When the VC hires a new partner from a top ten university, new alumni investments perform worse, with 0.016 more failure and 0.023 less acquisition, but only 0.006 less IPO. The alumni networks of non-top universities also negatively influence the performance, with relatively smaller magnitude and less significance in failure rate and acquisition rate, while stronger effects on IPO. These results suggest that the heterogenous effects among the top and non-top universities are mixed, but investments in top-ten alumni startups are riskier.

To sum up, VC companies are more likely to select less successful investments after forming alumni networks, which provides evidence of the strong influence of the preference channel. The negative impact on performance is more salient among young VC firms.

1.6 Structural Estimation

The empirical evidence that obtaining alumni networks leads to more investments but worse investment performance supports the existence of VCs' preference for alumni startups, but it cannot clearly disentangle the information channel and preference channel. Therefore, this section turns to model estimation to quantify the impact of information and preference. The estimation setup follows DellaVigna et al. (2017).

Recall that the model predicts the total investments by

$$P_g = \frac{1}{2b}(r - \eta_g - \mu)\left(\frac{\phi(x_g)}{x_g} - 1 + \Phi(x_g)\right)\left(1 - \Phi(x_g)\right),$$

and the expected return conditional on investments by

$$\Pi_g = \mu + \frac{\sigma}{\sqrt{1+\kappa_g^2}}\frac{\phi(x_g)}{1 - \Phi(x_g)},$$

where $x_g = \frac{\sqrt{1+\kappa_g^2}}{\sigma}(r - \eta_g - \mu)$. The key parameters include information accuracy $\kappa_{g=0}$, $\kappa_{g=1}$, and preference $\eta_{g=0}$, $\eta_{g=1}$. Since more precise information and stronger preference both increase the investment probability but influence the performance in opposite directions, matching investment probability and performance to the data allows us to identify these parameters.

I define a submarket by industry and deal stage, early or late. For each submarket i, I calibrate μ_i and σ_i by estimating the average and standard deviation of deal returns, and estimate the investment probability and average return on investments of alumni firms and non-alumni firms, respectively, as observed moments to match. Thirty-six submarkets with at least 100 deals are included, generating $36 \times 4 = 144$ observed moments denoted by $\hat{m}$. The vector of parameters ξ in the baseline estimation includes: (1) information precision for alumni startups and other startups κ_1, κ_0; (2) preference for alumni startups and other firms η_1, η_0; (3) searching cost parameter b. The vector of moments predicted by the theory $m(\xi)$ combines the investment probability and expected return of alumni and non-alumni startups in each submarket. The fixed-rate r is set to 28%, the average return of VC funds from 2010 to 2020. Since the predicted moments can be viewed as functions on κ_g and $r - \eta_g$, the optimization is invariant to the choice of r.

The estimation finds the best $\hat{\xi}$ to minimize the distance between predicted and observed moments $(m(\xi) - \hat{m})'W(m(\xi) - \hat{m})$. The weighting matrix is a diagonal matrix of the inverse of the variance of the moment. The estimated variance matrix of the estimator is $(\hat{C}'W\hat{C})^{-1}(\hat{C}'W\hat{V}W\hat{C})(\hat{C}'W\hat{C})^{-1}/N$, where $\hat{C} = \nabla_{\xi} m(\hat{\xi})$ and $V \equiv Var(m(\hat{\xi}))$.

The estimation results are shown in Table 1.16 Panel A. Column 1 shows the baseline estimation. VC companies have highly noisy signals on non-alumni startups, and the preference η_0 is small and not significantly different from zero, implying that the investors do not have a strong preference for non-alumni firms. The investors have improved information on alumni firms and a sharp increase in preference. To compare the effects of less information noise and stronger preference for alumni firms, Table 1.16 Panel B presents the changes in investment and performance when either of the channels is shut down. Column 1 shows that when there is no information improvement from the networks, i.e., $\kappa_1 = \kappa_0$, the investment in alumni firms will decrease by 11.3%. If the preference channel does not take effect, i.e., $\eta_1 = \eta_0$, the investment will shrink by 19.8%. In terms of performance, shutting down the information channel decreases the expected return on investments by 23.9%, and without the preference channel, the performance improves by 37.8%. This counterfactual evidence suggests that the preference channel has a more substantial impact.

Column 2 in Panel A estimates the κ's and η's separately for startups from the top and non-top universities. The estimation for non-top universities is close to that of the baseline estimation. However, η_0 is relatively high for founders educated in top universities, meaning that the investors are interested in startups from top universities even when they are not connected. Columns 2 and 3 in Panel B disentangles the two channels. For startups from top universities, the preference channel slightly outweighs the information channel, while for startups from non-top universities, the preference channel greatly outweighs the information channel. The mild impact of preference partly stems from the pre-existing preference for non-alumni startups from top universities.

The structural estimation reveals that information and preference channels both play a critical role in VC decision-making, while favoritism towards alumni plays a more significant part.

1.7 VC Partner Hiring

Given previous empirical evidence that hiring new VC partners results in excessive investments in new alumni startups of worse quality, how should VCs develop alumni networks through hiring? Should they hire a new partner with a similar educational background as incumbent partners to enhance existing alumni networks or a partner from a different university to expand new alumni networks? For example, for a VC company with partners from university A, is it better to bring in a new partner also from university A or a different university B? To address this hiring strategy question, this section compares the network effects of two types of hiring, *first hiring* (hiring a partner from university B) and *additional hiring* (hiring a partner from university A).

First, following a similar strategy in section 1.4, I test the network effects of two types of partner hiring on investment choice. The regression is

$$
\begin{aligned}
\text{Invest}_{f,v,t} = & \alpha_{t\times v} + \gamma_{t\times f} + \beta_1 \text{Alumni of New Partner-First}_{f,v} \\
& + \beta_2 \text{Alumni of New Partner-Additional}_{f,v} \\
& + \boldsymbol{\beta_3} \text{After Hiring}_{f,v,t} \times \text{Alumni of New Partner-First}_{f,v} \\
& + \boldsymbol{\beta_4} \text{After Hiring}_{f,v,t} \times \text{Alumni of New Partner-Additional}_{f,v} + \delta' X_{f,v,t} + \epsilon_{f,v,t}
\end{aligned}
$$

where Alumni of New Partner-First$_{f,v}$ equals one if the founder of startup f is an alumnus of the new partner of VC company v but not an alumnus of incumbent partners, Alumni of New Partner-Additional$_{f,v}$ equals one if the founder of startup f is an alumnus of both the new partner and current partners of VC company v. β_3 and β_4 estimate the alumni network effects of first hiring and additional hiring on investment probability, respectively.

Table 1.17 presents the results, with columns 1 and 2 regressing on the entire sample, column 3 on the top sample, and column 4 on the early funding rounds. Column 1 includes VC-year and startup-year fixed effects and shows a substantial increase in investments in new alumni startups after hiring a partner from different universities but no significant change after additional hiring. Column 2 adds VC-industry-year fixed effects and presents similar results as column 1, with a salient increase by 10.29% after *first hiring* and an insignificant 1.34% increase after *additional hiring*. Columns 3 and 4 indicate similar results, with network effects stronger in early-round financing after *first hiring* while no significant changes after *additional hiring*. Additional tests on heterogeneity are presented in the appendix.

Next, following section 1.5, I compare the changes in the performance of invested alumni firms after two types of hiring. Table 1.18 presents the results, with the regression sample and inclusion of controls and fixed effects being identical to that in Table 1.11. Columns 1 and 2 show that the failure rates of new alumni startups invested after *first hiring* increase by 7.9%, and by 25.8% when the VC company leads the deal. The increase in failure rates after *additional hiring* is not statistically significant but still meaningful in magnitude, with a 7.0% increase considering all deals and a 16.74% increase in leading deals. Columns 3 and 4 indicate that both types of hiring induce a decline in acquisition rates of invested new alumni firms, and the change is more significant after *additional hiring* events. Columns 5 and 6 present that after *first hiring*, the IPO rates in new alumni firms decline significantly by 30.2%, and the effects are doubled in leading rounds. *additional hiring* events show similar effects on IPO rates when the VC company is a leading investor. Additional tests comparing the effects on performance by two types of hiring events are included in the appendix.

To sum up, new partners with different educational backgrounds (*first hiring*) increase investments in new alumni startups but worsen the investment performance; new partners from the same universities (*additional hiring*) have limited impact on investment probabilities but still have negative effects on the performance of alumni investments. The results imply that although *first hiring* induces favoritism for alumni from new universities, it brings in information and more deal flows. However, hiring a subsequent partner with the same educational background does not improve the information but accumulates more preference for current alumni startups. Since both types of hiring suffer from biases, hiring partners with diverse academic backgrounds appears to be a better choice as VCs benefit from more information and deal flows by expanding the alumni networks. Appendix A presents tests on VC hiring patterns, finding that VC companies tend to hire more alumni partners, especially for small and young VC companies, which may result in more concentrated alumni networks and capital misallocation to alumni of a small set of universities.

1.8 Conclusion

Networks feature prominently in the venture capital industry, and both venture capitalists startup founders actively expand and manage their networks. Despite the widespread consensus on the importance of networks, understanding how networks take effect in the VC market provides meaningful guidance to both founders and investors.

This paper studies the causal effects of networks on venture capital investments and unpacks two channels, information and preference. Specifically, the paper focuses on alumni networks and identifies the causal impact by exploiting VC partner hiring events. After a new partner with a different educational background joins the VC company, startup founders from the same university as the new partner exogenously obtain alumni connections to the VC. Under a difference-in-difference framework, I show that startups with alumni networks are more likely to be invested in by VC companies. The effects are more salient in markets with higher risks and asymmetric information, and for less-represented entrepreneurs.

Despite the significant increase in alumni investments, the performance of these firms is less satisfactory. Firms invested in after forming alumni connections experience higher failure rates, lower exit rates, and lower return rates. The poor performance implies the existence of VCs' preference for alumni firms and this favoritism leads to excessive capital allocated to startup founders with similar educational backgrounds. To disentangle the network effects on private information and homophily preference, I estimate a model to quantify the two channels. The estimation indicates that the preference channel has a stronger impact on investments than the information channel. Moreover, by comparing the effects of hiring a new partner with a similar or diverse educational background, I find that the latter one tend to bring more benefits to the VC company. The above evidence highlights VCs' over-emphasis on alumni networks and potential efficiency loss in their investment strategies. However, we also observe stronger network effects on underrepresented founders, implying that alumni networks can play a positive role in enhancing diversity in VC markets. Therefore, it is meaningful to be aware of the mechanism of how alumni networks work and make the best use of them.

Considering the extensive effort universities, partners, and entrepreneurs invest in maintaining and expanding alumni networks, understanding why venture capitalists do not learn from past investments and still keep the preference for alumni firms is essential to improve VC capital allocation. Future work can aim to understand the formation of alumni preference and the network effects on other investment decisions, such as VC's ability to add value, deal contract design, and startup governance. This work can provide guidance to VC investors and startup founders, and help universities improve educational programs and alumni networks.

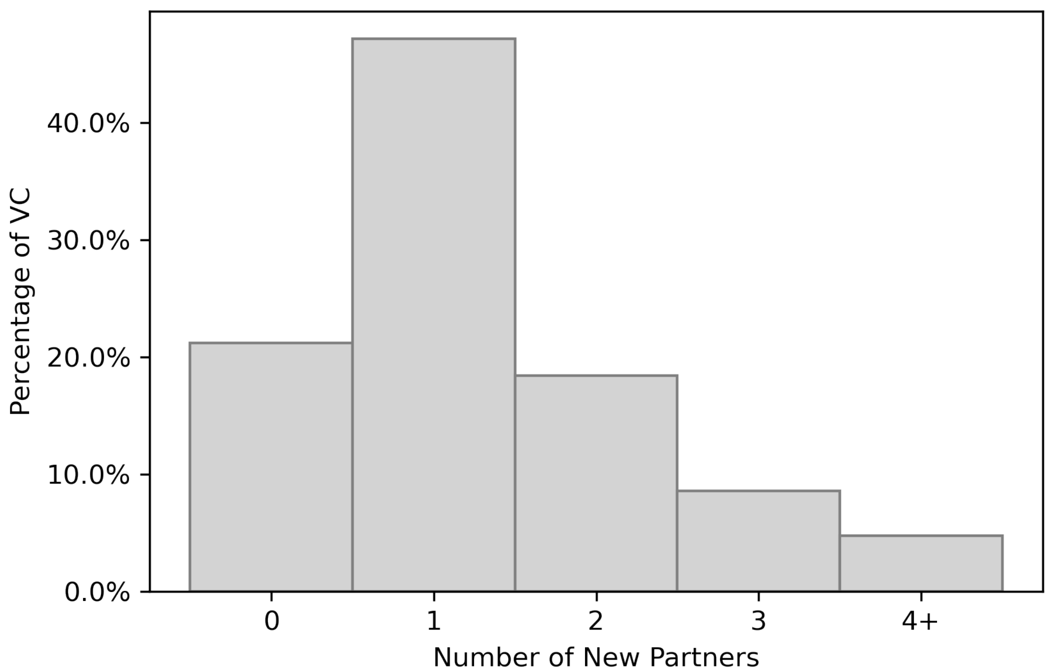
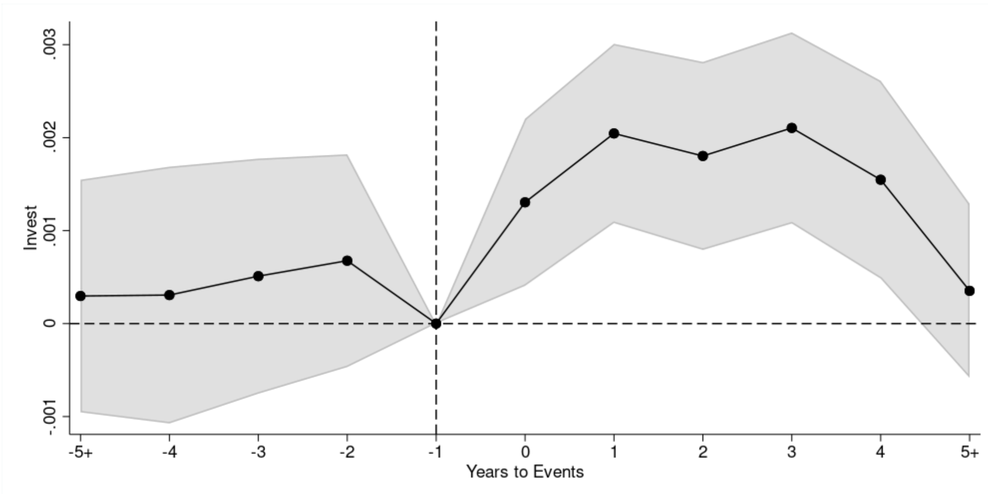


Figure 1.1: New Partner Hiring Frequency

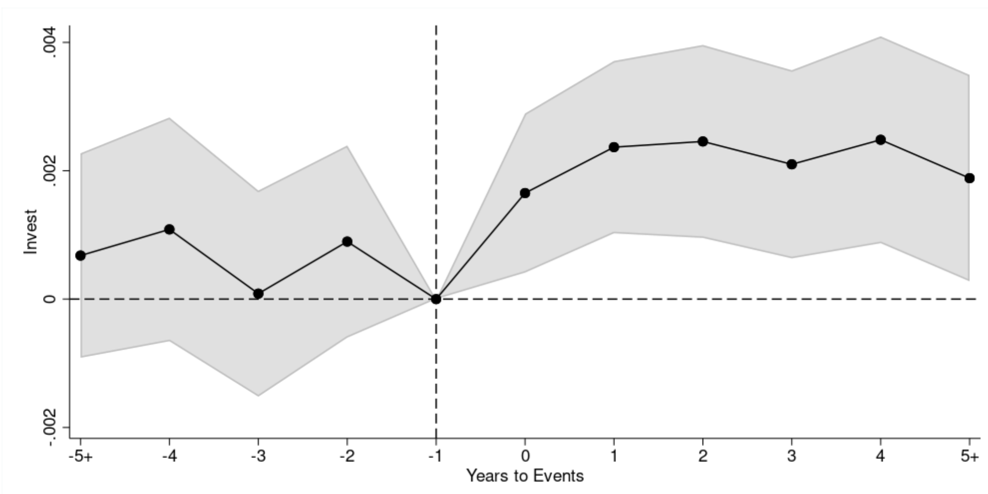
Note: This figure shows the frequency distributions of number of new partner hiring events among VC. The x-axis is the number of new hiring events, and the y-axis shows the percentage of VC companies having the according number of new hiring events.

Figure 1.2: Effect of Alumni Networks on Investment Choice

(a) Full Sample



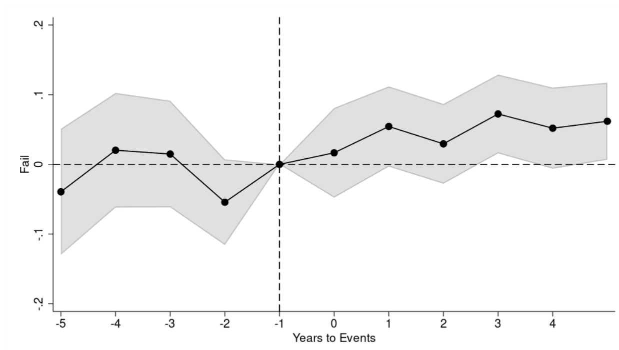
(b) Early Rounds



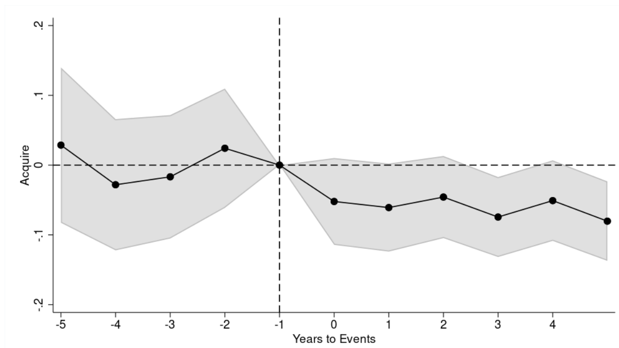
Note: These figures plot the effect of alumni networks on investment, obtained from estimating equation 1.3.2. The y-axis measures the effects on investment probability, the x-axis is the relative years to the event year, with -1 as the baseline year. The regression includes VC-industry-year fixed effects and company-year fixed effects, alumni status to incumbent partners, and professional connections to current partners. The upper graph regresses on the full sample, and the lower graph regresses on seed and earlier rounds. The gray area plots the 95% confidence intervals for each point estimates. Robust standard errors are clustered at VC level.

Figure 1.3: Effect of Alumni Networks on Investment Performance

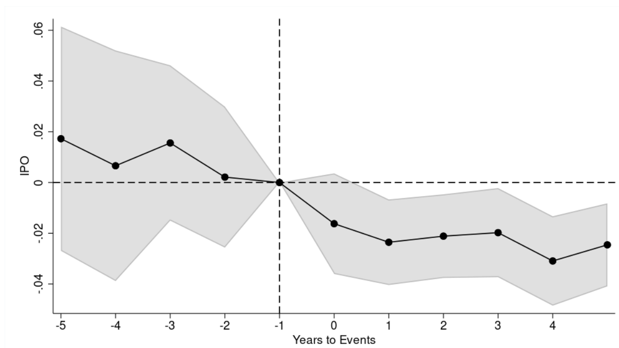
(a) Failure Rate



(b) Acquisition Rate



(c) IPO Rate



Note: These figures plot the effect of alumni networks on investment performance by year relative to the event year. The y-axis measures the effects on investment performance, the x-axis is the relative years to the event year, with -1 as the baseline year. The regression includes VC-industry fixed effects and industry-state-year fixed effects, alumni status with incumbent partners, professional connections, startup founding year, deal type, and the total number of investors in the deal. The upper graph tests the effects on failure rates, the middle on acquisition rates, and the lower on IPO rates. The gray area plots the 90% confidence intervals for each point estimates. Robust standard errors are clustered at VC level.

Table 1.1: Merge Diagnostics

(a) Panel A: Merged Startups

	Matched		Full	
	Mean	Median	Mean	Median
Total Raised	35.89	3.50	35.50	3.23
Employees	73,041	15	67,348	14
Latest Valuation	217.43	21.42	214.22	20.56
IT	0.45	0.00	0.44	0.00
Healthcare	0.16	0.00	0.17	0.00
Close	0.22	0.00	0.22	0.00
Acquire	0.16	0.00	0.16	0.00
IPO	0.01	0.00	0.01	0.00
Observation	27,127		30,357	

(b) Panel B: Merged Venture Capital

	Has New Partner		All Matched		Full	
	Mean	Median	Mean	Median	Mean	Median
AUM	750.62	100.00	2,343.90	100.00	2,526.02	100.00
IT	0.78	1.00	0.76	1.00	0.71	1.00
Healthcare	0.49	0.00	0.47	0.00	0.40	0.00
Observation	5,139		6,348		10,046	

(c) Panel C: Merged Deals

	Has New Partner		All Matched		Full	
	Mean	Median	Mean	Median	Mean	Median
Deal Size($M)	15.56	3.17	15.65	3.18	16.88	3.10
Valuation($M)	196.26	17.72	196.48	17.73	220.48	18.00
IT	0.48	0.00	0.48	0.00	0.47	0.00
Healthcare	0.15	0.00	0.15	0.00	0.16	0.00
Observation	63,089		83,485		101,662	

Note: This table shows summary statistics for merged datasets and input datasets. Panel A shows the merging results by startups. Panel B presents the merging results for VC and VC with new partner hiring. Panel C shows the statistics for merged deal sample, i.e. deals where both VCs and startups are merged.

Table 1.2: Summary Statistics

(a) Panel A: Full Matching Sample

	All			New Alumni			Non New Alumni		
	Mean	Std.	Median	Mean	Std.	Median	Mean	Std.	Median
Invest	1.014	10.0	0	1.383	11.680	0	0.836	9.105	0
Alumni of New Partner	0.325	0.468	0	1	0	1	0	0	0
Top University Founder	0.498	0.500	0	0.823	0.382	1	0.343	0.481	0
Professional Networks	0.140	0.347	0	0.303	0.460	0	0.061	0.240	0
Female Founder Share	0.144	0.254	0	0.153	0.238	0	0.139	0.261	0
Black Founder Share	0.014	0.082	0	0.014	0.066	0	0.015	0.089	0
Asian Founder Share	0.150	0.256	0	0.154	0.223	0.071	0.148	0.270	0
White Founder Share	0.597	0.339	0.660	0.608	0.283	0.649	0.591	0.364	0.667
Information Technology	0.765	0.424	1	0.755	0.43	1	0.77	0.421	1
California	0.795	0.404	1	0.787	0.409	1	0.799	0.401	1
Startup Founding Year	2012.75	2.154	2013.00	2012.50	2.19	2013.00	2012.87	2.126	2013.00
Observation		6,185,555			2,007,535			4,178,020	

(b) Panel B: Real Deal Sample

	All			New Alumni			Non New Alumni		
	Mean	Std.	Median	Mean	Std.	Median	Mean	Std.	Median
Bankrupt	0.126	0.332	0	0.11	0.313	0	0.139	0.346	0
Acquired	0.207	0.405	0	0.219	0.413	0	0.197	0.398	0
IPO	0.022	0.146	0	0.024	0.154	0	0.019	0.138	0
Leading VC	0.343	0.475	0	0.376	0.484	0	0.317	0.465	0
Deal Size ($M)	15.5	72.5	3.25	18.7	89.4	4	12.9	54.2	3
Post Valuation ($M)	196	1896	17.5	237	2185	20.38	158	1577	15.63
Alumni of New Partner	0.451	0.498	0	1	0	1	0	0	0
Top University Alumni	0.535	0.499	1	0.762	0.426	1	0.347	0.476	0
Professional Networks	0.211	0.408	0	0.369	0.482	0	0.082	0.275	0
Female Founder Share	0.162	0.274	0	0.165	0.262	0	0.16	0.284	0
Black Founder Share	0.015	0.078	0	0.015	0.067	0	0.016	0.086	0
Asian Founder Share	0.131	0.233	0	0.138	0.215	0.045	0.125	0.248	0
White Founder Share	0.639	0.317	0.667	0.638	0.278	0.667	0.64	0.346	0.667
Information Technology	0.527	0.499	1	0.52	0.5	1	0.532	0.499	1
California	0.494	0.5	0	0.505	0.5	1	0.485	0.5	0
Startup Founding Year	2012.87	2.32	2013	2012.71	2.32	2013	2012.99	2.31	2013
Observation		63,089			28,473			34,616	

Note: This table reports the summary statistics for the main sample. Panel A reports summary statistics on full matching sample with counterfactual pairs, used for investment choice analysis. Panel B reports the sample on real deals, used for performance analysis.

Table 1.3: University Alumni Share in VC companies and Startups

	Venture Capital		Startup	
Rank	University	Share	University	Share
1	Harvard University	17.68%	Harvard University	11.68%
2	Stanford University	15.82%	Stanford University	10.84%
3	University of Pennsylvania	11.10%	University of Pennsylvania	7.70%
4	University of California, Berkeley	9.57%	University of California, Berkeley	7.35%
5	Columbia University	8.47%	Massachusetts Institute of Technology	6.57%
6	Massachusetts Institute of Technology	7.90%	Columbia University	5.91%
7	Northwestern University	6.13%	New York University	5.15%
8	New York University	6.11%	Northwestern University	5.11%
9	University of California, Los Angeles	5.76%	University of California, Los Angeles	5.01%
10	University of Michigan	5.67%	University of Michigan	4.96%
	Average	0.0746%		0.0383%
	Median	0.0085%		0.0039%
	Skewness	22.59		22.08

Note: This table presents the universities with the most alumni shares in VC companies and startups. The alumni share is defined as number of VC companies/startups having partners/founders graduated from the focal university over the total number of VC companies/startups.

Table 1.4: Alumni Network Effects on Investment Choice

	Full Sample		Top Sample	Never Treated
	(1)	(2)	(3)	(4)
Alumni of New Partner ×After Hiring	0.107***	0.115***	0.110***	0.108***
	(0.0287)	(0.0287)	(0.0365)	(0.0288)
relative to Baseline	7.64%	8.21%	8.53%	7.61%
Alumni of New Partner	0.230***	0.220***	0.109***	0.217***
	(0.0260)	(0.0258)	(0.0315)	(0.0253)
Alumni of Incumbent Partner	0.144***	0.163***	0.143***	0.176***
	(0.0240)	(0.0239)	(0.0303)	(0.0228)
Professional Networks	0.318***	0.323***	0.321***	0.328***
	(0.0188)	(0.0192)	(0.0220)	(0.0190)
Startup×Year FE	Y	Y	Y	Y
VC×Year FE	Y			
VC×Year×Industry FE		Y	Y	Y
Observations	6,199,796	6,185,555	3,070,864	6,600,517
R-squared	0.226	0.220	0.215	0.221

Note: This table estimates the alumni network effects on investment choice by equation 1.3.1. The dependent variable is an indicator variable that equals to 100 if the VC invest in the startup and zero otherwise. *Alumni of New Partner×After Hiring*, the main variable of interest, is the interaction between *Alumni of New Partner*, an indicator that equals one if the startups is the alumni of the new partner, and *After Hiring*, the event variable taking one if the year is after the hiring. All columns control for alumni status among startup and VC's incumbent partners, and *professional networks*, which takes one if any founder worked in the same company as any partners in the VC in the past. Column 1 regresses on the full sample with investor-year fixed effects and company-year fixed effects. Column 2 also regresses on the full sample, except including VC-year-industry fixed effects and company-year fixed effects. Column 3 only includes startups having founders from top universities, defined as universities with top 10 business programs according to US News ranking in 2020. Column 4 includes VC companies that never hire new partners in the sample. Below the point estimation, the row "relative to baseline" reports the size of the effect relative to the baseline of outcome variable before events. The standard errors are clustered by VC companies. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table 1.5: Alumni Network Effects on Investment Choice-Market Characteristics

	(1) Early Rounds	(2) Late rounds	(3) Full	(4) Full	(5) Early Rounds
Alumni of New Partner ×After Hiring	0.169***	0.101**			
	(0.0425)	(0.0425)			
relative to Baseline	12.52%	6.69%			
Alumni of New Partner ×After Hiring ×Early Rounds			0.207***		
			(0.0480)		
Alumni of New Partner ×After Hiring ×Late Rounds			0.0649		
			(0.0421)		
Difference			0.142**		
			(0.0665)		
Alumni of New Partner ×After Hiring ×High HHI				0.278***	0.315***
				(0.0898)	(0.120)
Alumni of New Partner ×After Hiring ×Low HHI				0.0891***	0.104***
				(0.0251)	(0.0364)
Difference				0.189**	0.211*
				(0.0905)	(0.120)
Alumni of New Partner	0.255***	0.146***			
	(0.0324)	(0.0379)			
Alumni of Incumbent Partner	0.196***	0.149***	0.164***	0.154***	0.171***
	(0.0353)	(0.0239)	(0.0239)	(0.0241)	(0.0340)
Professional Networks	0.364***	0.301***	0.325***	0.319***	0.354***
	(0.0294)	(0.0217)	(0.0191)	(0.0197)	(0.0300)
Startup×Year FE	Y	Y	Y	Y	Y
VC×Year×Industry FE	Y	Y	Y	Y	Y
Observations	3,344,579	2,840,122	6,185,555	6,183,419	3,344,379
R-squared	0.219	0.224	0.220	0.222	0.223

Note: This table estimates the alumni network effects on investment choice with different market characteristics by equation 1.3.1. The dependent variable is an indicator variable that equals to 100 if the VC invest in the startup and zero otherwise. Column 1 regresses on the deals in seed rounds or earlier. Column 2 focuses on deals in other financing rounds. Column 3 uses full sample to estimates the effects in early rounds and later rounds. Column 4 regress on the full sample to compare the effects in high HHI and low HHI markets, and column 5 compare the effects using only seed or earlier deals. Below the point estimation, the row "relative to baseline" reports the size of the effect relative to the baseline of outcome variable before events. The row "difference" reports the difference between two point estimations above with standard errors. Some interaction terms are eliminated from reporting for brevity. The standard errors are clustered by VC companies. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table 1.6: Alumni Network Effects on Investment Choice-University Characteristics

	(1) Full	(2) Early	(3) Full	(4) Early	(5) Full	(6) Early	(7) Full	(8) Early
Alumni of New Partner ×After Hiring	0.358***	0.661***						
×Low Alumni Share	(0.0788)	(0.144)						
Alumni of New Partner ×After Hiring	0.0877***	0.107***						
×High Alumni Share	(0.0291)	(0.0409)						
Difference	0.270***	0.555***						
	(0.0792)	(0.141)						
Alumni of New Partner ×After Hiring			0.0909***	0.105**				
×Top			(0.0317)	(0.0452)				
Alumni of New Partner ×After Hiring			0.175***	0.286***				
×Non-Top			(0.0424)	(0.0660)				
Difference			-0.0837*	-0.180**				
			(0.0457)	(0.0705)				
Alumni of New Partner ×After Hiring					0.0810**	0.114**		
×Foreign					(0.0328)	(0.0489)		
Alumni of New Partner ×After Hiring					0.167***	0.238***		
×Domestic					(0.0409)	(0.0558)		
Difference					-0.0859*	-0.124**		
					(0.0440)	(0.0595)		
Alumni of New Partner ×After Hiring							0.0974***	0.136***
×Bachelor							(0.0345)	(0.0492)
Alumni of New Partner ×After Hiring							0.0626	0.0875
×Master							(0.0468)	(0.0636)
Alumni of New Partner ×After Hiring							0.139**	0.346***
×Ph.D.							(0.0673)	(0.100)
Alumni of New Partner ×After Hiring							0.0126	-0.0275
×MBA							(0.0545)	(0.0721)
Alumni of New Partner ×After Hiring							0.0735	0.157**
× Undeclared Degree							(0.0472)	(0.0662)
Startup×Year FE	Y	Y	Y	Y	Y	Y	Y	Y
VC×Year×Industry FE	Y	Y	Y	Y	Y	Y	Y	Y
Observations	6,185,555	3,344,579	6,185,555	3,344,579	6,185,555	3,344,579	6,185,555	3,344,579
R-squared	0.220	0.220	0.220	0.219	0.220	0.219	0.220	0.219

Note: This table estimates the alumni network effects on investment choice with different university characteristics by equation 1.3.1. The dependent variable is an indicator variable that equals to 100 if the VC invest in the startup and zero otherwise. Odd-numbered columns regresses on full sample and other columns focuses on deals in seed or earlier rounds. Columns 1 and 2 compare effects with universities of difference alumni share in startups. For each university in the data, I calculate the share of founders in the submarket (by industry state year) graduating from the focal university as alumni share, and define the variable $HighAlumniShare = 1$ if the alumni share of the university the new partner graduated from is above the median. Column 3 and 4 compare effects of top and non-top universities. Universities with top 10 business programs according to US News ranking in 2020 are defined as top universities. Column 5 and 6 compare alumni effects for foreign universities and domestic universities. Column 7 and 8 report the effects on different degree the founders obtain. Below the point estimation, the row "difference" reports the difference between two point estimations above with standard errors. Some interaction terms are eliminated from reporting for brevity. The standard errors are clustered by VC companies. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table 1.7: Alumni Network Effects on Investment Choice-Venture Capital Characteristics

	(1) Full	(2) Early	(3) Full	(4) Early	(5) Full	(6) Early
Alumni of New Partner ×After Hiring	0.239***	0.297***				
×Young VC	(0.0384)	(0.0534)				
Alumni of New Partner ×After Hiring	-0.0060	0.0352				
×Old VC	(0.0402)	(0.0636)				
Difference	0.245***	0.262***				
	(0.0533)	(0.0801)				
Alumni of New Partner ×After Hiring			0.0537	0.0464		
×High AUM			(0.0418)	(0.0733)		
Alumni of New Partner ×After Hiring			0.154***	0.225***		
×Low AUM			(0.0371)	(0.0493)		
Difference			-0.101*	-0.178**		
			(0.0540)	(0.0859)		
Alumni of New Partner ×After Hiring					0.0870***	0.119**
×Same State					(0.0324)	(0.0469)
Alumni of New Partner ×After Hiring					0.179***	0.241***
×Different State					(0.0491)	(0.0679)
Difference					-0.0916	-0.122*
					(0.0558)	(0.0737)
Alumni of Incumbent Partner	0.191***	0.216***	0.172***	0.207***	0.156***	0.171***
	(0.0272)	(0.0408)	(0.0259)	(0.0399)	(0.0242)	(0.0343)
Professional Networks	0.326***	0.366***	0.325***	0.367***	0.320***	0.355***
	(0.0192)	(0.0295)	(0.0194)	(0.0295)	(0.0197)	(0.0299)
Startup×Year FE	Y	Y	Y	Y	Y	Y
VC×Year×Industry FE	Y	Y	Y	Y	Y	Y
Observations	6,185,555	3,344,579	6,185,555	3,344,579	6,183,419	3,344,379
R-squared	0.220	0.219	0.220	0.219	0.222	0.223

Note: This table estimates the alumni network effects on investment choice with different VC characteristics by equation 1.3.1. The dependent variable is an indicator variable that equals to 100 if the VC invest in the startup and zero otherwise. Odd-numbered columns regresses on full sample and other columns focuses on deals in seed or earlier rounds. Columns 1 and 2 test the effects interacting with VC age. VC founded after 2010 are defined as young VC and old otherwise. Column 3 and 4 compare effects of VC with different asset under management (AUM). High AUM VCs are those managing AUM above the median. Column 5 and 6 compare alumni effects on VC and startup location. *Same State* indicates if the VC and the startup locate in the same state. Below the point estimation, the row "difference" reports the difference between two point estimations above with standard errors. Some interaction terms are eliminated from reporting for brevity. The standard errors are clustered by VC companies. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.8: Alumni Network Effects on Investment Choice-Founder Demographics

	(1) Full	(2) Early	(3) Full	(4) Early	(5) Full	(6) Early	(7) Full	(8) Early
Alumni of New Partner ×After Hiring	0.241***	0.320***						
×Female Led	(0.0812)	(0.0941)						
Alumni of New Partner ×After Hiring	0.115***	0.146***						
×Male Led	(0.0336)	(0.0422)						
Difference	0.125	0.175*						
	(0.0789)	(0.0892)						
Alumni of New Partner ×After Hiring			0.226***	0.228**				
×Has Black Founder			(0.0764)	(0.112)				
Alumni of New Partner ×After Hiring			0.104***	0.165***				
×No Black Founder			(0.0295)	(0.0428)				
Difference			0.122	0.0634				
			(0.0774)	(0.109)				
Alumni of New Partner ×After Hiring					0.107***	0.159***		
×Has Asian Founder					(0.0356)	(0.0544)		
Alumni of New Partner ×After Hiring					0.129***	0.179***		
×No Asian Founder					(0.0372)	(0.0512)		
Difference					-0.0216	-0.0195		
					(0.0437)	(0.0618)		
Alumni of New Partner ×After Hiring							0.102***	0.162***
×Has White Founder							(0.0304)	(0.0452)
Alumni of New Partner ×After Hiring							0.185***	0.197***
×No White Founder							(0.0532)	(0.0680)
Difference							-0.0833	-0.0351
							(0.0546)	(0.0696)
Alumni of Incumbent Partner	0.168***	0.195***	0.163***	0.196***	0.164***	0.197***	0.164***	0.196***
	(0.0284)	(0.0353)	(0.0239)	(0.0353)	(0.0238)	(0.0352)	(0.0238)	(0.0352)
Professional Networks	0.326***	0.364***	0.323***	0.364***	0.327***	0.370***	0.327***	0.368***
	(0.0234)	(0.0294)	(0.0192)	(0.0295)	(0.0192)	(0.0296)	(0.0190)	(0.0293)
Startup×Year FE	Y	Y	Y	Y	Y	Y	Y	Y
VC×Year×Industry FE	Y	Y	Y	Y	Y	Y	Y	Y
Observations	6,185,555	3,344,579	6,185,555	3,344,579	6,185,555	3,344,579	6,185,555	3,344,579
R-squared	0.221	0.219	0.220	0.219	0.220	0.219	0.220	0.219

Note: This table estimates the alumni network effects on investment choice with different founder demographics by equation 1.3.1. The dependent variable is an indicator variable that equals to 100 if the VC invest in the startup and zero otherwise. Odd-numbered columns regresses on full sample and other columns focuses on deals in seed or earlier rounds. Columns 1 and 2 test the effects interacting with founders' gender. *Female Led* takes one if the share of female founders in the startup is at least 0.5. Column 3 and 4 compare effects of black and non-black founders. *Has Black Founder* takes one if the startup has at least one black founder. Column 5 and 6 compare effects of Asian and non-Asian founders. *Has Asian Founder* takes one if the startup has at least one Asian founder. Column 7 and 8 compare effects of white and non-white founders. *Has White Founder* takes one if the startup has at least one white founder. Below the point estimation, the row "difference" reports the difference between two point estimations above with standard errors. Some interaction terms are eliminated from reporting for brevity. The standard errors are clustered by VC companies. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.9: Alumni Network Effects on Investment Choice-Hiring After Partner Leave

	Full Sample		Top Sample	Early Sample
	(1)	(2)	(3)	(4)
Alumni of New Partner ×After Hiring	0.0734**	0.0730**	0.125**	0.133**
	(0.0366)	(0.0366)	(0.0565)	(0.0564)
relative to Baseline	5.24%	5.21%	9.77%	9.96%
Alumni of New Partner	0.126***	0.115***	-0.00841	0.146***
	(0.0354)	(0.0356)	(0.0537)	(0.0495)
Alumni of Incumbent Partner	0.163***	0.177***	0.158***	0.209***
	(0.0281)	(0.0277)	(0.0333)	(0.0415)
Professional Networks	0.301***	0.301***	0.301***	0.353***
	(0.0202)	(0.0202)	(0.0241)	(0.0314)
Startup×Year FE	Y	Y	Y	Y
VC×Year FE	Y			
VC×Year×Industry FE		Y	Y	Y
Observations	4,312,352	4,303,167	2,136,298	2,349,578
R-squared	0.219	0.220	0.217	0.218

Note: This table estimates the alumni network effects on investment choice by equation 1.3.1, but restrict to the hiring events after current partners' leaving. The dependent variable is an indicator variable that equals to 100 if the VC invest in the startup and zero otherwise. Column 1 regresses on the full leaving sample with investor-year fixed effects and company-year fixed effects. Column 2 also regresses on the full leaving sample, except including VC-year-industry fixed effects and company-year fixed effects. Column 3 only includes startups having founders from top universities, defined as universities with top 10 business programs according to US News ranking in 2020. Column 4 only includes deals in seed or earlier rounds. Below the point estimation, the row "relative to baseline" reports the size of the effect relative to the baseline of outcome variable before events. The standard errors are clustered by VC companies. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table 1.10: Placebo Test: Effects of Alumni Networks to Non-Partners

	Full Sample (1)	Top Sample (2)	Early Sample (3)
Alumni of New Employee ×After Hiring	-0.0414	0.0176	0.00399
	(0.0273)	(0.0340)	(0.0391)
Alumni of New Employee	0.298***	0.169***	0.345***
	(0.0292)	(0.0287)	(0.0419)
Alumni of Incumbent Partner	0.166***	0.122***	0.200***
	(0.0235)	(0.0295)	(0.0358)
Professional Networks	0.318***	0.302***	0.378***
	(0.0193)	(0.0225)	(0.0310)
Startup×Year FE	Y	Y	Y
VC×Year×Industry FE	Y	Y	Y
Observations	5,371,672	2,667,494	2,925,793
R-squared	0.220	0.215	0.218

Note: This table estimates the effects of alumni networks to non-partner employees on investment choice by equation 1.3.1. The dependent variable is an indicator variable that equals to 100 if the VC invest in the startup and zero otherwise. Column 1 regresses on the full non-partner hiring sample with VC-year-industry fixed effects and company-year fixed effects. Column 2 only includes startups having founders from top universities, defined as universities with top 10 business programs according to US News ranking in 2020. Column 3 only includes deals in seed or earlier rounds. The standard errors are clustered by VC companies. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table 1.11: Alumni Network Effects on Investment Performance

Dependent Variable	All Deal Fail (1)	Lead Deal Fail (2)	All Deal Acquire (3)	Lead Deal Acquire (4)	All Deal IPO (5)	Lead Deal IPO (6)
Alumni of New Partner ×After Hiring	0.0128*	0.0500***	-0.0210**	-0.0461**	-0.00646**	-0.0202***
	(0.00703)	(0.0178)	(0.00915)	(0.0202)	(0.00308)	(0.00549)
relative to Baseline	7.69%	22.93%	-6.80%	-17.53%	-21.46%	-66.45%
Alumni of New Partner	-0.0200***	-0.0544***	0.00370	0.0210	0.00717**	0.0190***
	(0.00606)	(0.0153)	(0.00825)	(0.0180)	(0.00286)	(0.00553)
After Hiring	-0.0143**	-0.0416***	0.00585	0.0144	0.00166	0.0122***
	(0.00591)	(0.0136)	(0.00674)	(0.0143)	(0.00221)	(0.00420)
Top University	-0.0223***	-0.0193**	0.0136***	0.0224**	-0.00390***	-0.00381
	(0.00385)	(0.00862)	(0.00438)	(0.00953)	(0.00150)	(0.00241)
Alumni of Incumbent Partner	-0.00499	0.00687	-0.00290	-0.0117	-0.00357	0.000178
	(0.00471)	(0.00952)	(0.00585)	(0.0117)	(0.00249)	(0.00410)
Professional Networks	-0.0235***	-0.0240***	0.0154***	0.0172**	-0.0233***	-0.0218***
	(0.00438)	(0.00754)	(0.00485)	(0.00806)	(0.00250)	(0.00489)
Industry×State×Year FE	Y	Y	Y	Y	Y	Y
VC×Industry FE	Y	Y	Y	Y	Y	Y
Observations	63,090	17,917	63,090	17,917	63,090	17,917
R-squared	0.437	0.495	0.411	0.410	0.454	0.435

Note: This table estimates the effects of alumni networks on investment performance by equation 1.5.1. VC-industry fixed effects and industry-state-year fixed effects are included. Control variables includes alumni status with incumbent partners, professional connections, startup founding year, deal type, and the total number of investors in the deal. Odd-numbered columns regress on full deal sample, and even-numbered columns restrict to deals where the focal investor is a leading investor. The dependent variable in columns 1 and 2 is an indicator variable that equals to one if startup goes bankrupt. Columns 3 and 4 regress on an indicator variable that equals to one if startup gets acquired. Columns 5 and 6 focus on an indicator variable that equals to one if startup goes IPO. The row "relative to baseline" reports the size of the effect relative to the baseline of outcome variable before events. The standard errors are clustered by VC companies. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table 1.12: Alumni Network Effects on Investment Performance-Portfolio Return

Dependent Variable	All ln(Return) (1)	Lead ln(Return) (2)	All ln(IRR) (3)	Lead ln(IRR) (4)
Alumni of New Partner $\times$After Hiring	-0.291***	-0.305*	-0.0399*	-0.0904*
	(0.0906)	(0.160)	(0.0205)	(0.0480)
relative to Baseline	-24.87%	-35.92%	-20.36%	-60.67%
Alumni of New Partner	0.561***	0.634***	0.0948***	0.164***
	(0.0820)	(0.140)	(0.0174)	(0.0392)
After Hiring	0.226***	0.228	0.0433**	0.0678
	(0.0822)	(0.155)	(0.0203)	(0.0471)
Year FE	Y	Y	Y	Y
VC FE	Y	Y	Y	Y
Observations	15,868	4,945	13,014	3,944
R-squared	0.234	0.283	0.288	0.343

Note: This table estimates the effects of alumni networks on portfolio return by equation 1.5.2. Specifically, for each VC company at year t, I bundle all new alumni companies invested this year into a new-alumni portfolio at year t and other invested companies at t into a control portfolio. Then I define the return as the sum of the valuation of portfolio startups divided by the total amount invested in the portfolio. Columns 1 and 3 construct portfolios using all deals, and columns 2 and 4 construct portfolios by leading deals. The dependent variable in columns 1 and 2 is the logarithm of the portfolio return. Column 3 and 4 regress on the logarithm of the portfolio internal rate of return (IRR). VC fixed effects and year fixed effects are included. The row "relative to baseline" reports the size of the effect relative to the baseline of outcome variable before events. The standard errors are clustered by VC companies. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table 1.13: Alumni Network Effects on Investment Performance-Early Rounds

Dependent Variable	All Deal Fail (1)	Lead Deal Fail (2)	All Deal Acquire (3)	Lead Deal Acquire (4)	All Deal IPO (5)	Lead Deal IPO (6)
Alumni of New Partner ×After Hiring	0.0147	0.0590**	-0.0216*	-0.0491**	-0.00371*	-0.00433*
	(0.0109)	(0.0241)	(0.0126)	(0.0231)	(0.00221)	(0.00253)
Relative to Baseline	6.77%	21.38%	-7.85%	-22.84%	-33.64%	-55.84%
Alumni of New Partner	-0.0291***	-0.0653***	0.0102	0.0438**	0.00439**	0.00293
	(0.00866)	(0.0203)	(0.0110)	(0.0204)	(0.00206)	(0.00240)
After Hiring	-0.0136	-0.0363**	0.00686	0.00374	0.00316***	0.00495**
	(0.00915)	(0.0179)	(0.0101)	(0.0172)	(0.00118)	(0.00244)
Top University	-0.0276***	-0.0211*	0.0147**	0.0199*	0.00106	0.00205*
	(0.00651)	(0.0116)	(0.00646)	(0.0104)	(0.00103)	(0.00119)
Alumni of Incumbent Partner	-0.0104	0.00996	0.00708	-0.00804	-0.00178	0.00238
	(0.00829)	(0.0143)	(0.00955)	(0.0150)	(0.00145)	(0.00241)
Professional Networks	-0.0213***	-0.0189*	0.0128*	0.0146	-0.00373***	-0.00287*
	(0.00737)	(0.0114)	(0.00679)	(0.00973)	(0.00131)	(0.00168)
Industry×State×Year FE	Y	Y	Y	Y	Y	Y
VC×Industry	Y	Y	Y	Y	Y	Y
Observations	29,823	10,609	29,823	10,609	29,823	10,609
R-squared	0.476	0.500	0.446	0.395	0.533	0.441

Note: This table estimates the effects of alumni networks on investment performance in early rounds by equation 1.5.1. The sample only includes deals in seed rounds or earlier. VC-industry fixed effects and industry-state-year fixed effects are included. Control variables includes alumni status with incumbent partners, professional connections, startup founding year, deal type, and the total number of investors in the deal. Odd-numbered columns regress on full deal sample, and even-numbered columns restrict to deals where the focal investor is a leading investor. The dependent variable in columns 1 and 2 is an indicator variable that equals to one if startup goes bankrupt. Columns 3 and 4 regress on an indicator variable that equals to one if startup gets acquired. Columns 5 and 6 focus on an indicator variable that equals to one if startup goes IPO. The standard errors are clustered by VC companies. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table 1.14: Alumni Network Effects on Investment Performance-VC Age

Dependent Variable	All Deal Fail (1)	Lead Deal Fail (2)	All Deal Acquire (3)	Lead Deal Acquire (4)	All Deal IPO (5)	Lead Deal IPO (6)
Alumni of New Partner ×After Hiring	0.0212**	0.132***	-0.0326**	-0.104***	-0.0115**	-0.00801
×Young VC	(0.0102)	(0.0333)	(0.0145)	(0.0341)	(0.00465)	(0.00777)
Alumni of New Partner ×After Hiring	0.00730	0.0177	-0.0126	-0.0222	-0.00406	-0.0254***
×Old VC	(0.00950)	(0.0191)	(0.0119)	(0.0241)	(0.00405)	(0.00736)
Top University	-0.0223***	-0.0192**	0.0136***	0.0222**	-0.00396***	-0.00379
	(0.00386)	(0.00857)	(0.00439)	(0.00954)	(0.00150)	(0.00241)
Alumni of Incumbent Partner	-0.00249	0.00739	-0.00192	-0.00915	-0.00347	0.000127
	(0.00482)	(0.00917)	(0.00613)	(0.0119)	(0.00251)	(0.00408)
Professional Networks	-0.0233***	-0.0240***	0.0155***	0.0175**	-0.0233***	-0.0218***
	(0.00437)	(0.00756)	(0.00486)	(0.00803)	(0.00249)	(0.00488)
Industry×State×Year FE	Y	Y	Y	Y	Y	Y
VC×Industry	Y	Y	Y	Y	Y	Y
Observations	63,089	17,917	63,089	17,917	63,089	17,917
R-squared	0.437	0.495	0.411	0.410	0.454	0.435

Note: This table estimates the effects of alumni networks on investment performance under different VC ages by equation 1.5.1. VC-industry fixed effects and industry-state-year fixed effects are included. Control variables includes alumni status with incumbent partners, professional connections, startup founding year, deal type, and the total number of investors in the deal. VCs are defined as young if their founding year is after 2010. Odd-numbered columns regress on full deal sample, and even-numbered columns restrict to deals where the focal investor is a leading investor. The dependent variable in columns 1 and 2 is an indicator variable that equals to one if startup goes bankrupt. Columns 3 and 4 regress on an indicator variable that equals to one if startup gets acquired. Columns 5 and 6 focus on an indicator variable that equals to one if startup goes IPO. Some interaction terms are eliminated for brevity. The standard errors are clustered by VC companies. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table 1.15: Alumni Network Effects on Investment Performance-Top University

Dependent Variable	All Deal Fail (1)	Lead Deal Fail (2)	All Deal Acquire (3)	Lead Deal Acquire (4)	All Deal IPO (5)	Lead Deal IPO (6)
Alumni of New Partner ×After Hiring	0.0166**	0.0499**	-0.0232**	-0.0585**	-0.00577	-0.0160***
×Top	(0.00769)	(0.0200)	(0.0107)	(0.0255)	(0.00361)	(0.00609)
Alumni of New Partner ×After Hiring	0.00474	0.0519**	-0.0127	-0.0277	-0.00715*	-0.0245***
×Non-top	(0.0112)	(0.0264)	(0.0142)	(0.0237)	(0.00427)	(0.00786)
Alumni of Incumbent Partner	-0.00483	0.00731	-0.00242	-0.0112	-0.00341	0.000495
	(0.00474)	(0.00960)	(0.00585)	(0.0118)	(0.00249)	(0.00407)
Professional Networks	-0.0236***	-0.0240***	0.0157***	0.0175**	-0.0232***	-0.0218***
	(0.00439)	(0.00755)	(0.00484)	(0.00804)	(0.00249)	(0.00488)
Industry×State×Year FE	Y	Y	Y	Y	Y	Y
VC×Industry	Y	Y	Y	Y	Y	Y
Observations	63,089	17,917	63,089	17,917	63,089	17,917
R-squared	0.437	0.495	0.411	0.410	0.454	0.436

Note: This table estimates the effects of alumni networks on investment performance under different universities by equation 1.5.1. VC-industry fixed effects and industry-state-year fixed effects are included. Control variables includes alumni status with incumbent partners, professional connections, startup founding year, deal type, and the total number of investors in the deal. Universities with top 10 business programs according to US News ranking in 2020 are defined as top universities, and $Top = 1$ if the founder graduates from top universities. Odd-numbered columns regress on full deal sample, and even-numbered columns restrict to deals where the focal investor is a leading investor. The dependent variable in columns 1 and 2 is an indicator variable that equals to one if startup goes bankrupt. Columns 3 and 4 regress on an indicator variable that equals to one if startup gets acquired. Columns 5 and 6 focus on an indicator variable that equals to one if startup goes IPO. Some interaction terms are eliminated for brevity. The standard errors are clustered by VC companies. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table 1.16: Estimation Results

(a) Panel A: Parameter Estimation

	(1)	(2)	
θ	Base	Top	Other
κ_0	8.757	7.937	8.000
	(0.001)	(0.001)	(0.001)
η_0	0.081	0.126	-0.016
	(0.017)	(0.020)	(0.027)
κ_1	6.826	7.000	6.999
	(0.000)	(0.001)	(0.001)
η_1	0.205	0.200	0.201
	(0.017)	(0.021)	(0.021)
b	0.0025	0.0053	
# of Moments	144	288	
# of Parameters	5	9	
Func	11.001	18.641	

(b) Panel B: Counterfactual Results

	Base		Top		Non-Top	
	(1)		(2)		(3)	
	P	Π	P	Π	P	Π
κ	-11.3%	-23.9%	-6.0%	-12.7%	-6.4%	-13.5%
η	-19.8%	37.8%	-12.0%	22.8%	-35.1%	67.2%

Note: Panel A presents the parameter estimation results. Column 1 presents the baseline estimation. Column 2 differentiates the estimation for top university alumni (universities with top 10 business programs according to US News ranking in 2020). The standard errors are listed in parentheses. Panel B presents the counterfactual results. Row κ presents the investment change/performance change if shutting down the information channel, and row η shows the investment change/performance change if shutting down the preference channel. Left side (P) of each column shows the counterfactual changes in total investments, and right side (Π) shows the changes in investment performance. Column 1 presents the baseline results. Column 2 separates results for top alumni (universities with top 10 business programs according to US News ranking in 2020) and column 3 for non-top universities

Table 1.17: Alumni Network Effects on Investment Choice-Two Hiring Types

	Full Sample		Top Sample	Early Sample
	(1)	(2)	(3)	(4)
Alumni of New Partner-First	0.139***	0.144***	0.125***	0.202***
After Hiring	(0.0337)	(0.0334)	(0.0401)	(0.0504)
relative to Baseline	9.93%	10.29%	8.93%	14.43%
Alumni of New Partner-Additional	-0.000445	0.0187	0.0618	0.0256
After Hiring	(0.0576)	(0.0580)	(0.0610)	(0.0846)
relative to Baseline	-0.03%	1.34%	4.41%	1.83%
Alumni of New Partner-First	0.229***	0.220***	0.105***	0.251***
	(0.0258)	(0.0256)	(0.0329)	(0.0320)
Alumni of New Partner-Additional	0.164***	0.155***	0.101*	0.196**
	(0.0542)	(0.0544)	(0.0559)	(0.0792)
Alumni of Incumbent Partner	0.265***	0.274***	0.180***	0.324***
	(0.0281)	(0.0281)	(0.0364)	(0.0406)
Professional Networks	0.323***	0.327***	0.322***	0.369***
	(0.0187)	(0.0191)	(0.0220)	(0.0294)
Startup×Year FE	Y	Y	Y	Y
VC×Year FE	Y			
VC×Year×Industry FE		Y	Y	Y
Observations	6,199,796	6,185,555	3,070,864	3,344,579
R-squared	0.226	0.220	0.215	0.219

Note: This table compares the effects of alumni networks on investment probability after *first hiring* (hiring a new partner with a different educational background) and *additional hiring* (hiring a new partner with the same educational background). The dependent variable is an indicator variable that equals to 100 if the VC invest in the startup and zero otherwise. Column 1 regresses on the full leaving sample with investor-year fixed effects and company-year fixed effects. Column 2 also regresses on the full leaving sample, except including VC-year-industry fixed effects and company-year fixed effects. Column 3 only includes startups having founders from top universities, defined as universities with top 10 business programs according to US News ranking in 2020. Column 4 only includes deals in seed or earlier rounds. Below the point estimation, the row "relative to baseline" reports the size of the effect relative to the baseline of outcome variable before events. The standard errors are clustered by VC companies. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table 1.18: Alumni Network Effects on Investment Performance-Two Hiring Types

Dependent Variable	All Deal Fail (1)	Lead Deal Fail (2)	All Deal Acquire (3)	Lead Deal Acquire (4)	All Deal IPO (5)	Lead Deal IPO (6)
Alumni of New Partner-First After Hiring	0.0132*	0.0563***	-0.0144	-0.0284	-0.00914***	-0.0200***
	(0.00769)	(0.0203)	(0.0101)	(0.0214)	(0.00330)	(0.00579)
relative to Baseline	7.93%	25.81%	-4.53%	-10.65%	-30.23%	-66.45%
Alumni of New Partner-Additional After Hiring	0.0116	0.0365	-0.0377**	-0.0848**	0.000339	-0.0205**
	(0.0101)	(0.0234)	(0.0168)	(0.0376)	(0.00569)	(0.00874)
relative to Baseline	6.97%	16.74%	-11.97%	-31.94%	1.13%	-66.45%
Alumni of New Partner-First	-0.0190***	-0.0575***	0.000232	0.00996	0.00965***	0.0195***
	(0.00667)	(0.0170)	(0.00903)	(0.0190)	(0.00318)	(0.00601)
Alumni of New Partner-Additional	-0.0275**	-0.0550**	0.00613	0.0342	-0.000716	0.0153
	(0.0111)	(0.0255)	(0.0173)	(0.0400)	(0.00527)	(0.0109)
After Hiring	-0.0141**	-0.0414***	0.00612	0.0149	0.00176	0.0123***
	(0.00592)	(0.0135)	(0.00673)	(0.0143)	(0.00220)	(0.00419)
Top University	-0.0225***	-0.0196**	0.0133***	0.0220**	-0.00396***	-0.00390
	(0.00387)	(0.00861)	(0.00438)	(0.00960)	(0.00152)	(0.00245)
Alumni of Incumbent Partner	0.00219	0.0180	0.00668	0.00661	-0.00160	0.00381
	(0.00817)	(0.0157)	(0.0111)	(0.0243)	(0.00368)	(0.00746)
Professional Networks	-0.0234***	-0.0238***	0.0157***	0.0177**	-0.0233***	-0.0218***
	(0.00440)	(0.00756)	(0.00485)	(0.00803)	(0.00249)	(0.00490)
Industry×State×Year FE	Y	Y	Y	Y	Y	Y
VC×Industry FE	Y	Y	Y	Y	Y	Y
Observations	63,089	17,917	63,089	17,917	63,089	17,917
R-squared	0.437	0.495	0.411	0.410	0.454	0.435

Note: This table compares the effects of alumni networks on investment performance after *first hiring* (hiring a new partner with a different educational background) and *additional hiring* (hiring a new partner with the same educational background). VC-industry fixed effects and industry-state-year fixed effects are included. Control variables includes alumni status with incumbent partners, professional connections, startup founding year, deal type, and the total number of investors in the deal. Odd-numbered columns regress on full deal sample, and even-numbered columns restrict to deals where the focal investor is a leading investor. The dependent variable in columns 1 and 2 is an indicator variable that equals to one if startup goes bankrupt. Columns 3 and 4 regress on an indicator variable that equals to one if startup gets acquired. Columns 5 and 6 focus on an indicator variable that equals to one if startup goes IPO. The row "relative to baseline" reports the size of the effect relative to the baseline of outcome variable before events. The standard errors are clustered by VC companies. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Chapter 2

Signaling in Venture Debt and Capital

Coauthored with Sizhu Lu

2.1 Introduction

Conventional wisdom regards equity as the pivotal financing vehicle for new firms, especially high-tech firms that invest vast money in research and development before generating positive cash flows. Under greatly asymmetric information and a high risk of failure, classic corporate finance theory suggests that debt is not a wise choice in startup financing. Under the context of the agent problem, classic models predict that fast-growing industries and firms with negative cash inflows and high risks should have low leverage (Jensen and Meckling, 1976; Leland and Pyle, 1977; De Meza and Webb, 1987). Winton and Yerramilli (2008) find that under high uncertainty of continuation with risky cash flow distribution, low liquidation, low probability of success, and high returns if successful, equity financing is optimal. Ueda (2004) argues that entrepreneurs seek venture capital financing when they have little collateral and when they require larger investment amounts.

However, in contrast to the prediction classic theory makes, debt financing in the early-round financing market, known as venture debt, is unexpectedly active, and has experienced steady growth in recent years. Roughly \$1 - \$5 billion of venture debt in total are offered to startups annually (Ibrahim, 2010), and are observed in 28-40% of startups with venture financing (Davis et al., 2018). Venture debt is usually offered to venture-backed startups in the technology and health care industry at all stages, mostly after series A or series B. It has a maturity of three to five years, usually starting with a 6-to-12-month interest-only period. Unlike traditional loans, venture debt does not require property or equipment as collateral, which adds a higher risk to venture debt. To compensate for the high risk, venture debt investors ask for warrants of around 8% of the deal size, typically less than 1% of the company's total equity. Unlike convertible bonds with features similar to equity, venture debt is non-convertible. While seemingly risky, venture debt requires interest rates around 2% plus the prime rate, and the rates may be higher when the deal size is larger.

The puzzling existence of venture debt has attracted scholarly attention, with a growing literature documenting and studying the use of venture debt. A pervasive opinion explains the demand side of the venture debt market, arguing that startups are in favor of venture debt since it reduces dilution from equity financing and helps them reach milestones before the next round of funding. Both models and empirical results illustrate that venture debt helps firms that face high dilution and low pre-money valuation to reduce dilution by extending the runway (Ibrahim, 2010; Davis et al., 2018).

However, the supply side of venture debt remains understudied. To the best of our knowledge, this paper is the first to answer the puzzle of why venture debt investors are willing to offer seemingly risky debt at a relatively low interest rate. Are these debts mispriced, or are these debts far less risky than presumed? According to statistics from Silicon Valley Bank, which is a primary investor in venture debt, the default rate of venture debts in 2019 was 0.3%, a number that is fabulously low compared with the failure rate of venture-backed startups, which is around 25% according to industry reports. Based on this fact, this paper states that venture debt, though unsecured by conventional collaterals, is far less risky than presumed. This paper will elucidate a signaling channel that contributes to diminishing the risk of venture debt, supported by empirical results.

The startup financing market features tremendous asymmetric information. When venture debt does not play a role in startup financing, venture capital investors have to exert great effort to eliminate asymmetric information and invest in promising firms. However, the entrance of venture debt provides venture capital investors with more choices. The return of venture debt depends on the likelihood of a firm's continuing operation and getting the next round of funding from venture capital, so venture debt investors screen startups that have outstanding performance and may well succeed in raising funds. Venture debt prefers startups that are invested in by well-known venture capital investors in previous rounds, or with a steady and fast growth rate. Therefore, the firms venture debt investors select will on average have a higher probability of success and better performance than those without venture debt. In the next financing round, venture capital investors will take venture debt as a positive signal, presuming that firms with venture debt are more likely to be good firms. To save on the high cost of due diligence, venture capital investors investigate these firms less and invest. In this way, venture debt increases the probability of a firm getting next-round funding, making the venture debt more likely to be paid off and hence lower the risk of venture debt itself. Anticipating this logic, venture debt investors have the incentive to do a relatively rough screening and issue debt to a small portion of low-quality firms. Though the quality of firms with venture debt is not ideal, the signaling effect still exists as long as the overall performance is sufficiently better than firms without venture debt. Venture capital investors will still invest in firms with venture debt without careful due diligence since they can cover the loss of investing in low-quality firms by saving due diligence costs when they are sufficiently high.

To illustrate this signaling, we establish a three-period simplified model, involving three parties, startups, venture capital investors (VCs), and banks that provide the venture debt. There are two types of startups, high-type, and low-type. While only startups know their type, banks can choose the level of screening cost to filter out some portion of the low types, and VCs can uncover the type after investigation at some cost. At time 0, startups can borrow venture debt from the bank, and the bank decides how much effort to take in screening and whether to lend the venture debt. At time 1, firms have their states realized. After observing the realization of firms at time 1 and banks' investment decisions, the VC updates the conditional probability of a startup firm being high-type, then chooses whether to investigate at some cost to reveal the type of the firm and whether to invest in it. After the VC's decision, the debt starts to get paid. If the VC invests in the firm with debt, then the debt is paid off at the end of time 1. Otherwise, the firm defaults, and banks receive nothing. We solve for equilibria and prove the existence of the signal effect. When the due diligence cost is sufficiently high, if the firms with debt are in a good state at time 1, it is optimal for the venture capital investors to take banks' screening into account and invest in firms with debt without due diligence, to save costs. However, in terms of investment performance, the signal effect results in over-investment in low-type firms.

The model provides four predictions that will be supported by empirical results. First, firms with venture debt have a shorter time gap between funding rounds. As suggested in the model, in equilibrium, venture capital investors do less careful due diligence when assessing a startup firm with venture debt, and this will shorten the period of investigation. As a result, it takes less time for startups with venture debt to get the next round of funding than those without debt. Second, firms with venture debt have better long-term performance. Because banks filter out a portion of low-type firms by their screening, the pool of startups with venture debt will have better performance on average. However, our third prediction shows that conditional on getting the next rounds of venture capital, the performance of firms with venture debt is worse compared with the counterfactual world without venture debt. As we have seen, taking debt financing as a signal, VCs prefer to do less careful due diligence to save costs when confronting firms with venture debt. Since banks do not prudently filter out all low-type firms, next-round venture capital investors will invest in both high-type and low-type entrepreneurs. However, if VCs see a startup without venture debt in the next round of funding, they *will* do sufficient due diligence and invest only in high-type firms, resulting in a much lower probability of investing in low-type firms. Therefore, conditional on getting next-round financing, the average long-term performance of firms with venture debt is expected to be worse than the average of those without venture debt. Finally, our model predicts that more severe asymmetric information reinforces the signaling effect. When faced with more severe asymmetric information, namely higher due diligence costs, VCs get more benefit from taking the signal of venture debt and thus are more willing to get a free ride on the banks' screening results, leading to stronger signaling effects when venture capital investors have less information.

In the empirical section, we test these four predictions. We find strong empirical evidence in line with the model predictions. We use CrunchBase data, which contains the funding round history of startups with information on the investors, investment types, series rounds, funding size, and the announcement dates. First, we find that it takes a startup with venture debt a significantly shorter time to get the next round of funding, which indicates that VCs indeed take venture debt as a good signal when making their investment decisions. Comparing the length of time between two funding rounds, it takes around 93 days less for startups with venture debt to get funds from a next-round investor. Second, in the long time horizon, we find that startups with venture debt have significantly better performance. To measure long-term performance, we investigate several variables of a startup: whether it has closed, gone public, been acquired, or is still operating. Startups with venture debt show better long-term performance evaluated using different measures. They have a marginally (at mean) 3.33% lower probability of suspending operation (at a significance level of 0.01), and are 0.45% more likely to achieve an IPO. However, conditional on getting the next round of funding, our model predictions are reserved. Such startups with venture debt have relatively worse average long-term performance, which is indicated by a marginally (at mean) 1.90% higher probability to close and 1.47% lower likelihood of going public. Again, this results from the VCs taking signals from banks' decisions without careful due diligence and investing in a pool containing both high-type and low-type firms. Lastly, empirical results show that the signaling effect of venture debt is stronger when the asymmetric information problem between the startups and investors is more severe. We test and find that if startups are funded by experienced investors in the next round, the length of time for startups with venture debt to get next round funding is 39 days less compared with if they are invested by inexperienced investors.

Several types of robustness checks are provided to test the sensitivity of our results to different industries and startups founded during different years. The primary empirical evidence remains the same when we do the same types of empirical analysis in various industries and when we use the subsample by choosing companies founded in different time slots. Also, when we test both the unconditional and conditional long-term performance of startups, the startups used in the conditional test are a subsample. We check the robustness of our first empirical prediction on the subsample, which is precisely the same as what we used for the conditional long-term performance test. The pattern that it takes significantly less time for startups with venture debt to raise next-round funding is quite robust.

It is worth noting that when banks make decisions on whether to lend venture debt to a startup, they prefer startups backed by influential venture capital investors. Therefore, one possible concern is that the signaling effect can be caused by influential VCs rather than venture debt. Based on the extraordinary investment performance of these VCs, it is wise for future investors to follow them when making investment decisions. The well-known VCs can also send similar signals. To distinguish the signal of venture capital and venture debt, we do a robustness check to see the impact of being backed by a good venture capital investor. The result shows that while ever invested by a well-performed VC indicates better long-term performance, it also shows better conditional long-term performance, which is substantially distinct from the signaling effect of venture debt.

We enrich the traditional theoretical literature on early-stage startup financing. Agency problems and bankruptcy costs make equity a more favorable financing tool for firms with high risk and low value of the collateral (Jensen and Meckling, 1976; Leland and Pyle, 1977; De Meza and Webb, 1987; Harris and Raviv, 1991). These models do not capture the critical features of venture debt—that it is a relatively short-term contract, and its risk depends on the success of future funding rounds instead of a firm's final success.

We also contribute to the growing literature on venture lending in the early stage of startups by studying the rise of venture debt from the perspective of demand. De Rassenfosse and Fischer (2016) and Davis et al. (2018) show theoretical and empirical evidence that venture debt enables startups to attain more milestones and prevent further dilution. Davis et al. (2018) also find that firms demanding venture debt face higher potential dilution and exhibit lower pre-money valuations. From the perspective of investors, Hochberg et al. (2018) focus on patent-backed venture debt and find that the credibility of VC commitments increases lending. De Rassenfosse and Fischer (2016) argue that being backed by a VC company increases the probability that a startup will obtain venture debt. Cumming and Fleming (2013) study the determinants of returns of venture lending, highlighting the role of time allocation for due diligence and monitoring. This paper builds on this literature in the way that we address the signal effect of having venture debt and explains the seemingly low return of venture debt, which is a puzzle that none of the above papers explains.

We also relate to the literature on the impact of debt financing on startups and innovation in any stage of growth. In addition to the above literature that shows that debt reduces dilution, Geelen et al. (2019) find that while debt hinders innovation due to debt overhang, it encourages entry, fostering growth at the aggregate level. Hombert and Matray (2016) study how relationship lending determines the financing of innovation. Albertus and Denes (2019) document the emergence of debt financing by private equity funds. They find that funds using debt financing tend to reduce the amount of equity invested relative to fund size and delay capital calls. Our research contributes to this literature by providing evidence that as a result of the signaling effect, venture debt induces overinvestment from venture capital investors in later stages.

A branch of related literature studies the signaling effect in venture capital investment, mainly focusing on the patent signaling effect. Howell (2017) show that an early-stage award from the Department of Energy's SBIR grant program significantly increases the probability that a firm receives venture capital. Conti et al. (2013) find that patents serve as a positive signal to attract investors. Our paper also shows the evidence that venture capital investors exploit signal effects when making investment decisions, but focuses on the signal effect of having debt on the balance sheets, which has not been studied yet.

The rest of this paper is organized as follows: Section 2.2 develops our model and the equilibrium. Section 2.3 introduces the database we use in this paper and the variables of interest. It also presents the empirical implications of our model, with the test and results presented in Section 2.4. Section 2.5 provides several further robustness tests, and Section 2.6 concludes.

2.2 Model

In this section, we develop a three-period model to illustrate the signaling effect of venture debt. In the model, asymmetric information exists, and both banks (the venture debt providers) and VCs make efforts to screen the firms. Banks move first to do the screening at a cost and provide venture debt to firms that pass the screening. In the second stage, VCs observe the mid-stage realization of firms' valuation and decide whether to do due diligence and invest in the firms. As the banks filter out some bad firms, the VCs may well get a free ride and do less due diligence. Anticipating the VCs' behavior, banks will make fewer efforts in first-stage screening. The following subsections will formally model this intuition.

2.2.1 Model Setup

There are three periods $t \in \{0, 1, 2\}$ and three parties: startup firms, venture capital investors, and banks. Firms are either high type or low type with a proportion of α being a high type, which is private information to firms. In t_0, firms ask for venture debt from banks, and banks do costly investigations into these applicants. By choosing the level of screening cost C_B, banks can filter out part of the low-type firms thus increasing the proportion of high-type firms to $\alpha(C_B)$. After screening, banks provide debt of a fixed amount of D to firms passing the screening. $\alpha(C_B)$ is a concave monotonically increasing function whose value range is between α and 1, with $\alpha(0) = \alpha$ and $\lim_{c\to\infty} \alpha(c) = 1$. At time t_1, firms' mid-stage value is realized. The value of firms is $V = a$ with probability P_i, where $i \in \{H, L\}$ is an indicator of firm type, and $V = b < a$ otherwise. After observing the realization and the bank's investment decisions, the venture capital investor takes action. They decide whether to do due diligence, and whether to invest a fixed amount of I in the firms. Since VCs have more information sources and better knowledge in due diligence, unlike banks, they can reveal the type of the firms at a fixed cost of C. Afterward, startup firms that receive venture capital investment pay off the venture debt; otherwise, firms default and banks get 0. In t_2, firms' final valuations are realized, whose expectation in t_1 is μ_i, where $i \in \{H, L\}$, and VC's return is realized.

Banks are rational and maximize their payoff $E(R) - C_B - D$ by

$$\max_{C_B} \Pi_B = -C_B - D + P_I(C_B)R,$$

where P_I is the probability of debt being paid off, namely the probability of VC investing in the firms. Assuming a competitive venture debt market, banks receive payment of R such that

$$\Pi_B = -C_B - D + P_I(C_B)R = 0.$$

The maximization problem is equivalent to solving the following equations.

$$
\begin{aligned}
&-1+\frac{dP_I}{dC_B}R=0, \quad (\text{ F.O.C. })\\
&-C_B-D+P_IR=0.
\end{aligned}
$$

Suppose banks spend a screening cost of C_B^*, then the proportion of good types in firms with venture debt becomes $\alpha^*=\alpha(C_B^*)$. Since the firms without venture debt are low-type firms that are filtered out by banks, VC will only consider firms with venture debt. Based on banks' decisions, venture capital investors will update their belief of the probability of a firm being a high type conditional on having venture debt. When observing $V=a$, the updated probability of being a high type becomes

$$
\alpha_a^*=\alpha_a\left(c_B^*\right)=\frac{\alpha\left(c_B^*\right)P_H}{\alpha\left(C_B^*\right)P_H+\left(1-\alpha\left(C_B^*\right)\right)P_L},
$$

and VCs maximize payoff by

$$
\max\left\{-cI+\alpha_a^*\left(\frac{aI}{I+a}\mu_H-I\right),\frac{aI}{I+a}\left(\alpha_a^*\mu_H+\left(1-\alpha_a^*\right)\mu_L\right)-I,0\right\},
$$

where $c=C/I$. VCs compare the payoff of three options: doing due diligence and investing in high-type firms, investing in all firms without due diligence, or doing nothing. Three arguments in the maximization problem correspond to these three options. Solving the problem, we get the venture capital investor to do the following actions.

1. When $c<c_a(C_B^*)=(1-\alpha_a(C_B^*))\,\frac{I+a-a\mu_L}{I+a}$ and $c<\alpha_a(C_B^*)\left(\frac{a\mu_H}{I+a}-1\right)$, do due diligence and invest in high type firms.

2. When $c>c_a(C_B^*)$ and $\alpha_a^*\mu_H+\left(1-\alpha_a^*\right)\mu_L-1>\frac{I}{a}$, invest in all firms with venture debt.

3. Otherwise, do not invest.

Similarly, when observing $V=b$, the updated probability becomes

$$
\alpha_b^*=\alpha_b(C_B^*)=\frac{\alpha_b(C_B^*)\left(1-P_H\right)}{\alpha_b(C_B^*)\left(1-P_A\right)+\left(1-\alpha_b(C_B^*)\right)\left(1-P_L\right)},
$$

and the venture capital investor maximize

$$
\max\left\{-CI+\alpha_b^*\left(\frac{bI}{I+b}\mu_H-I\right),\frac{bI}{I+b}\left(\alpha_b^*\mu_H+\left(1-\alpha_b^*\right)\mu_L\right)-I,0\right\}.
$$

The actions VC takes are

1. When $c < c_b(C_B^*) = (1-\alpha_b(C_B^*))\frac{I+b-b\mu_L}{I+b}$ and $c < \alpha_b(C_B^*)\left(\frac{b\mu_H}{I+b}-1\right)$, do due diligence and invest in high-type firms.

2. When $c > c_b(C_B^*)$ and $\alpha_b^*\mu_H + (1-\alpha_b^*)\,\mu_L - 1 > \frac{I}{b}$, invest in all firms with venture debt.

3. Otherwise, do not invest.

Meanwhile, in a world without venture debt, which is equivalent to the circumstance that $C_B = 0$, we assume that VC will always do due diligence and invest in high-type firms. In order to restrict to this condition, μ_i and c are assumed to satisfy the following conditions.

Assumption 1 *The following conditions are satisfied.*

$$\begin{aligned}
&\mu_L < \frac{I+a}{a},\\
&\mu_H > \frac{I+b}{b},\\
&c < \alpha_a(0),\\
&c < \alpha_b(0)\left(\frac{b\mu_H}{I+b}-1\right),\\
&\alpha_a(0)\mu_H + (1-\alpha_a(0))\,\mu_L - 1 > \frac{I}{a},
\end{aligned}$$

where

$$\begin{aligned}
\alpha_a(x) &= \frac{\alpha(x)P_H}{\alpha(x)P_H + (1-\alpha(x))P_L},\\
\alpha_b(x) &= \frac{\alpha(x)\,(1-P_H)}{\alpha(x)\,(1-P_H) + (1-\alpha(x))\,(1-P_L)}, \quad \textit{and}\\
c_a(x) &= (1-\alpha_a(x))\,\frac{I+a-a\mu_L}{I+a}.
\end{aligned}$$

The first two assumptions ensure that there exists c that satisfies the other two criteria. The third and fourth assumptions allow the investment in high-type firms after due diligence to be the best choice for VC when there is no venture debt in the market. The last assumption assumes that the payoff of investing in all firms is positive, which will simplify the discussion of equilibria in the following subsection.

2.2.2 Equilibrium

According to the level of c, there are three possible equilibria in this model described formally below.

Theorem 1 (Equilibrium) *There are three possible equilibria in this model:*

1. *When $c < c_a(C_B^0)$, banks take efforts of C_B^0 and $R^0 = \frac{C_B^0+D}{\alpha(C_B^0)}$. In t_1, VCs always invest in high-type firms after due diligence.*

2. *When $c > c_a(C_B^2)$, banks take efforts of C_B^2 and $R^2 = \frac{C_B^2+D}{\alpha(C_B^2)+(1-\alpha(C_B^2))P_L}$. VCs invest in all firms with venture debt when $V = a$, and invest in high-type firms after due diligence when $V = b$.*

3. *When $c \in (c_a(C_B^0), c_a(C_B^2))$, banks take efforts of C_B^1 and $R^1 = \frac{C_B^1+D}{\alpha(C_B^1)+(1-\alpha(C_B^1))P_L p}$. When $V = a$, VCs play a mixed strategy of investing in all firms with venture debt with some probability of p and investing in high-type firms after due diligence with a probability of $1-p$. When $V = b$, VC invests in high-type firms after due diligence.*

To simplify the discussion on c, we impose one assumption on c.

Assumption 2 *Assume that $c_a(0) < c_b(C_B^0)$.*

This assumption allows that when $V = b$, the investigation is a better choice for VC regardless of c.

Theorem 1 demonstrates the equilibria in this model. The actions of VC highly depend on the level of due diligence cost, as deciding between doing due diligence or not is a trade-off between avoiding bad investment and saving due diligence cost. When VCs observe a bad state, the probability of a firm being a low type is high enough that if the due diligence cost is sufficiently small, although banks partially filter out bad firms, the expected cost of investing in low-type firms still exceeds the cost of due diligence. Under this circumstance, VC will not blindly invest in all firms with venture debt but will still carefully investigate firms. Therefore, there is no signaling effect in this equilibrium. However, when c is high enough to surpass the losses of investing in low-type firms, VCs will take a free ride on banks' screening results and invest in all firms with venture debt. In this case, the signaling of venture debt does take effect. When the due diligence is moderate, VC will partially take the signal by having a mixed strategy of doing due diligence or not. The signaling effect is mitigated but still exists under this condition.

In response to the behavior of VC, banks spend different levels of screening cost and set the payoff R accordingly. The first equilibrium can be treated as the benchmark under which condition there is no signaling effect, and banks spend C_B^0 investigating firms and ask for R^0 for return. In the other equilibria, due to the existence of the signaling effect, banks set the cost to C_B^1 and C_B^2. The relation between costs in different situations can be described in the following proposition.

Proposition 1 *Under the above settings and assumptions,*

$$C_B^0 > C_B^1 > C_B^2,$$
$$R^0 > R^1 > R^2.$$

This proposition indicates that the signaling effect causes banks to do less careful screening, as the probability of a venture debt being paid off is higher when VC regards having debt as a good signal. Therefore, as due diligence cost increases hence signaling effects increase, banks spend less effort on screening. Though lower efforts result in higher default risk, reduced screening cost covers the loss in less careful investment and overall lowers R. This proposition gives a possible explanation for the puzzle raised in the beginning that why seemingly highly risky venture debt asks for moderate interest rates.

Besides the nature of venture debt, the model also predicts the performance of firms. More related to the empirical test in the next section, another proposition is straightforward to show that the existence of venture debt providers increases the quality of the pool of firms via screening. Since banks filter out some low-type firms, the average performance of firms receiving venture debt will be better.

Proposition 2 *Under the model setup,*

$$\mathbb{P}(H|\,VD) = \alpha(c) > \alpha = \mathbb{P}(H)$$

However, as VCs take venture debt as a positive signal and skip the due diligence, the signaling effect can result in venture capital investors over-investing in low-type firms. In a counterfactual world without venture debt, VCs will always do careful due diligence and invest only in high-type firms. Nevertheless, with banks screening at the first stage, VC will save the due diligence cost at the expense of over-investing in low-type firms.

Proposition 3 (Over-investment) *Given the model setup, the introduction of venture debt results in less due diligence and over-investment of VCs in low-type firms, i.e.,*

$$\begin{aligned}
&\mathbb{P}^0(L|Invest) = 0,\\
&\mathbb{P}^1(L|Invest) = \frac{pP_L(1-\alpha(C_B^1))}{pP_L(1-\alpha(C_B^2)) + \alpha(C_B^1)} > 0,\\
&\mathbb{P}^2(L|Invest) = \frac{P_L(1-\alpha(C_B^2))}{P_L(1-\alpha(C_B^2)) + \alpha(C_B^2)} > \mathbb{P}^1(L|Invest),
\end{aligned}$$

where $\mathbb{P}^i(L|Invest)$ is the conditional probability of a startup firm invested by the VCs being a low-type in equilibrium i.

Proposition 3 indicates that when the signaling effect increases, the quality of the firms that VCs invest in decreases. As the venture capital investors rely more on the screening results from banks, the banks are more likely to get the debt paid off and thus do less careful screening, which increases the probability of VCs investing in low-type firms.

Based on the above propositions, we derive four predictions for empirical tests.

Prediction 1 *Firms with venture debt have a shorter time gap between funding rounds.*

As predicted in the model, in good states VCs will take venture debt as a positive signal and do less careful due diligence, accelerating the financing process.

Prediction 2 *Firms with venture debt have better long-term performance.*

Before banks issue venture debt, they do screening and filter out some low-type firms. Thus the average quality of firms that pass the screening and get debt will be higher than the total population.

Prediction 3 *Conditional on getting the venture capital, the performance of firms with venture debt is worse compared with the counterfactual world without venture debt.*

When there is no venture debt and signaling effect, VCs do due diligence, thus firms getting venture capital are high-type firms. However, when venture debt kicks in and VCs take the signaling, some low-type firms can also get funded, impairing the average performance of startup firms that get funding.

Prediction 4 *More asymmetric information reinforces the signaling effect.*

When the problem of asymmetric information is more severe, venture capital investors are more likely to get a free ride on banks' screening results and rely on the signal, resulting in a stronger signaling effect, thus stronger effects of venture debt on firms' performance.

2.3 Data and empirical method

2.3.1 Model predictions

In our theoretical model, there are four main testable predictions.

First, venture debt acts as a good signal for a startup's next-round financing. Therefore, it's easier for startups with venture debt to get future rounds of VC financing. The signaling effect causes VCs to do less careful screening and overinvest in low-type firms. Empirically, overinvestment causes startups with venture debt to get the next round of funding faster.

Second, as shown in the equilibrium, when c is sufficiently small, the probability of a firm with venture debt being a high type is higher than the proportion of high type in the population. There is an empirical prediction based on this theoretical result. Startups with venture debt are more likely to succeed since all high-type entrepreneurs are willing to use venture debt lending, while only some of the low types are willing to. Therefore, when we look at the long-term performance of all startups, it is not surprising to see those with venture debt are more likely to succeed.

Third, the model predicts that conditional on getting the next round of VC, the performance of startups with venture debt is worse compared to the counterfactual world without venture debt. From the model, in a world without venture debt, VCs will always do due diligence and invest in only high types after the screening. As venture debt currently acts as a good signal, VCs prefer not to do careful due diligence if they encounter a startup with venture debt, and will directly invest in it. As a result of overinvestment, more low-type startups can get VC financed because of the signaling effect of venture debt. Therefore, conditional on the startups achieving later rounds of VC investment, the pool of startups with venture debt performs worse in the long term.

Finally, the model suggests that the signaling effect of venture debt is stronger when the asymmetric information problem between the startups and investors is more severe. To test this prediction, we need suitable measures for the extent of both asymmetric information and the signaling effect. It is natural to think that experienced investors who have invested in a large number of startups or been involved in a large number of funding rounds are considered to have relatively more moderate asymmetric information problems, compared with investors who do not. Assuming this, we use whether an investor is experienced as a proxy of the severity with which it suffers from asymmetric information. As for the signaling effect, it is shown in prediction 1 that startups with venture debt are getting the next round of funding in a shorter period compared with those without venture debt. We utilize this shorter length of time as our measure of the strength of the signaling effect. We compare the number of days that startups with venture debt saved when the next round investor suffers from severe asymmetric information when it only has moderate asymmetry. The model predicts the length of time shortened between two rounds should be significantly less if the next round investor is an experienced investor.

We do our empirical analysis to test these four predictions and check the robustness of our results. We find that the empirical evidence is strong and in line with the model predictions. In the following subsections, we describe the dataset used for the empirical test, introduce our definitions of some important variables, and illustrate and talk in detail about our variables of interest. We show the final results in the next section.

2.3.2 Data

The data we use in this paper is from CrunchBase, a platform that collects comprehensive information about startups, where companies and investors can get market information or fund data on the platform in exchange for reporting their own information. Data in CrunchBase reports each funding round with information on the investors, investment types, series rounds, funding size, and the announcement dates. For startups, CrunchBase has information on their founded year, the total number of funding rounds, the total amount of funding, and their current status (operating, closed, IPO, or acquired). The industry and the number of employees are also available with some missing values. We use these data to test our model predictions in the following subsections.

2.3.3 Definition of venture debt

To use CrunchBase data to test our predictions, we need to empirically identify which funding round is venture debt in the data. Venture debt appears in a venture's early stage, usually before series B. Thus, we define funding round as *venture debt* if and only if its investment type is "debt financing" and it's an early round, while we define funding round as *early round* if and only if:

1. the announced date of this round is before the angel, seed, series A, or series B, or
2. the announced date of this round is no more than two years later than the angel, seed, and series A, or
3. this round is right after series A, or
4. this startup's total number of funding rounds is less than or equal to two rounds.

2.3.4 Variables of interests

In this subsection, we define and explain our variables of interest used in the empirical tests. To test our four model predictions, we need to get the following variables:

- an indicator of whether a funding round is a venture debt round (as defined in the previous subsection),
- the speed of getting next round financing,
- whether a firm ever used venture debt as a financing method,
- measures of the long-term performance of the firms,
- measures of the severity of asymmetric information problem, and
- the strongness of the signaling effect.

First, at the funding round level, the variable `vdebt` indicates whether a funding round is a venture debt round or not, which is equal to 1 if a round has the investment type "debt financing" and is an early-round, and 0 otherwise. Next, `date_diff` is defined as the length of the time interval between a funding round and its next round, and `date_diff2` is similarly defined as the length of the time interval between its next round and the previous round. We use both variables to measure how quickly a venture gets its next-round financing. Then, at the firm level, we define `have_vdebt` as an indicator of whether a startup ever used venture debt, which is equal to 1 if it has used, and 0 otherwise.

For the measure of the long-term performance of startups, we are mainly interested in three outcome variables:

1. `closed` is an indicator of whether a startup has already closed. It is set to be 1 if the startup is closed, and 0 otherwise. If a startup is already closed, we treat this as bad long-term performance.

2. `ipo` stands for whether a venture goes public, and is equal to 1 if it ever managed to go public. This variable is naturally an indicator of long-term success.

3. `acq` indicates whether a startup is acquired, and is equal to 1 if it is acquired. We consider being acquired by other companies as an indicator of good long-term performance for now and will have more discussion on this in the later sections.

It is possible that some startups first went public and got acquired after the acquisition. As for those firms, we treat their values of `acq` as 0 and `ipo` as 1, because it shows enough evidence of good performance if it ever succeeds in going public, and the decisions to acquire a startup and a public firm are very different, we would like to get rid of the latter case in our empirical test.

Finally, to test our last prediction, we need measures of the severity of asymmetric information problems and the strongness of the signaling effect. We define `num_round` as the total number of rounds a VC has ever got involved in and calculate this value for all investors in our dataset. Then, we use whether a VC's value of `num_round` is higher than or equal to the 90th percentile of the whole population. If it is higher, we treat the VC as an experienced investor that suffers from a less severe asymmetric information problem. Similarly, `num_company` is defined as the total number of companies a VC ever invested in, also calculated and compared with its 90th percentile to get another measure of an experienced investor. To check the stability of the cutoff (90th percentile), we also use the 95th percentile to measure whether the VC is treated as an experienced investor or not. We will show our empirical results in all the cases, and it does not make much difference which one we use, as they all lead to similar results.

Table 2.1 reports summary statistics of our variables of interest. Part A summarizes the 45,350 observations at the funding round level. Part B summarizes the 21,444 observations at the startup level. Part C reports summary statistics of the same variables as Part B but on a subsample of startups whose total number of funding rounds is greater than or equal to 4. This is the subsample we use for the conditional test in the latter part.

2.4 Empirical results

Test 1: Venture debt is a good signal for next round financing

In this subsection, we test whether venture debt acts as a good signal to get the next round of financing. If this is the case, it's easier for startups with venture debt to get funding in the next round; thus, it takes a shorter period for startups with venture debt to reach the next round of funding. Our empirical strategy is to regress the length of the time interval between the next funding round and this round on a dummy variable indicating whether this round is a venture debt round or not. Considering that all venture debts are in early rounds, to be comparable, our regression sample only contains early rounds. Our regression equation is

$$\texttt{date\_diff} = \alpha_t + \beta \cdot \texttt{vdebt} + \epsilon$$

where α_t is the fixed effects, `vdebt` is an indicator of whether the financing is a venture debt round, and `date_diff` is the length of the time interval between the next round of funding and this round of funding. In addition, we also do the same test using `date_diff2` as the outcome variable of interest, where `date_diff2` is defined as the length of the time interval between the next round of funding and the previous round of funding.

We report the regression results in Table 2.2. The results indicate that the coefficient of `vdebt` is significantly negative in all the settings, and is robust to whether the year-fixed effects are included or not. This verifies the first model prediction. On average, it takes a startup with venture debt about a hundred days shorter to get the next round of funding. VCs take venture debt as a good signal and do not screen it as carefully as those without venture debt. These results are robust to the two different definitions of the length of the time interval between financing rounds, `date_diff` and `date_diff2`.

Our results are robust to outliers. When we winsorize the dependent variables, the results do not change much, and the coefficient remains significantly negative in all cases. However, we should interpret these results with caveats. Common sense in the literature and industry reports is that venture debt rounds usually come together with or right after venture capital rounds in the early phases of startup financing. Even so, it is possible that some startups seek venture debt when they are about to achieve a milestone and want to use debt financing as a way to avoid equity dilution. In that case, the power of our results as a proof of model prediction is reduced.

Test 2: Long-term performance of startups with venture debt

We test the second prediction of the model in this subsection. The model predicts that in general, startups with venture debt are likely to have better long-term performance since all high-type entrepreneurs are willing to borrow venture debt while only some low types do so. In our empirical study, we would like to test whether startups with venture debt are more likely to succeed in the long term (e.g., IPO or acquisition), and less likely to get closed. Our empirical strategy is to use the Probit model to regress the measure of long-term performance on the dummy variable indicating whether a startup ever used venture debt or not. We use three measures of a firm's long term performance: `closed`, `ipo`, and `acq`, as defined in the previous section. Specifically, the Probit regression models we use are:

$$\begin{aligned}
\texttt{outcome}' &= \alpha_t + \beta \cdot \texttt{have_vdebt} + \epsilon,\\
\texttt{ipo}' &= \alpha_t + \beta \cdot \texttt{have_vdebt} + \epsilon,\\
\texttt{acq}' &= \alpha_t + \beta \cdot \texttt{have_vdebt} + \epsilon,
\end{aligned}$$

where

- α_t are the year fixed effects;

- `have_vdebt` is an indicator equal to 1 if a startup ever used venture debt in the financing history, and 0 otherwise;
- `closed` is an indicator of whether the startup is closed;
- `ipo` is an indicator of whether the startup goes public in the end;
- `acq` is an indicator of whether the startup is acquired by an acquirer; and
- $Y' = \Phi^{-1}(Y)$ for $Y \in \{\texttt{closed}, \texttt{ipo}, \texttt{acq}\}$, where $\Phi(\cdot)$ is the cumulative distribution function of standard normal distribution.

The sample we use here are startups founded between 2001 and 2011. CrunchBase uses a back-filling way to retrieve the data in the past. To avoid the measurement error caused by the fact that funding rounds information in the early years is not accurate enough, we decide not to use data on startups founded before 2001. On the other hand, many companies founded after 2011 are still operating now. Their long-term performances are yet to see and hard to predict, which is the reason we decide to exclude these startups from our test sample as well.

We report the regression results in Table 2.3. Startups with venture debt have a significantly lower probability of getting close, indicating their better long-term performance under the measure of closure, no matter whether the year fixed effect is considered or not. We also show results using IPO as a measure of success. Startups with venture debt also have a higher probability of going public, while the statistical power is limited. While going public seems to show the promise of young startups, recent literature documents that the number of public firms in the US has declined significantly recently, and one big reason some successful startups are shying away from IPOs is that public listings do not offer enough benefit to them Doidge et al. (2018). In our sample, we also see a vast number of startups that have been successfully operating for over a decade but never went public. Instead of not performing well enough to get into the public market, most of them do not seek going public as their ultimate goal and prefer to operate the business sustainably. Considering this fact, we construct a subsample of the firms excluding those with a low propensity to go public. We first predict each startup's intention of going public, then drop those with IPO probability less than the 10th percentile of the population. The prediction of the intention is based on their length of operation and the size of the company. This could be improved if a larger set of data on the properties of these companies is available. After excluding startups not willing to go public, the measure `ipo` is considered to be a better measure of success compared with not excluding them. The results of the same Probit regression on the subsample are reported in the last two columns of Table 2.3. Startups with venture debt have a significantly higher probability of going public than those without venture debt, which indicates the better long-term performance of firms that ever used venture debt.

The interesting results here are the ones using acquisition as dependent variables. Results indicate that startups with venture debt are significantly less likely to be acquired. Acquisitions have various purposes and are complicated in reality. Some promising startups may be purchased by some giant companies in the end, while others may feel not confident about their future and agree to sell the company at a low price. It will be more clear if we are able to distinguish between these two types of acquisition. We can use the premium of acquisition, defined as the ratio of the deal price over the book value of the company (Masulis and Nahata, 2011), as an indicator of whether an acquisition is a success for the startup or not. We then decompose the set of acquisitions into good and bad groups and use only the good acquisitions as a measure of success. This type of exercise is not doable due to the limit of our data and is left for future research.

Test 3: Conditional long-term performance of startups with venture debt

In this subsection, we test the conditional long-term performance of startups with venture debt, which is the third model prediction. Our model predicts that conditional on getting the next round of venture capital, the performance of startups with venture debt is worse compared with the world without venture debt. Our empirical strategy is similar to that in test 2. We use the Probit regression of different measures of long-term performance on the dummy variable indicating whether a startup ever used venture debt. The sample is also restricted to startups founded between 2001 and 2011 for the same reason stated in the last test. However, to test the *conditional* long-term performance, we only use startups getting enough next round funding (measured as total funding round greater than or equal to 4 in this case).

The regression equation and definitions of all variables are the same as in Test 2. The only difference is imposing the condition that startups in this subsample already get their next round of funding. For those startups with venture debt, we are able to track whether they get the next round of funding. However, we need to construct a comparable subgroup for those startups that have never borrowed venture debt. To deal with this problem, we construct the subsample by filtering the total number of funding rounds of startups and keep only those with the total number of funding rounds greater than or equal to 4. As shown in Table 2.4, conditionally, startups with venture debt have a significantly higher probability of closing, no matter whether we control for year-fixed effects or not, indicating they have worse long-term performance. As for going public, conditionally, startups with venture debt have a lower probability of going public. Similar to what we do in Test 2, we do the test of IPO on the subsample with a large enough predicted propensity to go public. The results of regressions run on the subsample are reported in the last two columns of Table 2.4. The results using IPO as the dependent variable verifies our prediction that startups with venture debt have worse long-term performance. When we use `acq` as the measure of success, the result is inconsistent with the others, which is not surprising since the acquisition indicator has the same problem as discussed in the previous test. A better method to solve this problem is in need to come up with a better way of measure of success.

Test 4: More asymmetric information reinforces the signaling effect

The last test provides empirical evidence for our fourth model prediction: the signaling effect of venture debt is stronger when the asymmetric information problem between the startups and investors is more severe. To test this, we construct measures of the extent of the signaling effect and measures of the asymmetric information problem severity as described in Section 2.3.

The regression equation we use for this test is:

$$\texttt{date\_diff} = \alpha_t + \beta_1 \cdot \texttt{vdebt} + \beta_2 \cdot \texttt{experienced} + \gamma \cdot (\texttt{vdebt} \times \texttt{experienced}) + \epsilon,$$

where

- α_t are the year fixed effects;
- `date_diff` is the length of the time interval between the next round of funding and this round of funding;
- `vdebt` indicates whether this is a venture-debt round; and
- `experienced` indicates whether the next round investor is an experienced investor, where an investor is defined as *experienced investor* if the total number of rounds they get involved is greater than the 90th percentile.

Table 2.5 reports the regression results. Consistent with test 1, the length of the time interval between the funding round and its next round is 100 days shorter when it is a venture-debt round. However, focusing on γ, the coefficient of the interaction term, we see if an experienced investor invests in the next round, the time shortened is significantly less, indicating the signaling effect of venture debt is a lot weaker among these experienced investors. Robustness checks in the next section use the 95th percentile and another variable—the total number of companies invested in—to define experienced investors. We show that our results are not sensitive to the threshold or the definition. All results provide strong empirical evidence of the fact that the signaling effect of venture debt tampers when the asymmetric information problem is more moderate.

2.5 Robustness testing

2.5.1 Effect of good venture capital investors

As we briefly mentioned in the introduction, when the venture debt issuers make decisions on whether to lend money to a startup, in addition to their own screening process, whether the startup is backed by an influential VC also matters. Influential VCs not only invest a considerable amount of funds to support the research and development of the startups they invested in but also provide them with extraordinary management and ensure they are in good shape when they grow. Venture debt lenders take this into consideration and prefer startups backed by good VCs, and following the well-known VCs when making investment decisions is a seemingly secure rule for them. Therefore, it is likely that whether a startup is backed by renowned VCs is highly correlated with whether a startup has venture debt, and we need to distinguish the effect of being backed by influential VCs and the signaling effect of venture debt. To distinguish these two effects, we do our first robustness check to test the effect of good VCs. The empirical regressions are similar to what we do in tests 2 and 3. We regress the measure of success on whether a startup is backed by good VCs. For all results reported in this robustness testing section, we omitted the regression with `acq` as the left-hand-side variable due to the unclearness of whether the acquisitions are successful or not in our data. The regression equations are:

$$\begin{aligned} \texttt{closed}' &= \alpha_t + \beta \cdot \texttt{have\_good\_vc} + \epsilon, \\ \texttt{ipo}' &= \alpha_t + \beta \cdot \texttt{have\_good\_vc} + \epsilon, \end{aligned}$$

where

- α_t are the year fixed effects;
- `have_good_vc` is an indicator equal to 1 if a startup is backed by at least one good venture capital investor, and 0 otherwise;
- `closed` is an indicator of whether the startup was closed;
- `ipo` is an indicator of whether the startup went public in the end; and
- $Y' = \Phi^{-1}(Y)$ for $Y \in \{\texttt{closed}, \texttt{ipo}\}$, where $\Phi(\cdot)$ is the cumulative distribution function of standard normal distribution.

We searched for top venture capital investors in the US and constructed the list referring to some ranking lists by some convincing and professional organizations. A detailed list of good venture capital investors is relegated to the appendix for the sake of conciseness. From results in Table 2.6, we observe that startups invested by good VCs have a lower probability of closing and a higher probability of going public than those not. This indicates that these good VCs significantly affect the long-term performance of the startups they invest in. Nevertheless, results on the conditional long-term performance in Table 2.7 indicate that even conditionally on getting enough funding rounds, startups backed by good VCs still have significantly better long-term performance. This helps us distinguish the effect of good VCs from the signaling effect of venture debt, where conditionally, the long-term performance of startups with venture debt is not as good as the world when there is no venture debt.

2.5.2 Industry

Startups in different industries may have significantly different growth models, and preferences for financing and operating. To check the robustness of our predictions, we want to see whether our signaling model predicts all different industries well. We check the robustness by doing similar regressions among various industries and to see whether the results depend on the industry or not. We do analyses on both unconditional and conditional long-term performance regressions for the technology industry, including all tech firms such as software, hardware, health care, etc. The results are reported in Table 2.8 and Table 2.9. Compared with previous results, we see most of the results are robust. Ventures with venture debt are more likely to be closed unconditionally. Also, conditionally, they have worse long-term performance, indicated by a higher probability of closing and a lower probability of going public and getting acquired.

2.5.3 Time Period

In order to avoid the trouble that lots of companies founded after 2011 are still operating, thus it's hard to tell their long-term performances yet, we choose to use startups founded between 2001 and 2011 to test our model predictions. Here, to test the robustness of the results, we try to use all startups founded from 2001 to 2016 to see whether the results are very sensitive to the time window we choose. The long-term performance of startups is reported in Table 2.10, and their conditional long term performance is reported in Table 2.11. Here, we can see that the results for the rate of close and going public do not change much, while the results of acquisition are kind of ambiguous. Similar to what we discussed before, this may be caused by the different nature of acquisitions. Being acquired is not a perfect indicator of success for ventures. From these results, we can see that the empirical predictions are robust no matter what specific time slot we use for the test.

Measures of experienced investors

To test the stability and robustness of our results on test 4, we use different measures of the severity of the asymmetric information problem and run the same set of regressions as in Section 2.4. We first use a different threshold, 95th percentile, when defining the experienced investors using the total number of investment rounds. Table 2.12 shows our results are robust to the threshold. Next, we define use another measure, the total number of companies a VC invests in, in the definition of experienced investors. Tables 2.13 and 2.14 report the results when the threshold is the 90th percentile an the 95th percentile, respectively. The results do not change much in scale and significance level.

2.6 Conclusion

In this paper, we study the supply side of venture debt, providing a resolution to the puzzle that there is growing venture debt with relatively low rates of return while bearing high risk. We build a model where startups use venture debt as a good signal for their financing, and the cost of due diligence for the VC is sufficiently high that venture capital investors prefer to utilize this signal instead of investigating by themselves. We test the four predictions of the model in our empirical study. First, startups with venture debt can get next-round funding faster than those without venture debt, as venture debt is a good signal. Second, in general, startups with venture debt tend to perform better in the long term. They have a lower probability of going out of business and a higher probability of going public. Third, conditional on the startups getting their next round of funding, those with venture debt have worse long-run performance compared with the world without venture debt. Finally, the signaling effect is stronger when venture capital investors suffer from more severe asymmetric information problems. We show strong empirical evidence which is in line with our predictions, with various robustness tests provided. This paper documents the signaling effect of venture debt from both theoretical and empirical perspectives.

Table 2.1: Summary statistics for funding rounds and startups

A. funding round level variables - early rounds

VARIABLE	N	mean	sd	max	min	p25	p50	p75
vdebt	45350	0.048	0.214	1	0	0	0	0
date_diff	45350	460.547	447.887	13283	0	185	362	593
date_diff2	22763	790.461	537.580	6797	0	427	686	1018

B. startup level variables - full sample

VARIABLE	N	mean	sd	max	min	p25	p50	p75
have_vdebt	21444	0.097	0.296	1	0	0	0	0
closed	21444	0.105	0.307	1	0	0	0	0
ipo	21444	0.024	0.153	1	0	0	0	0
acq	21444	0.190	0.393	1	0	0	0	0
# of funding rounds	21444	2.360	2.074	23	1	1	1	3

C. startup level variables - subsample conditional on # funding rounds $\geq$ 4

VARIABLE	N	mean	sd	max	min	p25	p50	p75
have_vdebt	4365	0.179	0.383	1	0	0	0	0
closed	4365	0.042	0.201	1	0	0	0	0
ipo	4365	0.063	0.242	1	0	0	0	0
acq	4365	0.263	0.441	1	0	0	0	1
# of funding rounds	4365	5.759	2.156	23	4	4	5	7

Note: This table summarizes the characteristics of the financing deals and startups in our data sample. Part A summarizes the three variables of interest at the funding round level. Part B and C summarize the five variables of interest at the startup level, where Part B describes the full sample, and Part C describes a subsample of startups whose total number of funding rounds is greater than or equal to 4.

Table 2.2: Test of signaling effect of venture debt

VARIABLES	(1) date_diff	(2) date_diff	(3) date_diff2	(4) date_diff2
vdebt	-121.9***	-93.05***	-141.7***	-118.6***
	(8.207)	(8.033)	(12.98)	(12.88)
Constant	466.4***	13,283	799.0***	607.0
	(2.169)		(3.699)	
year FE		✓		✓
Observations	45,350	45,350	22,763	22,763

Note: This table reports the effect of venture debt round on the length of time interval until the next round of funding. The treatment variable `vdebt` is an indicator of whether the funding is a venture debt round. The outcome variable of interest in the first two columns is `date_diff`, defined as the length of the time interval between the next round of funding and this round of funding. The outcome variable of interest in the last two columns is `date_diff2`, defined as the length of the time interval between the next round of funding and the previous round of funding. The regression models in Columns (1) and (3) are OLS regressions, while those in Columns (2) and (4) control for the year fixed effects. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.3: Effects of venture debt on long-term performance of startups

	full sample						subsample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	closed	closed	ipo	ipo	acq	acq	ipo	ipo
have_vdebt	-0.175***	-0.185***	0.0821	0.0898	-0.121***	-0.116***	0.108*	0.143**
	(0.0419)	(0.0420)	(0.0594)	(0.0611)	(0.0345)	(0.0355)	(0.0613)	(0.0642)
Constant	-1.238***	-1.413***	-1.988***	-1.615***	-0.866***	-0.410***	-1.947***	-1.505***
	(0.0120)	(0.0600)	(0.0197)	(0.0674)	(0.0103)	(0.0421)	(0.0203)	(0.0764)
year FE		✓		✓		✓		✓
Observations	21,444	21,444	21,444	21,444	21,444	21,444	18,786	18,786

Note: This table reports the effect of having venture debt on the long-term performance of startups. The treatment variable `have_vdebt` is an indicator of whether the startup ever used venture debt in the financing history. The outcome variables of interest stand for the exit status of startups, where `closed`, `ipo`, and `acq` are indicators of the startup going closed, public, and acquired, respectively. The first six columns report the regression results on the full sample. The last two columns are results of a subsample of likely-IPO startups that have a predicted propensity of going public larger than or equal to the 10th percentile. The odds number columns are Probit regressions with no fixed effects, while the even number columns control for the year fixed effects. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.4: Effects of venture debt on conditional long-term performance of startups

	full sample						subsample	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	closed	closed	ipo	ipo	acq	acq	ipo	ipo
have_vdebt	0.187**	0.230***	-0.176**	-0.142	-0.151***	-0.0872	-0.163*	-0.0938
	(0.0818)	(0.0832)	(0.0841)	(0.0876)	(0.0547)	(0.0565)	(0.0870)	(0.0926)
Constant	-1.762***	-1.586***	-1.508***	-1.273***	-0.607***	-0.151*	-1.466***	-1.231***
	(0.0383)	(0.131)	(0.0323)	(0.111)	(0.0224)	(0.0813)	(0.0334)	(0.126)
year FE		✓		✓		✓		✓
Observations	4,365	4,365	4,365	4,365	4,365	4,365	3,885	3,885

Note: This table reports the effect of having venture debt on the conditional long-term performance of startups, conditioning on the startups that have already received at least four rounds of funding. The treatment variable `have_vdebt` is an indicator of whether the startup ever used venture debt in the financing history. The outcome variables of interest stand for the exit status of startups, where `closed`, `ipo`, and `acq` are indicators of the startup going closed, public, and acquired, respectively. The first six columns report the regression results on the full sample. The last two columns are results of a subsample of likely-IPO startups that have a predicted propensity of going public larger than or equal to the 10th percentile. The odds number columns are Probit regressions with no fixed effects, while the even number columns control for the year fixed effects. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2.5: Intensity of signaling effects on severity of asymmetric information problem

	(1)	(2)	(3)	(4)
VARIABLES	date_diff	date_diff	date_diff2	date_diff2
vdebt $\times$ $1\{\#\text{rounds} \geq \text{p90}\}$	38.76**	3.475	92.23***	56.48*
	(19.31)	(18.95)	(29.68)	(29.03)
vdebt	-144.0***	-96.02***	-191.8***	-150.7***
	(14.75)	(14.58)	(20.52)	(20.46)
$1\{\#\text{rounds} \geq \text{p90}\}$	-3.315	7.204	-21.67**	-24.31***
	(5.328)	(5.213)	(9.006)	(8.760)
Constant	473.0***	13,283	814.6***	631.3***
	(3.531)		(5.781)	(8.760)
year FE		✓		✓
Observations	31,924	31,924	16,118	16,118

Note: This table summarizes the test results on whether more asymmetric information reinforces the signaling effect. Regression equations are following

$$\texttt{date\_diff} = \alpha_t + \gamma \cdot (\texttt{vdebt} \times \texttt{experienced}) + \beta_1 \cdot \texttt{vdebt} + \beta_2 \cdot \texttt{experienced} + \epsilon,$$

where `vdebt` is an indicator of venture debt round and `experienced` is an indicator of experienced investor, defined as whether the total number of investment rounds $\geq$ 90th percentile. The outcome variable of interest in the first two columns is `date_diff`, defined as the length of the time interval between the next round of funding and this round of funding. The outcome variable of interest in the last two columns is `date_diff2`, defined as the length of the time interval between the next round of funding and the previous round of funding. The main parameter of interest is γ, the coefficient of the interaction term in the second row. The regression models in Columns (1) and (3) are OLS regressions, while those in Columns (2) and (4) control for the year fixed effects. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.6: Effects of good venture capital investors on long-term performance

VARIABLES	(1) closed	(2) closed	(3) ipo	(4) ipo
have_good_vc	-0.198***	-0.192**	0.426***	0.462***
	(0.0751)	(0.0755)	(0.0659)	(0.0681)
Constant	-1.470***	-1.460***	-1.812***	-1.440***
	(0.0202)	(0.0905)	(0.0254)	(0.0875)
year FE		✓		✓
Observations	9,654	9,654	9,654	9,654

Note: This table reports the effect of having good venture capital investors on the long-term performance of startups. The treatment variable `have_good_vc` is an indicator of whether the startup is ever invested in by at least one good VC. The outcome variables of interest stand for the exit status of startups, where `closed` and `ipo` are indicators of the startup going closed and public, respectively. Columns (1) and (3) are Probit regressions with no fixed effects, while Columns (2) and (4) control for the year fixed effects. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.7: Effects of good venture capital investors on conditional long-term performance

VARIABLES	(1) closed	(2) closed	(3) ipo	(4) ipo
have_good_vc	-0.150	-0.133	0.338***	0.390***
	(0.107)	(0.108)	(0.0762)	(0.0795)
Constant	-1.710***	-1.579***	-1.586***	-1.317***
	(0.0364)	(0.134)	(0.0335)	(0.112)
year FE		✓		✓
Observations	4,293	4,293	4,293	4,293

Note: This table reports the effect of having good venture capital investors on the conditional long-term performance of startups, conditioning on the startups that have already received at least four rounds of funding. The treatment variable `have_good_vc` is an indicator of whether the startup is ever invested in by at least one good VC. The outcome variables of interest stand for the exit status of startups, where `closed` and `ipo` are indicators of the startup going closed and public, respectively. Columns (1) and (3) are Probit regressions with no fixed effects, while Columns (2) and (4) control for the year fixed effects. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.8: Effects of venture debt on long-term performance in the technology industry

VARIABLES	(1) closed	(2) closed	(3) ipo	(4) ipo
have_vdebt	-0.151***	-0.162***	0.0363	0.0463
	(0.0475)	(0.0475)	(0.0650)	(0.0665)
Constant	-1.285***	-1.414***	-1.921***	-1.619***
	(0.0142)	(0.0657)	(0.0215)	(0.0741)
year FE		✓		✓
Observations	16,182	16,182	16,182	16,182

Note: This table reports the effect of having venture debt on the long-term performance of startups in the technology industry. The treatment variable `have_vdebt` is an indicator of whether the startup ever used venture debt in the financing history. The outcome variables of interest stand for the exit status of startups, where `closed` and `ipo` are indicators of the startup going closed and public, respectively. Columns (1) and (3) are Probit regressions with no fixed effects, while Columns (2) and (4) control for the year fixed effects. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.9: Effects of venture debt on conditional long-term performance in the technology industry

VARIABLES	(1) closed	(2) closed	(3) ipo	(4) ipo
have_vdebt	0.179**	0.219**	-0.178**	-0.146
	(0.0885)	(0.0894)	(0.0887)	(0.0918)
Constant	-1.772***	-1.607***	-1.485***	-1.342***
	(0.0417)	(0.140)	(0.0345)	(0.121)
year FE		✓		✓
Observations	3,751	3,751	3,751	3,751

Note: This table reports the effect of having venture debt on the conditional long-term performance of startups in the technology industry, conditioning on the startups that have already received at least four rounds of funding. The treatment variable `have_vdebt` is an indicator of whether the startup ever used venture debt in the financing history. The outcome variables of interest stand for the exit status of startups, where `closed` and `ipo` are indicators of the startup going closed and public, respectively. Columns (1) and (3) are Probit regressions with no fixed effects, while Columns (2) and (4) control for the year fixed effects. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.10: Effects of venture debt on long-term performance of startups founded during 2001 - 2016

VARIABLES	(1) closed	(2) closed	(3) ipo	(4) ipo
have_vdebt	-0.0602*	-0.156***	0.148***	0.0748
	(0.0355)	(0.0365)	(0.0531)	(0.0559)
Constant	-1.457***	-1.199***	-2.193***	-1.723***
	(0.00936)	(0.0433)	(0.0163)	(0.0585)
year FE		✓		✓
Observations	43,526	43,526	43,526	43,526

Note: This table reports the effect of having venture debt on the long-term performance of startups founded during 2001 - 2016. The treatment variable `have_vdebt` is an indicator of whether the startup ever used venture debt in the financing history. The outcome variables of interest stand for the exit status of startups, where `closed` and `ipo` are indicators of the startup going closed and public, respectively. Columns (1) and (3) are Probit regressions with no fixed effects, while Columns (2) and (4) control for the year fixed effects. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.11: Effects of venture debt on conditional long-term performance of startups founded during 2001 - 2016

VARIABLES	(1) closed	(2) closed	(3) ipo	(4) ipo
have_vdebt	0.241***	0.284***	-0.153**	-0.139*
	(0.0701)	(0.0726)	(0.0770)	(0.0827)
Constant	-1.837***	-1.451***	-1.647***	-1.279***
	(0.0326)	(0.105)	(0.0285)	(0.0970)
year FE		✓		✓
Observations	6,608	6,608	6,608	6,608

Note: This table reports the effect of having venture debt on the conditional long-term performance of startups founded during 2001 - 2016, conditioning on the startups that have already received at least four rounds of funding. The treatment variable `have_vdebt` is an indicator of whether the startup ever used venture debt in the financing history. The outcome variables of interest stand for the exit status of startups, where `closed` and `ipo` are indicators of the startup going closed and public, respectively. Columns (1) and (3) are Probit regressions with no fixed effects, while Columns (2) and (4) control for the year fixed effects. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2.12: Intensity of signaling effects on severity of asymmetric information problem (experienced investors defined as total # of investment rounds ≥ p95)

VARIABLES	(1) date_diff	(2) date_diff	(3) date_diff2	(4) date_diff2
vdebt × 1{#rounds ≥ p95}	40.48**	9.348	94.83***	62.05**
	(19.18)	(18.81)	(29.74)	(29.07)
vdebt	-145.4***	-100.1***	-191.8***	-152.6***
	(14.27)	(14.18)	(20.08)	(20.07)
1{#rounds ≥ p95}	6.532	16.29***	-16.63*	-18.63**
	(5.405)	(5.289)	(9.160)	(8.909)
Constant	468.7***	13,283***	811.8***	625.6***
	(3.386)	(0.000734)	(5.586)	(8.909)
year FE		✓		✓
Observations	31,924	31,924	16,118	16,118

Note: This table summarizes the test results on whether more asymmetric information reinforces the signaling effect. Regression equations are following

$$\texttt{date\_diff} = \alpha_t + \gamma \cdot (\texttt{vdebt} \times \texttt{experienced}) + \beta_1 \cdot \texttt{vdebt} + \beta_2 \cdot \texttt{experienced} + \epsilon,$$

where `vdebt` is an indicator of venture debt round and `experienced` is an indicator of experienced investor, defined as whether the total number of investment rounds ≥ 95th percentile. The outcome variable of interest in the first two columns is `date_diff`, defined as the length of the time interval between the next round of funding and this round of funding. The outcome variable of interest in the last two columns is `date_diff2`, defined as the length of the time interval between the next round of funding and the previous round of funding. The main parameter of interest is γ, the coefficient of the interaction term in the second row. The regression models in Columns (1) and (3) are OLS regressions, while those in Columns (2) and (4) control for the year fixed effects. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.13: Intensity of signaling effects on severity of asymmetric information problem (experienced investors defined as total # of companies invested in $\geq$ p90)

VARIABLES	(1) date_diff	(2) date_diff	(3) date_diff2	(4) date_diff2
vdebt $\times$ 1{#companies $\geq$ p90}	41.34**	6.515	93.78***	58.83**
	(19.33)	(18.98)	(29.67)	(29.03)
vdebt	-145.4***	-97.74***	-192.7***	-152.0***
	(14.80)	(14.68)	(20.57)	(20.54)
1{#companies $\geq$ p90}	-4.620	7.056	-23.50***	-24.71***
	(5.328)	(5.215)	(8.997)	(8.753)
Constant	473.6***	13,283	815.4***	631.7***
	(3.531)		(5.799)	(8.753)
year FE		✓		✓
Observations	31,924	31,924	16,118	16,118

Note: This table summarizes the test results on whether more asymmetric information reinforces the signaling effect. Regression equations are following

$$\texttt{date\_diff} = \alpha_t + \gamma \cdot (\texttt{vdebt} \times \texttt{experienced}) + \beta_1 \cdot \texttt{vdebt} + \beta_2 \cdot \texttt{experienced} + \epsilon,$$

where `vdebt` is an indicator of venture debt round and `experienced` is an indicator of experienced investor, defined as whether the total number of companies invested in $\geq$ 90th percentile. The outcome variable of interest in the first two columns is `date_diff`, defined as the length of the time interval between the next round of funding and this round of funding. The outcome variable of interest in the last two columns is `date_diff2`, defined as the length of the time interval between the next round of funding and the previous round of funding. The main parameter of interest is γ, the coefficient of the interaction term in the second row. The regression models in Columns (1) and (3) are OLS regressions, while those in Columns (2) and (4) control for the year fixed effects. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.14: Intensity of signaling effects on severity of asymmetric information problem (experienced investors defined as total # of companies invested in $\geq$ p95)

VARIABLES	(1) date_diff	(2) date_diff	(3) date_diff2	(4) date_diff2
vdebt $\times$ 1{#companies $\geq$ p95}	39.44**	6.049	95.01***	59.81**
	(19.22)	(18.86)	(29.75)	(29.07)
vdebt	-144.6***	-98.16***	-191.6***	-151.4***
	(14.39)	(14.31)	(20.11)	(20.06)
1{#companies $\geq$ p95}	2.918	14.38***	-21.09**	-22.01**
	(5.402)	(5.288)	(9.164)	(8.905)
Constant	470.2***	13,283	813.6***	629.0***
	(3.390)		(5.580)	(8.905)
year FE		✓		✓
Observations	31,924	31,924	16,118	16,118

Note: This table summarizes the test results on whether more asymmetric information reinforces the signaling effect. Regression equations are following

$$\texttt{date\_diff} = \alpha_t + \gamma \cdot (\texttt{vdebt} \times \texttt{experienced}) + \beta_1 \cdot \texttt{vdebt} + \beta_2 \cdot \texttt{experienced} + \epsilon,$$

where `vdebt` is an indicator of venture debt round and `experienced` is an indicator of experienced investor, defined as whether the total number of companies invested in $\geq$ 95th percentile. The outcome variable of interest in the first two columns is `date_diff`, defined as the length of the time interval between the next round of funding and this round of funding. The outcome variable of interest in the last two columns is `date_diff2`, defined as the length of the time interval between the next round of funding and the previous round of funding. The main parameter of interest is γ, the coefficient of the interaction term in the second row. The regression models in Columns (1) and (3) are OLS regressions, while those in Columns (2) and (4) control for the year fixed effects. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Chapter 3

Bank Size and Deposit Market Competition

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3.1 Introduction

Deposit franchise, a critical and unique function of banks, plays a key role in understanding the competition in deposit markets and determining deposit rates. It is essential for grasping the competitive dynamics within the banking industry, as well as evaluating the impact of monetary policy transmission and the broader economic implications. This paper offers an in-depth analysis of how banks compete and set deposit rates.

The literature on competition in deposit markets is extensive and diverse. In the early 1960s, retail banking markets were commonly seen as local. Studies revealed that deposit interest rates correlated with local levels of bank competition[1], leading antitrust regulators to focus on local competition levels. However, research in the 1980s and 1990s began to question these conclusions, especially in light of banking deregulation, which permitted banks to have multiple branches[2]. Using 1996-97 deposit and loan data from the Bank Rate Monitor, Inc., Radecki (1998) discovered that many major banks set constant rates across large regions, and the local-level correlations previously observed had vanished. Later studies confirmed these findings using more recent data, demonstrating that while large banks tend to set uniform rates across extensive regions, smaller banks base their rates on local competitive conditions (see, for example, Radecki, 2000; Biehl, 2002; Heitfield, 1999; Heitfield and Prager, 2004; Park and Pennacchi, 2009). Strangely, these results on uniform pricing appear to have been overlooked in recent literature, which has refocused on the relationship between

[1] See, for example, Berger and Hannan (1989); Hannan (1991, 1997); Hannan and Berger (1991); Neumark and Sharpe (1992); Rhoades (1992); Sharpe (1997).

[2] For a description of banking deregulation in the U.S., see Berger et al. (1995).

cross-sectional variations in local bank competition and monetary policy (see, for example, Drechsler et al., 2017; Wang et al., 2022; Wang, 2022), as well as other topics of interest. In a recent working paper, Begenau and Stafford (2022) criticized this literature for not considering the uniform rate-setting policies of large banks.

Following recent discussions, this paper also identifies uniform rate policies, particularly among large banks. Our analysis uses weekly deposit rates at the branch level from RateWatch, revealing minimal rate variation within banks. Additionally, we examine factors contributing to rate variation, discovering that local market conditions, such as HHI and demographics, have little impact on deposit rate setting, which supports uniform rate policies. Bank size is the primary contributor to rate variation, emphasizing differences between large and small banks.

A notable distinction between large and small banks is the rate gap. Large banks set significantly lower deposit rates for all deposit products. Additionally, rate disparities exist among small banks influenced by the presence of large banks. Small banks in areas with a higher market share of large banks set relatively lower rates than those in regions with a smaller share of large banks.

How do large banks generate profit with low deposit rates and uniform rate restrictions? We contend that customer segmentation is the answer. Firstly, large banks typically operate in markets with similar characteristics, primarily in densely populated urban areas with higher household income, housing prices, and fewer elderly individuals. Secondly, large banks cater to customers who value complex financial services and are less concerned about low deposit rates, while small banks target customers who are more sensitive to deposit rates. This segmentation is evident in their asset and liability structures. Large banks hold more complex financial assets, including real estate loans, commercial loans, and mortgage-backed securities (MBS), while small banks possess more agriculture loans, catering to farmers and rural customers, as well as liquidity assets, in preparation for potential deposit withdrawals. Large banks also maintain a larger savings deposit base, whereas small banks hold more transaction deposits. These asset and liability structures suggest that large bank customers tend to have more assets and appreciate complex financial services.

To further support the notion that large bank customers exhibit lower deposit demand elasticity, we conduct a structural estimation of banks' demand elasticity following Wang (2022) and Xiao (2020). Banks are differentiated by offered deposit rates, convenience value, and product specialization. Large banks are characterized by high convenience value, while small banks provide higher deposit rates and more agricultural loans. Assuming homogeneous households choosing from available local market banks, we group nearby counties with small populations into county clusters, defining each cluster as one market. We estimate the deposit demand system on a cluster-by-cluster basis. After determining demand parameters, we calculate each bank's demand elasticity in each local market, finding that large banks experience significantly lower demand elasticity and are more likely to be located in markets with less elastic customers. This estimation reinforces the customer segmentation between large and small banks.

Our deposit rate setting framework has insightful implications for recent bank failures and discussions about bank interest risks. Previous literature finds that deposit franchises help hedge interest rate risk due to deposit spreads. However, as we observe different rate setting behaviors between small and large banks, small banks are more vulnerable in a tightening environment, as their customers are more sensitive to deposit rate changes, and they need to set high rates to retain deposits. Consequently, their deposit franchises have weaker hedging power. Furthermore, small bank customers are more susceptible to economic downturns, tending to withdraw deposits during quantitative tightening. Such withdrawals weaken the hedging power of deposit franchises even further.

The remainder of this paper is organized as follows: Section 3.2 details the data. Section 3.3 provides comprehensive tests on banks' deposit rate setting behavior, and Section 3.4 discusses the implications. Section 3.5 concludes.

3.2 Data

Our analysis relies on two major datasets for deposit rates. First, we investigate branch-level deposit rates using RateWatch Data. Held by S&P Global, RateWatch offers a comprehensive deposit and loan rate database covering nearly 100,000 institutions from 2001 to 2019. The deposit rate dataset collects branch-level advertised deposit rates for various products, such as CDs, savings accounts, and money market accounts, updated weekly. It is important to note that RateWatch manages their datasets by creating rate-setting networks, designating "rate setters" as parent branches and "followers" as child branches with identical deposit rates. However, "rate setters" do not necessarily have local officers setting rates in these branches and passing them on to follower branches. Instead, rate setters are partially arbitrarily selected from a pool of branches sharing the same rates, with head offices and branches in major cities being more likely to be chosen. RateWatch creates rate setter flags primarily for data storage purposes, so using only rate-setting branches for analyses involving branch-level information can be problematic. We utilize RateWatch data to examine rate variations within banks.

Once we establish that banks implement uniform rates, we shift our primary focus to bank-level deposit rates from the Consolidated Report of Condition and Income, known as Bank Call Reports. We calculate these rates by dividing deposit interest expenses by deposit balance. We also use Call Reports to obtain other bank-level characteristics. To further our analysis, we supplement Call Report data with the FDIC's Summary of Deposits, which reports branch-level total deposit balances. This additional data source allows us to calculate local market shares for demand elasticity analysis. To explore the demographics of customers and their potential impact on deposit rates, we rely on Infogroup data, which provides residential information on demographics, household wealth, and income from 2006 to 2019.

3.3 Deposit Rate Setting Behavior

3.3.1 Uniform Rates

Table 3.1 investigates deposit rate variations within banks, focusing on weekly deposit rates at the branch level from RateWatch between 2001 and 2019. It examines how various fixed effects contribute to deposit rate variations. Columns 1 and 2 concentrate on $10,000 12-month CD rates, with the R-square indicating that 87.8% of rate variation can be explained by time fixed effects. This suggests similar rate setting across branches and banks. Meanwhile, 98.8% of variance can be accounted for by bank-time fixed effects, signifying minimal rate variation within banks. The remaining columns examine $25,000 money market deposit rates and rates for savings accounts with balances below $2,500. These two deposit products exhibit more rate variation across branches and banks, with only around 60% of variations explained by time fixed effects. However, bank-time fixed effects still account for most of the rate variations, at 95%. Table 3.1 indicates that banks tend to set uniform rates across branches, with the majority of deposit rate variations arising across banks rather than within them.

Various reasons explain why large banks would implement uniform rates. First, a lack of local experts and high costs make it difficult for banks to analyze local markets and set deposit rates at the branch level. Second, setting different rates exposes banks to potential disputes of regional price discrimination. Uniform rate setting has crucial implications for bank deposit competition. Large banks operating in multiple regions and setting uniform rates face limitations when responding to changes and competition in local markets, instead determining rates based on national market conditions. Conversely, small and local banks can set rates locally, offering greater flexibility. Large banks leverage their extensive networks, operational efficiencies, and economies of scale to compete nationally, while small banks rely on local knowledge, personalized services, and community ties to compete within their specific regions. This results in a disparity in rate-setting behavior and business models between large and small banks.

To further support the view of a uniform rate-setting policy, Table 3.2 tests the contribution of local market characteristics to rate variations after removing time variation, implementing a two-step analysis. We first regress branch-level deposit rates on time fixed effects to extract the time effects, and then regress the residuals on the fixed effects of interest in the second step to evaluate their explanatory power for the remaining variations. As a baseline, we test bank-time fixed effects in the second step, finding that 90% of the remaining rate variation can be accounted for by bank-time in all three products. However, time-varying local HHI and local population have little explanatory power for rate variance, with only 2% for CD and savings rates, and less than 1% for money market account rates. In contrast, bank size has a relatively stronger explanatory power for rate variation. We denote the 19 Dodd-Frank banks as large banks and find that large-time fixed effects explain 21.5% of the remaining variance of CD rates, 10.7% of money market rates, and 15.4% of savings rates, which is over 10 times the impact from local characteristics. These results support the argument that local market conditions have minimal impact on deposit rate setting, particularly for large banks, and bank size plays a crucial role in understanding how banks set deposit rates.

3.3.2 Rate-Setting Gap Between Large and Small Banks

One salient difference between large and small banks is the rate gap. Since banks set uniform rates, we focus on the bank-level deposit rates from Bank Call Reports, calculated by dividing interest expense on deposit products by their deposit balance. Figures 3.1 plot the median deposit rates of the 19 Dodd-Frank large banks and other banks. Both small and large banks adjust deposit rates in tandem with the Federal funds rates, setting rates well below benchmark rates. Figure 3.1a displays the deposit rates on total deposits, revealing that small banks set significantly lower deposit rates than large banks. The gap widens when rates drop and narrows during the zero-rate period after 2009. Since banks set different rates on various deposit products, the rate gap may result from different product compositions between large and small banks. To account for this possibility, other figures plot the deposit rates on time deposits, savings deposits, and transaction deposits, demonstrating that small banks also set lower rates by product types. While time deposits have a narrower rate gap and align closely with the fed funds rates, small banks still set relatively lower rates. Savings deposit rates exhibit similar trends to total deposits, and transaction deposits display the most pronounced rate pattern differences between large and small banks.

Figures 3.2 present deposit rates from RateWatch, showing patterns similar to Call Reports. Small banks persistently set higher rates in money market accounts over 25k, 12-month CD of 10k, and saving transaction account of 2.5k.

To quantify the rate difference, we examine Table 3.3, which evaluates the rate gap through regressions based on RateWatch data. Branch-level deposit rates are collapsed into bank-level rates by taking the average rates weighted by branch deposit balance. Odd-numbered columns present regressions of deposit rates on 3-month LIBOR rates and a dummy variable indicating if the bank is among the 19 largest Dodd-Frank banks. Even-numbered columns display regressions of rates on the large bank indicator with time fixed effects.

Columns 1 and 2 show that large banks set 12-month CD rates 0.54% lower than small banks and 0.50% lower after controlling for fixed effects. The remaining columns implement the same tests, revealing that large banks set rates 0.25% lower for Money Market accounts of $25,000 and 0.31% lower for saving accounts below $2,500. It is important to note that saving accounts below $2,500 are very similar to checking accounts, except for limitations on the number of withdrawal times. As a result, the average rates are lower than those for MM25K accounts. However, the rate gap is even more significant for Saving2.5K accounts, suggesting that large banks are less competitive in the Saving2.5K product, which is more likely held by low-income groups, and are relatively more competitive in MM25K. Overall, large banks offer lower rates across all three products.

Interestingly, rate disparities also exist among small banks influenced by the presence of large banks. Small banks located in areas where large banks have a higher market share set relatively lower rates than small banks in areas with a smaller share of large banks. Figures 3.3 illustrate this fact using deposit rates of small banks from RateWatch, indicating that the deposit rates of all products have a negative relationship with the deposit share of large banks in the areas where the small banks operate. This pattern suggests that small banks do not necessarily experience increased competition in areas with large banks, nor do they need to set higher rates to compete effectively. In fact, as large banks establish uniform rates nationally, small banks encounter reduced competition, allowing them to set lower rates to attract deposits.

Although large banks set less competitive deposit rates, they account for the majority of the deposit share in the US. Figures 3.4 show that the total share of deposits owned by the 19 large banks grew steadily, exceeding 50% of total deposits in the US, with growth slowing down after 2009. Large banks hold relatively larger shares in savings deposits and transaction deposits compared to time deposits.

The traditional view that banks compete in local markets and set rates according to local competition has limitations in reconciling the above stylized facts. In the following sections, we propose a more comprehensive understanding of banks' rate-setting behavior with empirical evidence.

3.3.3 Market Selections of Large and Small Banks

Owing to the uniform rate constraints, large banks generally operate in similar markets. They are primarily found in densely populated urban areas, benefiting from the heightened demand for financial services and potential economies of scale. In contrast, rural areas are frequently occupied by small banks, which utilize local knowledge and community connections to address the region's specific needs.

Figure 3.5 demonstrates the geographical distribution of branches belonging to large and small banks. Counties are colored according to the proportion of branches held by non-top 19 banks in 2019. Evidently, large banks hold more shares in coastal and major cities, whereas the Midwest and Central South-regions with more rural and less populated areas-have a higher share of branches owned by small banks. Figures 3.6 further link location choices with geographical demographics, displaying the share of branches at the Zipcode-level on the y-axis and the variable of interest at the Zipcode-level on the x-axis with a band of one standard deviations above and below the mean, controlling for year fixed effects. The figures indicate that small banks hold a higher market share in areas characterized by lower population density, lower household income, lower housing prices, and a higher proportion of individuals over 65 years of age. Customers in these areas typically possess smaller overall wealth, and as a result, deposits may constitute a larger share of their household assets. Consequently, these customers are more sensitive to deposit rates. To cater to the needs of such customers and stay competitive in these markets, small banks need to offer higher deposit rates, ensuring they remain an attractive option for customers who place a greater emphasis on the returns they receive on their deposits.

These graphs suggest customer segmentation between large and small banks. Large banks target populated areas with higher-income populations, who are more likely to value complex financial services beyond deposits and be less sensitive to the low deposit rates offered by large banks. Small banks find it more challenging to compete with large banks in urban areas, thus holding more shares in rural and less-populated regions, where customers prioritize deposits and are more sensitive to deposit rates. Therefore, small banks must offer higher deposit rates to attract customers in these areas. This segmentation can persist due to the uniform rate-setting policy. If large banks were to expand into rural areas dominated by small banks, they would set the same low deposit rates as in other areas. Since customers in these markets are sensitive to deposit rates, large banks would struggle to compete with small banks offering better rates. Alternatively, large banks could raise rates to compete, but they would lose profits in urban areas as customers there are inelastic to deposit rates. Consequently, neither approach would be profitable for banks expanding into rural areas. Similarly, high deposit rates are not as competitive as better financial services for small banks attempting to compete in urban areas, which is where large banks excel.

The geographic distribution of large and small banks, along with the rate gaps between them, results in observable geographic deposit rate gaps. Figures 3.7 display the average deposit rates weighted by branches' deposit shares by county using RateWatch data from 2019. These figures can be compared to Figure 3.5, depicting the geographic distribution of small banks, indicating that areas with a higher share of small banks exhibit higher average deposit rates for CD, Saving, and Money Market Accounts. Consequently, rural and less-populated area populations significantly benefit from higher deposit rates, while urban populations are compensated with financial services provided by large banks. However, this also implies that low-income populations in urban areas are worse off due to the disparity, as they may prefer higher deposit rates over services but can only access low deposit rates set by market conditions.

The segmentation of customers is not only evident in the geographical distribution of large and small banks, but it also manifests in their asset structures. The distinct customer bases served by large and small banks lead to variations in their balance sheets, reflecting the different financial products and services they offer to cater to the specific needs and preferences of their respective clients.

Figures 3.8a and 3.8b display the asset and liability structures of banks with asset sizes in the lowest decile and those in the highest decile, highlighting significant differences in their compositions. Large banks tend to hold more real estate loans, accounting for about 50% of their total assets in recent years. This suggests that large banks focus more on mortgages, serving clients with real estate assets and more complex financial service needs beyond deposits. In contrast, small banks allocate 20% more of their assets to liquidity assets, such as cash, treasuries, government bonds, and Federal funds repos, and 10% more to agricultural loans. This implies that they support more farmers and rural populations, whose customers may be more sensitive to deposit rates and have more volatile deposit balances, requiring small banks to maintain higher liquidity levels to accommodate potential withdrawals.

Figure 3.8b illustrates the differences in liability structures between large and small banks. While deposits constitute the majority of liabilities for both types of banks, their deposit product compositions vary significantly. Large banks feature a growing share of savings deposits, which include money market accounts, reaching around 50% in recent years, compared to just 21% in small banks. Small banks, on the other hand, hold relatively more time deposits, which offer the highest deposit rates, and substantially more transaction deposits, such as checking accounts. These differences suggest that small banks serve a customer base with smaller deposit balances who are less sophisticated in their choice of deposit products. These customers may be more reliant on deposit services and more sensitive to deposit rate changes. Another notable difference is that large banks have more diverse funding sources beyond deposits. In most years, large banks borrow more from Federal funds repos than small banks, making them less dependent on deposit funding.

In summary, the asset and liability structures of small and large banks reflect a segmentation in their customer bases, which could contribute to the differences in their deposit rate-setting behavior.

3.3.4 Demand Elasticity

An essential premise embedded in the earlier analysis is that customers of large banks exhibit lower deposit demand elasticity. In this section, we present empirical evidence to reinforce this argument by utilizing methods from industrial organization literature following Wang et al. 2022 and Xiao 2018.

Model setup. In each market t, each customer i is endowed with one dollar, and can make a discrete choice to allocate this dollar to cash (denoted by $j = 0$), bonds (denoted by $j = J + 1$ and set as outside goods), or deposit in one of the banks (denoted by $j = 1, ..., J$) that are available in the market, based on product characteristics $X_{j,t}$ and product price $p_{j,t}$. We define the price for holding cash equals Federal Funds Rates, and the price for depositing in bank j is the deposit spread $r_t - d_{j,t}$, namely the difference between Federal Funds Rates and deposit rates. The customer choose the product that maximize her indirect utility:

$$U_{i,j,t} = \alpha p_{j,t} + \beta X_{j,t} + \xi_{j,t} + \epsilon_{i,j,t} \tag{3.3.1}$$

Where $\xi_{j,t} = \xi_j + \xi_t + \Delta\xi_{j,t}$ consists of bank fixed effects ξ_j, market fixed effects ξ_t, and unobserved product characteristics $\Delta\xi_{j,t}$, and $\epsilon_{i,j,t}$ is a mean-zero stochastic term capturing customer-product specific shocks, which follows the Type I extreme-value distribution with $F(x) = e^{-e^{-x}}$. Then, the market share of product j can be represented as

$$\begin{aligned} s_{j,t}(X_{\cdot t}, p_{\cdot t}; \alpha, \beta) &= \int \mathbb{1}_{i,j} dF(\epsilon) \\ &= \frac{\exp(\alpha p_{j,t} + \beta X_{j,t} + \xi_{j,t})}{1 + \sum_{k=0}^{J} \exp(\alpha p_{k,t} + \beta X_{k,t} + \xi_{k,t})} \end{aligned} \tag{3.3.2}$$

Note that we assume homogeneous price sensitivity in this case, thus the estimation of parameters $\theta = (\alpha, \beta)$ can be transmitted into plain logit regressions. The bank characteristics X_j include the logarithm of the number of branches the bank owns, the logarithm of the number of employees per branch, a dummy variable specifying if the bank's asset size is within the top 1%, and the share of agricultural loans in total assets, serving as a proxy for services targeting farmers or individuals in rural areas.

Identification. A shared difficulty in demand estimation is the endogenous determination of the price, which implies that $\Delta\xi_{j,t}$ is not independent from $p_{j,t}$, causing biased estimation if market shares are directly regressed on prices. To tackle this issue, we follow Wang et al. 2022 and employ supply shocks $Z_{j,t}$ as instrumental variables, including the ratio of staff salaries to the total assets in the prior year and the ratio of non-interest expenses on fixed assets to total assets in the previous year. The fundamental assumption is that customers are unlikely to be aware of these cost alterations and thus less prone to modify their demand in response, while banks might adjust prices due to shifts in marginal costs. As a result, the moment condition is

$$E[Z_{j,t}\Delta\xi_{j,t}(\theta)] = 0 \tag{3.3.3}$$

and we estimate θ utilizing linear IV GMM. Upon estimating θ, we determine the demand elasticity for each bank using the following equation:

$$\eta_{j,t} \equiv \frac{\%\Delta s_{j,t}}{\%\Delta p_{j,t}} = \frac{\partial s_{j,t}}{\partial p_{j,t}} \cdot \frac{p_{j,t}}{s_{j,t}} = -\alpha p_{j,t}\,(1 - s_{j,t}) \tag{3.3.4}$$

where $s_{j,t}$ is the fitted market share of bank j in market t.

Define market. Customers typically choose banks based on their local availability and accessibility. For instance, households in San Francisco are likely to opt for banks with branches in San Francisco, while they generally would not consider banks operating exclusively in New York. As a result, we define markets based on counties to reflect these local preferences. However, in counties with small areas and low populations, the consumer base is small, causing banks to draw customers from neighboring counties to compensate for limited local demand. Therefore, to create comparable markets, it is reasonable to combine these neighboring counties and treat them as a single market.

To form county clusters, considering the high skewness of population distribution, counties with populations below the 80% percentile are intended to group with neighboring counties, taking into consideration their geographical proximity and similarity in population size. This process continues until the total population of the formed cluster surpasses the 80% percentile threshold. To efficiently construct these clusters, we employ the Breadth First Search algorithm, which systematically searches through the county network to identify suitable groupings. Ultimately, 3075 counties are organized into 1366 clusters, and we separately estimate the demand system for each county cluster.

Estimation Results. We utilize data from Call Reports spanning 2001 to 2019, and assume that total household wealth is composed of cash, investments in treasury securities, money market funds, and deposits, which are gathered from Federal Reserve Economic Data (FRED). Given that FRED only provides this data at the national level, we assume that non-deposit wealth at the county level is proportional to county income obtained from the Bureau of Economic Analysis. Using the estimation method described earlier, we estimate the demand system for each cluster. Some clusters display unstable estimation due to their small sample size and high variable volatility. To mitigate the influence of these outliers, we trim the estimation of α at the 10% level. Table 3.4 presents the average of the point estimation, with t statistics in brackets. The average price sensitivity stands at -0.44, suggesting that when the deposit spread decreases by 1%, with other variables held constant, bank market shares rise by 0.44% on average. Market shares also increase when banks are larger, possess more branches, have more employees per branch, and offer more agricultural loans.

Based on the estimations, we compute the demand elasticities and compare them according to bank size. Table 3.5 panel A displays the summary of demand elasticities. The average elasticity is -0.372, indicating that when the deposit spread decreases relatively by 1%, the deposit quantity on average rises by 0.372%. Small banks have higher average demand elasticity, with a deposit increase of 0.412% following a 1% relative drop in deposit spread, while large banks only increase by 0.259%. The median elasticity of small banks is approximately four times that of large banks, suggesting that small bank customers are more sensitive to deposit rate changes. Note that a considerable share of elasticity estimations are positive, which is counterintuitive. These are primarily driven by insignificant estimations of α, implying that the elasticity is not significantly different from zero. To exclude the impact of insignificant estimations, we retain only those with t statistics of α greater than 1.5 and present the results in Table 3.5 panel B. The majority of the elasticities remain negative, and, likewise, small banks experience significantly higher demand elasticity by 0.239.

Figure 3.9 illustrates the distribution of deposit elasticity for large and small banks. The majority of large banks are centered around zero, which implies that their customer groups exhibit inelastic behavior, meaning they are less sensitive to changes in deposit rates. In contrast, the distribution of small banks leans more towards the left, indicating higher elasticity among their customers. This implies that customers of small banks are more sensitive to deposit rate changes and may switch banks based on the competitiveness of these rates. Small banks, therefore, might rely on offering attractive deposit rates to maintain and expand their customer base. Furthermore, Figure 3.10 demonstrates the geographic distribution of deposit elasticity by depicting the relationship between cluster average elasticity and the market share of large banks within each cluster. A clear correlation emerges, showing that in areas with a higher concentration of large banks, demand tends to be more inelastic.

In summary, the tests on demand elasticity underscore the different strategies adopted by large and small banks to cater to distinct customer segments. Large banks target customers who are less sensitive to deposit rates, likely offering additional services or benefits to maintain their base. Conversely, small banks focus on depositors more responsive to deposit rate changes, often providing competitive rates to differentiate themselves in the market.

3.4 Implications

The framework of bank deposit rate setting that we have established provides crucial insights into the phenomena of recent bank failures and the ongoing debates surrounding bank interest rate risks. Quantitative tightening, characterized by high interest rates, results in the contraction of bank assets. Existing literature argues that deposit franchises can serve as a hedge against this interest rate risk, primarily due to deposit spreads. In essence, when banks set low and sticky deposit rates, a high-rate environment allows them to enjoy higher deposit spreads. This, in turn, increases the value of the deposit franchise and serves as a buffer against the interest rate risk of assets.

However, our research findings indicate that small and large banks exhibit divergent rate-setting behaviors, rendering small banks more susceptible to risks in a tightening environment. Firstly, as customers of small banks display greater sensitivity to changes in deposit rates, these banks are compelled to set higher deposit rates in order to retain their depositor base. Given that small banks adopt higher rates and are more sensitive to interest rate shifts, they derive fewer benefits from deposit spreads. Consequently, their deposit franchises possess diminished hedging capabilities.

Secondly, the clientele of small banks predominantly consists of low-income households, a demographic that is particularly vulnerable during economic downturns and more likely to withdraw deposits during quantitative tightening. This withdrawal behavior further undermines the hedging power of deposit franchises. In an attempt to counteract the dual pressures of withdrawals and the need to maintain elevated deposit rates, small banks may be forced to liquidate their liquidity assets, such as treasuries, in order to satisfy capital requirements. This course of action can lead to the realization of capital losses.

Therefore, small banks encounter difficulties in utilizing their deposit franchises as a means of hedging interest rate risk, potentially resulting in bank failures, as exemplified by the experience of Silicon Valley Bank. This underscores the importance of understanding the distinct rate-setting behaviors and their implications for financial stability in the banking sector.

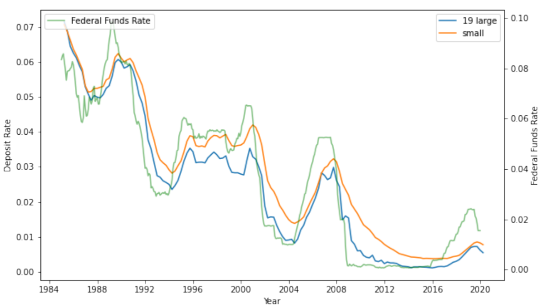
3.5 Conclusion

A comprehensive understanding of how banks set deposit rates is essential for researchers and policymakers. This paper conducts an extensive analysis of bank deposit rate-setting behavior and its implications on the competitive landscape of the banking industry. We find that banks tend to set uniform deposit rates across branches, with most rate variations occurring between banks rather than within them. The uniform rate policy causes large and small banks to compete at different market levels, with large banks setting rates based on national competition while small banks reacting to local markets. We also observe that local market conditions have minimal impact on deposit rates across banks, while bank size plays a crucial role in understanding how banks set deposit rates.

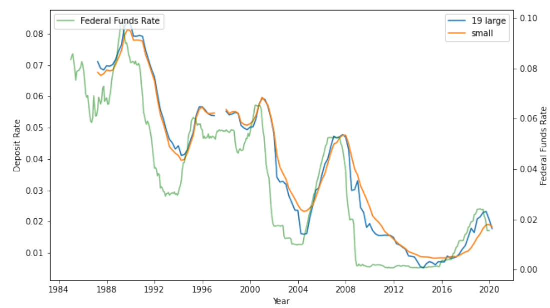
To accommodate the uniform rate-setting policy, large and small banks engage in customer segmentation in order to generate profits. Large banks target more populated areas with higher-income populations who value complex financial services beyond deposits and are less sensitive to the low deposit rates offered by large banks. Small banks, on the other hand, serve rural and less-populated regions where customers prioritize deposits and are more sensitive to deposit rates, necessitating higher rates to attract customers in these areas.

Moreover, we provide empirical evidence supporting the notion that customers of large banks exhibit lower deposit demand elasticity using structural estimation. The areas where most large banks operate are also populated with more customers with low demand elasticity.

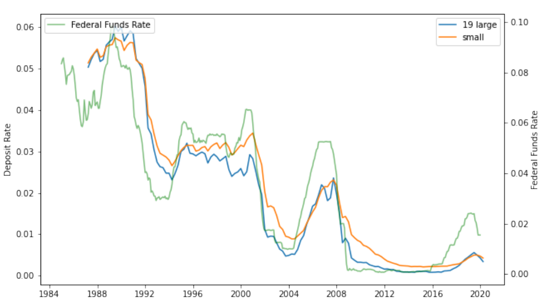
This research contributes to the understanding of deposit rate-setting behavior and competitive dynamics among banks, providing valuable insights for regulators and policymakers. Future research could explore the potential impact of new technologies, regulations, and the role of digital banking and fintech companies in shaping the deposit rates and competitive landscape of the banking industry. This would help assess whether they introduce new dimensions to deposit rate competition and affect traditional patterns in the banking sector.



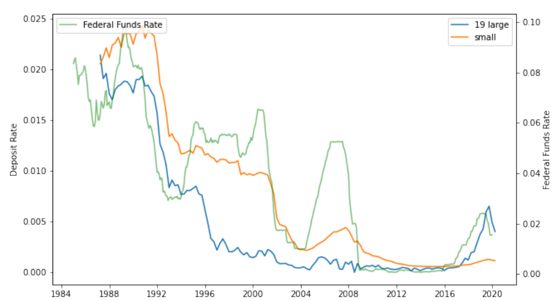
(a) Total Deposits



(b) Time Deposits



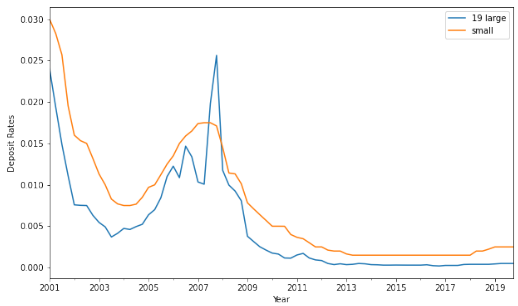
(c) Saving Deposits



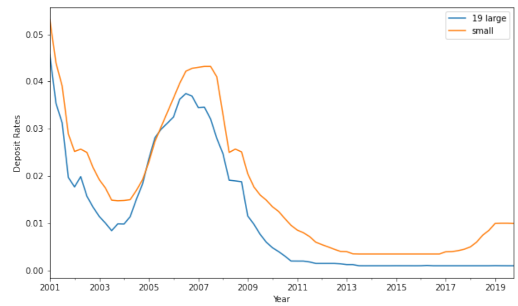
(d) Transaction Deposits

Figure 3.1: Median Deposit Rates-Call Report Data

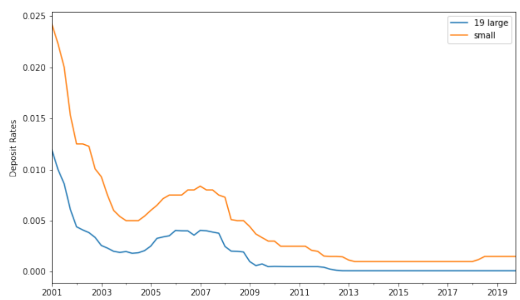
Note: The figures present the time series of median deposit rates for 19 large banks compared to other banks, using bank-level deposit rates calculated from Call Reports covering the period from 1985 to 2020. The charts display the implied deposit rates for total deposits, time deposits, saving deposits, and transaction deposits. The green lines represent the time series of federal funds rates, which serve as reference rates obtained from Federal Reserve Economic Data (FRED), plotted against the right y-axis.



(a) MM 25K



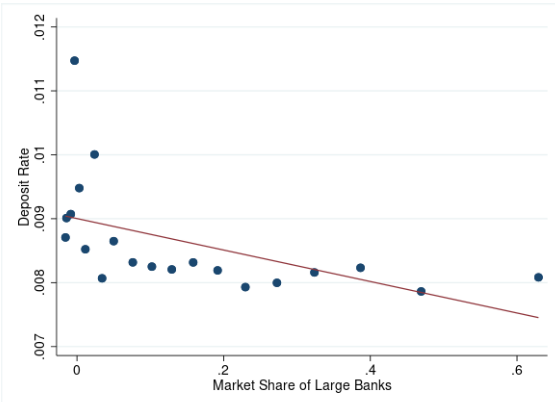
(b) 12M CD 10K



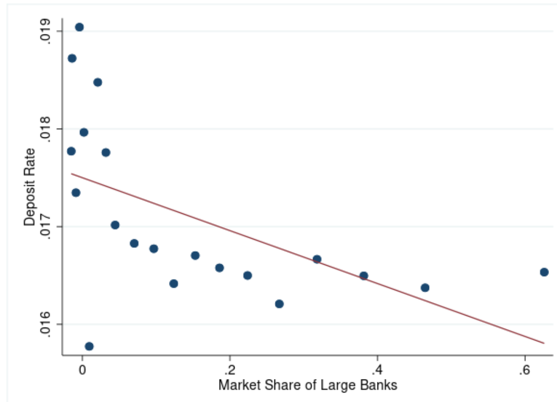
(c) Saving 2.5K

Figure 3.2: Median Deposit Rates-RateWatch Data

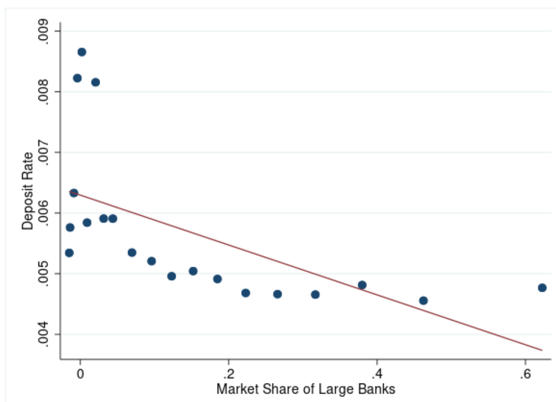
Note: The figures present the time series of median deposit rates for 19 large banks compared to other banks using RateWatch data from 2001 to 2019. The branch-level deposit rates are collapsed at bank level weighted by branch deposit balance. The charts display deposit rates of money market accounts of $25,000, 12 month CD of $10,000, and saving account below $2,500.



(a) MM 25K



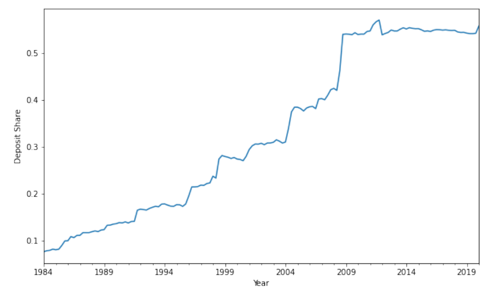
(b) 12M CD 10K



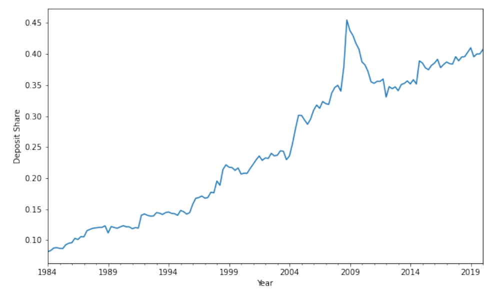
(c) Saving 2.5K

Figure 3.3: Deposit Rates and Market Share of Large Banks

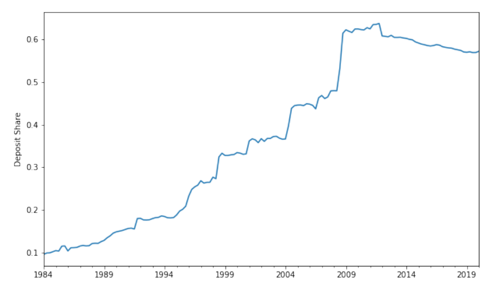
Note: These figures illustrate the relationship between deposit rates of small banks and the market share of large banks in the local market where small banks operate, using RateWatch data from 2001 to 2019. Branch-level deposit rates are collapsed at the bank level, weighted by branch deposit balance. The charts display deposit rates of money market accounts of $25,000, 12 month CD of $10,000, and saving account below $2,500. The market share of large banks is calculated at the Zipcode level by dividing the total deposits held by large banks by the total deposits within the Zipcode.



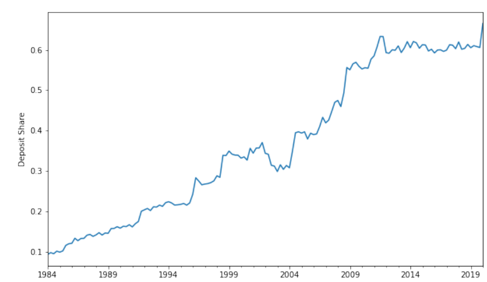
(a) Total Deposits



(b) Time Deposits



(c) Saving Deposits



(d) Transaction Deposits

Figure 3.4: Deposit Share of 19 Large Banks

Note: These figures plot the deposit share of 19 large banks using Call Report data from 1984 to 2020. The deposit share is calculated by dividing the total deposit held by the 19 large banks by the total national deposit. The figures also display the large bank deposit share for time deposits, saving deposits, and transaction deposits.

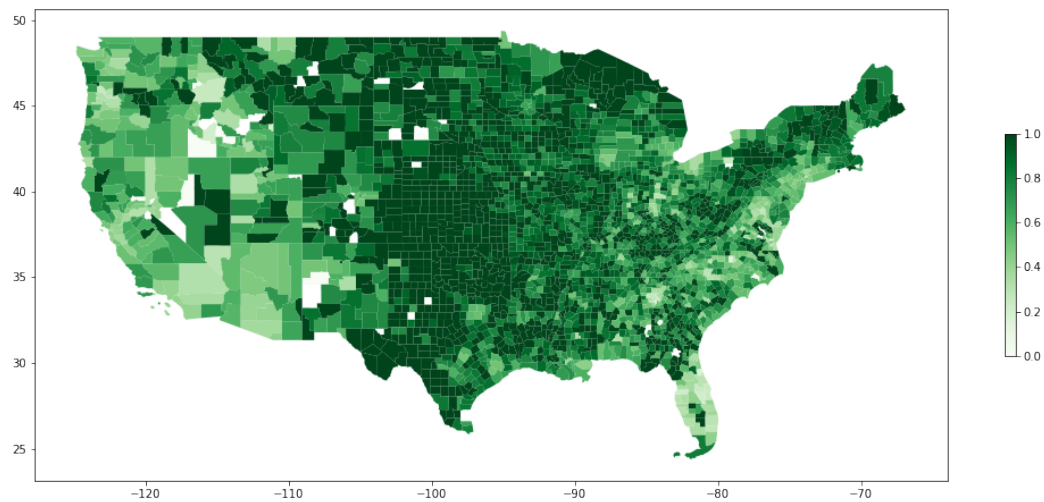
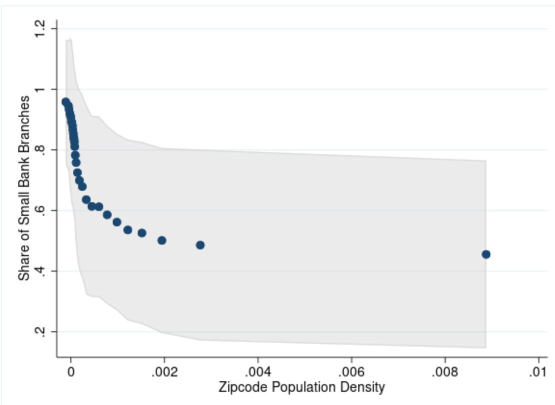
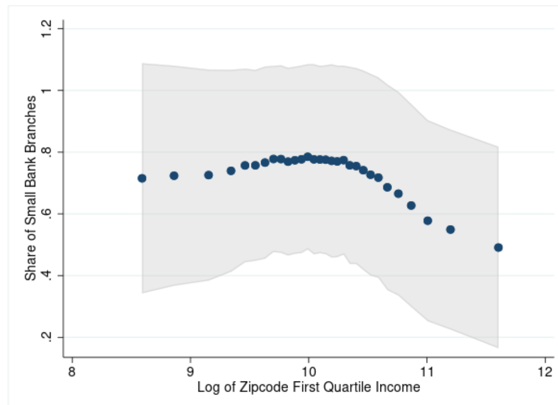


Figure 3.5: Share of Branches held by Small Banks

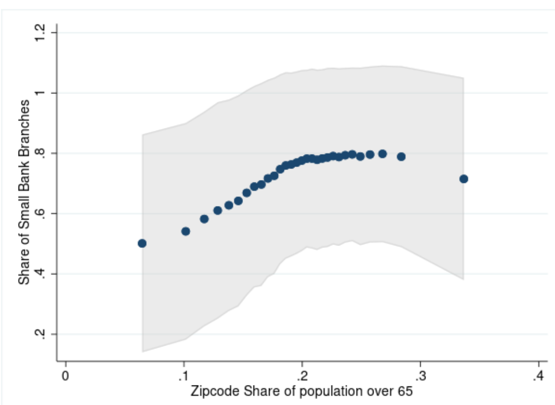
Note: This map displays the share of branches held by small banks at the county level in 2019. The share of small banks' branches is calculated by dividing the number of branches held by small banks by the total number of branches in the county. The intensity of the color represents the level of branch shares, with deeper shades indicating a higher share of small bank branches.



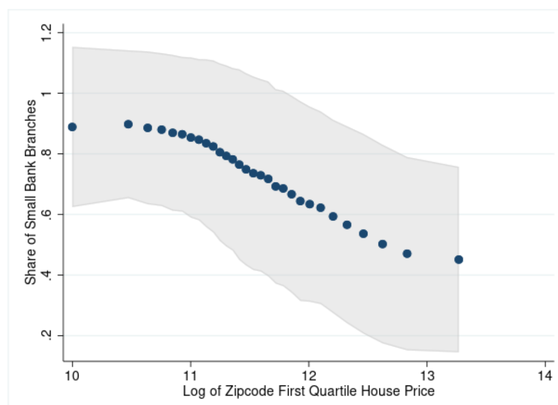
(a) Population



(b) Income



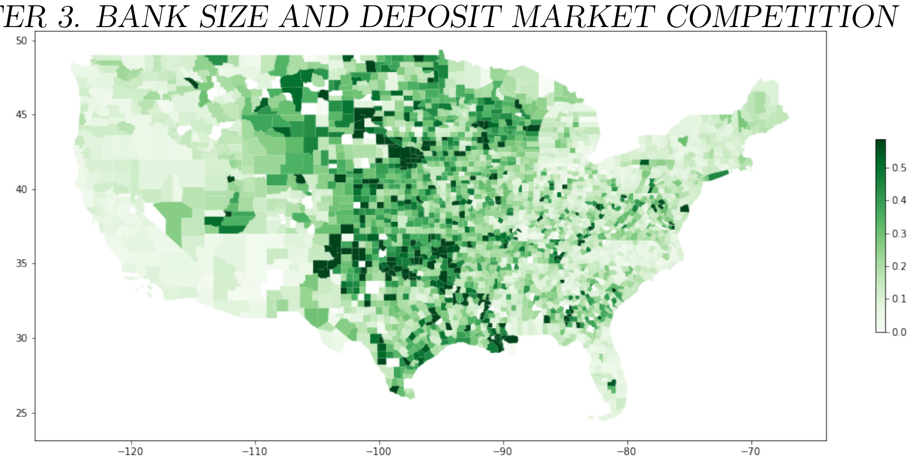
(c) Elderly Population



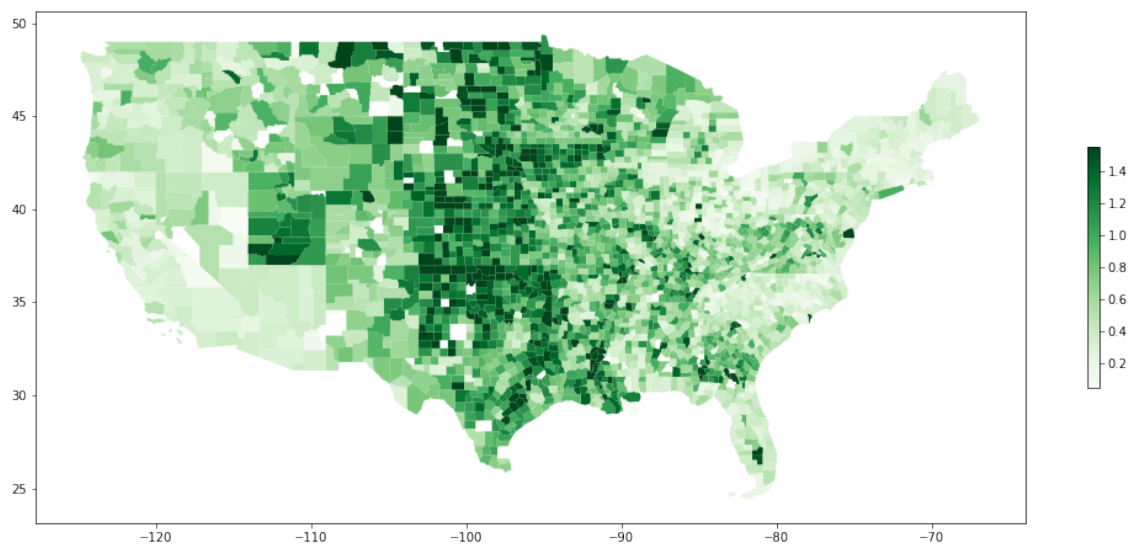
(d) Housing Price

Figure 3.6: Small Bank Share and Demographics

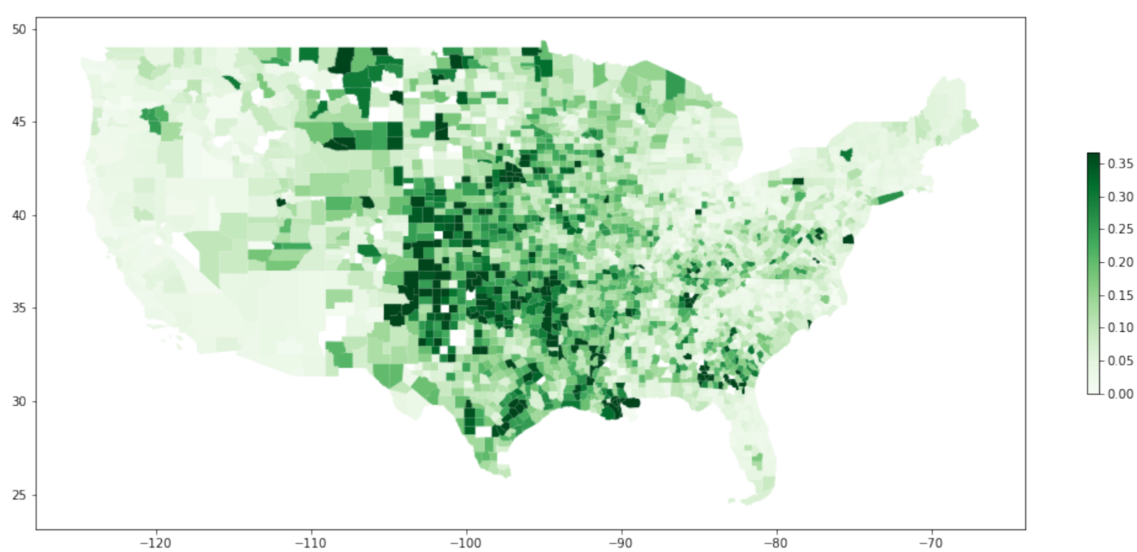
Note: These figures examine the relationship between the share of small bank branches and local population, income, elderly population, and housing prices from 2006 to 2019. Demographic data are sourced from Infogroup at the zipcode level. Income and housing prices represent the 25% quantile of the respective measures. The grey area in the figures illustrates one standard deviation below and above the average.



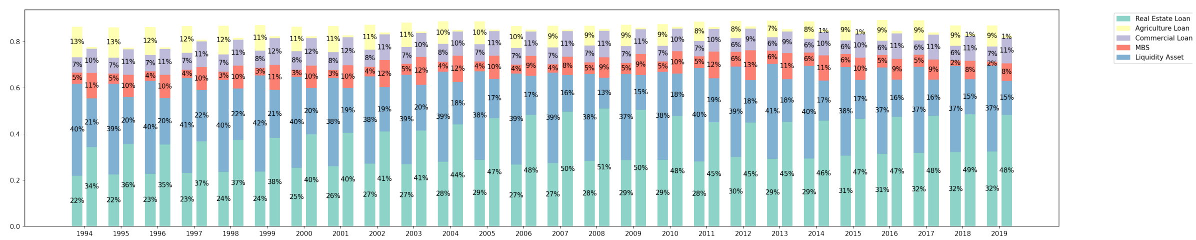
(a) MM 25K



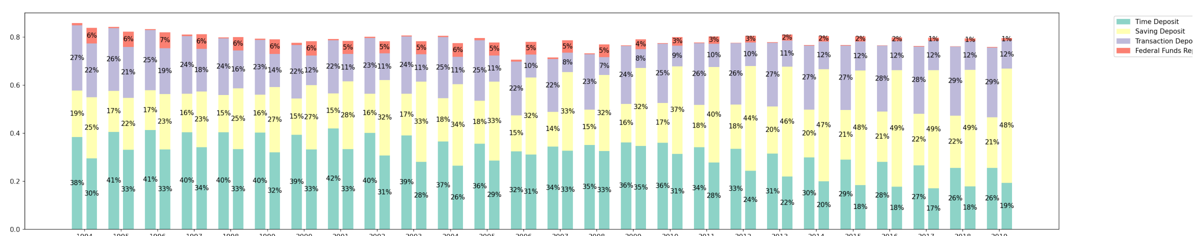
(b) 12M CD 10K



(c) Saving 2.5K



(a) Asset Structure: Lowest Asset Decile (Left) vs Highest Asset Decile (Right)



(b) Liability Structure: Lowest Asset Decile (Left) vs Highest Asset Decile (Right)

Figure 3.8: Asset and Liability Structure

Note: These figures display the asset and liability structures of banks based on Call Report data from 1994 to 2019. The asset (liability) share is calculated by dividing the specific asset (liability) of interest by the total assets (liabilities) at the bank level, and then plotting the average for each bank group. The left bar in each group represents data for banks with total assets below the lowest decile, while the right bar corresponds to banks with total assets above the highest decile.

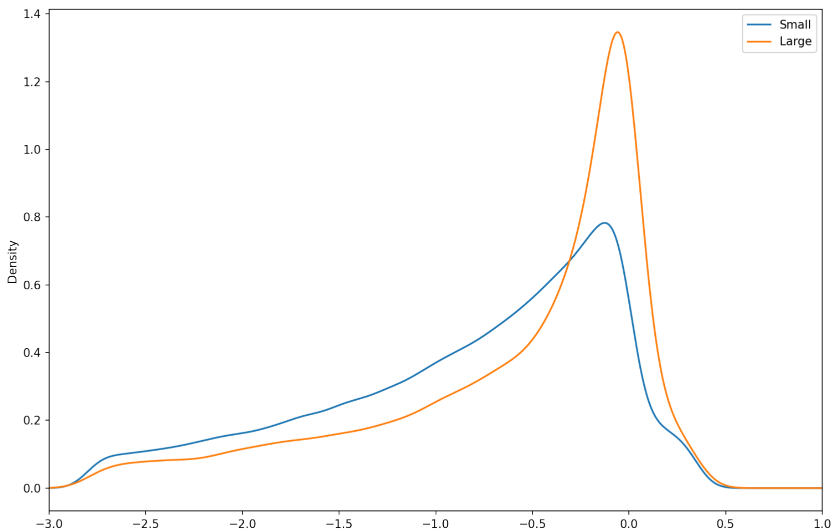
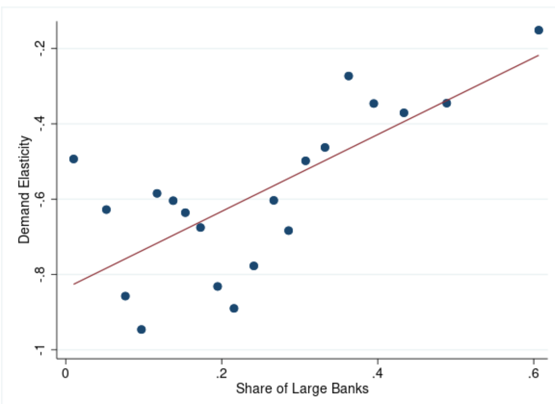
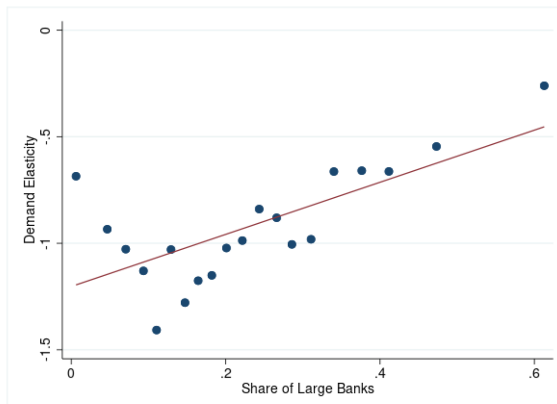


Figure 3.9: Density of Deposit Elasticity

Note: This figure plots the density graph of estimated deposit demand elasticity of large and small banks. Large banks are banks with assets above the 99% percentile.



(a) All



(b) T Statistics > 1.5

Figure 3.10: Deposit Elasticity and Large Bank Local Share

Note: This figure presents the relationship between bank demand elasticity and share of large banks. Share of large banks is calculated by dividing the number of large banks by the total number of banks in the county cluster. Large banks are banks with assets above the 99% percentile. The left figure plots on the full sample, and the right figure restrict sample to estimations with t statistics of price sensitivity greater than 1.5.

Table 3.1: Rate Variation Within Banks

	12M CD 10K		MM 25K		Saving 2.5K	
	(1)	(2)	(3)	(4)	(5)	(6)
FE	Time	Bank×Time	Time	Bank×Time	Time	Bank×Time
Observations	46,443,692	44,766,046	43,920,768	42,343,777	45,846,684	44,174,299
R-squared	0.878	0.988	0.610	0.950	0.557	0.949

Note: This table investigates the sources of deposit rate variation by conducting regression analysis. The regressions follow the equation:

$$Rate_{branch,t} = FE + \epsilon_{branch,t}$$

The data consist of weekly deposit rates from RateWatch, covering the period from 2001 to 2019 at the branch level. The selected deposit products include 12-month CDs with a balance of \$10,000 (columns 1 and 2), money market accounts with a balance of \$25,000 (columns 3 and 4), and savings accounts with balances below \$2,500 (columns 5 and 6). Odd-numbered columns incorporate time fixed effects, while even-numbered columns include time-bank fixed effects.

Table 3.2: Residual Analysis

	12M CD 10K			
	(1)	(2)	(3)	(4)
FE	Bank×Time	Large×Time	HHI×Time	Population×Time
Observations	44,766,046	44,766,046	44,749,523	44,266,697
R-squared	0.909	0.215	0.018	0.026
	MM 25K			
	(5)	(6)	(7)	(8)
FE	Bank×Time	Large×Time	HHI×Time	Population×Time
Observations	42,343,777	42,343,777	42,328,766	41,862,179
R-squared	0.879	0.107	0.005	0.007
	Saving 2.5K			
	(9)	(10)	(11)	(12)
FE	Bank×Time	Large×Time	HHI×Time	Population×Time
Observations	44,174,299	44,174,299	44,158,357	43,680,242
R-squared	0.896	0.154	0.024	0.027

Note: This table tests the contribution of local market characteristics to rate variations after removing time variation, implementing a two-step analysis and reporting the results of second stage.

$$Rate_{branch,t} = \alpha_t + \epsilon_{branch,t}$$
$$\hat{\epsilon_{branch,t}} = FE + \varepsilon_{branch,t}$$

The data consist of weekly deposit rates from RateWatch, covering the period from 2001 to 2019 at the branch level. The selected deposit products include 12-month CDs with a balance of $10,000 (columns 1-4), money market accounts with a balance of $25,000 (columns 5-8), and savings accounts with balances below $2,500 (columns 9-12). Fixed effects incorporated are bank-time, large-time (with "Large" as a dummy for 19 Dodd-Frank large banks), HHI-time (calculated at zipcode level), and population-time fixed effects.

Table 3.3: Deposit Rate Gaps Between Large and Small Banks

	12M CD 10K		MM 25K		Saving 2.5K	
	(1)	(2)	(3)	(4)	(5)	(6)
Libor	0.719***		0.345***		0.189***	
	(0.000201)		(0.000189)		(0.000149)	
Large	-0.00539***	-0.00502***	-0.00261***	-0.00254***	-0.00325***	-0.00314***
	(6.41e-05)	(3.58e-05)	(6.11e-05)	(4.50e-05)	(4.77e-05)	(3.20e-05)
T-FE		Yes		Yes		Yes
Observations	4,354,051	4,354,051	4,170,821	4,170,821	4,334,833	4,334,833
R-squared	0.746	0.921	0.443	0.698	0.270	0.672

Note: This table estimates the average deposit rate difference between large and small banks using RateWatch data. Branch-level deposit rates are collapsed into bank-level rates by taking the average rates weighted by branch deposit balance. The 19 Dodd-Frank large banks are defined as large banks, and the depend variables are deposit rates of 12 month CD of $10,000, money market accounts of $25,000, and saving account below $2,500. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table 3.4: Demand Estimation

	Estimation
Price	-0.441
	[-1.445]
Log(Branch Number)	0.476
	[2.709]
Log(Employee per Branch)	0.325
	[1.855]
Large	0.005
	[1.78]
Share of Agriculture Loans	1.586
	[1.501]
Year FE	Y
Bank FE	Y
Observations	322216

Note: This table reports the average estimated deposit demand parameters using county cluster-level market shares. The sample includes all U.S. commercial banks from 2001 to 2019. The data is from the Call Reports and the Summary of Deposits. Price is the difference between federal funds rate and deposit rates, Log(Branch Number) is the logarithm of total number of branches held by the bank, Log(Employee per Branch) is the logarithm of average number of employees per branch, Large indicates if the bank has assets above the 99% percentile, and the share of agriculture loans represents the proportion of agriculture loans in total bank assets. The estimation is performed on a county cluster-by-cluster basis and trims the estimation at the 10% level. The estimation column reports the average of county estimations, with t statistics provided in brackets.

Table 3.5: Demand Elasticity

(a) Panel A: All

	Mean	Std	25%	50%	75%
All	-0.372	0.638	-0.676	-0.152	0.021
Small	-0.412	0.657	-0.760	-0.202	0.017
Large	-0.259	0.564	-0.406	-0.053	0.028
Difference	-0.153***				
T-stats	59.704				

(b) Panel B: T Statistics>1.5

	Mean	Std	25%	50%	75%
All	-0.770	0.743	-1.229	-0.564	-0.167
Small	-0.827	0.744	-1.300	-0.642	-0.224
Large	-0.588	0.713	-0.940	-0.305	-0.054
Difference	-0.239***				
T-stats	53.426				

Note: These tables present the summary statistics of calculated demand elasticity after trimming the original estimation at the 10% level. The first row reports the summary statistics for all banks (after trimming). The "Large" row represents banks with assets above the 99% percentile, while the "Small" row reports the remaining banks. The "Difference" row displays the mean difference between small and large banks, with t statistics provided below. Panel A focuses on the full sample, while Panel B restricts the estimation to those with t statistics of price sensitivity greater than 1.5.

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Appendices

A Appendix of Chapter 1

VC Hiring Strategy

Previous empirical evidence explores the network effects of two types of VC hiring, *first hiring* and *additional hiring*. The two types of hiring also have different implications for VC network evolution. If VC companies hire partners with various educational backgrounds to expand alumni networks (*first hiring*), they allocate funding to more diverse startups. However, suppose VC companies prefer partners from the same universities to strengthen existing alumni ties (*additional hiring*), their alumni networks are increasingly centralized, and funds are disproportionally allocated to alumni startups of specific universities, thus amplifying the distortion. Therefore, it is meaningful to test how VC companies implement two types of hiring.

In this section, I test whether VC companies hire more alumni or non-alumni partners. Since data only consists of actual hiring, I construct counterfactual partner candidates that VC could have considered hiring but did not through matching. Assuming similar VC companies share the same partner candidate pools, I first split VC companies into groups by headquarters location, industry, AUM quintiles, and total investment quintiles. Partners hired by a VC would be plausible candidates for other VC companies within the same group. Therefore, I create partner-VC pairs within the group and construct the final sample.

The regression is

$$Hire_{v,t,p} = \alpha_t + \gamma_v + \eta_p + Alumni_{v,p} + \epsilon_{v,t,p}$$

where $Alumni_{v,p}$ indicates if the partner p is alumni of current partners in VC company v. The regression includes time, VC, and partner fixed effects.

Table A1 presents the results of hiring. Column 1 indicates that VC hires 0.014 more alumni partners, an increase of 18.6% compared with non-alumni partners. Column 2 tests if the alumni effects differ among new partners' degrees. The results show that partners who obtain alumni ties through MBA and undergraduate programs are significantly affected. The heterogeneous effects on degrees imply that VC companies value MBA and Bachelor's degrees more when hiring new partners, which differs from selecting startups relying more on bachelor's and Ph.D. degrees.

Table A1: Alumni Network Effects on Partner Hiring

	(1)	(2)	(3)	(4)
Alumni	0.0139***			
	(0.00276)			
Alumni×Bachelor		0.00829**		
		(0.00337)		
Alumni×Master		-0.00407		
		(0.00701)		
Alumni×Ph.D.		0.00679		
		(0.00839)		
Alumni×MBA		0.00998**		
		(0.00477)		
Alumni×Undeclared		0.00910		
		(0.00568)		
Alumni×High AUM			0.0109**	
			(0.00435)	
Alumni×Low AUM			0.0215**	
			(0.00984)	
Alumni×Large Team				0.0117***
				(0.00297)
Alumni×Small Team				0.0213***
				(0.00446)
VC FE	Y	Y	Y	Y
Partner FE	Y	Y	Y	Y
Year Fe	Y	Y	Y	Y
Observations	102,318	102,318	102,318	102,318
R-squared	0.271	0.270	0.271	0.271

Note: This table estimates the effects of alumni networks on partner hiring by

$$Hire_{v,t,p} = \alpha_t + \gamma_v + \eta_p + Alumni_{v,p} + \epsilon_{v,t,p}$$

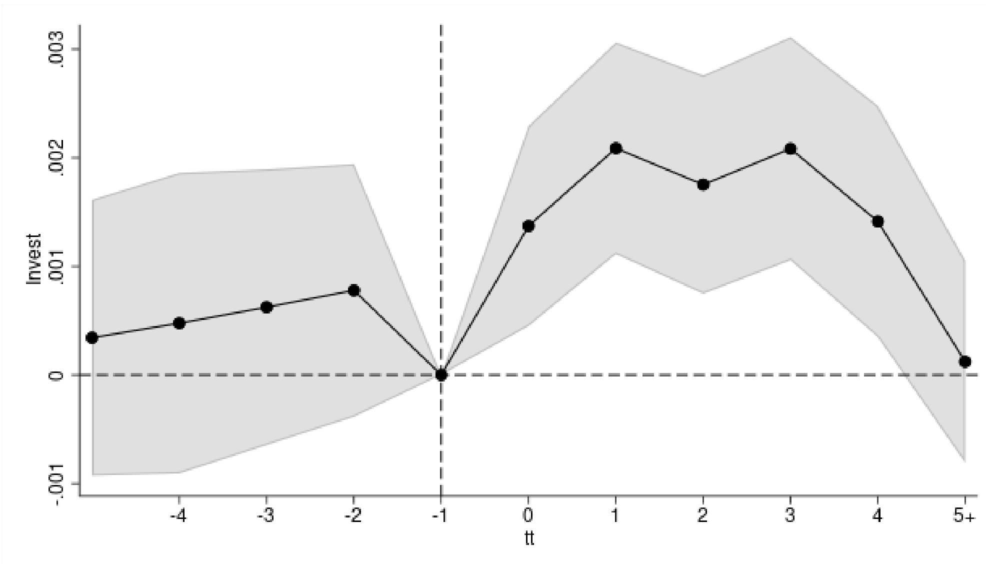
VC, year, and partner fixed effects are included. Column 1 provides the baseline estimation. Column 2 compares the effects for degrees new partners hold. Column 3 tests the heterogeneous effects on VC with different asset under management (AUM). High AUM VCs are those managing AUM above the median. Column 4 compares VCs with various investment team size. Large team VCs are ones with partner numbers above the median. Some interaction terms are eliminated for brevity. The standard errors are clustered by VC companies. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Columns 3 and 4 compare the effects among VC companies. Column 3 compares VC companies with high asset under management (AUM) versus low AUM. VC companies with AUM above the median are defined as a high AUM VC and vice versa. The results show that VC companies with smaller AUM have a higher tendency to hire alumni partners. Column 4 tests heterogeneous effects on VC with different investment team sizes. A VC is categorized as a small team VC if the number of current partners is below the median and a large team VC otherwise. The estimation implies that when the current partner team is small, the VC tends to hire more alumni partners. These two columns imply that when VC companies are relatively small and under a growing stage, they rely more on current alumni networks when expanding investment teams, and hire new partners with similar educational backgrounds, resulting in less diverse alumni networks.

The above evidence points out the increasing concentration of alumni networks within VC companies. VC companies continue hiring partners of the same educational background, most of whom are educated in prestigious universities. Therefore, they disproportionally allocate more capital to alumni founders but miss out on high-quality investments from less distinguished universities. As a result, both founders and venture capitalists from leading universities dominate the venture capital market.

Additional Tables and Figures

Figure A1: Effect of Alumni Networks on Investment Choice-Sun and Abraham Estimator



Note: These figures plot the effect of alumni networks on investment, obtained from estimating equation 1.3.2, under the estimation method proposed by Sun and Abraham (2021). The y-axis measures the effects on investment probability, the x-axis is the relative years to the event year, with -1 as the baseline year. The regression includes VC-industry-year fixed effects and company-year fixed effects, alumni status to incumbent partners, and professional connections to current partners. Robust standard errors are clustered at VC level.

B Appendix of Chapter 2

Proofs

Proof of Equilibrium

Proof. **No Venture Debt**

We first solve the world without venture debt, where only venture capital takes action. When the venture capital observes a firm with a valuation of a, the venture capital updates the belief of the probability of the firm being a high type as

$$\alpha_a = \frac{\alpha P_H}{\alpha P_H + (1-\alpha) P_L}.$$

If the venture capital investigates the firm and invests in high type, the profit of the venture capital is

$$R_I = -cI + \alpha_a \left(a\mu_H \cdot \frac{I}{I+a} - I \right).$$

If the venture capital does not investigate but invests in all firms, the profit of the venture capital is

$$R_{NI} = \frac{aI}{I+a} \left\{ \alpha_a \mu_H + (1-\alpha_a)\, \mu_L \right\} - I.$$

If the venture capital does not invest in any firms,

$$R_N = 0.$$

To optimize the return of venture capital investors, the action of the venture capital is as follows.

- When $c < (1-\alpha_a) \frac{I+a-a\mu_L}{I+a} = C_a^*$ and $c < \alpha_a \left(\frac{a\mu_H}{I+a} - 1 \right)$, the venture capital investigates and only invest in high type firms.
- When $c > c_a^*$ and $\alpha_a \mu_H + (1-\alpha_a)\mu_L - 1 > \frac{I}{a}$, the venture capital does not investigate and always invest in firms.
- Otherwise, there is no investment.

If the venture capital observes a firm in the state b, the venture capital updates the belief of the probability of the firm being a high type as

$$\alpha_b = \frac{\alpha\,(1-P_H)}{\alpha\,(1-P_H) + (1-\alpha)\,(1-P_L)}.$$

Similarly, to maximize the profits, the venture capital's action is as follows.

Table A1: Variable Definitions

Variable	Definition
	Startup Variables
Total Raised	Total amount of capital that the startup raised before exit or before 2021
Employees	Number of employees
Latest Valuation	The post valuation of the latest financing round or the exiting valuation
IT	Indicator variable that equals one if the startup is in the information technology industry
Healthcare	Indicator variable that equals one if the startup is in the healthcare industry
Close	Indicator variable that equals one if the startup goes bankrupt
Acquire	Indicator variable that equals one if the startup is acquired
IPO	Indicator variable that equals one if the startup goes IPO
	VC Variables
AUM	The size of VC's asset under management
IT	Indicator variable that equals one if the VC specializes in the information technology industry
Healthcare	Indicator variable that equals one if VC specializes in the healthcare industry
	Deal and VC-Startup Pair Variables
Deal Size	The total amount of investment in the deal
Valuation	Post-valuation of the deal
Top University Founder	Indicator variable that equals one if the startup has founders graduating from university with top ten ranked business program
Professional Networks	Indicator variable that equals one if the startup founder and VC partners have worked in the same company before
Female Founder Share	The share of female founders in the startup
Black Founder Share	The share of black founders in the startup
Asian Founder Share	The share of Asian founders in the startup
White Founder Share	The share of white founders in the startup
California	Indicator variable that equals one if the startup is located in California
Startup Founding Year	The year when the startup is founded
Leading VC	Indicator variable that equals one if the VC is the leading investor in the deal
Early Rounds	Indicator variable that equals one if startup is seeking for seed or earlier rounds of financing
Alumni of New Partner	Indicator variable that equals one if the startup founder and the new partner the VC hires went to the same university
Alumni of Incumbent Partner	Indicator variable that equals one if the startup founder and current partners of the VC went to the same university

Table A2: Alumni Network Effects on Investment choice–Sample Under 1-year Window

	Full Sample (1)	(2)	Early Round (3)	Top Sample (4)
Alumni of New Partner ×After Hiring	0.150**	0.181***	0.229**	0.194**
	(0.0701)	(0.0701)	(0.102)	(0.0926)
Alumni of New Partner	0.543***	0.540***	0.698***	0.274***
	(0.0658)	(0.0667)	(0.0842)	(0.0827)
Alumni of Incumbent Partner	0.231***	0.301***	0.373***	0.274***
	(0.0552)	(0.0565)	(0.0894)	(0.0700)
Professional Networks	0.646***	0.685***	0.748***	0.675***
	(0.0448)	(0.0461)	(0.0724)	(0.0500)
Startup×Year FE	Y	Y	Y	Y
VC×Year FE	Y			
VC×Year×Industry FE		Y	Y	Y
Observations	2,460,352	2,450,457	1,298,640	1,229,061
R-squared	0.275	0.244	0.243	0.243

Note: This table estimates the alumni network effects on investment choice by equation 1.3.1, but constructing sample with one-year investment window. The dependent variable is an indicator variable that equals to 100 if the VC invest in the startup and zero otherwise. Column 1 regresses on the full sample with investor-year fixed effects and company-year fixed effects. Column 2 also regresses on the full sample, except including VC-year-industry fixed effects and company-year fixed effects. Column 3 only includes deals in seed or earlier rounds. Column 4 only includes startups having founders from top universities, defined as universities with top 10 business programs according to US News ranking in 2020. The standard errors are clustered by VC companies. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

- When $c < (1-\alpha_b)\frac{I+b-b\mu_L}{I+b} = C_b^*$ and $c < \alpha_b\left(\frac{b\mu_H}{I+b} - 1\right)$, the venture capital investigates and only invest in high type firms.
- When $c > c_b^*$ and $\alpha_b\mu_H + (1-\alpha_b)\mu_L - 1 > \frac{I}{b}$, the venture capital does not investigate and always invest in firms.
- Otherwise, there is no investment.

With Venture Debt
Banks are rational and maximize their payoff $ER - C_B - D$ by

$$\Pi_B = -C_B - D + P_I(C_B)R.$$

Venture capital does not change their actions, except by updating the belief based on the bank's behavior. Therefore, the action of the venture capital is as follows. When the venture capital observes a firm with a valuation of a, the venture capital updates the belief

Table A3: Alumni Network Effects on Investment Choice-2SLS

	Full Sample (1)	Top Sample (2)	Early Sample (3)
First Stage	Dependent Variable: Alumni of New Partner×After Hiring		
Alumni of New Partner×After Leave	0.667***	0.687***	0.732***
	(0.0159)	(0.0145)	(0.0168)
Cragg-Donald Wald F	7.733e+06	3.847e+06	5.493e+06
Kleibergen-Paap Wald rk F	1769	2259	1897
Second Stage	Dependent Variable: Invest		
Alumni of New Partner×After Hiring	0.0941**	0.110***	0.173**
	(0.0456)	(0.0401)	(0.0758)
relative to Baseline	6.72%	8.53%	12.84%
Alumni of New Partner	0.240***	0.111***	0.261***
	(0.0295)	(0.0316)	(0.0403)
Alumni of Incumbent Partner	0.169***	0.144***	0.204***
	(0.0237)	(0.0303)	(0.0352)
Professional Networks	0.327***	0.322***	0.370***
	(0.0192)	(0.0220)	(0.0295)
Startup×Year FE	Y	Y	Y
VC×Year×Industry FE	Y	Y	Y
Observations	6,185,555	3,070,864	3,344,579
R-squared	0.226	0.220	0.221

Note: This table estimates LATE treatment effects by two-stage least square. *After Leave* takes one if current partners leave the VC companies. *After Leave* × *Alumni of New Partner* is treated as an instrument for *After Hiring* × *Alumni of New Partner*. The first-stage is regress *After Hiring* × *Alumni of New Partner* on *After Leave* × *Alumni of New Partner* and control variables including alumni status to partners, professional networks, and alumni to new partners. The dependent variable in second stage is an indicator variable that equals to 100 if the VC invest in the startup and zero otherwise. VC-year-industry and company-year fixed effects are included. Column 1 regresses on the full sample. Column 2 only includes startups having founders from top universities, defined as universities with top 10 business programs according to US News ranking in 2020. Column 3 focuses on deals in seed or earlier rounds. the row "relative to baseline" reports the size of the effect relative to the baseline of outcome variable before events. The standard errors are clustered by VC companies. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

Table A4: Alumni Network Effects on Investment Performance-Industry Portfolio Return

Dependent Variable	All ln(Return) (1)	Lead ln(Return) (2)	All ln(IRR) (3)	Lead ln(IRR) (4)
Alumni of New Partner $\times$After Hiring	-0.238***	-0.316*	-0.0383*	-0.0594
	(0.0908)	(0.185)	(0.0202)	(0.0413)
Relative to Baseline	-31.36%	-41.09%	-25.36%	-37.59%
Alumni of New Partner	0.651***	0.823***	0.118***	0.142***
	(0.0777)	(0.153)	(0.0168)	(0.0337)
After Hiring	0.116	0.145	0.0206	0.0408
	(0.0918)	(0.200)	(0.0204)	(0.0446)
Year FE	Y	Y	Y	Y
VC FE	Y	Y	Y	Y
Observations	22,370	5,686	21,717	5,452
R-squared	0.227	0.315	0.247	0.343

Note: This table estimates the effects of alumni networks on portfolio return by equation 1.5.2. Specifically, for each VC company at year t in industry i, I bundle all new alumni companies invested into a new-alumni portfolio at year t and other invested companies at t into a control portfolio. Then I define the return as the sum of the valuation of portfolio startups divided by the total amount invested in the portfolio. Columns 1 and 3 construct portfolios using all deals, and columns 2 and 4 construct portfolios by leading deals. The dependent variable in columns 1 and 2 is the logarithm of the portfolio return. Column 3 and 4 regress on the logarithm of the portfolio internal rate of return (IRR). VC fixed effects and year fixed effects are included. The standard errors are clustered by VC companies. * $p < 0.10$, ** $p < 0.05$, ***$p < 0.01$.

of the probability of the firm being a high type as

$$\alpha_a(C_B) = \frac{\alpha(C_B)P_H}{\alpha(C_B)P_H + (1-\alpha(C_B))P_L}.$$

- When $c < (1-\alpha_a(C_B))\frac{I+a-a\mu_L}{I+a} = c_a(C_B)$ and $c < \alpha_a(C_B)\left(\frac{a\mu_H}{I+a} - 1\right)$, the venture capital investigates and only invest in high type firms.
- When $c > c_a(C_B)$ and $\alpha_a(C_B)\mu_H + (1-\alpha_a(C_B))\mu_L - 1 > \frac{I}{a}$, the venture capital does not investigate and always invest in firms.
- Otherwise, there is no investment.

If the venture capital observes a firm in the state b, the venture capital updates the belief of the probability of the firm being a high type as

$$\alpha_b(C_B) = \frac{\alpha(C_B)\,(1-P_H)}{\alpha(C_B)\,(1-P_H) + (1-\alpha(C_B))\,(1-P_L)}.$$

Similarly, to maximize the profits, the venture capital's action is as follows.

- When $c < (1-\alpha_b(C_B))\frac{I+b-b\mu_L}{I+b} = c_b(C_B)$ and $c < \alpha_b(C_B)\left(\frac{b\mu_H}{I+b}-1\right)$, the venture capital investigates and only invest in high type firms.
- When $c > c_b(C_B)$ and $\alpha_b(C_B)\mu_H + (1-\alpha_b(C_B))\mu_L - 1 > \frac{I}{b}$, the venture capital does not investigate and always invest in firms.
- Otherwise, there is no investment.

According to the level of c, there are three possible equilibria in this model.

1. When c is sufficiently small, based on the reasoning above, venture capital always does due diligence and invests in high-type firms. Therefore, only high-type firms will be invested in by the venture capital. Given venture capital's behavior, $P_I(C_B) = \alpha(C_B)$. The level of screening cost C_B^0 banks take solves

$$\begin{cases} -1 + \alpha'(C_B)R = 0, \quad (\text{ F.O.C }) \\ -C_B - D + \alpha(C_B)R = 0. \end{cases}$$

   If venture capital always does due diligence, c must satisfy

$$c \leq (1-\alpha_a(C_B^0))\frac{I+a-a\mu_L}{I+a} = c_a(C_B^0).$$

   Therefore, when $c < c_a(C_B^0)$, banks spend C_B^0 on screening and venture capital always does due diligence and invests in good firms.

2. When c is sufficiently large, venture capital always takes the signal of venture debt and invests in all firms with venture debt when $V = a$, and does due diligence when $V = b$. In this case, $P_I(C_B) = \alpha(C_B) + (1-\alpha(C_B))P_L$. The level of screening cost C_B^2 banks take solves

$$\begin{cases} -1 + \alpha'(C_B)(1-P_L)R = 0, \quad (\text{ F.O.C }) \\ -C_B - D + (\alpha(C_B) + (1-\alpha(C_B))P_L)R = 0. \end{cases}$$

   If venture capital always invests in firms with venture debt when $V = a$, c must satisfies

$$c \geq (1-\alpha_a(C_B^2))\frac{I+a-a\mu_L}{I+a} = c_a(C_B^2).$$

   Therefore, when $c > c_a(C_B^2)$, banks spend C_B^2 on screening, and venture capital does due diligence and invests in good firms when observing $V = b$, and always invests in firms with venture debt when $V = a$.

3. When $c \in (c_a(C_B^0), c_a(C_B^2))$, there is no pure strategy for venture capital. If banks spend C_B^0, since $c > c_a(C_B^))$, venture capital will take the signal of venture debt. Anticipating that, banks will change their action and spend C_B^2 on screening. Then venture capital has the incentive to ignore the signal as $c < c_a(C_B^2)$. Therefore, venture capital will play a mixed strategy. When $V = a$, venture capital plays a mixed strategy of investing in all firms with venture debt with some probability p and investing in high-type firms after due diligence with probability $1 - p$. When $V = b$, venture capital invests in high-type firms after due diligence. In this case, $P_I(C_B) = \alpha(C_B) + (1 - \alpha(C_B))p \times P_L$. The level of screening cost C_B^1 banks take solves

$$\begin{cases} -1 + \alpha'(C_B)(1 - p \times P_L)R = 0, \quad (\text{ F.O.C }) \\ -C_B - D + (\alpha(C_B) + (1 - \alpha(C_B))p \times P_L)R = 0. \end{cases}$$

Since venture capital has no preference over doing due diligence or not when $V = a$, p must satisfies

$$c = (1 - \alpha_a(C_B^1(p)))\frac{I + a - a\mu_L}{I + a} = c_a(C_B^1(p)).$$

□

Proof of Proposition 2

Proof. To generalize the question, let C_B and R denote the solutions to

$$\begin{cases} -1 + \alpha'(C_B)(1 - q)R = 0, \quad (\text{ F.O.C }) \\ -C_B - D + (\alpha(C_B) + (1 - \alpha(C_B))q)R = 0. \end{cases}$$

C_B^0, R^0 solves the question when $q = 0$, C_B^1, R^1 solves when $q = p \times P_L$, and C_B^2, R^2 solves when $q = P_L$. We will prove the proposition by showing that C_B decreases in q and R decreases in q.

$$\begin{cases} -1 + \alpha'(C_B)(1 - q)R = 0, \quad (\text{ F.O.C }) \\ -C_B - D + (\alpha(C_B) + (1 - \alpha(C_B))q)R = 0, \end{cases}$$

$$\begin{aligned} &\Rightarrow \frac{1}{R} = \alpha'(c)(1 - q) = \frac{\alpha(c) + (1 - \alpha(c))q}{c + D}, \\ &\Rightarrow \alpha'(c)(1 - q)(c + D) = \alpha(c) + (1 - \alpha(c))q. \end{aligned}$$

Derive both sides with respect to q, we get

$$(\alpha''(c)(c+D)(1-q)+\alpha'(c)(1-q))\frac{dc}{dq}-\alpha'(c)(c+D)=\alpha'(c)\frac{dc}{dq}+1-\alpha-\alpha'(c)q\frac{dc}{dq},$$
$$\Rightarrow \alpha''(c)(c+D)(1-q)\frac{dc}{dq}=\alpha'(c)(c+D)+1-\alpha(c).$$

Since $\alpha''(c)<0, \alpha'(c)>0, q<1, \alpha<1$, we have $\frac{dc}{dq}<0$, namely C_B decreases in q.

Let $r=\frac{1}{R}=\alpha'(c)(1-q)$.

$$\begin{aligned}
\frac{dr}{dq} &= \alpha''(c)(1-q)\frac{dc}{dq}-\alpha'(c) \\
&= \alpha''(c)(1-q)\frac{\alpha'(c)(c+D)+1-\alpha(c)}{\alpha''(c)(c+D)(1-q)}-\alpha'(c) \\
&= \frac{\alpha'(c)(c+D)+1-\alpha(c)}{c+D}-\alpha'(c) \\
&= \frac{1-\alpha(c)}{c+D}>0, \\
\Rightarrow & \frac{dR}{dq}<0.
\end{aligned}$$

Thus R decreases in q. □

List of good venture capital firms

In this appendix, we describe the construction of our list of good venture capital firms in detail. We provide the name of these venture capitals and our reference of the rankings.
The list of good venture capital firms (in alphabetical order):

1. Accel Partners
2. Alexandria Venture
3. Alumni Ventures Group
4. Andreessen Horowitz
5. Bessemer Trust
6. Canaan Partners
7. Founders Fund
8. General Catalyst
9. Goldman Sachs
10. Greycroft
11. GV
12. Higher Ground Labs
13. Insight Venture Partners
14. Khosla Ventures
15. Kleiner Perkins Caufield & Byers
16. Lightspeed Venture Partners
17. New Enterprise Associates
18. Quake Capital Partners
19. Revolution
20. Sequoia Capital
21. Sinai Ventures
22. Social Capital

23. SV Angel
24. True Ventures

This list is constructed referring to three convincing venture capital rankings. We also attach their website addresses for reference:

1. https://www.angelkings.com/top-venture-capital-firms
2. https://www.forbes.com/sites/alejandrocremades/2018/07/18/top-10-venture-capital-investors-that-every-entrepreneur-should-be-pitching-right-now/#4017b74f1ced
3. https://pitchbook.com/news/articles/how-20-big-name-us-vc-firms-invest-at-series-a-b