Essays on Relationships in Private Capital Markets

by

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ABSTRACT

Private equity funds sometimes allow their limited partners to directly invest in a firm the fund is buying. This is puzzling because, when such co-investment is offered, the GP both reduces fees for the LP and allows them discretion over whether to make the additional investment. I show that co-investments are driven by an LP's bargaining power in the relationship with the GP. To establish a causal relationship, I consider the 2013 regulatory implementation of the Volcker Rule. This rule prevented banks from investing in private equity, thereby increasing the bargaining power of other LPs. I find that LPs invested in funds that were highly exposed to the loss of bank investors see a 36 basis point increase in co-investments (159% of the pre-2013 mean), compared to the control group. Overall, my results point to the use of co-investments as a device to increase flexibility in the contractual relationship between a GP and an LP. These results are relevant in light of recent regulations affecting LP-GP relationships.

How venture capital firms (VCs) exit their investments and the influence of underwriters on IPOs are the focus of an extensive literature. Still, whether underwriters influence how VCs exit their investments is unclear. We exploit underwriter acquisitions and closures to test whether underwriters affect startup IPOs. When a VC loses an underwriting relationship, startups' likelihood of an IPO falls in the five years following the loss. High employee turnover following underwriter mergers likely undermines the strength of established VC-underwriter relationships, and affected VCs pay higher underwriting fees when they establish new relationships, consistent with frictions in forming new relationships.

Network relationships are critical to the investment opportunity sets and outcomes of VC funds. I study whether VC funds are hesitant to make investment decisions that may damage important VC network relationships. I do so by investigating the effect of startup investments by the largest of VC funds on VC finance prospects for same-industry startups, similar in observable characteristics. Quarterly probability of VC finance for these competitor startups decreases by 0.84%, a 15% drop from the pre-event average. Quarterly amount of VC finance drops by about $22,700, a 5% decline from before. I find that bank and SBA loans do not meaningfully change for these firms after the event. The drop in VC finance is stronger among those previously backed by VCs with a syndicate history

with the super-large VC. I interpret this evidence as favoring a networks explanation above alternative hypotheses related to lower firm quality.

CHAPTER 1

Why do Private Equity Firms Co-invest With Their Limited Partners?

Private equity funds sometimes allow their limited partners to directly invest in a firm the fund is buying. This is puzzling because, when such co-investment is offered, the GP both reduces fees for the LP and allows them discretion over whether to make the additional investment. I show that co-investments are driven by an LP's bargaining power in the relationship with the GP. To establish a causal relationship, I consider the 2013 regulatory implementation of the Volcker Rule. This rule prevented banks from investing in private equity, thereby increasing the bargaining power of other LPs. I find that LPs invested in funds that were highly exposed to the loss of bank investors see a 36 basis point increase in co-investments (159% of the pre-2013 mean), compared to the control group. Overall, my results point to the use of co-investments as a device to increase flexibility in the contractual relationship between a GP and an LP. These results are relevant in light of recent regulations affecting LP-GP relationships.

1.1 Introduction

Private equity (PE) as an investment class has long been dominated by the intermediary fund structure. Investors, known as limited partners (LPs) commit capital to PE funds managed by general partners (GPs). GPs source and screen potential private firms in which

to invest, call capital from LPs, work to add value to the firm and eventually exit for a profit, deducting substantial fees for themselves. Despite recent slowdowns due to high-interest rate environments, growth in PE capital raised in recent years remains historically significant over the last decade.[1]

An increasingly common alternative to PE investment in this type of fund structure is co-investment by LPs and PE funds into private firms. In 2022 total capital raised for co-investments by PE fund managers was $10.3B, up from $3.0B in 2012.[2] Co-investment is when LPs invest directly in private firms alongside PE funds where they have committed capital. This benefits LPs because they are charged little to no fees on their direct investments made in co-investments. Because co-investments are discretionary on the part of the LP, they may also mitigate liquidity costs associated with locking up capital in traditional PE funds.

Given this surrender of fees and discretion to LPs, it is not immediately obvious why GPs agree to co-invest with LP investors. Fang et al. (2015) suggest that GPs offer lower-performing prospects to LPs. This explanation, however, seems to downplay the importance of long term relationships of GPs with their investing partners. In this chapter, I show that GPs provide co-investments to LPs with high bargaining power, suggesting that co-investments are important in order to improve these long-term relationships. This LP bargaining power arises in large part from the LP's availability of funds and willingness to continue investing (Hochberg et al. (2013), Da Rin and Phalippou (2017)).[3] In particular, I use a plausibly exogenous shock to the bargaining power of a set of LPs in PE funds and find that the likelihood of co-investment for these LPs increases by 36 basis points (a 159% increase relative to the pre-shock mean).

As preliminary evidence that co-investment is used to nurture long term relationships and

[1] Throughout this chapter, I use the term "PE" to refer to buyout firms. These firms buy mature private firms or public firms that they then take private, and actively manage the firms they purchase. In 2022, total deal volume in PE was $2.4T (https://www.bain.com/insights/private-equity-outlook-global-private-equity-report-2023/#).

[2] https://pitchbook.com/news/articles/pe-co-investments-credit-market-fundraising.

[3] As one anonymous practitioner puts it, "a GP's greatest asset is their relationship with LPs."

respond to LP bargaining power, I use OLS regressions to explore the relationship between measures of LP-GP bargaining power and co-investment. I find that when the proportion of a fund's committed capital coming from a particular LP increases by 10%, that LP sees a 57% increase (relative to the unconditional mean) in the probability of co-investing with the fund. Pairwise co-investment probability also increases when the LP has worked with more PE funds in the past, is older than the PE firm, has better returns (than the fund) on past investments, and invests in more PE funds that syndicate investment with the fund. These correlations are consistent with the hypothesis that GPs are more likely to offer co-investments to LPs with higher bargaining power. In addition, I find that LPs that co-invest with PE firms are 46.0% more likely than non-co-investing LPs to invest in the future funds of the firm, consistent with the idea that offering co-investments benefits GPs in their future access to LP capital.

While instructive, these simple OLS tests do not causally link LP bargaining power to more co-investments. The threats to identifying a causal impact of LP bargaining power on co-investment activity are of two types. First, bargaining power is a function of both GP and LP characteristics, and high quality LPs tend to also match with the best GPs. This cannot be solved simply by holding fixed the fund or the LP, as is done in the OLS tests. If top LPs only work with top funds, even if they work with several, we may never observe the counterfactual where top LPs have bargaining power with low-level PE funds. This endogenous matching process might bias the OLS coefficients downward.

In addition to endogeneity from the LP-GP match, another type of threat is that empirical measures traditionally associated with LPs' bargaining power, say experience, size, track record, or liquidity, may be correlated with co-investment for reasons unrelated to the pressure they can exert on GPs. For example, suppose a GP offers co-investments to all LPs, but only the largest have the cash on hand to make them. Cash on hand would then be an unobservable confounding variable driving both co-investment and the proxy for bargaining power, independent of whether a large LP has bargaining power or not.

In this chapter, I address these endogeneity issues by using a regulatory intervention made for reasons independent of LP-GP relationships, the late-2013 implementation of the Volcker Rule under the authority of the Dodd-Frank Act. This rule was passed with the express purpose of limiting the risk exposure of large banks deemed systemically large financial institutions. Included in the Volcker Rule was a prohibition on most forms of involvement of the majority of the US banking industry with PE investment, including fund investments as LPs. One (likely unintended) consequence of this prohibition is that the funds that had banking institutions as LPs prior to December 2013 suddenly could not raise capital from these LPs for future rounds of finance. I argue that this constitutes a shock to the relative bargaining power of the remaining LPs of such bank-reliant PE funds. In my tests, I consider a fund (active at the time of the shock) to be treated if it had at least one bank as an LP before the implementation of the Volcker Rule. All other active PE funds at the time constitute the control group. I find that after the shock, co-investment activity of treated funds with their LPs increases. This is consistent with the notion that LP bargaining power drives up co-investment with PE funds.

One alternative explanation for this result is that co-investment is simply a way GPs tap into additional capital, making larger and more investments feasible for the fund. After the loss of a bank LP, a PE fund likely faces a smaller supply of committed capital that it can draw upon to make investments, even if they are able to raise a new fund. This could cause GPs in treated PE funds to rely on co-investment capital to maintain pre-shock levels of investment, threatening the bargaining power interpretation. To examine whether the increase in co-investments was due to these types of short-term financial constraints, I exploit heterogeneity in the amount of capital available to treated funds at the time of the shock. I measure this by the proportion of dry powder remaining at the time of the shock, which is simply the fraction of the fund's committed capital that has not yet been called by GPs. I expect this heterogeneity in dry powder, observed at a seemingly arbitrary period of time as far as the fund is concerned, to be quasi-random, as this measure should decrease

by degree over the life of the fund as investments are made, regardless of unobservable fund characteristics. If the short-term financial constraints channel is the driver of my main results, I would expect the funds with proportionally more dry powder to co-invest with other LPs the least, as they could more easily use main fund capital to make large investments. Instead, I find that treated funds with higher amounts of dry powder at the time of the shock are the main source of variation driving the increase after the Volcker Rule. Overall, I interpret these results to suggest that GPs offer co-investments to nurture longer term relationships with important LPs.[4]

One reason that LPs benefit from co-investment opportunities is that they pay substantially lower fees to the GPs on these investments. However, as Begenau and Siriwardane (2023) show, GPs can customize fee levels across LPs within the same fund through side letter agreements. In other words, GPs may reduce fees for LPs with high bargaining power more directly. Why, then, do GPs use co-investments to benefit their LPs, instead of reducing fees directly? I argue that from the LPs' perspective, co-investments may be preferred in some cases to explicit fee reductions, since co-investments may reduce their liquidity costs (Giommetti and Sorensen (2021), Gourier et al. (2022)). That is, co-investments provide an *option* to deploy capital into PE. This mitigates the LP's costs of being obliged by prior capital commitments to deploy capital in states of the world where liquidity bears a premium.

Consistent with this explanation, I find that LPs that sell PE fund stakes on secondary markets also tend to actively co-invest with fund partners, suggesting that LPs concerned with managing liquidity use co-investments. In addition, PE funds that see an exodus of LP capital in secondary sales, on average, offer more co-investment opportunities to the remaining LPs in the fund. This finding demonstrates that LPs may accept co-investment as a substitute for secondary sales when managing liquidity. This may be consistent with the

[4]Moreover, I show that although all treated funds had at least one bank as an LP, none of the treated funds co-invested with these banking institutions prior to the Volcker Rule. This is inconsistent with the idea that treated funds had relied on bank co-investment capital prior to the rule, and simply switched to an alternative source after. In addition, this result helps rule out the possibility that after the Volcker rule, treated PE funds started offering co-investments to all of their LPs because of financial constraints, and only the cash-rich LPs agreed to co-invest.

notion that co-investments create gains from trade, with GPs deriving more value from the deeper relationships and future financing made possible more than the fees and discretion that they lose from co-investment.

My results are relevant for policy, especially in light of recent regulations of LP-GP relationships. On August 23, 2023, the SEC published a final ruling clarifying new measures it would enforce under the authority of the Investment Advisers Act of 1940.[5] In the name of full disclosure and LP protections, the SEC intends to prohibit undisclosed agreements creating preferential treatment for some LPs in the fund, as well as ban asymmetric (non-pro rata) fee schedules on co-investment vehicles. PE market participants claim this rule will greatly curtail their ability to co-invest with any LPs in the fund.[6] This chapter presents evidence suggesting that such regulations may limit the gains from trade for both LPs and GPs from co-investment deals. Namely, GPs lose a valuable relationship management tool, and LPs lose access to the low fee benefit created by co-investment deals. Yet, it is unclear whether LPs would decrease demand for co-investments (lower fee benefit) or increase demand (to be compensated for this loss of fees through liquidity breaks).

1.1.1 Literature

This chapter contributes to several major strands in the literature. The first is the literature studying the fund structure of PE investments and possible deviations from this structure. Explanations for why private equity is organized into funds are varied. GPs at PE funds point to the value these pooled resources create, allowing for PE firms to grow large enough to source the best private firms before investment, and monitor these firms after investment, particularly in an industry exhibiting scalablity (Gompers et al. (2016), Metrick and Yasuda (2010)). Axelson et al. (2009) develop a model that shows that funds can dominate direct LP investment due to agency frictions with GP and risk concerns. My chapter provides

[5] https://www.sec.gov/news/press-release/20235-15.

[6] For an example, see the April 22, 2023 comment on the SEC proposal by Ropes & Gray, LLP: https://www.sec.gov/comments/s7-03-22/s70322.htm.

some context to this discussion by proposing that LPs may benefit from investing outside the traditional fund structure due to liquidity concerns, and GPs may also benefit through improved LP relationships. Recent studies on co-investments have largely focused on investment performance rather than LP-GP relationships. Fang et al. (2015) find evidence consistent with GPs offering LPs co-investment opportunities in lower-performing portfolio companies. Braun et al. (2020) on the other hand, find no such co-investment underperformance. Lerner et al. (2022) provide evidence that, on average, co-investments may slightly underperform, but that much of this underperformance is driven by less sophisticated LPs. In a survey of LPs, Da Rin and Phalippou (2017) report that larger LPs were more likely to indicate a co-investment history with GPs. My chapter contributes to this literature by establishing that GPs offer such co-investments to nurture their long-term relationships with LPs by using a plausibly exogenous shock to LP bargaining power. In addition, I offer empirical evidence that liquidity considerations constitute a potential explanation for the LPs' preference for co-investments.

My chapter also contributes to the discussion surrounding bargaining power. Modern theory of two-way bargaining begins with the seminal study by Nash (1950) proposing a parameterized approach to bargaining power to solve the division of excess surplus created by trade in the presence of outside options. Rubinstein (1982) builds on this by demonstrating the importance of the negotiating protocol and costs of delay to bargaining power. Theory predicts that price dispersion may occur in equilibrium in the presence of bargaining power derived from information and search cost frictions (Salop and Stiglitz (1977), Burdett and Judd (1983), Bester (1988), Duffie et al. (2005), Allen et al. (2019)). My chapter shows that such price dispersion indeed occurs in PE relationships through the mechanism of co-investments.

In the PE literature, an LP's bargaining power in the LP-GP relationship is closely tied with the LP's size, i.e., the ability to finance future iterations of the GPs' fund (Lerner and Schoar (2004), Da Rin and Phalippou (2017), Clayton (2020)). Indeed, Hochberg et al.

(2013) highlight that the LP's threat of not re-investing in the next fund has implications to the GP of losing not only that LP's investment, but those of other LPs who interpret the action as a negative signal of the GPs' skill. Holding fixed an LP's ability to commit capital, large LPs also might have more bargaining power through their ability to afford better and larger teams of lawyers, analysts, and accountants (Morris and Phalippou (2011)). LP bargaining power might also arise through skill in processing public and private information, as well as forecasting future performance of PE firms and portfolio companies (Lerner et al. (2008), Sensoy et al. (2014)). Other LP characteristics may lead to concessions from the GP, such as the number of funds the LP invests in, past performance, and years of experience in PE (Begenau and Siriwardane (2023)). PE firms on the other hand generally earn bargaining power with LPs by driving up LP demand through persistently high performance in fund returns (Kaplan and Schoar (2005), Chung et al. (2012)). Relatively few academic papers discuss specific outcomes that result from the balance of LP-GP bargaining power, as mine does. Begenau and Siriwardane (2023) show that top LPs can demand lower fees through side-letter agreements with GPs at fund inception. Gompers and Lerner (1996) use venture capital data to show that more relaxed contractual covenants in limited partnership agreements were one way that GPs responded to an increase in LP demand for VC funds. Clayton (2020) documents several other avenues for large LPs to exert influence, such as co-investment rights, advisory board seats, and favorable future investment terms. In the context of co-investments, Lerner et al. (2022) show that co-investments of top LPs are less likely to underperform. My chapter adds to these studies by highlighting a new outcome affected by LP-GP bargaining power—the incidence of co-investments—and using a quasi-random shock to establish this link.

In addition, my chapter complements the literature investigating the liquidity environment for private equity investors. Private equity in particular is a market faced with high liquidity costs induced by restrictions on capital usage over long periods of time, estimated to be even as economically significant to LPs as fees paid to GPs (Sorensen et al. (2014), Ang et al.

(2018)). Furthermore, Robinson and Sensoy (2016) show that PE cash flows (particularly distributions) are highly procyclical, exacerbating liquidity concerns. Secondary markets for PE stakes provide a partial solution for LP investors, however, they have been shown to be a costly recourse to liquidity constrained sellers (Nadauld et al. (2019), Bollen and Sensoy (2022), Gourier et al. (2022)). In the model of Giommetti and Sorensen (2021), risk-tolerant LPs face opportunity costs due to liquidity buffers the LP holds to protect against untimely capital calls from PE funds. The theory of Gourier et al. (2022) discusses the possibility that co-investment may alleviate this friction by giving LPs discretion on how and when part of their capital may be deployed, without holding excess cash or reducing PE exposure. My chapter is the first to my knowledge to provide empirical evidence supporting this proposition.

Finally, my chapter joins a literature that exploits changes in LP composition or commitments to investigate causal impacts of VC or PE capital on various outcomes. Changes to when and where LPs deploy capital have been shown to lead to more innovation (Kortum and Lerner (2000), González-Uribe (2020)), firm founder control (Ewens and Farre-Mensa (2021)) productivity and employment (Bernstein et al. (2010)) and startup risk (Nanda and Rhodes-Kropf (2013)). Chen and Ewens (2021) use the Volcker Rule as a shock to bank investments in VC funds. Differently from my chapter, their focus is on the financial constraints channel and the resulting geographic disparity in entrepreneurship, whereas my focus is on the bargaining power channel and its effects on co-investments by buyout funds.

1.2 Institutional background

1.2.1 Co-investments

Traditionally, investors in private markets invest in private equity (PE) funds, which may include various categories of funds, such as buyout and venture capital (VC). For the purposes of this chapter, PE will generally refer to funds in the buyout category. These investors

(limited partners, or LPs) form limited partnerships with fund managers known as general partners, or GPs, to invest in these funds. The legal mechanism behind this arrangement is known as a limited partnership agreement (LPA). The LPA specifies compensation structures, GP duties and obligations, and so on. The GPs are employed by a PE firm, and typically manage several overlapping funds that invest LP capital into private firms. These funds usually have a 10-year lifespan, with options for extensions at the end. Investments into private companies are made early in the fund's life, with GP time and fund resources being dedicated toward development and exit later on. Generally, a new fund will be raised 4 to 6 years after the start of the previous fund.

While LPs continue to invest in PE funds, the direct investing of LPs into private firms alongside the PE fund is becoming more commonplace. As of a 2015 survey, 69% of GPs reported actively offering co-investments with LP investors, and more than half of LPs reported that they have co-invested with GPs.[7]

There are several reasons why an LP might find it optimal to co-invest to some degree with PE funds. First, and most obvious, is that exit returns from a direct stake in a private firm are not subject to management fees or carried interest that is charged on the LP's portion of exit returns from the main fund. These fees, with some variation, are 2% on all invested capital, and 20% on all returns above a certain hurdle rate, usually 8% (Metrick and Yasuda (2010)). Second, LPs may co-invest in order to manage the illiquidity endemic in private equity investment. PE funds have a typical lifespan of 10 years or so, and after making commitments, LPs are uncertain when their money will actually be called in order for GPs to deploy in private investments. Several recent papers theoretically explore the liquidity management decisions made by LPs in PE. Giommetti and Sorensen (2021) suggest that LPs, particularly risk-tolerant LPs, are constrained to maintain a "liquidity buffer" against capital calls. Gourier et al. (2022) make similar arguments, and point out that selling LP stakes on the secondary market is suboptimal for some LPs. Co-investments, combined with

[7] https://docs.preqin.com/reports/Preqin-Special-Report-Private-Equity-Co-Investment-Outlook-November-2015.pdf.

strategic main fund commitments, could be seen as useful to LPs as a fine-tuned alternative to selling entire stakes, and reducing the need to maintain additional liquidity buffers that could be optimally deployed elsewhere in the LP's portfolio.

GPs often commit to co-invest with LPs in some form through the use of a *side letter*. While much of the LPA is boilerplate and uniform across LPs, side letters are a legal tool to allow the GP to customize some contract terms with specific LPs. Several important aspects of the LPA that can be altered through side letters are the level fees required from the LP (Begenau and Siriwardane (2023)) and commitments to provide co-investment opportunities when the fund invests. Critically, the side letter allows GPs to offer favorable terms (e.g. co-investment rights) to some LPs and not others. However, the SEC recently clarified a rule under the authority of the Investment Advisers Act of 1940 to prohibit such activity.[8] Industry experts are still unsure how this ruling will affect a PE firm's ability to offer preferential treatment to some fund LPs, including through co-investment.[9]

Co-investment can happen in several ways. One method is the institution of a parallel co-investment fund. In this case, the LP or syndicate of LPs contribute capital to an entirely separate fund, which is then given the option to front a small percentage of any investment the main fund makes. Alternatively, the LP can directly secure co-investment opportunities from the GPs through the use of side letters at fund inception. Finally, GPs have discretion to offer co-investments at any time to any fund LP, including LPs that source the investment themselves. Whatever the case, some form of consent from the GP and take up from the LP is required for a co-investment to take place.

1.2.2 The Volcker Rule

After the collapse of several major banks in the financial crisis, banks in the United States faced increased scrutiny with regard to speculative investment. In July 2010, the US congress passed the Dodd-Frank Wall Street Reform and Consumer Protection Act, commonly re-

[8] See footnote 5.

[9] https://pitchbook.com/news/articles/SEC-private-investment-fund-managers-rules.

ferred to as Dodd-Frank. The legislation had the broad purpose of introducing new regulations on financial markets in order to mitigate the broad risks associated with widespread financial collapse. One particularly important provision of Dodd-Frank was the introduction of the so-called Volcker Rule, named after former Federal Reserve Chairman Paul Volcker, a strong advocate for its implementation. The Volcker Rule introduced a number of restrictions on banks and financial institutions in order to prevent speculative trading activities that could put the broader financial system at risk.

One of the provisions of the Volcker Rule is a general prohibition on proprietary trading, or ownership interest in a category of assets called covered funds.[10] While some ambiguity on the precise definition of a covered fund existed at the time Dodd-Frank was passed (Chen and Ewens (2021)), the definition was generally thought, correctly, to encompass private equity and hedge funds at the time. The Volcker Rule was originally set to go into effect on July 21, 2012, but due to a lengthy revision process involving large banks, regulators, reform groups, and politicians, the necessary approval was delayed until December 10, 2013. The rule finally came into effect in July 2015. To summarize, this law essentially prohibited banks from investing as LPs in private equity and venture capital funds, which banks had been doing for some time. However, the law did not require the immediate exit of prohibited assets by banks upon effective date. The initial approval provided for a one-year grace period, which was subsequently renewed thrice.[11]

In its original form, the Volcker Rule, with limited exemption, also forbade financial institutions from co-investing with private equity funds. Of particular concern was that by allowing co-investment, banks could create a loophole whereby they could continue to speculatively invest in private equity without putting capital into a private equity fund.[12] This particular restriction, along with several others, including exempting venture capital from the definition of covered fund, was relaxed by action of regulators in a revision finalized

[10] https://www.govinfo.gov/content/pkg/FR-2020-07-31/pdf/2020-15525.pdf

[11] https://www.reuters.com/article/us-usa-fed-volcker-exclusive-idUSKCN10M2E4, https://www.reuters.com/article/us-usa-banks-volcker-idUSKBN18Y2YB.

[12] https://www.fdic.gov/news/speeches/2020/spjun2520d.html.

in 2020.[13]

1.3 Data

In this section, I detail construction and summary statistics on the data I will use to produce the main results of the chapter. I rely on the PitchBook database to construct the sample. The database has broad coverage of private equity fund information, along with these funds' GPs, PE firms, portfolio firm investments, and LP investors. It also contains information on the direct investments of these LPs into private companies. As detailed below, the sample is a panel dataset at the PE fund-LP-quarter level. To this basic framework, I add treatment information defined by the presence of a bank LP investor at a fund, and co-investment information between funds and their other LP investors.

1.3.1 PE funds

I begin by identifying all US-based buyout funds that were active at the time of the Volcker Rule implementation. I consider a fund to be active if I observe at least one fund investment before and after 2013, or a fund vintage between 2009 and 2013. I drop all PE funds with vintage years prior to 2009 in order to restrict attention to funds likely to be in their investment phase of life. I am left with a sample of 600 buyout funds. Summary statistics can be found on Panel A of Table 1.1. The average (median) fund was formed in 2011 (2011), and made its last investment in 2017 (2018), had 10.0 (5.0) LP investors, and had a size of \$644.7 M (\$244.1 M). The average (median) fund was the 10th (4th) fund opened by its associated PE firm, made 21.4 (14.0) investments over its lifetime, which investments had an average deal size of \$228.3 M (\$23.9 M). The average fund made 1.89 co-investments over its lifetime, with about 15% of funds reporting any co-investment activity. These buyout funds tend to cluster geographically into states known for PE activity, as 14% are located

[13] https://www.fdic.gov/news/press-releases/2020/pr20075.html.

in California, 25% in New York, and 8% in Massachusetts.

1.3.2 Limited partners and panel construction

I next isolate all non-bank entities in the database that were limited partners for the 600 funds listed above. Since I am interested in the Volcker Rule's impact on the relative bargaining power of the *remaining*, non-bank LPs, I drop all LPs from this set that are listed as banking institutions. A standard practice in LPAs is to require the GPs at the PE firm to provide some of the fund's capital, in order to align incentives. I therefore drop LPs from the data that are listed as this direct investment arm of a PE firm's GPs. I am left with 1,331 LPs. Summary statistics for these LPs are presented in Panel B of Table 1.1. The types of LPs represented include corporate pensions (23%), public pensions (18%), foundations (19%), insurance companies (13%), funds-of-funds (6%), endowments (5%), high-net-worth individuals (1%), and other types (16%). LPs appear to be more geographically dispersed than their PE funds, as 9% are located in California, 12% in New York, and 6% in Massachusetts. As of 2013, the average (median) LP began PE investment in 1997 (1998), making investments with 11.7 (3.0) buyout funds. I observe 11% of LPs make a co-investment over the sample period, with the average LP making 4.0 co-investments.

From this set of fund-LP pairs, I construct an unbalanced panel, creating a dataset at the fund-LP-year level. The pair enters the sample in the fund's vintage year. PE funds typically shift focus from fund investing to portfolio management and exit midway through the average fund's life (5-6 years). To prevent a lack of overall investment from influencing co-investment results, I therefore have each fund exit the sample 6 years after its vintage year. Main results are qualitatively and statistically robust to a shorter or longer sample exit window. I begin the sample in 2009 to exclude the peak of the financial crisis, and end the sample in 2017 to keep the window around the shock year (2013) symmetric. Results are robust to including more years in the post-period. I am left with a sample of 35,520 fund-LP-year observations.

1.3.3 Co-investment data

The outcome I am interested in studying is the allocation of co-investments to non-bank LPs in response to the shift in LP bargaining power caused by the implementation of the Volcker Rule. Therefore, my primary dependent variable for my sample is $\mathbb{1}\{Co-investment\}_{ijt}$, an indicator for PE fund i making at least one co-investment with LP j in year t. I also perform alternate tests designed to measure the change in the number of co-investments as well.

As mentioned in Section 1.2, GPs can arrange co-investments by starting a fund parallel to the main fund, backed by a subset of fund LPs. However, co-investments can also be made outside this structure, either through side letters signed at the fund's inception or negotiated directly between GPs and LPs over the life of the fund. I can observe both types of co-investments in PitchBook. First, at the fund level, I can observe whether the fund is classified as buyout or co-investment. I then connect the main buyout fund to a parallel co-investment fund by each fund's association with the buyout firm in a given year. I then consider each investment made by the parallel fund a co-investment between the associated buyout firm and the LPs which invested in the fund. I identify 63 such co-investment funds parallel to buyout funds, who made 653 co-investments from 2009-2020. A very small amount of these co-investments were associated with a fund from my sample, so I omit them from the main sample. I suspect this to be due to the fact that parallel funds are a more recent phenomenon, and at the time more co-investments were made directly (i.e. outside a parallel fund).[14] Another reason I do not include these co-investments in my main sample is that it would be difficult for a fund GP to offer new co-investments to LPs through a parallel fund in response to a sudden shock like the Volcker Rule. This is because parallel fund contract terms are negotiated at the fund's inception. I do, however, use this data in other tests related to LP bargaining power (see subsection 1.3.4).

[14] See the trend in parallel fund capital in footnote 2.
See also: https://www.ropesgray.com/en/newsroom/podcasts/2022/june/podcast-fully-invested-co-investments.

The other way I classify an investment as a co-investment is by considering the direct investment behavior of the LP. The PitchBook database flags investments on the basis of whether one of the direct investors on the deal is also a limited partner for a PE fund. If a buyout fund investment also has a flag for a direct investment from one of the fund's associated LPs, I classify it as a co-investment. I identify an additional 1,679 such direct co-investments, 350 of which are made by a sample LP.

1.3.4 Data used in other tests

In this chapter, I perform other tests not directly related to the Volcker Rule. For this data, I am not restricted to only funds active at the time of the Volcker Rule implementation in 2013. Therefore, I construct an enlarged sample of fund-LP pairs to include all observable pairs from 2009 to 2020. I refer to this sample as the expanded sample. The expanded sample consists of 16,512 fund-LP pairs consisting of 2,151 funds and 2,275 LPs.

Furthermore, in this expanded sample, I am able to use the entire sample of buyout co-investment deals for this sample, 2,332 deals. I provide summary statistics for all co-investments I identify in Table 1.1, Panel C. The average (median) co-investment had a size of $ 392.5M ($23.02M) and was made in 2012 (2013). 28% of these co-investments were made through parallel funds, and 15% (350 deals) were made by funds in the Volcker Rule test sample described above.

1.4 LP incentives and bargaining power

Before using the Volcker Rule to identify an exogenous shock to an LP's bargaining power relative to the funds in which they invest, I use OLS to examine what pair-specific factors might contribute to an LP's standing with the fund, and how that in turn predicts the co-investment activity for the pair. In this section, I do not seek to establish a causal link between the measures of LP bargaining power relative to their PE funds, but rather

investigate whether plausible measures of this bargaining power correlate with LP-fund co-investment.

The first step in this exercise is to consider what variables may stand in for LP bargaining power. The most obvious candidate may be how large the LP is, or how much capital the LP provides to the fund. I construct two variables capturing this idea. The variable $SizeRatio_{ij}$ equals the AUM of LP j divided by the size of fund i. $FracCommitted_{ij}$ calculates the proportion of observable committed capital in fund i that comes from LP j.

Next, as is standard in many bargaining models, the LP's bargaining power relative to a GP can be viewed as an increasing function of her outside options. Better LP outside options then increase the share of surplus captured by the LP through investment. I construct two measures of the LP's outside options. First, $OtherFunds_{ij}$ counts the number of other PE funds that LP j has invested in at the start of fund i. These funds may be active or concluded. I hold that this variable can reasonably represent the quantity of alternatives to fund i that LP j has in terms of PE investing. The next variable related to LP outside options, however, is more qualitative than quantitative. $ReturnRatio_{ij}$ represents the average return on past funds that LP j has achieved divided by the eventual return that fund i will earn. Returns are measured by a fund's most recently observed total value to paid-in capital (TVPI). While $OtherFunds_{ij}$ will be correlated with the number of outside options the LP has to the fund, $ReturnRatio_{ij}$ can be seen as capturing the quality of those alternative PE funds for the LP based on past performance.

This bargaining equilibrium should also be a function of the outside options of the fund GPs. To get at this, I construct a variable related to the outcome a GP might expect in the absence of support from a prior LP investor. $NetworkInvestments_{ij}$ counts the number of syndicate PE funds of fund i to whom LP j has provided capital. I consider another PE fund to be a syndicate partner of fund i if they invest in the same deal at least once over the life of fund i. I believe this to be a measure of the GPs' outside options because it may represent the degree to which the LP may harm the fund GPs' reputation in the eyes of

network partners if the LP foregoes reinvestment with the GP.

Finally, I would expect the LP's experience in PE investing to be relevant to their bargaining position with fund GPs. While primarily included to measure outside options, $OtherFunds_{ij}$ will certainly be representative of LP experience. I also construct a variable that shows how much older LP j is than the firm managing the associated fund i, $AgeDiff_{it}$.

The parameters I will estimate are coefficients from the following regression:

$$y_{ij} = \alpha + \beta \times LPBargainingPower_{ij} + \phi_i + \epsilon_{ij}$$

Where i and j index PE funds and Limited Partner entities, respectively. The outcome variable y_{ij} is either an indicator variable for fund i making at least one co-investment with LP j, or the natural log of 1 plus the number of co-investments between i and j. $LPBargainingPower_{ij}$ are variables described above which may reasonably be expected to correlate with the bargaining power LP j enjoys relative to fund i. The parameter of interest is β, which represents the degree to which the measure of bargaining power is correlated with fund-LP co-investment. I also estimate these coefficients jointly for all measures to get a sense for how these measures correlate with one another, and which are the most predictive of co-investment.

Results are presented in Table 1.2. The coefficients on all variables in their separate regressions in columns (1) through (6) are positive and statistically significant, except for $SizeRatio_{ij}$, which appears to have a null effect. The interpretations are as follows. A 10% increase in the fraction of committed capital coming from the LP is associated with a 0.5% increase in probability of co-investment, a 57.0% increase relative to the sample mean. Increasing the number of PE funds the LP invests in before the fund by one is associated with a 0.9 basis point increase in probability of co-investment, a 1.0% increase relative to

the sample mean. A 10% increase in the return ratio is associated with a 0.2% increase in probability of co-investment, a 21.9% increase relative to the sample mean. Increasing the number of the fund's network partners the LP invests by one is associated with a 1.0% increase in probability of co-investment, which increase is more than double the sample mean. Finally, increasing the age difference between the LP and the fund's PE firm is associated with a 1.6 basis point increase in co-investment probability, which represents a 1.7% increase relative to the sample mean. These results are qualitatively comparable to the joint regression of all variables in column (7), except for the return ratio, which becomes negative and statistically insignificant.

I interpret these results to indicate that, in equilibrium, LPs that have more experience, skill, outside options relative to the GP tend to co-invest more with funds. However, as mentioned before, this is not the demonstration of a causal effect. These variables were not randomly assigned, and both they and the co-investment outcome could simultaneously be a result of optimization decisions of funds and LPs, which are, in turn, a function of unobservable variables. Therefore, a more rigorous approach is appropriate here.

1.4.1 Do co-investments predict future LP investments?

Before turning to the well-identified test using the Volcker Rule, I find it prudent to demonstrate the basic fact that more LP co-investment is correlated with more LP investment in future funds. Results of simple regressions of LP-GP collaborations on LP-GP co-investment activity are presented in Table 1.3. I use LP-fund data from the expanded sample. The dependent variables are indicators for the LP investing in the GPs' next fund, investing in any fund the GP opens within the next 5 years, and investing in any of the GPs' future funds. The independent variable is an indicator taking on the value of 1 if the LP makes a co-investment alongside the fund. I also include fund and LP fixed effects. In each case, LP-fund co-investment is a strong predictor of LP investment in the GPs' future funds. Taking the coefficient from the first column, I find that LPs that co-invest with their PE funds are

16% more likely to participate in the next iteration of the fund. This constitutes a 90.7% increase relative to the sample mean of 17.1%.

This test is likely subject to a selection bias. It is plausible that better LPs demand more co-investments, and simultaneously are large and stable enough to continue PE investments. Also, more skilled GPs may be better able to afford the lower fees associated with co-investments and simultaneously more attractive to LPs for future investments. Either of these simple explanations could drive the results seen in Table 1.3.

1.5 Empirical design

In this section, I describe empirical challenges I face in testing my hypothesis, and potential solutions for these challenges.

In an ideal setting, I would begin with a set of identical LPs who are invested with separate yet identical GPs. I then would randomly assign varying levels of bargaining power to these LPs. This could possibly be implemented through an increase in the LP's outside options without altering other LP characteristics, like size. Assuming the GP faces no other financial constraints in making fund investments, and under the hypothesis that co-investments are a tool GPs actually use to attract LPs, I would expect the GPs of the high-bargaining-power LPs to offer more co-investments than their peer GPs whose LPs experienced no such shock. Under these circumstances, the GPs' choice of co-investment strategy is not correlated with unobservable factors that are unrelated to the LP's relative bargaining power, such as LP size, short term financial constraints, GP skill, GP performance, or macroeconomic factors.

In practice, however, it is not feasible to "assign" bargaining power to a PE investor. A naïve approach would be to regress co-investment opportunities on some measure of LP reputation (e.g. size, cumulative PE returns, experience). However, it is entirely plausible that these measures correlate with propensity to co-invest for reasons other than GP seeking to improve LP relationships. For example, a skilled LP may source these investments them-

selves, and want to invest directly. Otherwise, a larger LP may have more liquidity to spare on co-investing than a small LP. Perhaps large LPs have larger risk tolerance, and want more on-demand exposure to an asset-class known for fat-tailed return distribution. Moreover, these endogenous measures of LP bargaining power are likely noisy, measured with error, and difficult to compare cross-sectionally.

Therefore, to directly determine whether LP bargaining power incentivizes the GP to share surplus through co-investment, I would need variation in relative LP bargaining power uncorrelated with these other channels. I solve this problem by exploiting variation in an LP's importance to the GP that arose as a result of the implementation of legislation intended to address the risk load of non-PE financial institutions, rather than from these endogenous reasons. In particular, I use the Volcker Rule as an exogenous shock to relative bargaining power in a differences-in-differences (DID) regression framework. As outlined in section 1.2, after December 2013, banks were no longer permitted to invest as limited partners in PE funds under federal law. Furthermore, this law was not passed with the intent to drastically alter decision-making at PE funds. Therefore, it is plausible that its passage was independent of the endogenous matching process between certain types of LPs and funds likely to offer co-investments to investors, as well as within-LP changes in bargaining power related to skill or increased liquidity.

Yet, it is intuitive to suppose that this change indeed increased the bargaining power of all remaining non-bank LPs with their funds that traditionally relied, at least in part, on bank capital to raise new funds. I believe this framework provides a strong setting to test whether GPs indeed view co-investments as a way to retain important LPs with high bargaining power for investment in future funds. LPs see greater financial flexibility as well as lower fees paid on co-investment. With this in mind, and under the hypothesis that co-investments are used to foster financial relationships, I would expect to see an increase in co-investment activity between bank-reliant GPs and non-bank LPs after the Volcker Rule implementation. Alternatively, if co-investments are not seen by GPs as a first-order way to

attract future capital by counterbalancing LP bargaining power, I would not expect a rise in co-investments post-Volcker Rule. Thus, I consider a fund in my sample to be treated by the Volcker Rule if at least one LP investor in the fund was a banking institution. Many of the funds in my sample (4.1%) may be categorized as treated under this definition.

Figure 1.1 provides evidence supporting the relevance of the Volcker Rule shock to PE capital supply. Average annual volume of PE funding from banks decreased from $2.1B in the years prior to the Volcker rule to an average of $0.8B per year in the subsequent years.

1.5.1 Differences-in-differences design

I use a differences-in-differences regression framework to demonstrate the impact of the Volcker Rule implementation on the co-investment activity of treated funds:

$$y_{ijt} = \alpha + \beta \; Treated_i \times Post_t + \eta \; Treated_i + \pi \; Post_t + \epsilon_{it} \tag{1.1}$$

Data is at the PE fund i, LP j, year t level. The dependent variable y_{ijt} takes on the value $\mathbb{1}\{Co\text{-}investment\}_{ijt}$, an indicator variable for LP j co-investing with a PE fund i at least once in year t. $Treated_i$ is an indicator for fund i having at least one bank LP prior to December 2013. The control group are all fund-LP pairs ij such that $Treated_i = 0$. $Post_t$ is an indicator for year t being greater than 2013. A $\beta > 0$ would indicate that LP exposure to the Volcker Rule by investment in bank-reliant PE funds is associated with an increase in co-investment activity post-2013. This result would be consistent with the notion that GPs co-invest with LPs with high relative bargaining power in order to secure important financial relationships.

It could be the case that the likelihood of having a bank investor pre-2013 is correlated with some fund- or LP-level characteristic that predicts co-investment. For example, if a particular fund has more senior partners, it may be that it simultaneously has more network

connections to large banks (classified as treatment), as well as more human capital to devote to seeking out co-investment opportunities (more co-investment). Furthermore, as discussed in section 1.1, there is a significant time trend in the data. If somehow $Treated_i$ correlates with participation in this trend, it would confound results. To account for this, I include alternate specifications which include fund, LP, and year fixed effects:

$$y_{ijt} = \alpha + \beta \; Treated_i \times Post_t + \phi_i + \lambda_j + \gamma_t + \epsilon_{it} \tag{1.2}$$

While the inclusion of fixed effects should control for time-invariant fund- or LP-characteristics as well as time trends, it is possible that within fund changes unrelated to the Volcker Rule occurred over the sample period. If these changes altered co-investment incentives differently for treated funds than for control funds, it could explain a positive β_1 coefficient regardless of the Volcker Rule's bargaining power effect. This type of asymmetric firm- and time-level variation across treated groups is of paramount concern in a discussion of parallel trends in a DID framework. The parallel trends assumption in this case is that treated and control pairs would have had parallel trajectories in co-investment outcomes over time in the absence of the Volcker Rule shock. While it is impossible to test this counterfactual, I present evidence consistent with parallel trends in the pre-period, which would suggest a similar pattern would have ensued had no regulatory intervention impacted the treated group. To this end, I specify a dynamic DID test:

$$\begin{aligned} y_{ijt} = \alpha + \sum_{\tau=\underline{\tau},\tau\neq 2013}^{\overline{\tau}} \beta_\tau \; Treated_i \times \mathbb{1}\{t=\tau\} \\ + \eta \; Treated_i + \phi_i + \lambda_j + \gamma_t + \epsilon_{it} \end{aligned} \tag{1.3}$$

Where τ is an index representing each year in the sample period, omitting 2013 as the basis of comparison. The sample beginning and end, respectively, are denoted by $\underline{\tau} \equiv 2009$ and $\overline{\tau} \equiv 2020$. $\mathbb{1}\{t=\tau\}$ is an indicator variable that takes on the value of 1 for an observation it

whenever $t = \tau$. γ_t are time fixed effects. For robustness, I also include other specifications with time and firm fixed effects. All other variables are defined the same as in equation 1.1. Again, coefficients $\beta_\tau > 0 \ \forall \tau \geq 2014$, would be consistent with the hypothesis that LPs exposed to the Volcker Rule through their funds (i.e. have a shock to their relative bargaining position with their GPs) co-invest more in the post-period. β_τ coefficients which are statistically not different from 0 for τ in interval 2009-2013 would be consistent with the notion that treated and control funds had similar co-investment time trends before 2014. Extrapolating a result like this to the unrealized counterfactual state where there was no regulatory intervention would suggest that a steeper time trend post-2014 would not be the cause of positive post-treated coefficients.

1.6 Results

In this section, I take my research question to the data and test whether an exogenous shift in relative bargaining power between GP and LP in a PE fund is associated with an increased level of co-investing.

1.6.1 Main results

Results from the baseline regressions (equations 1.1 and 1.2) are reported in Table 1.4. I find that treated firms see an increase of about 0.36% per year in probability of co-investment in the baseline specification, significant at the 95% level in most specifications. This represents a 159.0% increase relative to the unconditional mean in annual co-investment probability of roughly 0.23% in the pre-period.

Figure 1.2 plots the coefficients β_τ over the sample period, as well as 95% confidence intervals. Coefficients begin to increase after the shock in 2013, peaking in 2015 and remaining high, though not statistically significant in 2017. Co-investment activity seems to spike in 2015 rather than the year directly following the implementation. It is possible that this is

due to the staged implementation of the Volcker Rule, as many large banks were extended grace periods by the Fed to exit their investments. Also, on average, PE fundraising takes place only once every 4–6 years. It is possible that some GPs were able to delay post-Volcker LP renegotiation involving co-investments until their new fund was raised several years later, rather than immediately in 2014.

This analysis is also useful for the purpose of investigating the parallel trends assumption in the pre-period. None of the coefficients for $\tau < 2014$ are significant at the 95% level. This would suggest that after controlling for the level shift in co-investment activity predicted by LP treatment, there is no significant difference in pre-trends between the two groups. There is a slight uptick in co-investment for the exposed LPs in the wake of the passage of Dodd-Frank (2011). This may be an artifact of the delay between passage and implementation of the Volcker Rule, however, the figure clearly shows the bulk of the effect taking place after the rule came into effect.

1.6.2 LP-level analysis

Throughout my main results, I consider a fund (and associated LPs) to be treated when I observe a bank as a limited partner prior to the Volcker Rule. However, some GPs who did not partner with bank LPs before the Volcker Rule may have been affected by its implementation indirectly by losing *prospective* bank LPs. That is, GPs who did not *yet* have a bank investor, but may have been courting bank investors when the Volcker Rule was implemented may have experienced a drop in bargaining power relative to their prior LPs, but would be classified as control GPs under prior specifications. To address this concern, I follow the procedure of Chen and Ewens (2021), and construct treatment as a function of the geographic distribution of an LP's fund investments. The logic here is that the dependency of a GP on bank capital prior to the Volcker Rule could be captured by how reliant local GPs are on bank finance. I detail the sample construction in section 1.6. Data is constructed at the LP level, where the variable $Treatment_j$ measures LP j's geographic exposure to decreases in

their GPs' bargaining power. I perform a static and dynamic DID test in the same spirit as the main regression specifications:

$$y_{jt} = \alpha + \beta\ Treatment_j \times Post_t + \eta\ Treatment_{i=j} + \pi\ Post_t + \epsilon_{jt} \tag{1.4}$$

$$\begin{aligned} y_{jt} = \alpha + \sum_{\tau=\underline{\tau},\tau\neq 2013}^{\bar{\tau}} \beta_\tau\ Treatment_j \times \mathbb{1}\{t=\tau\} \\ + \eta\ Treatment_j + \gamma_t + \epsilon_{jt} \end{aligned} \tag{1.5}$$

Results from equation 1.4 and 1.5 may be found in Table A.1 and Figure A.1, respectively. Results are qualitatively similar to those of the main test. I interpret these findings to suggest that indirect treatment due to prospective bank-LPs does not bias results significantly.

1.7 Mechanisms

1.7.1 Short-term financial constraints

An alternative explanation consistent with the main results is that GPs use co-investments to enable the fund to make larger investments when main fund capital is limited. If the GP comes across a large, NPV-positive investment at a time in the life of the fund when the remaining committed capital (also called dry powder) is low, she may not be able to invest without the use of an LP co-investment. Similarly, a risk-averse GP may decide the initial outlay for the investment is too large for her preference to diversify the fund across many investments. This is an example of a GP incentive for co-investment that is not related to bargaining power. This alternative is consistent with the results as they stand because the Volcker Rule can be seen as an instrument that affects both the relative bargaining power of the LP-GP pair, and the supply of private capital for treated funds. For example, suppose PE firm A enjoyed a close relationship with J.P. Morgan's private equity investment arm

prior to the Volcker Rule. It could be the case that, after the Volcker Rule's implementation, all other LPs of PE firm A enjoy a position of strength with firm A and use that to demand co-investment opportunities from firm A. However, it could also be the case that firm A depended on capital from J.P. Morgan to undertake large LBOs. In this scenario, firm A might freely request co-investment capital from its non-bank LPs after 2013 to continue making these large investments made previously with commitments from J.P. Morgan.

Should this alternative entirely explain the main results of the chapter, one might expect this effect to be stronger for LPs associated with funds with more capital remaining in the fund. If short term financial constraints truly drive the main results, funds which have little or no dry powder remaining would be limited to one of two options: reduce or stop fund investment, or co-invest with a new partner. Conversely, funds still flush with yet-untapped capital commitments still have the option to make investments using money from the main fund. Thus, this hypothesis is most consistent with the treatment effect being driven by treated funds with a relatively low proportion of remaining dry powder.

I therefore test whether low dry powder treated firms actually drive the main results, beginning by constructing a measure for each fund's dry powder at the time of the shock. I first calculate the fraction of fund dry powder remaining at the time of the Volcker Rule. I calculate this as the aggregated size of deals invested in by the fund before 2014 divided by the sum of all deal sizes before and after 2014. For simplicity, the variable I use in the following regressions, $DryPowder_i$ is an indicator for the fraction of remaining dry powder at fund i being above median for all funds in the sample. The sample size shrinks slightly to 27,234 as some funds had missing investment data necessary to calculate $DryPowder_i$.

I use the following triple-difference design to ascertain whether the baseline effect is driven entirely by this short term financing constraint, measured in $DryPowder_i$:

$$
\begin{aligned}
y_{ijt} = \alpha &+ \beta_1 \; Treated_i \times DryPowder_i \times Post_t \\
&+ \beta_2 \; Treated_i \times DryPowder_i \\
&+ \beta_3 \; Treated_i \times Post_t \\
&+ \beta_4 \; DryPowder_i \times Post_t \\
&+ \beta_5 \; Treated_i + \beta_6 \; DryPowder_i + \beta_7 \; Post_t + \lambda_j + \epsilon_{it} \qquad (1.6)
\end{aligned}
$$

A $\beta_1 < 0$ would suggest that I could not rule out the possibility that the main results are entirely driven by PE funds using co-investments after the Volcker Rule to continue making investments after experiencing a shock to available capital in the short-term. On the other hand, an insignificant or positive β_1 would instead suggest that even PE funds with a lot of capital to fall back on still offer co-investments to their remaining LPs following the loss of an important LP for future funds. I also estimate specifications that include LP and year fixed effects.

Results for this test are presented in Table 1.5. I estimate $\beta_1 > 0$, and fail to reject the null that $\beta_1 = 0$, despite a persistently positive β_3. Therefore, I fail to find evidence to support the conjecture that the entire effect in the main results is due to short term financial constraints. A careful inspection of the estimates of β_3 shows a lack of statistical significance. However, I am not overly concerned about this, as much of the increase in co-investments occurs for funds with above median dry powder ($\beta_1 > 0$). Despite both coefficients being statistically insignificant independently, when I drop the triple interaction term from the regression, the coefficient on $Treated \times Post$ once again is positive and significant.

A special case of the short term financing explanation is that the bank LPs were an important source of *co-investment capital* to make these large investments. First, I do not believe this to be consistent with the results from the dry-powder test presented above. I would expect, under this alternative, that GPs with fewer capital commitments to draw

upon would be most affected, which is not supported by my results. I also note that in the data, I do not observe co-investment between the treated funds and any of their bank LPs prior to the Volcker Rule. I find it difficult to reconcile an increase in co-investments being due to this short-term financing friction left by the loss of bank LPs if bank LPs did not seem to be important co-investing partners of the treated funds in the pre-period.

Another related, but slightly different, interpretation of the Volcker Rule relates to co-investment take up and the cash availability of the non-bank LPs. Under this explanation, the GP has the flexibility to offer co-investments only to the LPs with the highest bargaining power before the shock to the GP's need for capital. After the shock, the GP lowers her standard, and, desperate for capital, invites all LPs to co-invest out of necessity to continue making investments. However, only those LPs with large cash-reserves are able to accommodate the GP. I would, of course, expect cash reserves to correlate with bargaining power. In this case, it would be difficult to disentangle whether these LPs co-invested more due to the GPs concern for raising future funds or their availability of cash. I argue, however, that it is important to consider why the GP invites all LPs to co-invest. If the GP invites all LPs to co-invest due to concern for raising future funds, I would consider this consistent with all parties' increasing in bargaining power, and only some LPs having the cash to accept. On the other hand, if the GP is more concerned about making co-investments in the current fund regardless of how future funds are raised, then I would suggest this would be most pronounced for GPs with low dry powder remaining. However, the evidence presented in this section seems inconsistent with this case.

Moreover, given the low fees and liquidity of the parallel co-investment fund, I would expect all LPs (especially cash constrained LPs) to participate if invited. However, this is not what I observe. In the expanded data, only 70% of LP investors in the average fund associated with a parallel co-investment fund also sponsor the co-investment fund, with a standard deviation of 50%. Therefore, I would conjecture that the evidence does not support all LPs being invited to co-invest.

Overall, this result may appear counter-intuitive: the GPs with the most available capital seem to be at least as hesitant to draw down on dry powder as those with less available capital. Perhaps this surprising result is due to the fact that GPs use co-investments to spare important LPs from capital calls in tight credit markets. This may prove to be an interesting question to more completely explore in future research.

1.7.2 LP incentives

As described in section 1.2, Limited Partners must agree for a co-investment to be realized. The most obvious reason an LP might agree to such a deal is that, assuming similar gross returns for co-investments, returns will be better for the LP due to the lower fees on the co-investment. In the Preqin survey mentioned in section 1.1, 61% of co-investing LPs list lower fees as a perceived advantage of co-investment. To ascertain the attractiveness of the co-investments from a return perspective, I take averages of value multiples for firms that received co-investment capital. I calculate these multiples by dividing the value of the firm at PE exit by the total PE dollars received by the firm. I compare this average to the average of the universe of PE-backed firms in the PitchBook database with non-missing PE investment and exit sizes. I also calculate the percentage of these groups that exit via IPO, acquisition, and write-offs. Results are reported in Table **?**. I find that the co-investment group of firms has higher multiples, is more likely to have a successful PE exit through IPO or acquisition, and less likely to go out of business. I do note that the difference in the means between the two groups is not statistically significant. Nevertheless, this analysis suggests that, even under the most strict interpretation, multiples are no worse for the co-investment group. This dovetails nicely with the analysis of Braun et al. (2020) who find that portfolios of co-investments outperform main-fund investments.

Another reason LPs choose to co-invest is for reasons of control (listed by 34% of LPs as a reason to co-invest). Specifically, co-investing gives LPs the *option* to deploy capital in a given period rather than an obligation mandated by the LPA. Capital commitments in the

main fund do not expire for 10–12 years, and the timing and magnitude of capital calls from GPs is uncertain. Co-investments can be seen as a potential solution to this problem. In essence, the prospect of co-investing with the fund gives LPs the discretion to deploy capital in the same asset class *after* they observe the state, including macro factors, idiosyncratic portfolio performance, and shifting preferences.

One option that has been traditionally used to offset this liquidity risk is the sale of LP stakes on secondary markets. However, this course of action is fraught with costs of its own. In a secondary sale, the LP has no option to choose specific fund portfolio firms from which to exit. All shares sold in these markets include exposure to each of the fund's portfolio firms (Gourier et al. (2022)). Nadauld et al. (2019) find that the average LP seller in these markets takes a 5% haircut when exiting their position. However, LPs that alternatively start with a smaller position in the main fund may have the option to fine tune their exposure to private equity, so to speak, through co-investments without paying a premium.

To investigate whether co-investments are valuable as a liquidity management tool, I explore whether LPs that show a propensity to sell stakes in secondary markets also tend to co-invest with their fund GPs. Using my expanded sample of 2,275 LPs, I construct a dataset at the LP level. I regress a dummy for LP co-investment over the period 2009-2020, and regress it separately on two measures of LP secondary sales (the number of LP secondary sales, and a dummy for any secondary sales over the same period). In case an LP's size drives both co-investment and willingness to engage in the secondary markets, I include the LP's last known assets under management (LPAUM) as a control variable. I also include the LP vintage year fixed effects. The regression equation is given as:

$$\mathbb{1}\{Co\text{-}investment_j\} = \alpha + \beta_1\ SecondarySales_j + \beta_2\ LPAUM_j + \phi_j + \epsilon_j \tag{1.7}$$

where j indexes LPs and ϕ_j represents a vintage year fixed effect.

Results are presented in Table 1.7. I find in each case that LPs that have sold shares

over their lifetime also tend to co-invest more with PE funds than their peers. I perform a similar test at the fund level (using the 2,151 funds from the expanded sample), testing whether funds that saw LPs sell shares on the secondary market also happened to co-invest more with LPs. The equation is given as:

$$\mathbb{1}\{Co\text{-}investment_i\} = \alpha + \beta_1 \ SecondarySales_i + \beta_2 \ AUM_i + \phi_i + \epsilon_i \tag{1.8}$$

where i indexes PE funds, AUM is the last known AUM of the PE firm managing the fund, and ϕ_i is a fund vintage year fixed effect. Results are presented in Table 1.8. Once again, I find that funds whose LP's sell stakes tend to co-invest more with any given fund LP compared to other PE funds. Using this cross-sectional analysis, it is possible that secondary sales and co-investment take place years and years apart, making any connection between the two likely spurious. To address this concern, I expand this data to the LP-year level in Table 1.9. Using this dataset allows me to identify how much of this co-investment activity happens after and around the LP's secondary sales. I find that co-investments are more likely in years following a secondary sale, and that co-investments are predicted by higher cumulative LP secondary sales to date. Another reason this refinement is useful, as it allows me to take advantage of the rich variation of these liquidity measures in the time series. I interpret these two results to suggest that co-investments are seen as complements to secondary stake sales to LPs looking for tools to manage the illiquidity of PE investment.

1.8 Conclusion

Despite the increased incidence and apparent importance of co-investment to participants in private equity markets, the literature has not, of yet, satisfactorily determined why such cooperation between GPs and LPs is acceptable to both parties. LPs certainly benefit from additional exposure to PE investments with lower fees and greater flexibility. At face value, it would appear that this increase in LP wealth and discretion comes directly at the

expense of fund GPs. In this chapter, I present evidence consistent with the hypothesis that GPs accept these costs in order to improve and maintain relationships with LP investors whose participation in future endeavors is a critical component of the GPs' future value. I identify natural, pseudo-random variation in regulatory practice that reduces a set of GPs' bargaining power, and find evidence that the GPs' LP investors demand more co-investments in exchange for their continued financial collaboration. I rule out an alternative explanation related to short-term financial constraints. I also document why co-investments, in particular, are useful to GPs in ways that direct price discrimination through fees is not, namely, transferring investment discretion to LPs. The possibility that co-investments create gains to trade for both GPs and LPs is important, considering the recent disclosure and fair-treatment requirements ruled by the SEC, which may impede co-investing activities.

There are many open questions surrounding this relatively new practice of co-investment. For example, it is unclear what impact the SEC ruling will have on the GPs' ability to give some, but not all, LPs preferential treatment (see footnote 5). Will disclosure requirements force GPs to choose between the interests of different factions of LP investors in their funds? Will GPs be required by law to charge proportionally equal fees to all LPs, including on co-investment vehicles? Will LPs with bargaining power demand more co-investments to compensate for this rule's impact on fees?

Another interesting question is why co-investments have become so popular only recently. What industry or macroeconomic trends might explain the rise of these alternative investments within the last decade or so? Answering questions such as these may help us better understand private markets and anticipate the future of the industry as a whole.

Table 1.1: **Summary statistics**

This table presents summary statistics for my main sample, which covers 600 buyout funds and 1,331 LPs over the sample period 2009-2017. Variation in observations is due to missing data for some variables. I present means, standard deviations, minimum and maximum variable values. I also present variable medians, as well as 25th and 75th percentiles. Variable definitions can be found in the appendix. Panel A presents descriptive statistics for sample funds. Panel B presents descriptive statistics for limited partners in the sample. Panel C presents descriptive statistics for the sample of co-investments, but over the expanded sample, 2009-2020.

	N	mean	std	median
Panel A: Funds, N = 600				
Vintage	600	2010.71	1.16	2011.00
Closing Year	483	2017.14	3.11	2018.00
Size ($M)	576	644.66	1,376.51	244.05
Fund Number	600	9.84	14.52	4.00
$\mathbb{1}\{CA\}$	600	0.14	0.35	0.00
$\mathbb{1}\{NY\}$	600	0.25	0.43	0.00
$\mathbb{1}\{MA\}$	600	0.08	0.27	0.00
# Investments	477	21.42	28.00	14.00
Avg Deal Size ($M)	428	228.32	678.70	56.98
# LPs	600	9.96	15.64	5.00
# Co-investments	600	1.89	11.17	0.00
$\mathbb{1}\{$Co-investment$\}$	600	0.15	0.36	0.00
Treated	600	0.04	0.18	0.00

Table 1.1: **Summary statistics, continued**

This table presents summary statistics for my main sample, which covers 600 buyout funds and 1,331 LPs over the sample period 2009-2017. Variation in observations is due to missing data for some variables. I present means, standard deviations, minimum and maximum variable values. I also present variable medians, as well as 25th and 75th percentiles. Variable definitions can be found in the appendix. Panel A presents descriptive statistics for sample funds. Panel B presents descriptive statistics for limited partners in the sample. Panel C presents descriptive statistics for the sample of co-investments, but over the expanded sample, 2009-2020.

	N	mean	std	median
Panel B: LPs, N = 1,331				
Year Founded	1331.00	1997.43	8.20	1998.00
# PE Funds	1331.00	11.71	20.96	3.00
# Funds	1331.00	68.75	107.41	27.00
Fund Size ($M)	1331.00	1895.38	1364.89	1636.36
𝟙{Corporate Pension}	1331.00	0.23	0.42	0.00
𝟙{Endowment}	1331.00	0.05	0.21	0.00
𝟙{Foundation}	1331.00	0.19	0.39	0.00
𝟙{Fund of Funds}	1331.00	0.06	0.24	0.00
𝟙{High-net-worth investor}	1331.00	0.01	0.08	0.00
𝟙{Insurance Company}	1331.00	0.13	0.34	0.00
𝟙{Other Type}	1331.00	0.16	0.37	0.00
𝟙{Public Pension Fund}	1331.00	0.18	0.38	0.00
𝟙{CA}	1324.00	0.09	0.29	0.00
𝟙{NY}	1324.00	0.12	0.32	0.00
𝟙{MA}	1324.00	0.06	0.23	0.00
# Co-investments	1331.00	4.02	35.68	0.00
𝟙{Co-investment}	1331.00	0.11	0.32	0.00
% Treated Funds	1331.00	0.12	0.27	0.00
𝟙{Treated}	1331.00	0.28	0.45	0.00
Panel C: Co-investments, N = 2,332				
Size ($M)	1258.00	392.49	2095.85	23.00
Year	1674.00	2012.14	6.10	2013.00
𝟙{Parallel Fund}	2332.00	0.28	0.45	0.00
𝟙{In Volcker Sample}	2332.00	0.15	0.36	0.00

Table 1.2: **LP bargaining power: indicator for co-investment**

This table reports OLS coefficients for regressions of fund-LP co-investments on various measures of LP bargaining power relative to the fund. Data is at the fund-LP level for each fund-LP pair over the sample period 2009-2020. The dependent variable is an indicator taking on the value of 1 if the fund co-invested with the LP, and 0 otherwise. Independent variables are defined in the appendix. Fund fixed effects are included in each regression. Standard errors are clustered at the fund level. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	$\mathbb{1}\{\textbf{Co-investment}\}\times 100$						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SizeRatio	0.000						-0.000
	(0.000)						(0.000)
FracCommitted		5.126***					2.601*
		(1.608)					(1.401)
OtherFunds			0.009***				0.008***
			(0.002)				(0.003)
ReturnRatio				2.005**			-0.712
				(0.945)			(0.638)
NetworkInvestments					1.019***		0.475**
					(0.195)		(0.212)
AgeDiff						0.016***	0.014***
						(0.003)	(0.004)
Fund Fixed Effects	YES	YES	YES	YES	YES	YES	YES
R-squared	0.207	0.218	0.187	0.185	0.211	0.197	0.198
No. observations	16,024	10,065	16,793	14,798	16,811	14,798	8,091
Dep. Var. Mean	0.961	0.775	0.846	0.933	0.916	0.973	0.816

Table 1.3: **Future fundraising**

This table presents results of OLS regressions of future LP-fund collaboration on LP-fund co-investment patterns. The data is at the fund-LP level, with one observation for each LP that is an investor in each fund from the expanded sample. I omit funds that were the last observable fund managed by the PE firm as of 2020. The sample period covers 2009-2020. The dependent variables are defined in the appendix. The independent variable is an indicator for a co-investment between the LP and the fund. All three regressions include fixed effects at the fund and LP levels. Standard errors are clustered at the PE fund level. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	Dependent Variable:		
	$\mathbb{1}\{$Next Fund$\}$	$\mathbb{1}\{$Any Fund $<$ 5yr$\}$	$\mathbb{1}\{$Any Future Fund$\}$
$\mathbb{1}\{$Co-investment$\}$	0.16***	0.46***	0.46***
	(0.04)	(0.05)	(0.05)
Fund Fixed Effects	YES	YES	YES
LP Fixed Effects	YES	YES	YES
R-squared	0.001	0.004	0.004
No. observations	13,004	13,004	13,004

Table 1.4: **Main results: co-investment outcomes for LP-fund pairs exposed to Volcker Rule change**

This table displays results from the DID test related to the change in co-investment activity for GPs exposed to the Volcker rule. Data is at the PE fund-LP-year level over the years 2009-2017. The dependent variable is $\mathbb{1}\{\text{Co-investment}\}$, which is an indicator variable for a co-investment deal between the fund and the LP in a given year. Treated is an indicator for the PE fund having at least one bank LP pre-Volcker Rule. Post is an indicator for a given year being 2014 or later. The strictest specification includes fund, LP, and year-level fixed effects. Standard errors (reported in parentheses) are clustered at the fund level. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	$\mathbb{1}$**{Co-investment}**				
	(1)	(2)	(3)	(4)	(5)
Treated × Post	0.004**	0.003**	0.004**	0.004**	0.003*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Treated	-0.001		-0.001	-0.001*	
	(0.001)		(0.001)	(0.001)	
Post	-0.000	0.000	0.000		
	(0.001)	(0.000)	(0.001)		
Fund Fixed Effects	NO	YES	NO	NO	YES
LP Fixed Effects	NO	NO	YES	NO	YES
Year Fixed Effects	NO	NO	NO	YES	YES
R-squared	0.000	0.039	0.140	0.000	0.175
No. observations	35,520	35,520	35,520	35,520	35,520

Table 1.5: **Dry powder**

This table presents results of tests investigating whether PE funds exposed to the Volcker rule are more likely to co-invest post 2013 if they had limited capital remaining at the time of the shock. Data is at the PE fund-LP-year level. The sample period is 2009-2017. The dependent variable is $\mathbb{1}$**{Co-investment}**, which is an indicator variable for a co-investment deal between the fund and the LP in a given year. Treated is an indicator for the PE fund having at least one bank LP pre-Volcker Rule. The important interaction variable in these tests is DryPowder, which is an indicator for a fund being above-median in the fraction of fund capital that was not deployed as of December 2013. These tests are of triple-difference design, done with the intent to measure whether there is a statistically different treatment effect on co-investment activity for high-dry powder funds, measured by Treated × DryPowder × Post. To isolate this effect, I also include all other two-variable interactions and levels of the three variables Treated, DryPowder, and Post in the regressions. I include LP and year fixed effects. Standard errors (reported in parentheses) are clustered at the fund level. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	$\mathbb{1${Co-investment}**}			
	(1)	(2)	(3)	(4)
Treated × DryPowder × Post	0.004	0.003	0.004	0.003
	(0.003)	(0.002)	(0.003)	(0.002)
Treated × DryPowder	-0.002	-0.001	-0.002	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Treated × Post	0.000	0.001	0.000	0.001
	(0.002)	(0.002)	(0.002)	(0.002)
DryPowder × Post	-0.005***	-0.005***	-0.005***	-0.005***
	(0.001)	(0.001)	(0.001)	(0.001)
Treated	-0.001	-0.001	-0.000	-0.001
	-0.001	-0.001	-0.000	-0.001
DryPowder	0.002	0.002	0.002	0.002
	(0.001)	(0.001)	(0.001)	(0.001)
Post	0.002*	0.003**		
	(0.001)	(0.001)		
LP Fixed Effects	NO	YES	NO	YES
Year Fixed Effects	NO	NO	YES	YES
R-squared	0.000	0.119	0.001	0.119
No. observations	27,234	27,234	27,234	27,234

Table 1.6: **Comparing exit outcomes**

This table displays summary statistics of exit outcomes for the universe of US-based, PE-backed private companies in PitchBook with non-missing exit data. Statistics are taken with respect the firm's group, that is, whether the firm received co-investment capital from LPs (group = 1) or not (group = 0). Data is at the firm level, and for each firm the exit event is the first observable PE exit after the final observable PE investment. Statistics related to the valuation of the firm at PE exit are displayed in Panel A. The first comparison variable is the ratio of exit value to the total observable PE dollars invested in the firm. The second comparison variable is the ratio of exit value to the size of the final PE investment made in the firm. Both of these ratios are set to 0 for firms that went out of business. Both are winsorized at the 99% level to mitigate the effect of outliers. Statistics related to the type of exit are presented in Panel B. The comparison variables in this panel are dummy variables for, respectively, the firm going public, the firm being acquired, the firm going out of business.

Panel A: Exit valuation

	Value / Total PE $		Value / Final PE Investment	
Group	0	1	0	1
N	5,861	96	5,245	84
mean	7.00	12.95	7.64	14.17
std	34.32	49.94	35.39	52.21
min	0.00	0.00	0.00	0.00
25%	0.00	0.06	0.00	0.10
median	0.00	0.55	0.00	0.83
75%	1.74	1.77	1.95	1.85
max	270.06	270.06	265.87	265.87

Panel B: Exit type

	$\mathbb{1}\{\text{IPO}\}$		$\mathbb{1}\{\text{Acquired}\}$		$\mathbb{1}\{\text{Out of Business}\}$	
Group	0	1	0	1	0	1
N	15,993	184	15,993	184	15,993	184.00
mean	0.06	0.18	0.59	0.72	0.35	0.10
std	0.24	0.39	0.49	0.45	0.48	0.30

Table 1.7: **LP secondary sales**

This table relates an LP's probability of co-investment to the propensity of the LP to sell their stake in PE funds on the secondary market. The data is at the LP level for all LPs making at least one PE fund investment over the period 2009 to 2020. The dependent variable is an indicator for an LP making at least one co-investment with at least one of their PE funds. The independent variables are defined in the appendix. The sample period is 2009 to 2020. I control for the size of the PE fund and include fund vintage year fixed effects. Standard errors are clustered at the LP level. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	𝟙{Co-investment}			
	(1)	(2)	(3)	(4)
# Secondary Sales	0.006***		0.005***	
	(0.001)		(0.001)	
𝟙{Secondary Sale}		0.145***		0.135***
		(0.021)		(0.022)
LPAUM			0.000***	0.000***
			(0.000)	(0.000)
Vintage Year Fixed Effects	YES	YES	YES	YES
R-squared	0.139	0.147	0.163	0.170
No. observations	2,275	2,275	1,997	1,997

Table 1.8: **Fund secondary sales**

This table relates a PE fund's probability of co-investment to the number or presence of LP secondary stake sales in the fund. Data is at the PE fund level for funds with a vintage year between 2009 and 2020. The dependent variable is an indicator for a fund making at least one co-investment with LPs over the life of the fund. The independent variables are defined in the appendix. I control for the size of the PE fund and include fund vintage year fixed effects. Standard errors are clustered at the fund level. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	𝟙{Co-investment}			
	(1)	(2)	(3)	(4)
# Secondary Sales	0.036***		0.036***	
	(0.005)		(0.005)	
𝟙{Secondary Sale}		0.090***		0.092***
		(0.012)		(0.013)
AUM			-0.000	-0.000
			(0.000)	(0.000)
Vintage Year Fixed Effects	YES	YES	YES	YES
R-squared	0.104	0.075	0.107	0.078
No. observations	2,151	2,151	2,072	2,072

Table 1.9: **LP co-investments and secondary sales timing**

This table reports results from regressions of LP secondary-sales variables on LP co-investments. The data is at the LP-year level, with one observation for each LP for each year the LP was active from 2009-2019. The dependent variable is an indicator for whether the LP made any co-investments in the given year, scaled by a factor of 100. The first independent variable of interest is Post-Secondary Sale, which is an indicator variable for whether the LP had sold a share in any PE fund in the current year or any prior year relative to the observation year. The second independent variable of interest is Total Secondary Sales, which enumerates the number of times that the LP had sold a share in any PE fund up to the observation year. I also control for the size of the LP (as observed in 2022) and include year fixed effects. Standard errors are clustered at the LP level. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	𝟙{Co-investment}*100			
	(1)	(2)	(3)	(4)
Post-Secondary Sale	4.572***		4.270***	
	(0.965)		(0.965)	
Total Secondary Sales		0.681***		0.649***
		(0.238)		(0.246)
AUM			0.000**	0.000*
			(0.000)	(0.000)
Year Fixed Effects	YES	YES	YES	YES
R-squared	0.009	0.013	0.010	0.013
No. observations	38,213	38,213	38,213	38,213

1.9 Figures

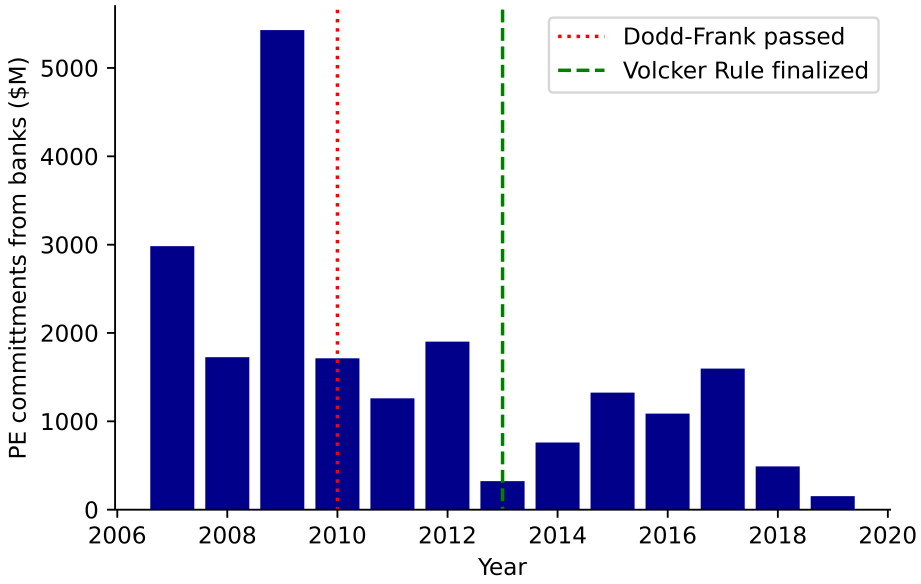


Figure 1.1: **Bank LP capital commitments to PE, 2000-2020**

This chart presents the levels of observable bank commitments to PE funds in each year over the period 2000-2020. The x-axis is year, and the y-axis represents the total amount of LP commitments from banks (in $M) in a given year to PE funds. The red dotted line denotes the year of the passage of the Dodd-Frank act, and the green dashed line shows the year of the Volcker Rule's adoption.

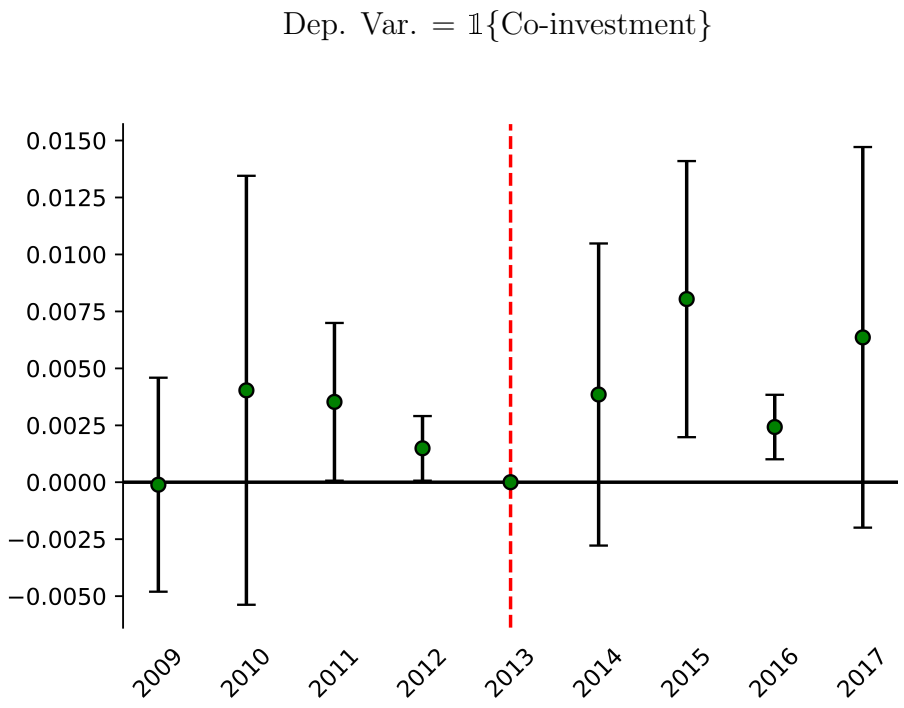


Figure 1.2: **Coefficients from dynamic DID**

This graph plots coefficients from the dynamic DID regression. Data is at the PE fund-LP-year level. The dependent variable is a dummy variable for at least one co-investment deal between the fund and the LP in a given year. The treatment variable takes on the value of 1 if the PE fund had at least one bank LP as of December 2013. The independent variables are interaction terms, the products of the treatment indicator and indicators for each year in the sample period (except the comparison year, 2013). The coefficients on these regression terms represent the effect of treatment status on the probability of co-investment in a given year of the sample relative to the untreated funds. These coefficients are plotted on the y-axis, with the year they represent on the x-axis. 95% confidence intervals are also included.

CHAPTER 2

Effect of Underwriter Mergers & Closures on IPOs[1]

2.1 Introduction

Venture capital (VC) is a prominent form of financing for innovative startups, offering entrepreneurs not only financial resources but also strategic guidance, networks, and expertise. As part of their investment strategy, VC firms seek to exit their investments by selling their stakes to other investors or by taking startups public through initial public offerings (IPOs). The IPO process is a critical event for VC-backed startups, as it enables them to realize their value by providing liquidity to their early employees and investors, and allowing them to raise new capital from public investors. However, the success of an IPO hinges not only on the startup's quality and the reputation of its VC investors, but also on the underwriting services of investment banks. IPO underwriters are crucial in pricing, marketing, and distributing IPOs. Their reputation and market relationships significantly influence startups' abilities to transition smoothly to public markets.

Existing research explores the importance of underwriters and VC expertise for startup outcomes, but the direct effect of underwriters on VC-backed-startup exits is less clear [Gompers et al. (2020), Megginson and Weiss (1991), Lerner (1994b), Hsu (2004a), Sorensen (2007)]. We test how changes in VC-underwriter relationships impact startup IPOs. While

[1]Co-authored by Emmanuel Yimfor, Columbia Business School, Columbia University

VCs finance less than 0.1% of new firms, these firms represent nearly 50% of all IPOs [Puri and Zarutskie (2012); Lerner and Nanda (2020)], suggesting VC-underwriter relationships facilitate the IPO process. Thus, our tests focus on whether changes to VC-underwriter relationships negatively affect VCs' ability to take their portfolio companies public or if these relationships are easily replaceable. Our study contributes to the literature on the declining number of IPOs and the increasing acquisitions of private companies, focusing on the influence of underwriters on this trend [Doidge et al. (2017), Ewens and Farre-Mensa (2020), Gao et al. (2013)].

The relationship between VC-underwriter ties and startup exits is not clear ex ante. On one hand, some studies suggest that these relationships can facilitate startup IPOs [Fernando et al. (2012), Megginson and Weiss (1991)]. They argue that underwriters supply valuable information, mitigate IPO uncertainties, and boost investor confidence. Thus, VCs that work repeatedly with the same underwriters can more precisely prepare startups to meet the underwriter's expectations. Conversely, VCs may forge a diverse set of underwriter relationships, such that no underwriter is important enough to impact how they exit their investments. Therefore, in a competitive market, VCs that have lost underwriting relationships could easily replace these relationships so that underwriter closures have no effect on how their portfolio companies exit.

We find that VCs that lose underwriters to mergers or closures (treated VC firms) take fewer startups public. Specifically, these treated VC firms see a 9.3% decrease in the ratio of IPOs to all forms of exit (IPOs and acquisitions).[2] One likely contributing factor is the significant employee turnover post-merger, where only 23.4% of the underwriter's key personnel remain with the merged entity after an acquisition. Such high turnover likely hinders the continuation of the VC-underwriter relationship with the merged entity, an effect that is stronger for smaller VCs. Further supporting the importance of these relationships, we show that IPOs are an important mechanism through which the loss of underwriters

[2]We do not expect M&A exits to be significantly affected by the closure of a VC's past IPO underwriter. For the average VC in our sample, only 6.9% of IPO underwriters also provide services for M&A exits.

impacts VC-fund returns. Treated VCs see their fund multiples decrease by approximately 12.9%. Consistent with the difficulties in establishing new VC-underwriter relationships, we also see that underwriting spreads are 22.3 basis points higher per issuer when treated VCs form new relationships in the aftermath of a lost relationship.

Of course, the relationship between an underwriter and a VC is not random. Better-quality VCs are more likely to establish and maintain relationships with underwriters (and therefore more likely to take startups public). Thus, we use quasi-exogenous variation in underwriting relationships: closures or mergers of underwriters [Asker and Ljungqvist (2010)]. Our identifying assumption is that an underwriter merger or closure is independent of the characteristics of a startup that received funding from a VC firm, as this financing typically occurs before the merger or closure. Thus, we use variation in underwriting relationships that is not caused not by the quality or performance of portfolio companies, but is due mainly to the business concerns of the underwriter, of which IPO underwriting is only a small part.

This approach allows us to estimate the causal effect of lost underwriting relationships on VC performance while controlling for unobserved characteristics of the VC firm and startups. We verify the validity of our identification strategy by testing the balance of observable characteristics between the treated VCs and the control group; we find no significant differences in terms of fund size, age, the composition of investments, average deal size, or location. We also find that our results persist if we restrict our attention to the subset of underwriter closures and mergers that were least likely to be anticipated or caused by the underwriting activity of the investment bank. Specifically, when we focus on VC firms that had previously worked with large banks that failed suddenly as a result of the mortgage backed security crisis, we observe a larger decrease in IPOs following the closures. Our results are also robust to the inclusion of VC firm fixed effects. Further, Figure 2.4 plots the average treatment effect estimates proposed by Callaway and Sant'Anna (2021). The lack of pre-trends further supports our identifying assumption.

In a mechanism test, we investigate whether VC funds that are under pressure to liquidate their investments are more reliant on underwriting relationships and therefore more vulnerable to lost underwriter relationships. Our results show that the decrease in the IPO rate for treated funds is 2.7% larger for VC funds near the end of their typical ten-year lifespan. This finding emphasizes the importance of underwriting relationships in managing the liquidity risk of VC funds, and suggests that the pressure to liquidate exacerbates the impact of lost relationships. We also show that the significant drop in IPOs for treated VC firms is a short-term effect that dissipates four years following the closure. This finding suggests that matching frictions exist between VC firms and underwriters, as the affected VC firms must put time and effort into establishing and building trust with new underwriters.

There are several mechanisms through which the loss of an underwriter could negatively impact a VC firm's ability to exit investments via IPO. First, VC firms that work repeatedly with an underwriter develop relationships that allow the underwriter to more efficiently sell the VC's portfolio companies in public markets. We provide evidence suggesting that observable, informal, human ties between the VC and underwriter are correlated with more IPO collaboration by the pair. Moreover, underwriters are sometimes late-stage investors in some of the VC's startups and can learn directly about the startup through their active involvement as an investor. Should the underwriter go out of business or be acquired, the trust relationship between the VC and underwriter is likely interrupted. In these cases, the underwriter can no longer signal quality to uncertain IPO investors, and acquisitions may be optimal for portfolio companies on the margin.

Second, repeated VC firm–underwriter interactions may allow the VC firm to resolve the underwriter's uncertainty by learning about, and adapting to, the underwriter's IPO practices and preferences. When the relationship is lost, the VC firm may find it difficult to easily and quickly learn and adapt to the preferences of new underwriters, which in turn may render an IPO suboptimal for a marginal portfolio company at the VC firm. We show evidence that IPOs decline even more for VC firms that previously worked with underwriters

with a revealed preference for small IPOs, a finding that is consistent with information costs associated with relearning and readjusting to new underwriter preferences.

Lastly, it is possible that some underwriters assist VC firms in taking startups public by offering specialized expertise for a specific subset of startups within an industry. After losing such an underwriter, the VCs and startups in that industry may have to choose from a remaining pool of underwriters that are either less willing or less capable of efficiently guiding them through the IPO process. However, when we control for a portfolio company's industry and time period, our main results do not change, implying that this factor is unlikely the primary driver of our findings.

One reasonable question that arises under our analysis is: how does the IPO underwriter benefit from relationships with VC firms? If underwriters find potential issuers using their networks, it not clear why they would benefit from building relationships with VCs.

We believe that VCs constitute an important channel through which underwriters source IPOs, reducing search costs and risk. Economic models of lender behavior support this logic. Using a model involving lenders choosing whether to employ costly monitoring for opaque borrowers, Diamond (1991) shows that lenders rely on the track record of borrowers to determine whether monitoring is optimal. In other words, when lenders observe borrowers making "good" decisions, they are less likely to monitor, and more likely to extend future credit. This, in turn, benefits the lenders through lower monitoring costs. Similarly, Malenko and Malenko (2015) show that prospective lenders in LBO transactions can use the reputation of a PE firm sponsor as a proxy for the quality of a portfolio company. Extending this logic, underwriters can use information on potential issuers through their relationship with VCs to reduce search and screening costs.[3]

[3]Figure 2.1 shows that the average VC-backed startup goes public just nine years after company formation, compared to the average non-VC-backed startup, which goes public 18 years post-formation. This disparity suggests that VC-underwriter relationships likely facilitate the going public process. Further, Table 1 in Lerner and Nanda (2020) shows additional characteristics of VC and non-VC-backed firms at IPO. These data show that VC-backed startups have weaker observable characteristics, which further suggests a significant role of underwriters.

2.1.1 Related literature

Our results add to the growing body of literature examining the time series variation in the number of IPOs and acquisitions. Previous studies, such as those by Doidge et al. (2017) and Ewens and Farre-Mensa (2020), show that a decrease in the number of IPOs can be partly explained by factors such as the availability of capital in private markets. Gao et al. (2013) show that increased economies of scope have led to more exits through acquisitions. Our study contributes to this literature by showing that underwriting relationships influence a startup's likelihood of exiting through an IPO. Thus, underwriter consolidation likely contributes to the trend of the decline in IPOs over time.

Our study also sheds light on how venture capital firms add value to their portfolio companies, which is of great practical and academic interest. Previous research, such as that by Bernstein et al. (2016) and Ewens and Marx (2018), shows that startups tend to perform better when VC firms can visit them more frequently, and that VC firms can add value by replacing founders. However, the role of VC firms' established relationships in adding value has received less attention. Our study contributes to this literature by demonstrating that VC firms add value to their portfolio companies through their relationships with underwriters. This finding enhances our understanding of the factors that contribute to the persistent performance of venture capital firms [Nanda et al. (2020)].

Our research also sheds new light on the value of underwriting relationships in general, and in the venture capital industry in particular. In public markets, Fernando et al. (2012) show that underwriting clients experienced abnormal returns of -5% in the seven days following Lehman's bankruptcy. Examining the impact of underwriter closures on startup exits, we find that underwriting relationships play a crucial role in the trajectory of venture-backed startups. Our findings suggest that the loss of an underwriter can significantly decrease the likelihood of an IPO exit, particularly for smaller venture funds and funds under pressure to liquidate their investments. These insights have important implications for the performance of venture capital firms and the welfare of their portfolio companies' employees and

consumers.

2.2 Underwriter selection

The selection of a lead underwriting investment bank (also known as bookrunner or "lead-left") is considered an important early step that a potential issuer makes as part of its pre-IPO checklist. Members of the board of directors at the issuing startup—usually some combination of founders, investors (VC firms), and outside directors—make this decision [Ewens and Malenko (2022)]. Industry sources indicate that in most cases, the candidate pool for the bookrunner selection consists of banks which have built up familiarity with the issuer over time, and that relevant employees at these potential bookrunners (managing directors, analysts, and associates) frequently have relationships with important parties at the portfolio company (company lawyers, general partners (GPs) at VC firms, executives, etc.).[4] In some cases, the advising lawyers will recommend a set of banks to the board; in others, the investment bank will initiate contact with partners at VC firms in order to source potential IPOs to underwrite.[5]

A formal underwriter selection process, sometimes referred to as a "bakeoff," ensues. In this process, representatives from both sides engage in a series of formal and informal meetings, often over the course of months or years. These meetings allow the board at the portfolio company to become familiar with the investment bank, and let the bank's analysts perform and report due diligence on the prospect of taking the startup public. Important considerations during the underwriter selection process include a bank's reputation/experience, the bank's expertise, the quality/salesmanship of the IPO team, the bank's proposed timetable, and pair-specific synergy/future prospects for issuer stakeholders [Fernando et al. (2005), Liu and Ritter (2011)].[6] In selecting portfolio companies, underwriters are likely to consider their own risk preferences, the information they have on the startup, and the trust they have

[4] https://www.lw.com/admin/upload/SiteAttachments/lw-us-ipo-guide.pdf
[5] https://mergersandinquisitions.com/win-investment-banking-deals/
[6] https://www.orrick.com/en/Insights/2018/06/Selecting-an-Underwriter-for-an-IPO

developed in any individuals or parties at the startup.

Once selected, a lead underwriter has several main responsibilities. Of course, the lead will be required to take the largest stake and act as intermediary for the issuers' shares in the IPO. Other important tasks include cobbling together a syndicate of underwriters for the IPO, filing prospectuses with the SEC, and developing and participating in the roadshow.[7]

A strong VC firm–underwriter relationship could facilitate the startup–underwriter matching process in several ways. First, without strong ties between bank personnel and senior partners at the VC firm, there might not be a strong pool of (or any) lead underwriter candidates for consideration, especially if VCs and underwriters tend to specialize in a particular type (e.g., industry) of startup. Second, a strong relationship could convince an underwriter to take a risk (underwriting and reputation risk) on a prospective issuer the underwriter does not know well. If the underwriter only deals directly with startup board members with whom it has no prior relationship, the underwriter might perceive the startup as too risky. This trust could prove important not only in the underwriter's decision to back the issuers' shares, but also in the underwriter's confidence when pitching the sale to investors during the roadshow. Finally, a VC firm that has a longstanding familiarity with an underwriter may better understand that underwriter's particular objectives and idiosyncrasies, easing frictions associated with communicating relevant information and facilitating the underwriter's due-diligence process.

2.3 Data

2.3.1 Sample construction

The data for our analysis comes from PitchBook. To identify underwriters that were acquired or closed, we start with a set of investment firms in PitchBook that were listed as underwriters on at least one IPO between 2001 and 2016. We drop underwriters with missing IPO date

[7] https://pitchbook.com/blog/ipo-process-explained

or size data, keep only US-based firms, and require that each of these underwriters has at least one IPO for a firm backed by at least one venture capital firm in the PitchBook data. This leaves us with a set of 252 underwriters involved in 2,324 unique IPOs. The average IPO took place in the second quarter of 2009, and the average underwriter was associated with 24.2 IPOs.

Using this set of underwriters, we collect information on all mergers and acquisitions where the underwriter was listed as a target in PitchBook. This yields a base sample of 121 mergers and acquisitions. We hand-inspect this list of M&A deals to ensure that there was a change in effective ownership. An example of a deal eliminated at this stage is Lazard Middle Markets being "acquired" by Lazard. After this step, we are left with 79 unique underwriting targets. Applying the filter that the underwriter must underwrite at least one VC-backed IPO between 2001 and 2016 leaves us with 47 unique underwriter acquisitions. The average underwriter merger happened in 2010, with the mode year being 2008 with seven deals. We hand-searched whether any of the 252 underwriters closed, and identified five unique underwriter failures in our sample: two in 2010, two in 2012 and one in 2016. Overall, our final sample of treated underwriters (ones that were in a merger or failed) comprises 52 underwriters, or roughly 20% of the U.S.-based underwriters in our sample.

We next identify the set of venture capital firms that lost underwriting relationships (treated firms). This set comprises all VC firms that backed at least one startup that went public over the years 2006-2016, 1,255 firms.

2.3.2 The unit of observation

The unit of observation in our final sample is a VC firm (i)–quarter (t) pair for each quarter between 2006 and 2016. The primary dependent variable is IPOs_{it}, or the number of portfolio companies of VC firm i that go public in quarter t. For an observation to be included in the sample the VC firm must have collaborated with at least one underwriter in the past five years. Our final sample comprises 26,343 VC firm–quarter observations.

2.3.3 Key independent variable

Our primary independent variable, *FracClosed*, captures the exposure that a VC firm has to underwriter closures and mergers in a given quarter. Specifically, it equals the proportion of underwriting partners from the previous five years that closed or were acquired. We choose five years as the time window over which we consider closures to mirror the average investment to exit time frame. VC firms raise new funds every 3–6 years, and the majority of investments are exited in the latter half of the fund's 10-12 year life. In any given quarter of our sample, the average VC firm has 1.92 unique underwriters with whom they have conducted at least one IPO over the prior five years.

2.3.4 Summary statistics

Table B1 shows our sample of "lost" underwriting relationships, how the relationships were lost, the number of VC firms that lost these relationships, and the number of unique portfolio company investments that the VC firm made before the lost relationship. Lehman Brothers, Thomas Weisel Partners Group, KBCM Technology Group, ThinkEquity Partners, and Bear Stearns are amongst the largest underwriter closure events. The KBCM Technology Group's acquisition in 2014 left 308 potential underwriting relationships at risk, while the ThinkEquity Partners bankruptcy and closure in 2012 left 125 underwriting relationships at risk.

Table 2.1 shows statistics for the VC firms in the sample. Panel A focuses on VC-level variables, while Panel B focuses on VC–quarter level variables. On average, a VC firm was founded in 1996 and manages $1.34 billion in assets. These VCs make 4.4 investments on average per quarter, with an average round size of $9.4M. Sector-wise, 15% of investments are in healthcare startups, 21% in tech startups, and 20% of these companies are based in California. In terms of exits, the average VC sells 0.47 portfolio companies through acquisitions and takes 0.16 companies public through IPOs. Importantly, 35% of the VCs in the sample experienced the loss of at least one underwriting relationship in the past 20

quarters.

To investigate whether VC firms that lost an underwriting relationship were systematically different from their counterparts that did not, we compare averages of VC firm characteristics across the two groups in Table 2.2. We refer to the set of VCs that lost an at least one underwriting relationship over the sample period as the *Lost Underwriter* group, and to the set of VCs that did not experience the closure of at least one underwriter as the *No Lost Underwriter* group. The first column presents the averages for the descriptive variables for the *Lost Underwriter* group, and the second column presents the averages for the *No Lost Underwriter* group. The third column displays t-statistics of the differences in means. The two groups are statistically similar when considering these variables, except that VCs that lost underwriters are slightly older. However, a close inspection reveals that the economic magnitude of this difference is small.

Figure 2.2 shows the univariate relationship between underwriter closures and the likelihood of an exit via an IPO. The first bar shows that when the VC firm recently lost between 0%-20% its underwriting relationships the likelihood of an IPO is 14%. This likelihood decreases to less than 6% when the VC loses 20%–40% its underwriting relationships, and dips further as the proportion of lost underwriting relationships increases (eventually falling below 1% for 60-80% of lost relationships). This univariate evidence suggests that losing an underwriting relationship impacts whether the VC firm exits its investments through an IPO.

Figure 2.3 shows how IPOs as a fraction of total exits evolves in response to lost underwriting relationships. For all VC firms that lost a relationship, the graph plots averages of the fraction of IPOs to total exits in event time, relative to the earliest loss of a relationship. This IPO ratio remains relatively constant and above 30% for the four years leading up to the event, then decreases by an average of 14% in the three subsequent years that follow. Interestingly, the ratio rises to near pre-closure levels by year five. This trend suggests that the drop in the IPO propensity is temporary, reflecting a period of adjustment during which

the VC forms new relationships.

The event study in Figure 2.3 is akin to a staggered difference in differences. To address the bias from comparing underwriters who lost a relationship later in the sample period (late switchers) with those who lost it earlier (early switchers) [Goodman-Bacon (2021)], we estimate treatment effects using the approach suggested by Callaway and Sant'Anna (2021). The results, presented in Figure 2.4, reassuringly show no pre-trends.

2.3.5 Startup–VC firm level sample

To complement the VC firm-quarter sample, we also construct a startup–VC firm sample, to perform tests that control for variation in startup characteristics such as the primary industry of operation, VC firm relationships, and number of early-stage investors. We identify 39,945 unique startups that received at least one investment from any of the 1,255 VC firms in the VC firm–quarter sample. For a startup to be included in the sample, we require that at least one of its VCs has a recent (within five years) interaction with an underwriter through an IPO. This leaves 34,170 startups. Finally, we impose the restriction that the startup's exit outcome is observable in the data, either through IPO, acquisition, or bankruptcy. This leaves us with a final sample of 13,340 startups. After applying these filters to the set of startups, our set of VC firms shrinks from 1,255 to 1,134, giving us a final sample of 30,267 unique startup–VC firm observations.

2.4 Results

2.4.1 Main results

In this section, we present and interpret the regressions which form the basis of our tests of the effects of lost underwriter relationships on exits.

2.4.1.1 Baseline regression: IPOs and underwriter closures

To understand how underwriting relationships affect startup exits, we run the following reduced form Poisson regression:

$$\#IPOs_{it} = exp\{\alpha + \beta\ FracClosed_{it} + X'_{it}\ \gamma + \eta_t + \epsilon_{it}\} \tag{2.1}$$

The unit of observation is a venture capital firm i, in quarter t. The dependent variable is the number of IPOs associated with VC firm i in quarter t. The independent variable is the fraction of underwriting relationships over the past five years that VC firm i lost, either through an acquisition or a failure between period $t-20$ and t.

X_{it} comprises control variables such as the number of underwriting relationships, whether the VC firm is U.S.-headquartered, whether the VC firm is California-headquartered, the amount of VC funding startups raised in that quarter in the VC's state, the number of years since the VC firm was formed, the most recent fund size raised by the VC firm, the number of past investments the VC firm has made as of that quarter, and the average investment size of deals made by the VC firm as of that quarter. η_t represents quarter fixed effects. We believe the structure of the data lends itself to Poisson Maximum Likelihood estimation [Cohn et al. (2022)], though we also run analogous OLS regressions for robustness.

Our hypothesis is that $\beta < 0$, which we interpret as evidence that lost underwriting relationships affect the VC firm's choice to take its portfolio company public. Table 2.3 presents results of the baseline test specified in equation 2.1. Across all columns, we see that VCs that lose underwriting relationships take fewer startups public. In a given quarter, the average VC lost 7% of its underwriting relationships (the average *FracClosed* is 0.07). Thus, the coefficient from our most stringent specification in column (4) suggests that the number of IPOs decreased by 22.28% ($(e^{-3.601*0.07} - 1) * 100$) following the loss of these relationships. This translates to 0.03 fewer IPOs per quarter for the average VC firm. We present analogous OLS results in Table B2.

The effect of our control variables on the number of IPOs is consistent with intuition. VC firms making large investments take more companies public, as larger investments likely imply high valuations, which means a smaller set of potential acquirers. Similarly, VC firms that have raised larger funds take more startups public, likely because they have the experience and expertise. The fact that California-based VC firms have higher IPO rates, and that VC firms with more underwriting relationships take more startups public is consistent with intuition.

In addition, the negative coefficient on the *RegionVCSupply* variable is consistent with the effect, documented by Ewens and Farre-Mensa (2020), where an abundance of capital in private markets is negatively associated with the likelihood of an IPO.

Our results thus far suggest that lost underwriting relationships due to mergers or closures are negatively associated with the number of IPOs in a VC firm's portfolio. Until now, we have assumed that mergers and closures are as good as randomly assigned. However, it is possible that the VC firms affiliated with underwriters that were targets of acquisitions or underwriters that closed might also be less likely to select portfolio companies that become successful enough to undergo an IPO. If these VCs chose to work with underwriters more likely to close, it is also possible that they also selected bad portfolio companies, leading to the lower IPO rates.

We next introduce VC-firm fixed effects to control for VC firm–specific unobservables that are not time varying. We use an OLS fixed effects specification for this test to avoid issues associated with the incidental parameters problem that can result from using the non-linear Poisson regression with fixed effects for each firm [Neyman and Scott (1948)]. Results are presented in Table B3. Given that we are using an OLS specification and cannot take the log of zero for VCs with no IPOs in a given quarter, the dependent variable is now a dummy variable that equals one if VC firm i had an IPO in quarter t and zero otherwise. Including VC firm- then quarter fixed effects (moving from column (2) to (4)) actually increases the magnitude of the coefficient (from -0.218 to -0.300), and remains significant at the 1% level.

This suggests that treated VC firms being more likely to have made bad investment decisions does not explain our findings.

2.4.1.2 Startup-VC-level data

In the previous subsections, we documented a decrease in IPOs following the loss of an underwriting relationship. In this section, we will test whether this effect is industry-specific using the startup-VC-level dataset we described in Subsection 2.3.5.

A startup is now treated if any of its VCs lost an underwriting relationship (over the previous five years). In the appendix, we also use a second, continuous measure, *FracClosed*, which is the average proportion of lost underwriting relationships across all the startup's VCs. We run the following regression:

$$\mathbb{1}\{Exit_{ijst} = IPO\} = \alpha + \beta\ Treatment_{jst} + X_s'\ \gamma + \phi_j + \eta_{it} + \epsilon_{ijst} \tag{2.2}$$

where s denotes startup, i is the startup's industry, j indexes the VC firm, and t is the first year the firm received VC funding. The dependent variable is a dummy that equals one if startup s exits via an IPO by June 2022. X_s is a vector of startup characteristics.

To account for the effect of VC firm–level skill (adding value by selecting and guiding startups to IPO) we include VC firm fixed effects, ϕ_j. We also include industry-year fixed effects, η_{it}, to test whether the loss of an underwriting relationship affects IPO prospects through the underwriter's relationship with the VC and not through a shock to the IPO prospects for all startup within that industry when the underwriter closes, a channel which could explain our findings from Subsection 2.4.1.1.

We present our results in Table 2.4. The most stringent specification, Column (4), shows that a startup whose VC lost an underwriting relationship (is treated) is 4.7% less likely to go public. This coefficient is economically meaningful. Given the sample IPO rate of 17.8%, it implies a more than 25% decline in the likelihood of an IPO. We obtain qualita-

tively similar results using the continuous treatment variable, which we show in Table B4. These results support our conclusion that the mechanism through which lost underwriter relationships affect startup IPOs is through the underwriter's relationship with the VC and not a specialization effect, where the closure or merger of a specialized underwriter within an industry affects the IPO prospects of all issuers within that industry.

2.4.1.3 VC firm or startup relationships?

The link between the underwriter and the startup in an IPO can be formed through the underwriter-VC relationship, but also through the underwriter directly identifying startups to take public.

Our main claim is that VC-underwriter channel is an important deal-sourcing technique for underwriters. If this were the case, we would expect the number of VC-underwriting relationships to be positively correlated with the likelihood of an IPO. We use the cross-section of 13,340 startups from the previous section, and regress an indicator of an IPO exit on the log number of unique underwriting relationships the startup's VCs had at the time of investment. Results are presented in Table B5. We find a significant and positive effect of more VC-underwriter relationships on IPO probability in all specifications. In economic terms, if a startup goes from 1 to 10 unique underwriters having relationships with VC-backers, the probability of an IPO increases from 0.008 * (log(10) - log(1)) $\approx$ 0.02, which represents a 15% increase relative to the sample mean of 0.12. We interpret this result to suggest that startups with VCs that are better-connected to IPO underwriters are more likely to go public.

2.4.1.4 Underwriter closures and exit outcomes

Next, we consider what happens to IPOs as a proportion of all exit outcomes (IPOs and acquisitions) following the loss of the underwriting relationship by the VC firm. From Figure 2.3, we see that the fraction of all the VC's startup exits that were IPOs decreases

significantly in the years following the loss of a relationship. We also test this in a multivariate regression by regressing the proportion of exits that were IPOs on the *FracClosed* variable. The dependent variable is the proportion of all the VC firm's startup exits over the past year (acquisitions and IPOs) that were IPOs.

From Table 2.5, we see that increasing *FracClosed* from 0 to its mean at 0.07 is associated with a 3% (-0.408*0.07) decrease in #IPOs/#Exits, a roughly 9% decrease relative to the unconditional mean. Figure 2.3 shows that this proportion recovers five years following the loss of the relationship. This recovery is consistent with the possibility that some IPOs are delayed until the VC establishes new underwriting relationships.

This finding is also consistent with a decline in M&As, albeit at a lower rate than the decline in IPOs. The business of an investment bank advising on an M&A deal is quintessentially a relationships business, arguably even more so than an IPO. As such, one might imagine that VC firms that experience an interruption in any underwriting relationship may also struggle to execute M&As, and observe a larger effect for M&As than IPOs. However, in our data, there is very little overlap in IPO and M&A underwriting. Less than 7% of IPO underwriters in our sample are also service providers for the VC's M&A exits (less than 3.6% in the full PitchBook database). Accordingly, we do not expect M&As to be strongly affected by the loss of an IPO underwriting relationship, a hypothesis we confirm in untabulated results that we would gladly provide upon request.

2.4.1.5 Exogenous mergers/closures

Next, we address the possibility that the loss of an underwriting relationship is only quasi exogenous. If lower quality VCs match with lower quality underwriters, the loss of a relationship could signal the low quality of the VC, which may correlate with fewer IPOs. To assess this, we focus on lost underwriting relationships most likely to be independent of the VC's skill. Specifically, we restrict our attention to the unexpected closures of only Lehman Brothers and Bear Stearns. We believe these events are more plausibly exogenous to the

VC's skill. [Fernando et al. (2012)]. These banks were seen as premier institutions essentially until their demise, and their sudden failures surprised many in the financial world and were related to their involvement in the mortgage market. We do not, therefore, expect that the VCs that partnered with these underwriters prior to their sudden closures were lower quality.

Table 2.6 presents estimates of Equation 2.1 where the independent variable is an indicator for VC firms that had an underwriting relationship with Lehman Brothers or Bear Stearns (*CrisisBankFlag*). Compared to VC firms that did not lose an underwriting relationship, VCs that lost one took fewer startups public in a given quarter following the closures. The statistically significant coefficient in column (4), -0.307, translates into a decrease of $-(e^{-0.307}-1)*100 = 35.9\%$ in quarterly IPOs in the five years following the shock. This, on average, corresponds to 0.05 fewer IPOs in a quarter. Of course, restricting our set of lost relationships to Lehman and Bear Stearns means our tests focus on the period following the financial crisis, a period when few companies went public. However, our tests have quarter fixed effects to capture time series variation in IPO rates. Thus, the *CrisisBankFlag* coefficient shows the decline in IPO rates *relative* to other VCs were not exposed to these crisis banks, holding the overall IPO rate in a given quarter fixed. Results are robust to using a shorter panel, OLS regressions, and startup-VC level data (see Tables B6 B7, B8, and B9).

2.4.1.6 Underwriter bankruptcies versus acquisitions

In this section, we test whether the merger of underwriting banks actually interrupts the human relationships between GPs at the VC firm and employees at the target bank. That is, does a change in ownership affect the information-relationship economics at play between underwriter employees and GPs? We investigate this dynamic by testing whether employees at target underwriters indeed continue working at acquiring investment banks.

We use the universe of public profiles on LinkedIn from CoreSignal to look at the employment histories of 144,771 people that worked for 91 underwriters acquired between 2017 and 2020. This sample does not overlap with our main sample, as 2017 is the first year of

data available through CoreSignal. We calculate the fraction of employees at the target that continue to work for the acquiring firm following the merger, and present the results in Panel A of Table 2.7. We find that only 23.4% of target employees later work for the acquirer.

We investigate which employees remain by sorting the sample of target employees and classifying their job titles by seniority. We also check for keywords common in the job titles of employees that work on IPO underwriting.[8] We present the proportion of employees that later work at an acquiring investment bank by job titles in Panels B&C of Table 2.7. Fewer junior employees (16.04%) but more employees with job titles associated with IPO underwriting (26.6%) continue working for the acquiring firm. Anecdotal evidence suggests that these figures are similar to employee turnover rates for the target underwriters in our main sample.[9]

Next, we test whether target underwriter employees that are not working for the acquirer go on to positions at other underwriters, bringing their VC relationships with them. However, we find significant disruption in this channel too, as our data show that 44% of target underwriter employees do not go on to jobs in the banking or financial institution industries.

Our evidence on employee turnover and career path changes after mergers suggest that GP-underwriter human capital relationships are disrupted by these closures, a likely channel for the drop in IPO rates.

2.4.2 VC fund/underwriter characteristics

In this subsection, we dig deeper into mechanisms by testing how various VC fund and underwriter characteristics affect a VC firm's exposure to the loss of an underwriter. In particular, we investigate 1) the age of the VC fund at the time of the underwriter's closure, 2) how much time had passed since the closure, 3) the frequency of the lost underwriter's IPO activity, and 4) the average size of the IPOs the closed underwriter participated in.

[8] See B.2 in the appendix for details on this process.

[9] For instance, approximately two-thirds of Lehman employees did not work at Barclays several years after the acquisition (https://libertystreeteconomics.newyorkfed.org/2019/01/customer-and-employee-losses-in-lehmans-bankruptcy/).

We use the main VC-panel sample and employ the following Poisson MLE regression:

$$\begin{aligned} \#IPOs_{it} = \exp\{\alpha + & \beta_1 \ FracClosed_{it} \times InteractionTerm_{it} + \\ & \beta_2 \ FracClosed_{it} + \beta_3 \ InteractionTerm_{it} + \\ & X'_{it} \ \gamma + \eta_t + \epsilon_{it}\} \end{aligned} \tag{2.3}$$

where the interaction term is a particular characteristic that may be related to the sensitivity of IPOs to lost relationships. Below, we carefully describe the construction of the various interaction terms that we investigate. The coefficient of interest is β_1. A negative (positive) sign on β_1 suggests that exposure to the loss of underwriting relationships is more (less) severe for VC firms with higher values of the interaction term.

2.4.2.1 VC fund age

We first turn our attention to the interaction between fund age and lost relationships. VCs raise funds for a limited life of ten years (although there is typically an option to extend for two more); the funds invest for the first five to seven years, then guide the companies towards an exit over the remaining years of the fund. Older funds, therefore, are exiting from investments made earlier in their life cycle. Since underwriting services are more critical to a fund's exit concerns than to its investment concerns, we expect that older funds will have less time to react to the sudden closure of an underwriter. Conversely, younger funds can form new relationships before they need to exit. Bian et al. (2023) show that the fund age constraint sometimes forces VCs to sell their startups at fire-sale prices, suggesting that the age constraint is binding.

In Table B10, we test whether the decrease in the quarterly IPO rate following an underwriter closure is bigger for VC firms with older funds, which would be consistent with the causal effect of these relationships on IPOs. We calculate the log average recent fund age, *RFA*, of all the active funds the investment firm is managing at the time of the observation

(the year-quarter in our VC-firm-quarter panel). We consider only funds that are less than 12 years old at the time of the observation, where the age of the fund is the number of years between the observation year and the year the fund was first raised (its vintage year).

We see that VC firms that manage older funds take fewer startups public in an IPO, and that the effect is exacerbated when the firm recently lost a relationship (see the negative coefficient on *FracClosed* $\times$ *RFA*). Taking the coefficient (-0.493) from the most stringent model, we calculate the effect of increasing the age of the most recent fund from 0 to 6 (changing *RFA* from 0 to 0.78). Using the average *FracClosed* value of 0.07 and holding all else equal, we find that the impact of increasing the fund age from 0 to 6 further decreases quarterly IPO probability by $-(e^{-0.493*0.07*0.78}-1)*100 = 2.7\%$ This coefficient, however, is not statistically significant in columns (1) and (4), but the economic magnitude (especially in Column (4) with VC-firm fixed effects) is large.

The negative effect of fund age on the likelihood of an IPO is likely because the oldest funds face the highest pressure from fund investors (limited partners, or LPs) to exit in a timely manner. Due to lockup requirements imposed by the SEC following IPOs, VCs cannot easily exit their positions, thus older funds likely choose acquisitions over IPOs.

We run an analogous test using the startup-VC-level data instead of the VC firm-quarter panel and report results in Table B11. We find that, following lost relationships, the startups with older VC funds see a larger decrease in the likelihood of an IPO than startups with younger VC funds.

2.4.3 VC fund size

Our next test is whether our results are stronger for smaller VC firms. *VCSize* is the natural log of the number of VC deals the VC made prior to that quarter. Our results are the same if we instead measure size as the size of all deals made, or the number of previous IPOs involving the VC.

We present the results in Table B12. We find that the β_1 from equation 2.3 is positive,

suggesting that smaller VCs are more affected by lost relationships than large VCs. To provide a sense of the economic magnitude of this effect, we first take the coefficient (0.723) from the most stringent model. Increasing the VC's number of deals from 0 to the sample average of 45 changes *VCSize* from 0 to 3.8, in log terms. Using the average *FracClosed* value of 0.07, we find that the increasing *VCSize* from 0 to the average level reduces the impact of lost relationships on IPOs by $(e^{0.723*0.07*3.8} - 1) * 100 = 21.4\%$, which, on average, represents 0.02 fewer IPOs per quarter. This effect can be seen visually in Figure 2.5, where we chart the fraction of all VC exits that were IPOs in quarters relative to the loss of an IPO underwriter relationship, with the sample split by size of the VC firm, measured in different ways. Across all measures, we see that the drop in # IPO / # Exits ratio is most pronounced for smaller VC firms.

2.4.3.1 Underwriter characteristics

We now consider characteristics of VC firms' underwriters that could affect the VCs ability to match with other underwriters following the loss of a relationship. We do this to test whether the effect of lost relationships is more severe when VCs lose underwriters with these characteristics, which would suggest that the relationship loss is driving our estimates of the decline in IPOs above. We focus on underwriting frequency and the size of the IPOs.

The existence of VC-underwriter relationship could plausibly suggest some alignment in the two party's preferences for IPOs. One example of such preferences is the frequency of IPO underwriting. A VC firm that loses a bank partner who is very active in the IPO underwriting market may struggle to find new underwriters wiling to take startups public at the same frequency. Similarly, a VC firm that deals primarily in smaller IPOs may find it difficult to replace a lost relationship given the rapid pace of bank consolidation over our sample period.

To test whether either of these characteristics makes a VC firm more vulnerable to losing an underwriting relationship, we run regression 2.3 after constructing variables to measure

the VC's exposure to these underwriter characteristics.

To measure the activity of a given underwriter in the IPO market, we first calculate, in each year, the number of unique IPOs the underwriter participated in over the previous five years. Then, in each year, we identify the underwriters in the top quintile of IPO activity. Finally, for each observation (VC i, time t), we calculate the proportion of VC i's recent underwriter partners that were top-quintile in IPO activity in year t. We call this variable *HighFrequency*. We construct our measure for the VCs exposure to small-IPO underwriters, *SmallSize*, similarly. This measure is the proportion of the VCs recent underwriters that were in the bottom quintile of the average size of the IPOs underwritten over the previous five years. Our prediction is that β_1 from equation 2.3 for both of these interaction terms will be negative.

Table B13 shows that VCs that lost very active underwriters had even larger drops in their quarterly numbers of IPOs following a closure. Using the mean *FracClosed* of 0.07, the incremental impact of increasing *HighFrequency* from 0 to 1 (i.e., from having *no* highly active underwriters to having *only* highly active underwriters) on quarterly IPO rate is $(e^{-3.603*0.07} - 1) * 100 = -22.3\%$ (or 0.02 IPOs per quarter). Here, we used the coefficient on the interaction term from column (3).

Table B14 displays results related to the second interaction term, *SmallSize*. Using the coefficient from column (3) and the average level for *FracClosed*, we can calculate the change in the quarterly rate of an IPO when *SmallSize* goes from 0 to 1 (i.e., from having *no* small-IPO underwriters to having *only* small-IPO underwriters) as $(e^{-6.97*0.07} - 1) * 100 = -38.6\%$ which corresponds, on average to 0.04 fewer IPOs per quarter.

Taken together, these last two results indicate that while all interrupted VC firm–underwriter ties seem to predict lower IPO rates for VC firms, VC firms with a dependency on highly specific underwriters (highly active underwriters or underwriters that underwrite small IPOs) are the most adversely affected.

2.4.4 Impact of closures on VC firm returns

Our results thus far are consistent with the idea that a disruption in a VCs access to underwriters is associated with a lower likelihood that portfolio companies go public. This association likely has implications for VC firm value. Figure 2.6 shows that there is a positive correlation between IPOs and VC fund returns. In this subsection, we use multivariate regressions to test the direct impact of these lost underwriter relationships on VC firms' returns.

2.4.4.1 Fund returns

To test the impact of lost relationships on fund returns, we limit our sample to VCs with information on fund returns in PitchBook. This sample of funds is a subset of the 1,255 VC firms from our earlier analysis. Of course, we also require that the firm associated with the fund have a documented relationship with an underwriter no more than five years prior to the fund's vintage year (the year the fund was formed). Our final sample has 954 unique funds managed by 296 unique VC firms.

We test the impact of underwriter closures on fund performance using the following regression:

$$\log(TVPI_{ijt}) = \alpha + \beta\ Treatment_{ijt} + \gamma\ NumUnderwriters_{ijt} + \phi_j + \eta_t + \epsilon_{ijt} \qquad (2.4)$$

where i indexes VC fund, j indexes VC firm, and t represents fund vintage. The dependent variable measuring fund performance is the natural log of the fund's most recently reported (as of the second quarter of 2021) total value to paid-in capital multiple, $log(TVPI_{ijt})$. TVPI is the sum of net asset value of deals the fund is still holding plus the value of all fund distributions to date, divided by the total commitments paid into the fund.[10] $Treatment_{ijt}$

[10]For instance, consider a situation where the only limited partner invested \$100 in a fund, which in turn, invested in two companies. If one company is sold for \$100 and the other is valued at \$180 in the year that we measure TVPI, then the TVPI value is $(100 + 180)/100 = 2.8$.

is constructed two ways. The first, *FracClosed*, is the proportion of all the VCs underwriting partners that closed over the life of the fund, which is 12 years in our sample. The second is an indicator variable that equals one if any of the VCs underwriters closed sometime over the life of the fund. $NumUnderwriters_{ijt}$ is the number of underwriting partners active for VC firm j over the lifetime of fund i. ϕ_j and η_t are VC firm and fund vintage year fixed effects, respectively.

We present our results in Table 2.8. The coefficient in column (4) is negative and statistically significant. Increasing *FracClosed* from 0 to the fund mean of 0.18 decreases TVPI by $(e^{-0.776*0.18} - 1) * 100 = -12.9\%$. The average fund's TVPI in the sample is 1.4, so, for the average VC fund, this effect would correspond to a TVPI decrease of roughly 18%. This implies that around 18 cents less are returned for each dollar invested in the fund. We found similar effects with our binary treatment variable and show those results in Table B15.

These results provide evidence that after controlling for time-varying factors, VC funds that are exposed to plausibly exogenous losses of underwriting relationships perform more poorly than funds *within the same VC firm* not experiencing such losses. The results are consistent with the assertion that restricting a VCs exit choice set has negative consequences for the returns.

2.4.4.2 Underwriting fees

One source of ambiguity in the effect of lost underwriter relationships on VC firm value is the potential for underwriter rent-seeking in the IPO process. Should this problem be sufficiently severe, it could outweigh the additional returns the VC firm enjoys, on average, from an IPO relative to an acquisition. Thus, VCs that repeatedly interact with the same underwriters may be particularly exposed to such underwriter expropriation. The evidence in Subsection 2.4.4.1 does not support this argument, as the closure or merger of an underwriter is associated with a *decline* in VC fund returns—not an increase. The evidence is instead consistent with the idea that, on average, the placement of IPO shares by underwriters

outweighs any rent-seeking losses. Nonetheless, we conduct two more tests on whether the loss of a relationship ameliorates or exacerbates measures commonly associated with underwriter rent-seeking.

First, we look at the spread charged by the underwriter. We get data on underwriting spreads for US-based IPOs from 2006 to 2016 from SDC Platinum. We identify all VC firms which were investors in startups issuing equity in these IPOs and perform a text-based matching algorithm to link this data to PitchBook VC firms. From our original sample of VC firms, we are able to match 561 to VC firms listed as backers on 481 IPOs, giving us 8,158 VC firm–deal observations. In the fee dataset, we find that 72.5% of IPOs have an underwriting spread of 7%, consistent with the literature on IPO spreads [Chen and Ritter (2000), Hansen (2001)]. However, there is non-trivial variation in the remaining 27.5% of the deals and thus in the sample as a whole, which has a standard deviation in underwriting spread of 201 basis points. To examine the impact of lost underwriter relationships on future underwriting spreads, we consider a VC firm–deal observation to be treated if the VC firm lost at least one underwriting relationship prior to the current IPO. We then regress underwriting spreads on this binary variable, including time and VC firm fixed effects.

Results from this test are presented in Table 2.9. We find that the underwriter spreads for treated VC firm deals are 22.3 basis points higher. Thus affected VCs pay higher fees to the new underwriters they match with, a finding that is consistent with the decrease in fund returns that we found previously, and inconsistent with the "rent-extraction" hypothesis.

2.4.4.3 Underpricing

Another measure of underwriter rent-extraction is the degree to which the underwriter seeks to manage risk by underpricing the issuer's shares in the IPO [Loughran and Ritter (2002)]. We gather data on one-day IPO underpricing from IPOScoop's website and match 306 unique IPOs from 2006 to 2016 to PitchBook. These IPOs are associated with 721 unique VC firms from our original sample, giving us 2,359 VC firm–deal observations. Once again, we

consider an observation as treated if the VC firm experienced the loss of at least one former underwriter in a period prior to the current IPO. We regress the one-day price change in issuer shares on the treated dummy, including time and VC firm fixed effects. We find that treated VC firm deals are associated with a 2.2% larger one-day price change, but the estimate is insignificant when we include VC firm fixed effects.

2.5 Discussion: understanding mechanisms

In this section, we discuss potential mechanisms driving the decline in IPOs for VCs that lost underwriting relationships, and relate these mechanisms to the results presented in this chapter.

2.5.0.1 Personal relationships, trust, and information asymmetry

Why does the closure of an underwriter affect whether a VC firm exits its portfolio company via an IPO? Consider a perfectly competitive market of identical IPO underwriters in a setting with no other major frictions. In this case, the value of the IPO option to the VC firm (and portfolio company) will be determined in a competitive equilibrium, and will not change based on past collaborations with underwriters. So, in theory, the loss of an underwriting relationship should have no effect on the VC-backed startups likelihood of going public.

In practice, however, VC firm–investment bank relationships are based on trust, and take time to build. Thus, it may be difficult for a VC firm to seamlessly replace one trusted underwriter with another. This, in turn, leads to a reduced likelihood that the VC firm with interrupted relationships will be able to effectively take its portfolio companies public. We believe this effect drives the results on lost underwriter relationships and IPOs. Further, the results in Table 2.7 are consistent with this argument that underwriter acquisitions disrupt these relationships, especially since the personnel at the target underwriter are less likely to

work for the combined firm.

The trust aspect of a VC firm–investment bank relationship is likely to be especially important when the underwriter knows little about the startup. With its reputation at stake, the underwriter may feel that the trustworthiness of the VC firm is essential, as it allows the underwriter to accurately represent the startup's risk and opportunities to prospective investors. Due to the risk of backing an unknown startup in an opaque market, some banks may underwrite IPOs only when they have trusting relationships with the startup's VC firm partners.

This information asymmetry mechanism is likely more binding for some VC firms than for others. We would expect that lost relationships would impact VCs that are more informational opaque rather than well-known, large VCs. Indeed, from Figure 2.5, see (B12) for the regression, the magnitude of the drop in IPO frequency relative to acquisitions following the loss of a relationship is much larger for smaller VCs, where we measure size using deal frequency, deal size, and IPO frequency.

One important way that VC firm–underwriter relationships might be built is through previous underwriter employees who now work for the VC firm. Figure 2.7 plots the relationship between the overlap in personnel between an IPO underwriter and VC firms and the likelihood that the underwriter and each VC firm jointly participate in an IPO.

To build this figure, we use résumé data from LinkedIn on the senior partners at the VC firm to generate VC firm–underwriter pairs. The X axis shows the number of senior partners at VC firm i who have worked for underwriter j. The Y-axis shows the number of startups backed by VC firm i, whose IPOs were underwritten by underwriter j. We averaged Y for each level of X, truncating the number of employee overlap at three partners. The tick marks surrounding the bars are 95% confidence intervals.

We see that VC firm–underwriter pairs that share more employees tend to take startups public together more frequently. Specifically, if a VC firm has no senior partners who have an employment history at a particular underwriter, the VC firm–underwriter pair collaborates,

on average, on less than one IPO. However, the number of collaborative IPOs jumps to almost two when there is one shared (former) employee and to more than three when there are two or more overlapping employees.

Another mechanism that could potentially explain the dynamics of VC-underwriter matching is the investment behavior of the underwriter, as well as pre-IPO services they provide to the VC firm. Figure 2.8 shows that for approximately 15% of IPOs during our sample period, the underwriter either served as a facilitator in a deal (assisting the startup in soliciting investors), or, more directly, was as a late-stage investor. Such intimate ties underscore the hypothesis that an underwriter's loss might not be effortlessly mitigated, which could lead to the impacts on returns and IPOs that we have documented.

These relationships seem to be founded not only on the employment history between the two entities, as shown in the previous figure, but also on their joint engagement in deals. Thus, this multi-faceted involvement of the underwriter in the operations of the VC firm further intensifies the implications of the loss of an underwriting relationship.

2.5.0.2 Underwriter practices and preferences

Another factor that could add friction to a VCs search for a new underwriter following the loss of a relationship are the business practices of the underwriting firms. That is, underwriters may vary in their preferences for industry, issue size, IPO frequency, and the type of due diligence they perform prior to agreeing to "audition" for the underwriting role.

As the general partners at the VC interact with an underwriter, they learn about their specific preferences and practices, which in turn facilitates the IPO process. In addition, this learning may incentivize the GPs to adapt their own *standard* procedures to match those of the underwriter. Consequently, if a repeat underwriter undergoes a merger or closes in a bankruptcy, the GP faces uncertainty about the preferences of potential replacements. Because the GP's *ability to accommodate new underwriter preferences* is constrained by the pair's abbreviated track record, the GP may find it difficult to match with a new underwriter

soon after a lost relationship.

This channel is difficult to test because much of this underwriter heterogeneity is unobserved. As discussed in Subsection 2.4.3.1 and presented in Tables B13 and B14, a key variable that we *can* observe is the underwriters' revealed preferences for the frequency and size of IPOs. Using these preferences, we showed that VCs that lose underwriters that dealt in smaller or more frequent IPOs suffered more from the loss of a relationship.

2.5.0.3 Underwriter expertise

The final mechanism we discuss that relates VC–underwriter relationships to VCs' IPO propensity is the expertise of the underwriter. As discussed briefly in Subsection 2.4.1.2, if a startup's industry matches the underwriter's specialty, an IPO is more likely for the startup. There are several reasons for this. First, there may be lower search costs for the startup and the VC firm, as, holding all else equal, a specialized underwriter will be more willing to accept IPOs in their area of expertise.[11] Second, the underwriter may be recognized by IPO investors as a superior institution—one that is highly skilled in sorting opaque startups of that particular type. As a result, the expected returns of the IPO are higher and the IPO exit option for the portfolio company is more attractive to the VC firm. This form of underwriter value-add is different from the stories above in that it is specific not necessarily to the VC–underwriter pair but to the underwriter and the characteristics of the startup.

We explore this channel in several ways. The first is using VC firm fixed effects (see Tables 2.4 and B3). VC firms tend to manifest a high degree of industry specialization [Gompers et al. (2009)]. Therefore, if the main results of this chapter are entirely driven by the loss of industry-specialist underwriters, we would expect much of this variation to be picked up at the VC firm level. However, the inclusion of VC fixed effects strengthens our main results.

The startup-VC-level data also allows us to test the industry expertise mechanism. By

[11] See e.g., https://www.cnbc.com/2018/11/09/jp-morgan-wants-to-lead-tech-ipos-vs-morgan-stanley-and-goldman-sachs.html; https://www.svbsecurities.com/healthcare-investment-banking; https://www.axios.com/2023/03/11/silicon-valley-bank-rip

taking the data to this level, we can directly control for the shocks to startup industry environments in each year. Under the expertise channel, we would expect that when an important industry-specialized underwriter (or team at the underwriter) closes, the effect on the likelihood of a public offering for startups in that industry will be absorbed by industry-by-year fixed effects. However, after including these fixed effects, we find neither a change in the significance of our estimate nor a substantial drop in magnitude.

Further, while this test helps to rule out the possibility that our results are mainly driven by underwriters' industry expertise, there may be other, unobservable startup characteristics that underwriters specialize in. If any of these unobservable characteristics drive the prior VC firm–underwriter interaction, then underwriter expertise/specialization may still be a viable interpretation of our results. Nevertheless, we maintain that our main results are still consistent with the importance of underwriting relationships with VCs for startup exits via an IPO.

2.6 Conclusion

Despite substantial evidence that venture capital firms add value to their portfolio companies, the sources of such value are not well understood, nor do we have a clear understanding of how different frictions impact this value creation. In this chapter, we present evidence that exogenous shocks to the underwriting relationship impact value creation by reducing access to IPOs. We test this effect in numerous ways and show how a variety of characteristics such as VC firm size amplify the VCs exposure to lost underwriter relationships.

Our results raise intriguing questions about the repercussions of underwriter consolidation and failures, as well as the downstream effects these might have on the employees of startup firms. For instance, as we show in Panel D of Figure 2.8, the recently bankrupt Silicon Valley Bank played dual roles as both an active late-stage syndicate partner and an underwriter for many VC firms. How such closures may affect not only the VC firm itself (see Table B16 for

the list of VCs most exposed to Silicon Valley Bank's closure), but also the career trajectories of startup employees is a fruitful area for future research.

Table 2.1: **Summary statistics**

This table presents statistics of venture capital firms at the firm level and at the firm-quarter level. We show means, standard deviations, minimum values , medians, and maximum values. We define all variables in the appendix. To mitigate the influence of outliers, we winsorize AUM and DealSize at the 1% and 99% levels.

	N	mean	std	min	median	max
Panel A: VC-level statistics (N = 1,255)						
Founding Year	1,072	1995.61	12.67	1833.00	1998.00	2014.00
$\mathbb{1}$(HQ=CA)	1,255	0.34	0.47	0.00	0.00	1.00
AUM ($M)	660	1,343.26	3,090.08	0.00	362.34	24,000.00
Panel B: VC-quarter level statistics (N = 26,343)						
VC Firm Age	26,343	17.40	11.46	0.00	15.00	176.00
VC Fund Age	16,343	5.25	2.38	0.00	5.25	10.00
$\mathbb{1}$(New Fund)	26,343	0.14	0.35	0.00	0.00	1.00
# Underwriters	26,343	1.92	0.59	0.69	1.79	4.06
$\mathbb{1}$(Lost Underwriter)	26,343	0.35	0.48	0.00	0.00	1.00
FracClosed	26,343	0.07	0.13	0.00	0.00	1.00
# Investments	26,343	4.43	7.68	1.00	1.00	223.00
DealSize ($M)	26,343	9.39	16.89	0.00	0.00	99.31
% Healthcare	26,343	0.15	0.32	0.00	0.00	1.00
% IT	26,343	0.21	0.32	0.00	0.00	1.00
% California	26,343	0.20	0.35	0.00	0.00	1.00
% US	26,343	0.41	0.47	0.00	0.00	1.00
# Acquisitions	26,343	0.47	0.96	0.00	0.00	15.00
# IPOs	26,343	0.16	0.44	0.00	0.00	5.00

Table 2.2: **Covariate balance**

This table presents statistics comparing VC firms with at least one underwriter closure to VC firms with no closures. *Lost Underwriter* indicates VC firms that experience the closure of at least one underwriting partner over the sample period. We report means for each characteristic, as well as the t-stat of the difference between the two groups. We define all variables in the appendix. All continuous variables were winsorized on the right tail at the 1% level. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	Lost Underwriter	No Lost Underwriter	
	Mean	Mean	t-stat
Panel A: VC Firms			
Founding Year	1994.69	1993.19	1.51
$\mathbb{1}$(HQ = CA)	0.39	0.37	0.38
AUM ($M)	2,405.08	3,006.97	-0.46
VC Firm Age	14.59	12.90	2.24**
Fund Age	5.02	4.83	0.72
$\mathbb{1}$(New Fund)	0.18	0.14	1.33
Panel B: Investments			
% Healthcare	0.13	0.15	-0.94
% IT	0.25	0.23	0.75
% California	0.21	0.20	0.41
% US	0.43	0.41	0.62
Deal Size ($M)	11.59	9.48	0.81

Table 2.3: **Association between underwriter closure and IPOs (Poisson regression)**

This table presents results testing the association between closures of a VC firm's underwriters and the IPO frequency of startups in the VC firm's portfolio. We present the coefficients from Poisson regressions at the VC firm–quarter level, with standard errors in parentheses. Standard errors are clustered at the VC firm level. The sample includes 1,255 VC firms that have had recent interactions (in the past five years) with at least one underwriter through an IPO. The dependent variable is the number of IPOs backed by the VC firm in a given quarter. The primary independent variable, *FracClosed*, is the percentage of a VC firm's recent underwriters that have closed in the previous 20 quarters. Additional independent variables are defined in the appendix. Note that the number of observations may vary across columns due to missing information in the *RegionVCSupply* variable for some VC firms. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	# IPOs			
	(1)	(2)	(3)	(4)
FracClosed	-3.618***	-3.140***	-3.668***	-3.601***
	(0.314)	(0.292)	(0.307)	(0.300)
NumPartners	1.037***	1.064***	1.192***	0.867***
	(0.044)	(0.048)	(0.052)	(0.045)
USDummy			0.492***	0.629***
			(0.118)	(0.176)
CaliforniaDummy			0.048	0.036
			(0.099)	(0.087)
RegionVCSupply			0.228	-0.011
			(0.216)	(0.191)
VCAge				-0.272**
				(0.134)
MostRecentFundSize				0.163*
				(0.097)
NumInvestments				-0.026
				(0.113)
AvgInvestmentSize				1.186***
				(0.162)
Quarter Fixed Effects	NO	YES	YES	YES
Pseudo R-squared	0.09	0.15	0.17	0.19
No. observations	26,343	26,343	21,319	21,269

Table 2.4: **Relationship between closures of a startup's VC firm's underwriters and IPOs**

This table presents results testing the association between an underwriter's closure and the exit outcomes for startups associated with the venture capital (VC) firm that subsequently lost an underwriting relationship. We present coefficients from OLS regressions run at the portfolio firm–VC firm level, with standard errors in parentheses. Standard errors are clustered at the VC firm level. The sample comprises 13,340 startups. This set of VC firms is identical to those in earlier tests. The dependent variable is an indicator for the portfolio firm going public by the second quarter of 2021. The independent variable of interest is *Treated*, which is an indicator taking a value of 1 if, over the life of the startup, at least one of the underwriters associated with the investing VC firm closes. In particular, the life of the startup encompasses the years from the VC firm's first investment date in the startup and the exit date. We include VC firm and industry-year fixed effects. The other independent variables are indicators for startups headquartered in California, the natural log of the number of unique VC firms that have invested in the startup, and the natural log of the number of unique underwriters with whom the VC firms had recently (in the previous five years) interacted. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	$\mathbb{1}$**{IPO}**			
	(1)	(2)	(3)	(4)
Treated	-0.042***	-0.054***	-0.047***	-0.047***
	(0.009)	(0.007)	(0.007)	(0.006)
CADummy		-0.055***	-0.058***	-0.053***
		(0.005)	(0.005)	(0.005)
NumVCs		0.038***	0.039***	0.038***
		(0.001)	(0.001)	(0.001)
NumUnderwriters		-0.001***	0.001***	-0.001***
		(0.000)	(0.000)	(0.000)
VC Fixed Effects	NO	YES	NO	YES
Industry-Year Fixed Effects	NO	NO	YES	YES
R-squared	0.003	0.281	0.235	0.328
No. observations	30,267	30,267	30,267	30,267

Table 2.5: **Association between underwriter closure and proportion of IPO exits (OLS regression)**

This table presents results testing the association between closures of a VC firm's underwriters and the fraction of a VC's startup exits that were IPOs. We present coefficients from OLS regressions run at the VC firm–quarter level, with standard errors in parentheses. Standard errors are clustered at the VC firm level. The sample includes 1,255 VC firms that have had recent interactions (in the past five years) with at least one underwriter through an IPO. The dependent variable is the fraction of the VC's startup exits from the past year that were IPOs. We drop all observations from quarters where the VC did not have any startup exits in the previous four quarters. The primary independent variable, *FracClosed*, is the percentage of a VC firm's recent underwriters that have closed in the previous twenty quarters. The other independent variables are defined in the appendix. The number of observations varies across columns because some VC firms have missing information in the *RegionVCSupply* or *VCAge* variable. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	# IPOs / (# IPOs + # Acquisitions)			
	(1)	(2)	(3)	(4)
FracClosed	-0.523***	-0.441***	-0.462***	-0.408***
	(0.030)	(0.030)	(0.030)	(0.026)
NumPartners	0.016**	0.015**	0.026***	0.085***
	(0.006)	(0.006)	(0.007)	(0.007)
USDummy			0.061*	0.038
			(0.032)	(0.026)
CaliforniaDummy			-0.022	-0.014
			(0.016)	(0.012)
RegionVCSupply			0.002	0.004
			(0.004)	(0.003)
VCAge				-0.090***
				(0.008)
MostRecentFundSize				0.000
				(0.003)
NumInvestments				-0.096***
				(0.006)
AvgInvestmentSize				0.037***
				(0.005)
Quarter Fixed Effects	NO	YES	YES	YES
R-squared	0.053	0.110	0.113	0.240
No. observations	18,867	18,867	15,403	15,395

Table 2.6: **Test for association between the closure of Lehman Brothers/Bear Stearns and IPOs (Poisson regression)**

This table presents results testing the association between closures of a VC firm's underwriters and the IPO frequency of startups in the VC firm's portfolio. In this table, we limit the closure events to the unexpected and sudden closures of Lehman Brothers and Bear Stearns. We present coefficients from Poisson regressions run at the VC firm–quarter level, with standard errors in parentheses. Standard errors are clustered at the VC firm level. The sample includes 1,255 VC firms that have had recent interactions (in the past five years) with at least one underwriter through an IPO. The dependent variable is the number of IPOs backed by the VC firm in that quarter. The primary independent variable, *CrisisBankFlag*, is an indicator for whether the VC firm was associated with Lehman Brothers or Bear Stearns. A VC is associated if it backed a firm that went public, and Lehman Brothers or Bear Stearns was the underwriter in the five years before their closures in 2008. The other independent variables are defined in the appendix. The number of observations varies across columns because some VC firms have missing information in the *RegionVCSupply* or *VCAge* variable. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	# IPOs			
	(1)	(2)	(3)	(4)
CrisisBankFlag	-0.754***	-0.261**	-0.336***	-0.307***
	(0.108)	(0.110)	(0.112)	(0.108)
NumPartners	1.047***	1.040***	1.166***	0.841***
	(0.044)	(0.047)	(0.050)	(0.044)
USDummy			0.433***	0.571***
			(0.111)	(0.171)
CaliforniaDummy			0.040	0.037
			(0.099)	(0.088)
RegionVCSupply			0.192	-0.065
			(0.216)	(0.195)
VCAge				-0.412***
				(0.132)
MostRecentFundSize				0.144
				(0.098)
NumInvestments				-0.039
				(0.114)
AvgInvestmentSize				1.250***
				(0.165)
Quarter Fixed Effects	NO	YES	YES	YES
Pseudo R-squared	0.08	0.14	0.16	0.18
No. observations	26,343	26,343	21,319	21,269

Table 2.7: **Percent of IPO underwriter employees that work at acquiring underwriter**

The following table documents the employment patterns of bank employees working at an IPO underwriter that was acquired by another IPO underwriter. We use LinkedIn data from CoreSignal, which covers 144,771 employees of target underwriters. These acquisitions are identified from PitchBook data spanning 2017-2020, matching the years when we have CoreSignal data. We calculate the percentage of target employees who also worked for the acquiring underwriter following the merger. Panel A shows the fraction of all employees working for the target following the merger. In Panel B, we classify job titles by seniority and show turnover for the different groups. In Panel C, we classify employment experiences based on whether job titles contain keywords common to titles of bank employees who work in IPO underwriting. See B.2 for detail on how we classify these job titles.

	% Acquirer Employed	N
Panel A: All employees		
	23.4%	144,771
Panel B: Sorted by title seniority		
Senior	27.3%	31,306
Middle	22.6%	31,566
Lower	16.0%	24,794
Uncategorized	24.8%	57,105
Panel C: IPO title keywords		
Yes	26.6%	7,583
No	23.6%	137,188

Table 2.8: **Impact of underwriter closure on VC fund performance**

This table presents the results of a regression analysis examining the relationship between the performance of a VC fund, as measured by the natural log of the fund's total value to paid-in capital ratio (TVPI), and the closure of underwriting partners associated with the investment firm managing the fund. An underwriting partner is an underwriter that previously collaborated on an IPO with the VC investment firm managing the fund. The unit of observation in the analysis is a VC fund, with data consisting of 954 funds associated with 296 VC firms. The dependent variable is the natural log of the last reported TVPI (fund return data is as of the second quarter of 2021), and the independent variable is the proportion of the investment firm's recent underwriters that closed during the lifetime of the fund. The lifetime of the fund is defined as the time period between the fund's vintage year and the year we observe the last reported TVPI. The analysis controls for the total number of underwriting partners the VC firm had on previous IPOs as of the fund's vintage year, and also includes VC firm and vintage year fixed effects. The data includes all funds with documented relationships with at least one underwriter, and excludes funds with missing return data. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	log(TVPI)			
	(1)	(2)	(3)	(4)
FracClosed	-0.077	-0.263	-0.488**	-0.776***
	(0.129)	(0.167)	(0.246)	(0.252)
NumUnderwriters		0.009***	0.017***	0.006
		(0.002)	(0.006)	(0.005)
Vintage Year Fixed Effects	NO	YES	NO	YES
VC Fixed Effects	NO	NO	YES	YES
R-squared	0.001	0.244	0.460	0.603
No. observations	954	954	954	954

Table 2.9: **Association between underwriter closures and future IPO underwriting fees**

This table presents the results of a regression analysis examining the relationship between the closure of a VC firm's underwriting partners and the underwriting fees paid by the VC firm in future IPOs. The data is at the VC firm–IPO level. We include the subset of VC firms from the original sample that we could match to a VC firm involved in an IPO from SDC Platinum, leaving us with 516 unique VC firms participating in IPOs from 2006 to 2016. The dependent variable is the gross spread paid to underwriters as a percentage of the share price. The independent variable is *Treated*, an indicator that equals one if the VC firm lost at least one underwriter before the year of the IPO. We account for the IPO's size by including the natural log of IPO proceeds, *IPOProceeds*, as a control variable in the regressions. We add IPO quarter and VC firm fixed effects sequentially in columns (2) and (3). We cluster standard errors at the VC firm level. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	PerShareUnderwriterSpread		
	(1)	(2)	(3)
Treated	-0.021	0.009	0.223***
	(0.038)	(0.039)	(0.054)
IPOProceeds	-0.640***	-0.639***	-0.536***
	(0.038)	(0.040)	(0.053)
Quarter Fixed Effects	NO	YES	YES
VC Fixed Effects	NO	NO	YES
R-squared	0.084	0.215	0.333
No. observations	8,158	8,158	8,158

Table 2.10: **Association between underwriter closures and IPO underpricing**

This table presents results relating the closure of a VC firm's underwriting partners to underpricing in the future IPOs of startups in the VC firm's portfolio. The data is at the VC firm–IPO level, with 2,359 observations. We include the subset of VC firms from the original sample that took a firm public that we could match to IPO data from IPOScoop (https://www.iposcoop.com/). The dependent variable is the first-day percentage change in the startup's stock price. The independent variable is *Treated*, an indicator for the associated VC firm's losing at least one underwriter before the year of the IPO. We add IPO quarter and VC firm fixed effects sequentially in columns (2) and (3). Standard errors are clustered at the VC firm level. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	Day1%PriceChange		
	(1)	(2)	(3)
Treated	3.325*	3.385*	2.187
	(1.827)	(1.960)	(5.280)
Quarter Fixed Effects	NO	YES	YES
VC Fixed Effects	NO	NO	YES
R-squared	0.002	0.102	0.517
No. observations	2,359	2,359	2,359

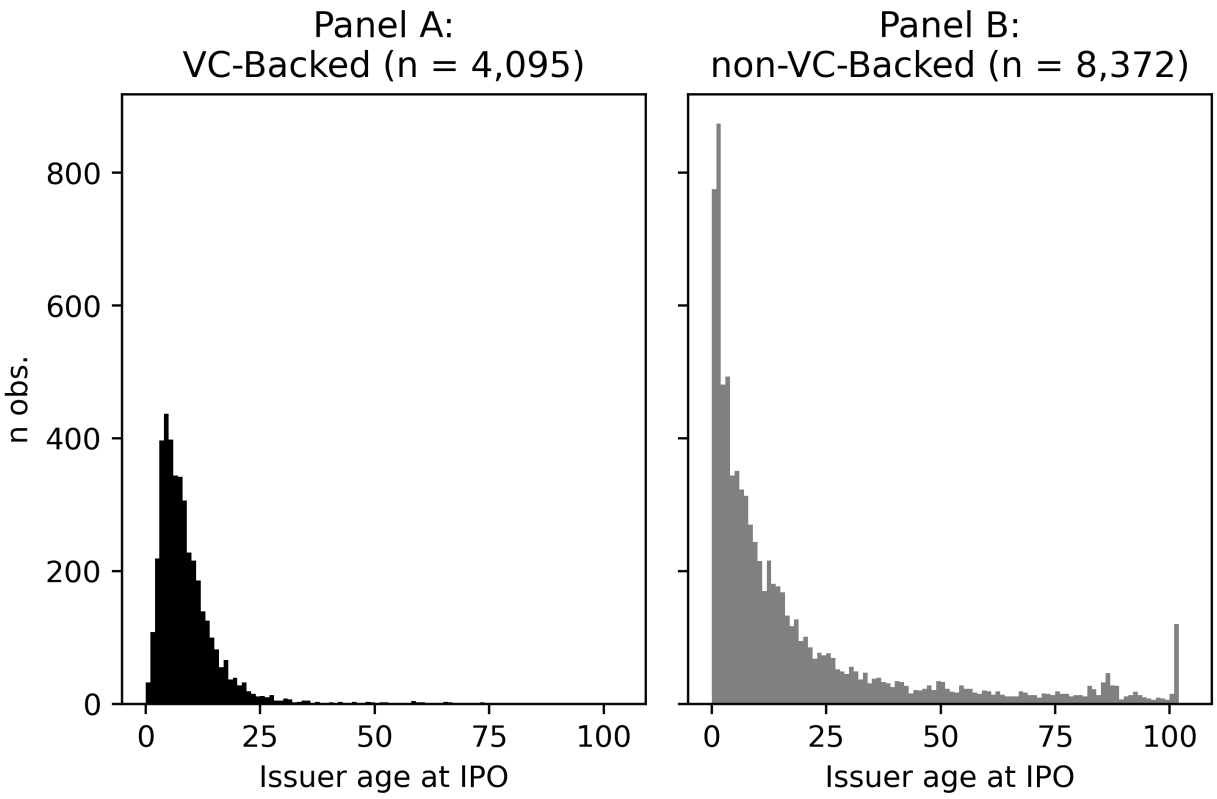


Figure 2.1: **Underwriter closures/acquisitions and future IPOs**

This figure plots the density of the age of IPO issuer firms, split by VC-backed status. The data is a collection of 12,467 non-SPAC US IPOs from 1975-2023 containing issuer founding date and VC-backed status, which we dowloaded from Jay Ritter's website. The X-axis is issuer age at the time of IPO (which we truncate at 101 years), and the Y-axis shows the frequency for the group. Panel A shows the histogram considering only VC-backed issuers, and Panel B shows the histogram for non-VC-backed issuers. The median (mean) age for the VC-backed group is 7 (8.76) years, and the median (mean) age for the non-VC-backed group is 8 (18.18) years.

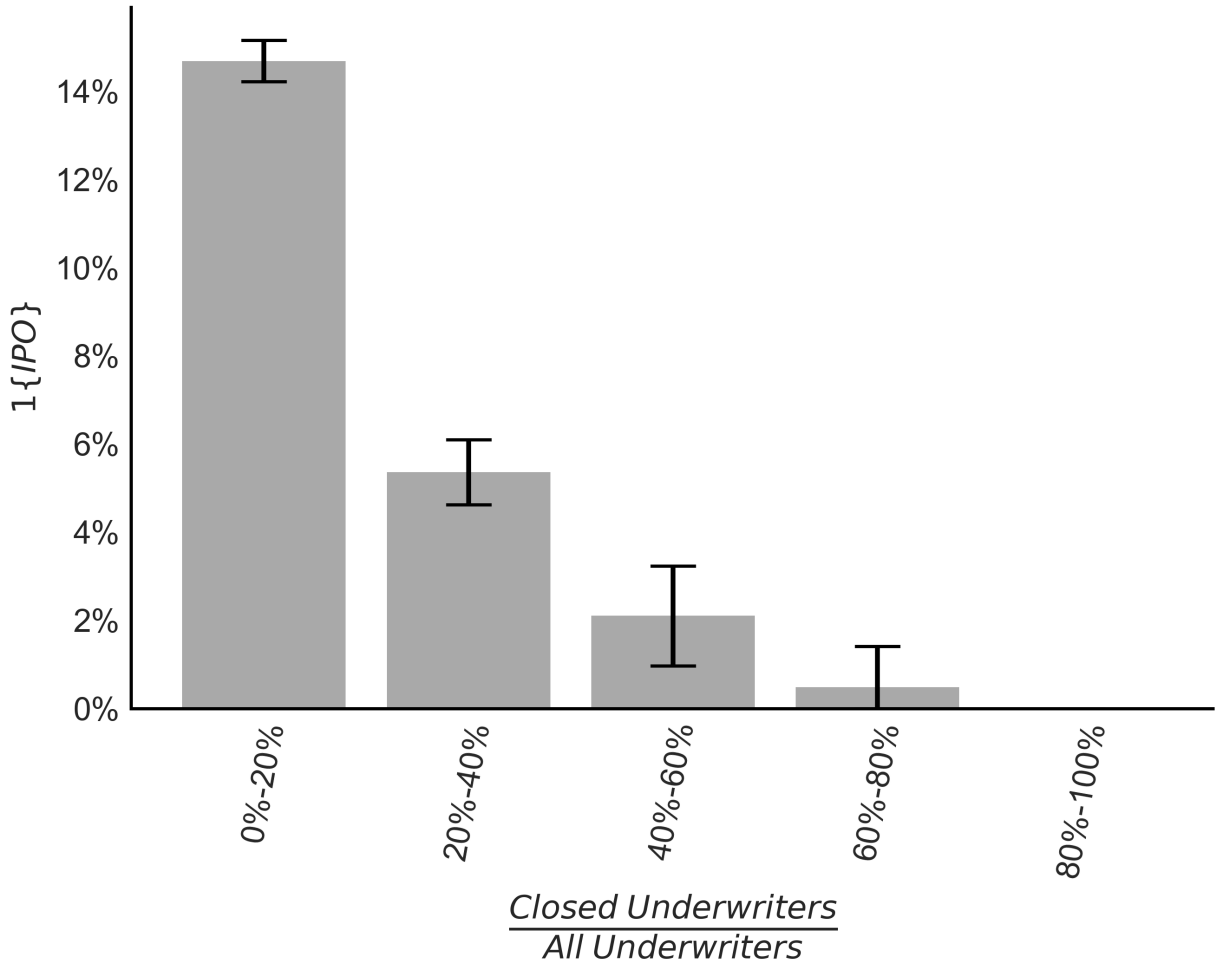


Figure 2.2: **Underwriter closures/acquisitions and future IPOs**

This figure plots averages of a quarterly indicator variable for a startup in a VC firm's portfolio undergoing an IPO, $1\{IPO\}$, against the fraction of a venture capital firm's underwriters that closed or were acquired over the previous five years. Data is at the VC firm quarter level. The X-axis displays the proportion of the VC firm's underwriters from the last five years that have closed as a result of bankruptcy or acquisition. The Y-variable is calculated by averaging a binary variable that assigns a value of 1 if a portfolio company conducts an IPO in that quarter, and 0 if not. Averages are taken over five bins of the X-value. Tick marks surrounding the bars show the 95% confidence intervals.

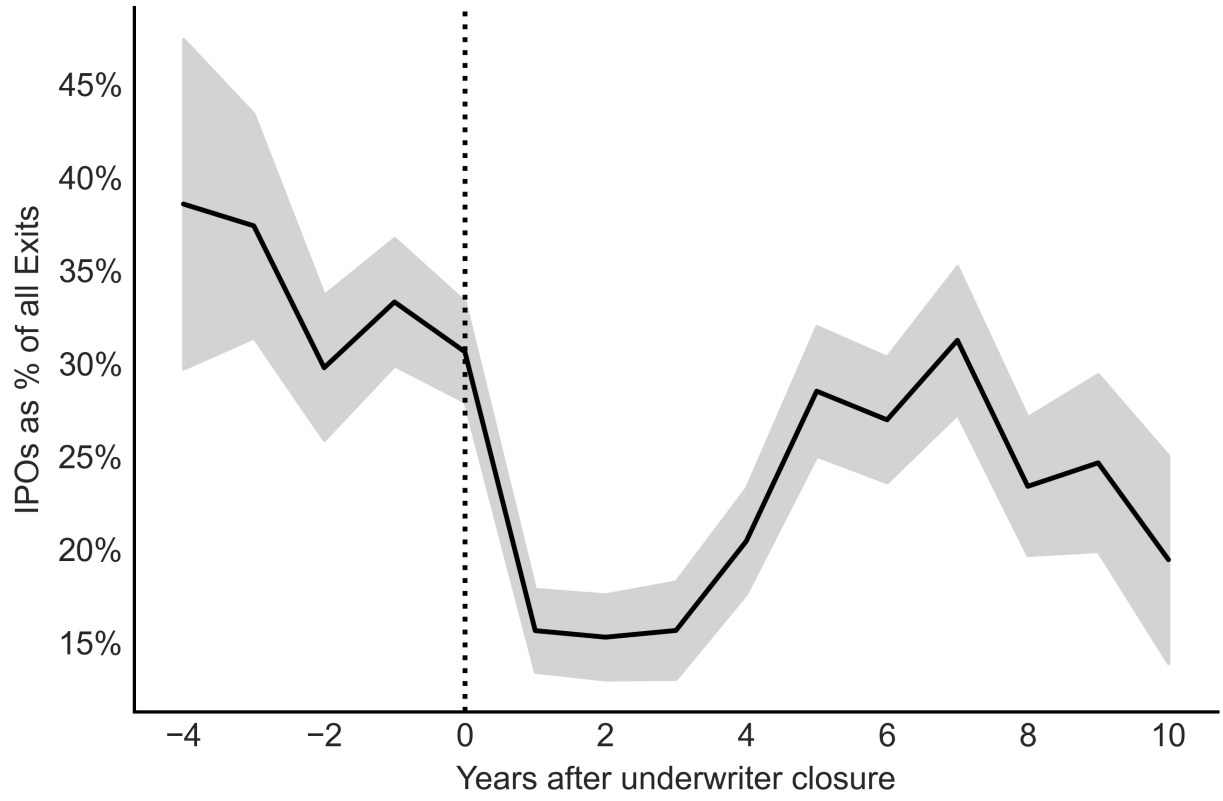


Figure 2.3: **IPOs as a fraction of total exits and underwriter closure**

This figure plots the average fraction of a VC firm's exits that were IPOs in event time relative to the earliest closure of a recent underwriter. Data was assembled at the event time–VC firm level for all treated VC firms, using the earliest closure of an underwriting partner as the event. The Y-axis shows the average of #IPOs/(# Acquisitions + # IPOs) for each event year. # IPOs and # Acquisitions represent the number of IPOs and acquisitions of startups in the VC firm's portfolio, respectively. The shaded areas represent 95% confidence intervals.

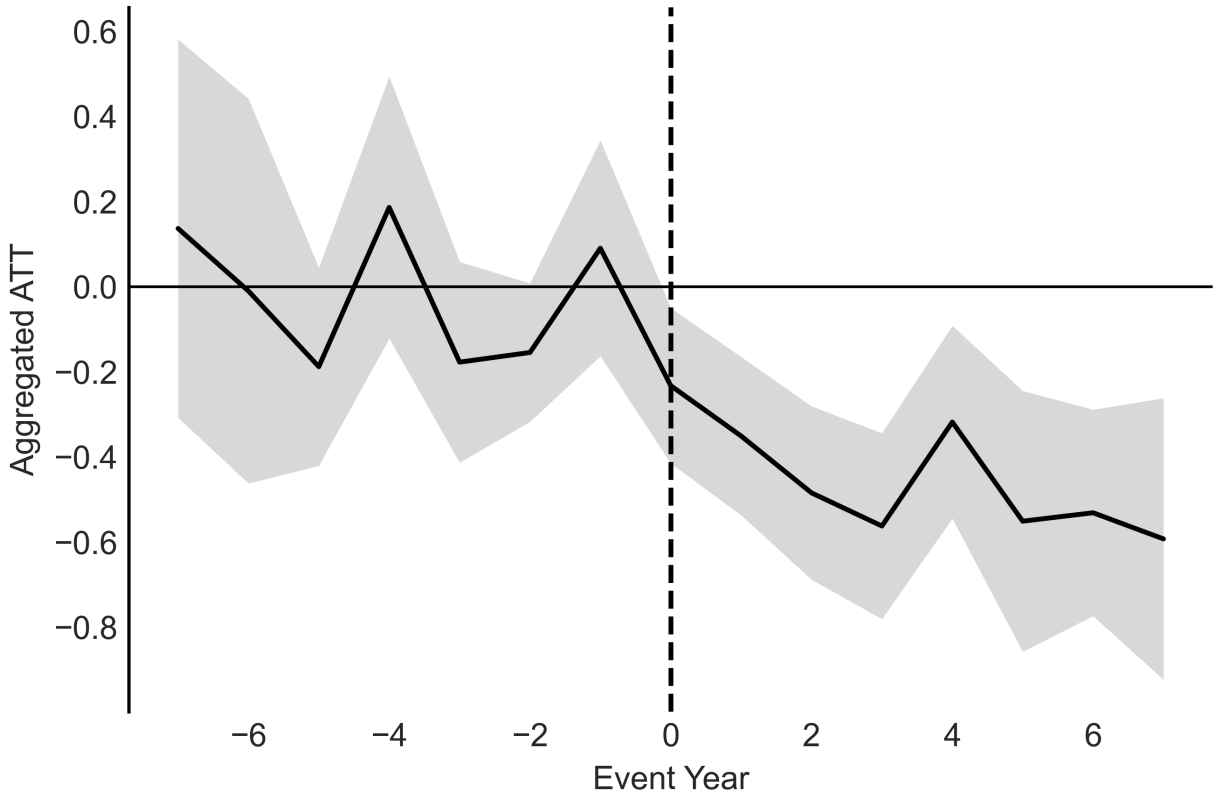


Figure 2.4: **Staggered timing and heterogeneous treatment effects**

This figure plots the average treatment effect on the treated (ATT) estimates proposed by Callaway and Sant'Anna (2021) of underwriter closures on IPO frequency. Because the closures are staggered and likely heterogeneous, we calculate doubly-robust ATT estimates at the year and treatment cohort level, which we aggregate at the event-time level. We define a VC firm's treatment cohort as the year of its first underwriter closure. The dependent variable is the number of startups the VC firm takes public in a given year. In our estimation procedures, We use only observations from not-yet-treated VCs as control, but results are qualitatively robust to also including never-treated observations in the control group.

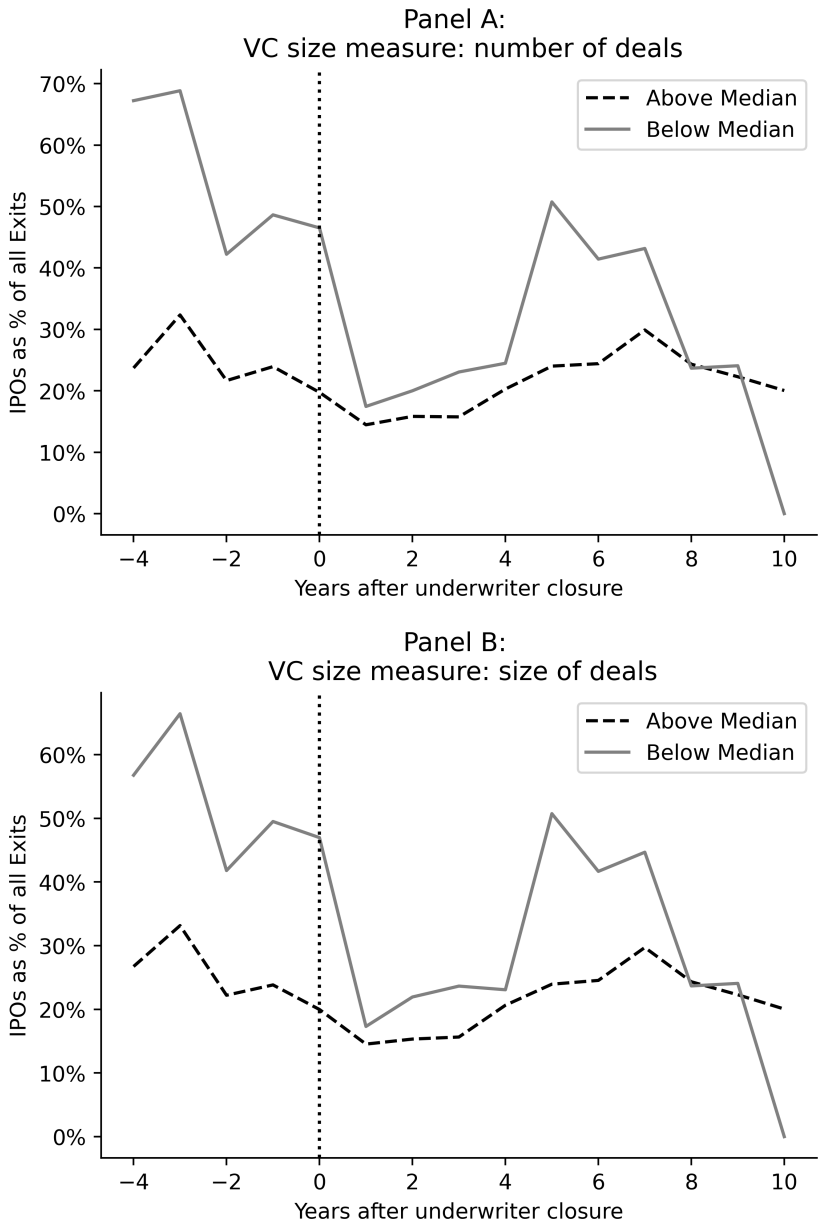


Figure 2.5: **VC exit outcomes by VC size**

This figure plots the average fraction of a VC firm's exits that were IPOs in event time relative to the earliest closure of a recent underwriter. Outcomes are split based on whether the VC is above or below median size. Data was assembled at the event time–VC firm level for all treated VC firms, using the earliest closure of an underwriting partner as the event. The Y-axis shows the average of #IPOs/(# Acquisitions + # IPOs) for each event year within a size group. # IPOs and # Acquisitions represent the number of IPOs and acquisitions of startups in the VC firm's portfolio in a given year, respectively. Size is measured by the number of past deals done by the VC firm in Panel A, and is measured by the average size of past deals done by the VC firm in Panel B. The solid gray lines represent averages of the below-median VCs, and the dashed black lines represent averages of the above-median group.

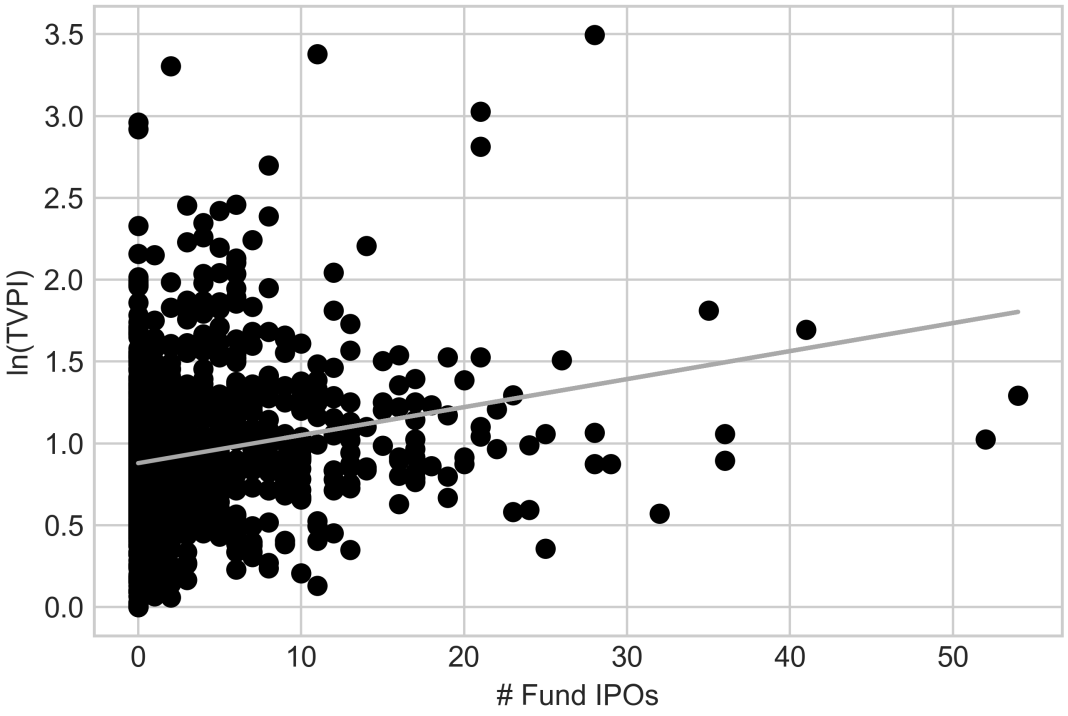


Figure 2.6: **Fund IPOs and returns**

This figure plots the number of startups in a VC fund's portfolio that go public through an initial public offering (*# Fund IPOs*), against the natural log of the fund's total value to paid-in capital multiple (*Ln(TVPI)*). TVPI is the sum of net asset value of deals the fund is still holding plus the value of all fund distributions to date, divided by the total commitments paid into the fund.

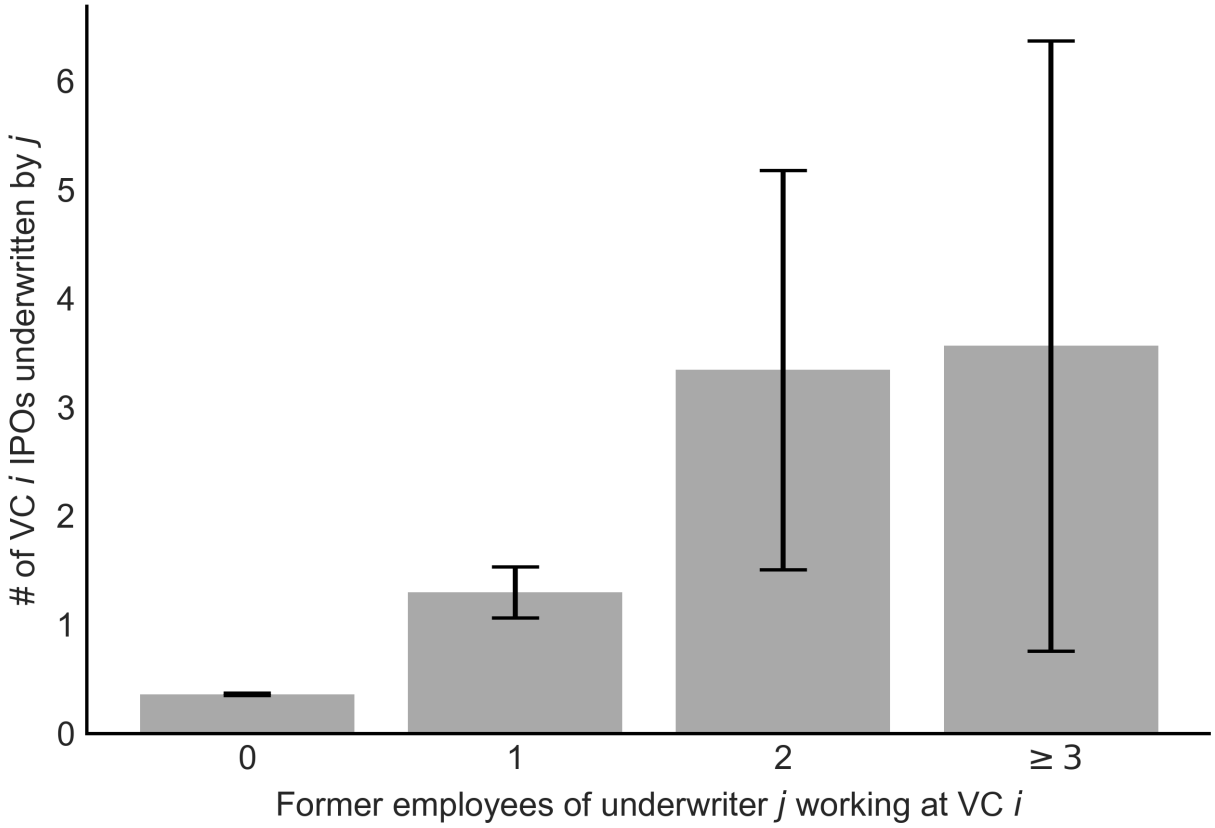


Figure 2.7: **IPO matching and employment overlap**

This figure plots the relationship between the overlap in personnel between the underwriter and VC firm and the number of IPO deals in which the underwriter and VC firm jointly participate. Here, we use LinkedIn data detailing the employment history of the people listed as senior partners at the VC firm in PitchBook. The dataset used to generate the graph is at the VC firm (i) underwriter (j) level. The X-axis shows the number of senior partners at i that have employment history at j prior to joining the VC firm. The Y-axis documents the number of startups backed by firm i, whose IPO was underwritten by underwriter j. We form the Y-values by averaging over bins of the X-values, which we have grouped into four bins. The tick marks surrounding the bars display 95% confidence intervals.

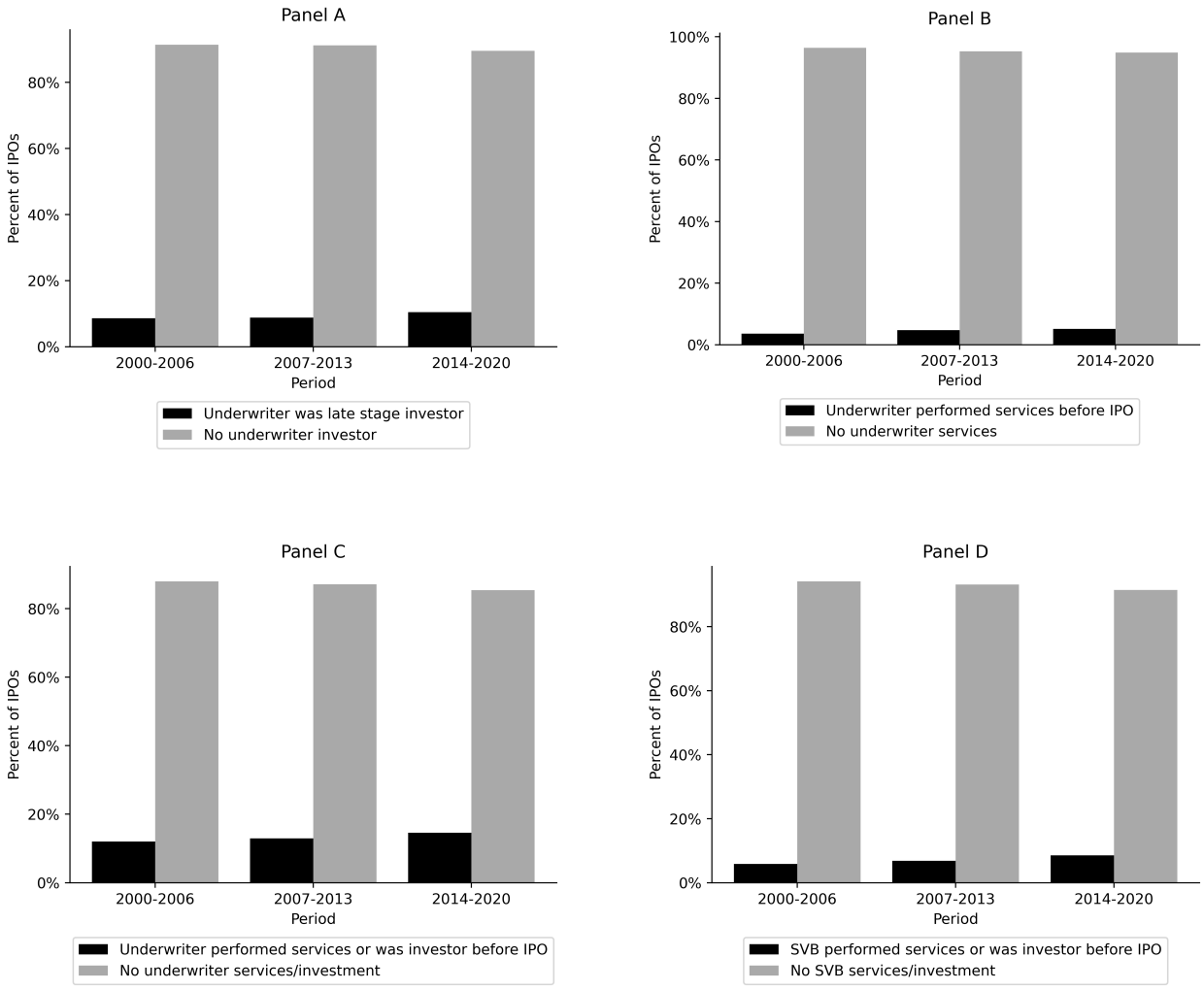


Figure 2.8: **Pre-IPO VC-underwriter collaboration**

In this figure, we chart the percentage of VC-backed startups that went public from 2000 to 2020 where the VC firm had pre-IPO interactions with at least one IPO underwriter. Panel A shows the proportion of Initial Public Offerings (IPOs) that involved startups that had received a late-stage investment from at least one of their IPO underwriters prior to the IPO itself. Panel B shows the percentage of IPOs where the startup, through their VC firm, received some form of general service from an IPO underwriter before the IPO. These services could include consulting, business banking, or legal advisement, among others. Panel C shows the proportion of startups that received either an investment or a service from an IPO underwriter prior to the IPO. Panel D shows the proportion of startups whose IPOs were underwritten by Silicon Valley Bank (SVB), where SVB was also a late-stage investor or provided banking services to the startup prior to the IPO.

CHAPTER 3

Do Venture Capital Networks Discourage Investment?

3.1 Introduction

Venture capital (VC) financing constitutes a critical step in determining the outcomes for many young firms. To put this into perspective, an estimated 60% of non-financial firms that went public from 1999 to 2009 were VC-backed (Kaplan and Lerner, 2010). Understanding how VC firms source and execute investments is therefore of paramount importance. Informal networks among managers of VC funds are perhaps the most important way VC firms find firms to add to their portfolio. Indeed, surveys of VC general partners indicate that more than 51% of all VC deals are sourced from other investors and professional networks (Gompers et al., 2020). Very large VC funds (what I hereafter refer to as super-large VCs) wield enormous clout as central nodes in such networks. Not only can they deploy larger amounts of capital, but they may also serve as gatekeepers of new investment opportunities to smaller VC funds. What happens when a VC firm is forced to choose between keeping strong ties with super-large VCs and an investment in a startup that it deems worthy of finance?

In this chapter, I attempt to answer this question using the VC finance outcomes of startup firms that compete with startups recently backed by a super-large VC fund. I define a super-large VC fund as a fund with assets under management (AUM) of greater than $500 Million. If VC finance decreases for these competitors, it may be because other VC funds

are hesitant to alienate super-large VC funds by investing in a firm whose operations may lower the large fund's profits. I find that the quarterly probability of receiving VC finance in periods directly after the super-large VC investment for these firms declines by 0.84% relative to similar firms in other industries, while quarterly VC finance drops by $22,700. When compared to the pre-event quarterly averages for VC finance probability and amount, this roughly corresponds to a decline by 15 and 5%, respectively. This effect is the strongest in the periods immediately following the super-large VC investment. Using a placebo set of small VC investments and their competitor startups, I find no change in VC finance outcomes.

Of course, it may be that these firms were of lower quality at the time of the investment or made to be so because of the investment. Iyer and Manso (2022) capture this dynamic, modelling a strategic recommender who influences product quality, creating a feedback effect. If this indeed happens in the super-large VC setting, other sources of finance may also drop or impose harsher terms than before. Under the hypothesis that the drop in VC finance was caused by network considerations, however, I would not expect outside finance to also decrease immediately. Upon examination, bank and certain government loans for the competitor startups do not meaningfully change after the investment. Average interest rates remain flat through the event. I interpret this consistency in non-VC finance to reflect similar fundamental quality of the competitor firms. Differences in the contract terms and security design between debt and equity lead to limitations of this analysis. To address these limitations, I provide evidence to suggest that these firms have similar return outcomes to those that received super-large VC investment.

Under the networks hypothesis, I anticipate that VC firms with a syndication history with a super-large VC fund would be more likely to reduce funding for competitors than other VCs. Consequently, I would expect that the drop in VC finance will be especially strong for competitor startups that aspire for follow-on finance from such a networked-in VC fund. Such aspirants must include those previously backed by a VC fund that syndicates with the

super-large VC. I find that such prior backing does indeed predict a stronger drop in VC finance than other firms.

What factors do VCs take into consideration when contemplating investment in a small firm? Of course, a first order concern for the general partners in the VC firm is to select deals likely to offer the highest return to their principals, the limited partners of the fund. General partners (GPs) therefore consider the qualities of the potential investments that increase the magnitude or likelihood of future payoffs to limited partners. When attention is restricted to a particular investment, such concerns include the GP's assessment of the firm's management team, business plan, product line, competitive environment, and so on. However, VC investments affect future cash flows in ways other than simply those directly attributable to the firm in which the investment was made. Network concerns fall into this category. If a certain investment may damage the VC's relationship with a critical market player, the VC may refrain from providing capital when it otherwise might.

Consider the following example detailing how investments may weaken VC network ties. A mid-size VC firm identifies an NPV-positive investment in private firm A. Suppose that firm B is a direct competitor to firm A. Now imagine that Sequoia Capital (a very large VC), with whom the other VC firm has enjoyed a syndicate relationship, invests in firm B. An investment by the VC into firm A will lower the payoffs from Sequoia's investment into firm B. This move may be viewed by Sequoia as a betrayal by the smaller VC of their network relationship. Consequently, Sequoia may be less inclined to share lucrative investment opportunities with the VC firm. Furthermore, any investment in firm A would preclude the VC firm from participating in future rounds of firm B financing, which now has a greater chance of being a home-run investment. Thus, the VC firm must now trade off these lost opportunities against the perceived benefits of investing in firm A when making its decision, in addition to the risks posed by the project before Sequoia's investment. This new factor must now weakly reduce the level of VC investment in firm A. I refer to the explanation of a decline in competitor VC financing due to VC firms' fear of alienating network allies as

the *networks* hypothesis.

The experience of the private firm NextSpace provides a useful example of this type of dynamic in the VC lending space. NextSpace, founded in California in 2008 by entrepreneur Jeremy Neuner, was a company providing shared office space to workers of different companies. As such, NextSpace was a direct competitor of the (soon-to-be) infamous WeWork. From 2011 to 2017, WeWork obtained billions of dollars in VC financing from some of the most recognizable names in venture capital. NextSpace, seen by some as having a more sustainable business plan, also sought VC finance to fuel growth around the same time. NextSpace, however, received less than $1 million in total VC financing, and would go on to a quiet acquisition four years later. How could NextSpace hope to obtain VC finance when an such an investment might be viewed as crossing a VC giant like SoftBank or Benchmark Capital?

A recent article details the plight of Neuner to obtain VC finance in this"cliquish" environment dominated by VC syndicates:

"Venture capitalists began telling Jeremy Neuner that making piddly investments in his company wasn't worth their time... For decades, venture capitalists have succeeded in defining themselves as judicious meritocrats who direct money to those who will use it best. But examples like WeWork make it harder to believe that V.C.s help balance greedy impulses with enlightened innovation.[1] *"*

This type of network friction is investigated in Akerlof and Holden (2016), who construct a model of project-entrepreneur-investor matching. In equilibrium, investors only invest as syndicates in the best-networked entrepreneur, even when other potential entrepreneurs may be more skilled. The key friction in the model is a restriction of investors' investment opportunity sets to those entrepreneurs that they are aware of, which is a function of their informal network. Because *awareness* of potential investment opportunities are limited to what investors learn from their networks, investors prefer to invest in ways that lead to

[1] https://www.newyorker.com/magazine/2020/11/30/how-venture-capitalists-are-deforming-capitalism

stronger networks rather than in skilled entrepreneurs alone. In my chapter, I seek to determine whether VC firms ever face such a network-vs-quality trade off in practice.

Network considerations do not constitute the only economic mechanism that would reduce VC finance for competitor firms of super-large VC-backed startups. Imperfect competition among portfolio companies and their financial backers provides another interpretation of this drop. Super-large VCs may operate through various means to quell competition in its portfolio firm's product market. VCs structure financial contracts with portfolio firms to align the entrepreneurs' incentives with their own (Kaplan and Stromberg, 2001). VCs exercise influence on portfolio firm policy through the appointment of board members (Amornsiripanitch et al., 2015). By virtue of its size, the super-large VC may use its resources to restrict competition in the product markets of its investment. Dixit (1980) provides a simple model of duopoly, in which the incumbent firm may engage in costly capacity investment to discourage/reduce the impact of entry on profits. In the previous example of firms A & B, I held fixed the benefits of firm A investment for the VC firm. In this case the super-large VC investment made other VC investment in firm A more costly by increasing opportunity and future search costs. Now suppose a simple cash burning or Dixit-type game is initiated between the VC firm and Sequoia after the investment in firm B. Sequoia may now deploy such measures as investment or cash burning to encourage the exit of firm A from firm B's product market to capture monopoly rents. These measures reduce the intrinsic incentive that the VC firm may have had to invest in firm A in the first place. This competition effect should not, however, depend on whether the other VC firms have a relationship with the super-large VC, a relationship critical to the network explanation. I will exploit this difference to disentangle these two effects.

A very similar, yet distinct, possibility is that other VCs are rationally responding to a series of signals on investment opportunities, the most recent of which is the super-large VC's investment. Anderson and Holt (1997) detail an experiment showing that people often rationally choose to ignore private information that contradicts public signals, in a

process called an information cascade, first documented by Bikhchandani et al. (1992). This is especially true when agents believe that prior signals are drawn with greater accuracy. When the super-large fund's choice signals that one firm may be the best among a set of competitors, the VCs may rationally ignore private information that would suggest an investment in a competitor.

Imperfect competition among startups and information cascades among VCs are examples of mechanisms that lower the attractiveness of competitor firms to other VCs. I refer to this category of explanations for a drop in VC finance as the *value-adding* hypothesis, as the super-large VC enhances the quality of investments rather than selecting those with higher quality ex ante.

Of course, it is entirely possible that the largest of VC firms also have the best ability to screen and investigate potential investment opportunities. All other firms passed over, including and especially the competition, are then less deserving of VC finance. Therefore, selection could also be to blame for an observed decrease in VC finance for these startups. I refer to this interpretation as the *selection* hypothesis

The main tasks of this chapter are two-fold. First, I seek to establish whether a drop in VC finance occurs for competitors of firms recently receiving finance from a super-large VC. Secondly, I seek for evidence which either lends support or casts doubt upon the various economic channels described above. Through the tests I run in the chapter, I find that, indeed, VC finance declines on extensive and intensive margins for competitors of startups recently invested in by super-large VCs. Further, I present results consistent with the hypothesis that networks may be an important factor driving this drop. I do not find evidence supporting the value-add or selection hypotheses.

A growing literature highlights the role that informal VC networks have in the economy. The vast majority of this literature speaks to the benefits of these networks for investor and entrepreneur. As the motivation of the chapter makes clear, networks are an important way in which VCs source potential investments (Gompers et al., 2020). As with other forms of

finance, awareness itself of promising investment opportunities, hindered by spatial distance, is mitigated through VC networks (Sorenson and Stuart, 2001; Hong et al., 2005; Pool et al., 2015). Portfolio firms are more likely to offer favorable contract terms to VCs with "high reputation" (Hsu, 2004b). Entrepreneurial projects with increasing returns, but high startup costs may also incentivize VC firms to form networks and invest jointly (Akerlof and Holden, 2019). The case that there may also be "undeserving" losers from this network apparatus is not as strong. Hochberg et al. (2010) argue that withholding of syndication for a period of time is a standard punishment for VC firms that syndicate with VC entrant firms against cartel wishes. To my knowledge, however, my chapter is the first to argue that VC firms fear retribution from their networks due to startup competition.

Investment in syndicates also seems to boost portfolio firm performance. In fact, Brander et al. (2002) suggest that the value-adding benefit of syndicates outweighs concern for future deal sourcing as the primary reason for VCs syndicate formation. VC networks mitigate problems associated with information asymmetry in a market fraught with uncertainty. To paraphrase Bygrave (1988), the sharing of knowledge stands beside the sharing of investment opportunities as the most important of VC network purposes. Syndicated investments help ensure that VC ownership stakes do not become diluted as a result of future financing rounds (Lerner, 1994a). The portfolio companies of better-networked VCs enjoy a higher percentage of successful exits from investments than do those of other VCs (Hochberg et al., 2007). Furthermore, syndicate-backed portfolio firms receive a higher-premium in M&A type deals and perform better in the long run (Que and Zhang, 2021). My chapter furthers this literature by pointing out that the value-adding benefits of networks may be a factor in discouraging VC investment that could jeopardize the strength of the those VC firms' networks.

This chapter also speaks to the aversion that VC firms may have to common ownership. While many studies find that common ownership of competing firms has anti-competitive consequences (Gilo et al., 2006; Azar et al., 2018), others find a lack of such an effect

(Dennis et al., 2019; Lewellen and Lowry, 2021; Koch et al., 2021). Looking at the VC industry in particular, Li et al. (2021) demonstrate that VC firms which invest in R&D-heavy pharmaceutical companies stifle competition within their portfolio by shutting down particular ventures. Doubling down on an investment that may end up competing with strong opportunities for future investment may not, then, be the best course of action for VCs. My results indicate that VCs refrain from follow-up investments in firms that compete with the startups invested in by a super-large VC with which they are networked. This hesitation could be an artifact of a VC desire to invest in a firm it sees as a rising star combined with a distaste for committing more capital toward a common ownership arrangement.

3.2 Data

My data comes primarily from the PitchBook database of deals related to private firms over the period 1990-2021. This database contains wide coverage of global angel investor, VC, PE, M&A, and IPO markets for entrepreneurial firms. I begin by identifying the list of super-large VC funds as any VC fund with assets under management AUM of greater than $500 million, as of 2021. Summary statistics are provided in Table 3.1, along with those of all other funds in the database for comparison. The main results of this chapter are robust to instead defining the super-large fund as one with AUM of greater than $1 billion. I next identify each investment made by these super-large funds over the sample period. This leaves me with a set of 550 super-large VC funds and 15,382 deals over the sample associated with said funds. These funds, though comprising only 2.85% of all VC funds in the PitchBook database, account for 11.01% of all VC fund-startup investment pairs and 30.94% of the market AUM.

For each firm in the database, I construct a quarterly time series of observations ranging from the period in which the firm was founded until the date of acquisition, IPO, bankruptcy, or last date in the sample (for those firms that are presumably still operating). This panel

includes data on VC financing and other firm characteristics. PitchBook industry data classifies each firm into one of 215 industries and one of 7 sectors. This classification is analogous to one- and three-digit SIC codes, but with fewer categories.

From Thompson's Dealscan database, I obtained private firm debt information. I also use data on the US government's SBA Small Business Innovation Research (SBIR) loans, which is publicly available from the SBIR portal.[2] Since PitchBook, Dealscan, and SBIR do not contain cross-identification information, I merge these three main datasets using a text-based fuzzy matching algorithm. After inspecting the data by hand, I was left with 8577 and 5664 matches with Dealscan and SBIR respectively, 259 and 758 of which appear in the final sample.

3.3 Sample construction & empirical design

In an ideal setting, I would have a pool of identical VC firms, in the sense that they have an equal ability to add value to and screen their investments. Half would be assigned to a group allowed to network and source deals from one another, and the other half would be assigned to a group allowed to source no deals from other VC firms. I would then have a pool of identical startups, which would randomly be assigned a competitor relation to all other startups. The VC firms with the highest degree of network centrality would randomly draw dates in which to invest. Their choice of startups must also be random. I would calculate the before and after difference in each type of VC finance for competitor and non-competitors of the randomly drawn investments. If competitor firms have lower VC finance than non-competitors, I would first conclude that the super-large VC investment caused a decrease in VC finance for competitors. Moreover, if the competitor minus non-competitor difference is even less for networked VC finance than non-networked VC finance, I would also conclude that fear of alienating network-central VCs discouraged VC investment.

As stated in the introduction, the first main task I seek to accomplish is to determine

[2] https://www.sbir.gov/sbirsearch/award/all

whether VC finance rises, falls, or remains constant for competitors of startups invested in by super-large funds after the event. For this first task, the ideal data would suggest a differences-in-differences approach, in which the before and after differences are compared across competitor statuses. My next task is to determine whether I can rule out or fail to rule out networks as contributing to this decrease in VC finance. Again looking at the ideal setting above, an additional difference may be taken into account (i.e. the networked vs. non-networked VC finance). I address this in Section 3.5.

Obviously, the data I have available does not directly correspond to the ideal situation I have described. Certainly, it may be folly to assume that the firms selected by super-large VCs are random (as I also discuss later). However, it may be more reasonable to attempt to identify viable competitors to the firms invested in by the super-large funds. I do so by employing a matching algorithm. For each super-large fund investment, I compile all active portfolio companies in the database in the same industry and state/province as the firm involved in the investment. I exclude any firm associated with a super-large VC deal. For this subset of firms, I calculate the absolute difference between the firm's total funding up to the date of the investment and founding years and those of the portfolio company associated with the super-large VC investment. I then sort the selected firms on the combined z-scores of these absolute differences. Using this measure, I select the closest firm to be a part of the treated group of my sample. The intuition behind this procedure is that, subsequent to the startup receiving super-large VC finance, the VC finance prospects of another firm in the same industry and state with similar characteristics will be the most affected by the investment compared to all other firms. I refer to this group as the *treated* firms.

Table 3.2 presents averages of relevant covariates for different groups of firms. Variables include VC finance, total finance, number of VC, bank, and SBA investments, year of founding, and firm characteristics, such as geography and ownership status. Column (1) firms are those invested in by the super-large VC (I refer to these as "investment firms"). Column (2) firms are competitor firms put in the treated group by the matching algorithm. Column (3)

firms are other PitchBook firms representative of the industries associated with investment firms. Columns (4) and (6) present differences between the investment and treated and investment and other firms, respectively. t-statistics of the differences are reported in columns (5) and (7), respectively. Panel A presents averages taken at the time of the super-large VC investment. Panel B presents averages taken with all available data. Results show that firms matched into the treated (i.e. competitor) group have a financial profile much closer to investment firms than do the remaining firms. t-statistics are smaller across the board for investment/treated firm differences than investment/other firm differences, with the exception of IPO and out-of-business indicators. Similarity is particularly strong at the time of the investment. This fact seems consistent with the idea that treated firms are similar to investment firms at the time of the investment, but see a divergence in outcomes as a result of less VC finance.

The motivation for the construction of a corresponding group of control firms is similar. The goal is, for each treated firm, to identify a firm with similar characteristics, yet which operates *outside* the industry in which the associated super-large VC investment was made. Accordingly, I again employ a matching algorithm to compile the control group. To reduce the effect of the super-large VC's investment on VC finance in the control group, I exclude firm-quarter observations that are soon after the first period in which the firm in question was a close candidate for treatment based on the treated firm matching process discussed above. For each treated firm, I again match the remaining set of portfolio companies in the database on state/province, but on firm *sector* rather than industry, excluding the industry of the treated firm. I then calculate the similarity score based on financing to date and founding year as before, keeping each treated firm's closest match.

For any firms that enter the treated group more than once, I set the treatment date as the first of the super-large VC investment dates that the firm is associated with. The same attribution of event time is performed for control firms. I am left with an unbalanced panel of 18,184 firms: 10,106 treated and 8,078 control.

Table 3.3 compares covariate balance between treated and control firms against that of treated and non-matched firms. The same variables from Table 3.2 are included. Column (1) firms are those matched into the treated (i.e. competitor) firms. Column (2) firms are the control firms selected by the matching algorithm from out-of-industry, but within sector and state/province to be similar to the treated firms. Column (3) firms are other PitchBook firms representative of the sectors associated with treated firms, outside the treated firm industries. Column (4) is the difference between treated and control firms, while column (6) is the difference between treated and other firms. t-statistics of the differences are reported in columns (5) and (7), respectively. Panel A presents averages taken at the time of the super-large VC investment. Panel B presents averages taken with all available data. Once again, covariate balance is much stronger between treated and control firms than treated and within-sector, outside-industry firms. t-statistics are generally smaller for the treated-control difference, but are slightly larger in some cases, such as IPO indicators or last available interest rates. The gap between treated and control firms, however, doesn't seem to widen as much over time as the gap between investment and treated firms. This covariance balance between the investment firms and the matched treated firms, and again the matched treated and control firms speaks to the added value of the matching process over using all available firms.

To tackle the first main task of this chapter, I propose an empirical test to investigate whether VC finance decreases for competitor firms relative to the control group after the investment. As mentioned before, the treatment event (the super-large VC investment) is not truly exogenous. However, in the spirit of the ideal tests discussed previously I use an interaction term regression strategy that mimics the framework of a differences-in-differences test. The regression equation is:

$$Y_{it} = \alpha + \beta\ Post_{it} \times Treat_i + \pi\ Post_{it} + \gamma_i + \tau_t + \epsilon_{it} \tag{3.1}$$

where i indexes firms and t indexes calendar time. Y_{it} is the outcome variable. I use both an indicator for VC finance and amount of VC finance in quarter t for firm i as the baseline dependent variables. $Post_{it}$ is an indicator for the observation occurring after the super-large VC investment event, and therefore is specific to each firm i, hence the subscript it. $Treat_i$ is an indicator for being in the treated group, and γ_i and τ_t are firm and quarter fixed effects, respectively.

The main identifying assumption in a true DID framework is "parallel trends," or that in the absence of treatment, the control and treated groups would have experienced the same trends in outcomes before and after the event. While my strategy is not a true DID, I posit that the control group outlined in the previous section constitutes a reasonable counterfactual along these lines.

Figure 3.1, Panel (a) provides evidence to support the parallel trends conjecture. The figure plots the share of firms in the treated and control groups that received VC finance by event time (quarter), relative to the super-large VC investment. In the 12 quarters before the event, the share of firms is slightly increasing for both groups, suggesting that, in the absence of treatment, the treated and control groups would have had similar changes in outcomes over time. Furthermore, patterns in Figure 3.1, Panel (b) indicate that the competitor firms indeed experienced lower levels of finance than they would have in absence of the super-large VC investment. Although the control group does experience a drop in VC finance post-event, the treated group seems to experience a sharper drop on impact, and decreases at a faster pace.

It is not entirely obvious why the control group also experiences a decrease after the event. I hypothesize that the close match based on the observables selects a control group which was also exposed to the super-large investment, albeit to a lesser degree. To demonstrate why this match is still of use, I construct an alternate control sample by using only the founding year variable as an axis along which to match, as opposed to both founding year and finance to date. Event time averages are plotted for this alternate control group and

treated firms in Figure 3.1 Panels (c) and (d) for the two VC finance outcome variables. It does appear that when the alternate match is made, the control group experiences no drop at event time = 0. Results of the interaction regression are strengthened using this control group. Quarterly finance probability drops -1.46%, quarterly VC finance amount drops by $38,900 (see Appendix C, Table C1 and Figure C1). However, this comes at the cost of firm similarity, as can be seen in the wider gap and different pre-event time trend between the treated and control groups than was present with the original control group. Thus, for the sake of similarity, I retain the original control group, with the understanding that it may be exposed to a similar, but smaller effect than the treated group. This being the case, the counterfactual to the treated group will represent what would have happened if the treated firm was *less* exposed to an effect, rather than not exposed at all. As a result, β may even be understated when using this group.

I mainly employ the static test in Equation 3.1 as a way to summarize the effect in one clean parameter, β. However, for a more revealing exposition of how super-large VC investments may change the investment landscape for the treated firms, I use a dynamic interaction term regression:

$$Y_{it} = \alpha + \sum_{e=-T, e\neq -1}^{T} \beta_e \ \mathbb{1}\{e = t - E_i\} \times Treat_i + \pi \ Post_{it} + \gamma_i + \tau_t + \epsilon_{it} \tag{3.2}$$

Where E_i is the quarter in which treatment occurred for treated firm i, e indexes event time, and $\mathbb{1}\{\cdot\}$ is the indicator function. T represents the window of observation about the event that is used, which I set to 12 quarters. Results are large and almost all statistically significant using any $T \in \{12, 10, 8, 6, 4, 2, 1\}$ (see Appendix C, Table C2). This specification, if properly identified, allows me to obtain a summary parameter β_e representing the strength of the treatment in each event time relative to the reference period (here standardized to e = -1). In both regressions, standard errors are clustered at the firm level. All regressions hereafter are also presented alongside a similar regression without firm or time fixed effects

(in which case the $Treated_i$ variable is introduced as an RHS variable).

The error term, ϵ_{it} will contain any time- and firm-varying factors that impact VC financing, such as the departure of a CEO, increased firm investment in R&D, or changes in a firm's competitive environment. The validity of using this regression hinges on the error term being uncorrelated with treatment. Causal identification will be threatened if treatment timing and assignment are related to time- and firm-varying factors that impact the outcome variable. Put differently, time- and firm-varying factors that affect VC financing should not be different between the treatment and control groups. As discussed previously, if the parallel trends assumption holds, then it is reasonable that much of the time- and firm-varying factors that affect treated observations also affect control observations, and are not significantly correlated with $Treat_i$.

One factor that may cause ϵ_{it} and the regressors to be correlated, threatening identification, would be selection of firms for treatment. It is plausible to suppose that the super-large VC fund had access to superior information or resources that allows them to "cherry-pick," or simply identify and invest in the best portfolio companies. If this problem is severe enough in the data, it could mean that the close competitors were of inherently lower quality and were passed over. Thus, decreased VC finance in subsequent periods may be lower not because of networks, or cash burning effects, but rather because of low, unobserved startup quality or "skill." I address this question in Section 3.5.

3.4 Results

Results of equations (3.1) and (3.2) are given in Tables 3.4 and 3.5, using an indicator for VC finance and VC finance amount as the dependent variables. Column (2) of Table 3.4 shows (using the β coefficient from the static interaction term regression) that firms which compete directly with startups that receive super-large VC funding experience a 0.84% larger decrease in probability of VC funding after the event than the decrease of the control firms.

The coefficient is statistically significant. This number is also economically large, since probability of obtaining VC finance is only 5.74% before the super-large investment. Thus the 0.84% drop constitutes about a 15% decrease in probability magnitude relative to before. Column (4) reports the results of Equation 3.1 using amount of VC finance received in quarter t by firm i as Y_{it}. The results indicate that firms in the control group can expect to receive an average of \$22,700 less VC investment per quarter after the investment event than can the a firm in the control group. Average quarterly VC finance for the treated group was about \$415,300 before the event, so a \$22,700 decline corresponds to a 5% decrease.

Table 3.5 reports event time coefficients from the dynamic regression from Equation 3.1. These coefficients are plotted in Figure 3.2 along with a 95% confidence interval. The graph demonstrates that a major portion of the 0.84% larger drop from the static regression is driven by a lack of VC finance in the first several quarters after the super-large VC investment event. Indeed when using the VC Indicator as the dependent variable in Equation 3.2, β_e goes from -0.006 to -0.0325 between event quarters $e = -2$ to $e = 0$, and remains relatively low thereafter. Under a networks or value-add hypothesis, I would not expect VC finance to begin to decline nor suddenly drop in the periods before the super-large VC investment. Consistent with these channels, all event-time-treated interaction coefficients are statistically significant and negative. Since the coefficients represent a comparison with the event time $e = -1$ period, we do see a building of VC finance outcomes just prior to the investment date. Furthermore, Figure 3.2 shows a nearly monotonic increase in interaction coefficients as event time nears the investment date from the past.

I interpret the results of the baseline specifications to imply that VC finance decreases on both the intensive and extensive margins more for direct competitors of firms invested in by super-large VCs than for similar firms outside the industry.

In order to ensure that the main results of this chapter are unique to super-large VCs, I conduct a placebo test using the smallest of VC firms. To do this, I reconstruct the entire sample in precisely the same way, changing only the very first step. Instead of identifying

all +$500 Million AUM VC funds, I identify all funds with an AUM less than $10 Million. The results are robust to choosing a different placebo level (e.g. $5 Million, $ 50 Million, $100 Million). I use $10 Million since it produces a sample of very similar size to that of the original sample. I then form the treated and control groups using the very small VC's investments rather than those of the very large funds. Under the networks hypothesis, any decrease in VC finance for the remainder of the industry should be small or negligible, as these smaller market players do not exert the same influence on their networks as would a super-large VC fund. I note that this would also be the case under the selection and value-adding hypotheses as well, as small VCs would have less reputation and resources to pick or make winners when compared to large VCs. Using this alternate placebo sample, I run equations (3.1) and (3.2) for the two outcome variables. The results for the static regressions are given in Table 3.6, running each regression with and without fixed effects. According to column (2), firms in the same industry experience a decline in probability of VC finance of 0.15%, and is statistically insignificant. Column (4) reports that the placebo treated group has a drop in period VC finance of $5,740, significant at the $p = 0.05$ level. Although of the same sign and the second figure having a small degree of statistical significance, these results are economically minor, especially in comparison to the 0.84% drop in likelihood and $22,700 drop in amount of quarterly VC finance from the baseline results in Table 3.4. To demonstrate this visually, I plot the dynamic coefficients and 95% confidence intervals of the placebo regression in red alongside those from the baseline in blue in Figure 3.3. Panels (a) and (b) correspond to the period VC finance indicator and amount dependent variables, respectively. The upon impact decline in VC finance (relative to the quarter before the event) is apparent in the baseline blue points, while little-to-no downward variation occurs in the placebo. I therefore verify that the main results are specific to super-large VC investment events.

3.5 Alternative explanations

3.5.0.1 Selection and Value-Adding

Based on results from Section 3.4, I conclude that competitor firms are less likely to receive VC finance and receive less per quarter after a super-large VC invests in their rivals. As discussed in the introduction, however, it does not directly follow that this decrease is due to the networks hypothesis I have proposed. For example, the decline in funding could result from super-large VCs selecting the highest-quality firms, while those remaining in the industry "deserve" less investment (selection).

Or, perhaps VCs do not wish to engage a better-funded and better-managed opponent in tight strategic competition (value-adding). If engaged, a super-large VC fund can deploy a larger array of resources to combat competitive pressure from other startups than could other VCs. As such, other VCs contemplating an investment in a competitor of a startup recently funded by a super-large VC should hesitate for reasons that have nothing to do with network strength. The startup may be maneuvered out of business by the efforts of the super-large VC, costing the value of the investment. Even if they outlast their competition, the cost of doing so may far surpass the benefit of winning. VC firms may also place heavy weight on the signal generated from super-large VC investment in a Bayesian update against their own signal of the competitor. This additional signal, perceived as accurate by the VC, could change the minds of VCs on the margin of investing in the competitor firm. It does not matter whether the super-large VC received the correct signal in the first place, as long as it is considered strong by the other VCs.

Both selection and value-adding explanations fall under the category of poor firm fundamentals, whether created or marked by the investment in a rival. Neither are mutually exclusive with the networks channel. Under these hypotheses, one would expect a treated firm to also struggle to obtain outside finance, or at the very least, see their cost of borrowing increase after the super-large VC investment. This prediction is reversed in the case of the

networks hypothesis. Bankers, and especially government loan agents, surely have a lower incentive to fear VC network exclusion, if any. Therefore a drop in outside finance sources, such as bank or SBA loans, for the treated group post-investment could be consistent with any combination of the three hypotheses. On the other hand, no change or an increase in bank or SBA loans would be consistent with a VC network effect, while simultaneously providing evidence contrary to a value-adding or selection story.

To execute this test, I run the same regression as Equation 3.1, but using other financial outcomes as dependent variables. I first use indicators for bank and SBIR loans. Results for these regressions (with and without fixed effects) are given in Table 3.7. The dependent variable, Y_{it} for columns (1) and (2) is an indicator for firm i receiving SBIR finance in quarter t. The dependent variable, Y_{it} for columns (3) and (4) is an indicator for firm i receiving bank finance in quarter t. Probability of quarterly SBIR finance drops by 0.54% more for the treated firms after the super-large VC investment event than for the control firms, and is not statistically significant. Probability of quarterly bank finance actually rises by 0.20% more for the treated firms than the control firms after the event, and is significant at the $p = 0.05$ level. While the standard errors of the point estimates may be slightly raised by the relatively few matches from SBIR and Dealscan to PitchBook (508 and 204, respectively), the point estimates are quite small. The regressions indicate that there is no strong relationship between the super-large VC investment events and treated group debt or SBIR finance. To show this graphically, I plot treated- and control-group averages in the SBIR and bank finance indicators by event time in Panels (a) and (b) of Figure 3.4.

One could imagine, however, that the constant nature of outside finance could still exist despite the presence of worse treated firm fundamentals. Perhaps the decrease in VC finance strengthened the bargaining position of non-VC financiers. In a simple Nash bargaining game, treated firms would be willing to accept a higher cost of capital than before, while control firms would remain relatively unaffected.

I test for increased rates using Equation 3.1, where Y_{it} is the last available bank loan

rate from Dealscan for each firm that received at least one loan by date t. Results for the regressions (with and without fixed effects) are presented in Table 3.7. It would appear that rates remain relatively stable for treated firms through the event, raising slightly by 11 more basis points for treated than control firms. This result is far from statistically significant (the firm-clustered standard error is 13). Furthermore, the post period in general is associated with an offsetting 9 bps lower rate. To add perspective to these quantities, taking an average over quarters and firms with the incorporated LIBOR information yields an average rate of 5.26%. If a treated firm had this rate before the event, the coefficient would indicate a new rate of 5.28% afterward (taking both $Post_{it}$ and $Post \times Treat_{it}$ coefficients into account). Interestingly, this effect seems to change sign when considering average rates weighted by size of loans (see Figure 3.4, Panel (c), which plots these weighted averages by group against event time). This weighted average drops and actually tends *downward* relatively fast for treated firms, but slightly rises and trends downward slowly for control firms. Taking together the results on bank loans and rates, I conclude that changes in bank finance prospects after the super-large VC investment do not materially differ across treated and control groups. This finding suggests that at a minimum, bankers and government loan officers did not immediately revise financing terms negatively for treated startups. This interpretation is consistent with the notion that these firms did not decrease in quality following the super-large VC investment event.

Several limitations to this analysis arise due to several factors. First, the term-loan structure of bank finance may mechanically reduce the elasticity of bank debt to market shocks. Second, debt and equity are different financial contracts. Debtholders, who often have senior claim to a firm's cash flow, may still be willing to lend to startups who can produce modest cash flows. On the other hand, VC investors are more interested in a firm's ability to generate a right-tailed return outcome. Plausibly, a super-large VC investment in the industry thins out the tails of a startup's return distribution without significantly reducing the ability of an industry participant from meeting basic debt obligations. If so, the above

result concerning bank loans would hold, despite true reduced return prospects. To address these limitations, I calculate averages for several variables related to the exit outcomes of treated startups. I compare these exit outcomes to those of the control startups, as well as the group of startups that received super-large VC finance. These variables are, first, the ratio of exit value to total VC finance received at time of exit[3], and second, indicator variables for various exit outcomes: IPO, M&A, and out of business. Results are presented in Table 3.11. In all cases I find that average exit ratios and incidence of successful exit (IPO, acquisition) are higher for the treated group than control group. Interestingly, the average exit multiple is higher for the treated group than even the group that received super-large VC finance. I note that these averages are not statistically different, yet, at worst, we cannot statistically conclude that treated firms have worse exit outcomes than the control (or super-large-VC-backed) startups. I would not expect to find these results if the treated firms turned out to be low-quality following the super-large VC investment.

Taking together the results on bank loans and rates, I conclude that changes in bank finance prospects after the super-large VC investment do not materially differ across treated and control groups.

3.5.0.2 Networks

Returning once again to the ideal experiment to test for a VC network effect (see beginning of Section 3.3), I would have a set of VC firms with and a set without the ability to source deals via VC networks. Under such an assumption, I would be able to observe which VC firms were more or less willing to cross their network companions through investment decisions. Equipped with this knowledge, I would be free to reject the networks hypothesis if competitor VC finance displays similar patterns across these two VC types (networked and non-networked). Clearly, no VC firm is forced to source deals in a vacuum, and so this is an unrealistic assumption. However, given a particular super-large VC, I can identify the VC

[3]This ratio is set to 0 for firms who went out of business

firms most likely to be affected by a network-estrangement incentive.

Using the PitchBook database's information on VC fund investments, I am able to create a list of all syndicate pairs in the data over the sample period, along with the dates of each pair's first joint investment. Recall that, by definition, each treated firm is associated with a super-large VC startup investment, and by association, an investment date and a super-large VC fund (see sample construction discussion in Section 3.3). Exploiting this fact, I then create a variable $Network_i$ to take the VC network information to the firm level. $Network_i$ is an indicator for firm i having received pre-event VC finance from a VC fund that had previously invested in a syndicate with the associated super-large VC. I run a set of triple-difference regressions to compare the post-event VC finance outcomes for the firms such that $Treated_i = 1, Network_i = 1$ against those where $Treated_i = 0, Network_i = 1$:

$$\begin{aligned} Y_{it} = \alpha + \beta\ Post_{it} \times Network_i \times Treated_i + \eta\ Post_{it} \times Network_i \\ + \nu\ Post_{it} \times Treated_i + \pi\ Post_{it} + \gamma_i + \tau_t + \epsilon_{it} \end{aligned} \tag{3.3}$$

Where Y_{it} is once again either an indicator for receiving VC finance or total VC finance received by firm i in quarter t. If well-specified, the test should be informative about several interesting parameters. First, it should identify the relationship between having a prior, networked-in VC-backer and VC finance after the super-large VC investment in the treated firm's competitor (η). It also should tell us whether this effect is stronger for firms in the treated group (β). The main idea behind this regression is that VC firms that have previously funded a portfolio firm are on the short list of VCs likely to participate in a future round for that firm. In regression (3.3), all treated firms are, in theory, exposed to the same level of super-large VC value-adding and selection considerations depressing the incentive of the other VC firms to invest post-event. However, firms for which $Network_i = 1$ should be more exposed to the network friction than other treated firms, since a major potential VC backer has an important, documented relationship they risk losing, should they invest. A negative β

coefficient from Equation 3.3 would suggest that the decline in VC finance has a component originating from the network explanation.

To better quantify the effect of $Network_i$ on the treated group alone, I also run the following regressions using only the observations from the treated firms:

$$Y_{it} = \alpha + \beta \ Post_{it} \times Network_i + \pi \ Post_{it} + \gamma_i + \tau_t + \epsilon_{it} \tag{3.4}$$

Results of these regressions are presented in Tables 3.8 & 3.9 using the outcome variables of an indicator for VC finance and the dollar amount of VC finance in a given startup-quarter. In each table are four columns, one for each combination of fixed effect and subsample (i.e. full sample, using Equation 3.3 or treatment group only, using Equation 3.4). Column (4) of Table 3.8 reports that a treated firm with prior backing from a well-networked VC firm experiences, on average, a 5.77% greater drop in likelihood of VC finance in the periods directly following the super-large VC investment event than those without such a backer. Similarly, column (4) of Table 3.9 reports that such firms experience, on average, a drop of $263,000 in VC finance in excess of the remaining firms after the event. Indeed, the negative effect of being treated seems to be stronger among those treated firms that were backed by a VC firm that has a previous relationship with the associated super-large VC fund.

Furthermore, the effect of $Network_i$ after the investment event may be stronger for treated firms than control firms (column (2) of the tables). Treated firms with networked prior backers experience a drop of 0.48% in quarterly probability of VC finance greater than those control firms with networked prior backers. This estimate, however, is not statistically significant. The corresponding figure using quarterly VC finance as Y_{it} shows a drop of $128,000, and is significant at the $p = 0.01$ level. I conclude that even the control group sees lower finance when backed by a networked VC. Due to the standard errors on the regression from column (2) of Table 3.8, I cannot reject the null that the treated-networked-backed firms are no less likely than the control-networked-backed firms to receive VC finance (although

point estimates are negative). Evidence in column (2) of Table 3.9, however, is consistent with treated-networked-backed firms receive significantly less VC finance.

I now turn to the question of exogeneity in this regression. It is possible that the variable $Network_i$ and its interactions are correlated with the error term. One example is that the VC firms that are able to invest in syndicates with super-large funds like Sequoia may be better than most VC funds, and have a better signal of firm quality (selection). Or perhaps the relationship between the VC fund and Sequoia makes the fund aware of private information related to strategic decisions Sequoia is making to bury their prior investment (value-adding). Either case could create scenarios in which the networked VC fund would refrain from returning for a follow-on round. Under this way of thinking, the results presented in Tables 3.8 and 3.9 could be present without reference to the network hypothesis.

As before, however, under the selection and value-add hypotheses, I would expect a decrease in outside forms of finance if firm quality was poor ex ante or was made poor. On the other hand, firm quality as perceived by bankers or the government would not be any lower simply because of well-networked VC backers under the network hypothesis. I therefore use equations (3.4) (for treated and control) and (3.3) (treated only) to test whether outside finance decreases for firms with networked VC backers. Results are displayed in Table 3.10. Point estimates for the $Network_i$ interactions are generally smaller than those related to VC finance from Tables 3.8 and 3.9. None are statistically significant. It would seem, therefore, that having prior backing from VC firms networked with the super-large VC fund predicts a decrease in VC finance. Such backing, however, does not predict significant change in other forms of finance. I argue that this test also lends support to the networks hypothesis over selection and value-adding.

Results from the $Network_i$ interaction regressions complement the regressions earlier in this chapter concerning SBIR and bank loans to treated firms. Treated firms have no meaningful difference from control firms in the changes in outside finance before and after the event. It is therefore unlikely that there is a large change in firm fundamentals, either

by negative selection or steepened competition. Consequently, the large decrease in VC finance appears unexplained before this last test. This result, on the other hand, provides an economic story consistent with such a drop. It indicates that there is a large negative association between having prior backers with a relationship with the relevant super-large VC and future VC finance prospects. I find it reasonable to conclude that these former investors are hesitant to return for a future round in the treated firm, as it could alienate an important node in their VC network.

3.6 Conclusion

The question that I seek to answer in this chapter is whether a super-large VC, through investments of its own, may discourage VC investment in competing firms. I seek to disentangle the effect of the selection and value-adding capacities of the super-large VC from the effect from its network influence on other VCs. I find that for startups that compete with the super-large VC investment, VC finance reduces in both frequency and intensity in the period directly after the event more than for other startups. I fail to discover evidence that this drop may be attributed to poorer quality firms being passed over by the super-large VC. Bank loan frequency, bank interest rate terms, and SBIR loan frequency do not deteriorate in step with VC finance for these competitor startups. Finally, I show that networks do seem to play a role in this decline. Backing from VC firms with prior relationships to the super-large VC is correlated with even larger drops in VC finance directly after the super-large VC investment.

The practice of VC firms investing in syndicate with other VC firms has been well studied. Motivations for the formation of VC networks such as better contract terms, stronger portfolio performance, lesser information risk, brighter exit prospects, and post-exit profitability have been documented. However, while these explanations would make it seem that networks serve to benefit VC firms, entrepreneurs, and follow-on investors, networks may

not be strictly beneficial to all market participants. My chapter demonstrates another side to this practice that keeps finance away from startups that appear to be equally deserving.

This dynamic becomes more poignant when considered alongside examples such as the debacle that was the failed IPO of WeWork. A clever business idea, when backed by a large VC and mismanaged by less competent founders may pollute the startup pool, simply because VC firms dare not harm their own networks by investing in healthier competitors. Understanding that informal VC networks may create these finance droughts may be the first step in differentiating failing startups from those that are healthy but left out in the cold unnecessarily. If investors can make this distinction, it could be not only profitable, but also help usher in the next generation of innovative, society-improving young businesses.

3.7 Tables

Table 3.1: **Summary statistics**

This table displays summary statistics comparing all VC funds (Panel A) to super-large VC funds (Panel B). A super-large VC fund is defined as a fund with assets-under-management of $500 Million or more.

Variable	N	mean	std	min	25%	50%	75%	max
Panel A: All VC funds								
# Investments	15,127	17.14	28.97	1.00	3.00	8.00	21.00	695.00
Fund Size ($M)	14,376	122.98	329.95	0.00	12.98	45.00	121.56	15,300.00
Vintage Year	15,540	2011	8.38	1960	2006	2014	2017	2021
% of Portfolio Out of Business	12,647	0.06	0.15	0.00	0.00	0.00	0.04	1.00
% of Portfolio Acquired	12,647	0.24	0.32	0.00	0.00	0.06	0.43	1.00
% of Portfolio Public	12,647	0.06	0.16	0.00	0.00	0.00	0.00	1.00
% of Portfolio US-based	12,647	0.75	0.38	0.00	0.53	1.00	1.00	1.00
% of Portfolio California-based	12,647	0.32	0.34	0.00	0.00	0.22	0.57	1.00
Fund California Based	19,315	0.22						
Fund US Based	19,315	0.55						
Panel B: Super-large VC funds								
# Investments	550	51.90	59.17	1.00	13.00	35.00	73.00	695.00
Fund Size ($M)	550	994.70	1089.64	500.00	560.43	700.00	1000.00	15300.00
Vintage Year	529	2011	7.24	1995	2006	2014	2018	2021
% of Portfolio Out of Business	541	0.04	0.08	0.00	0.00	0.00	0.06	0.50
% of Portfolio Acquired	541	0.30	0.30	0.00	0.00	0.20	0.53	1.00
% of Portfolio Public	541	0.11	0.16	0.00	0.00	0.05	0.15	1.00
% of Portfolio US-based	541	0.81	0.30	0.00	0.79	0.95	1.00	1.00
% of Portfolio California-based	541	0.42	0.27	0.00	0.20	0.44	0.62	1.00
Fund California Based	550	0.40						
Fund US Based	550	0.73						

Table 3.2: **Comparing startups receiving super-large VC finance to treated startups and within-industry startups**

This table displays summary statistics comparing investment firm, treated firm, and other firm averages. Standard errors (in parentheses) are displayed below means. Investment firms are those invested in by super-large VC funds. The treated group is the set of competitor firms selected by the matching algorithm. The other firms are firms from the industries of the investment firms, which were present at the median super-large VC investment in that industry. Investment and treated averages in columns (1) and (2) are taken directly over firms. Other firm averages in column (3) are computed as weighted averages of industry averages. Industry weights are the proportion of investment firms from that particular industry. Columns (4) and (5) are the differences between columns (1) - (2) and (1) - (3), respectively. Panel A measures averages using data up to the super-large investment date. Panel B measures averages using all available data.

	Investment Firms	Treated Firms	Same Industry Firms	Investment - Treated	Investment - Same Industry
	(1)	(2)	(3)	(4)	(5)
Panel A: Variables measured at time of super-large VC investment					
All Finance Received to-Date ($M)	11.36	11.36	7.21	0.00	4.16
	(1.31)	(0.57)	(0.41)	(1.43)	(1.38)
VC Finance Received to-Date ($M)	31.09	7.35	1.24	23.74	29.85
	(2.58)	(0.27)	(0.04)	(2.59)	(2.58)
# of VC Investments	1.99	0.92	0.30	1.07	1.69
	(0.01)	(0.01)	(0.00)	(0.02)	(0.01)
Quarterly Prob. of VC Finance	0.19	0.06	0.02	0.14	0.17
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Avg. Quarterly VC Finance ($M)	2.47	0.42	0.06	2.06	2.41
	(0.10)	(0.02)	(0.00)	(0.10)	(0.10)
# of Bank Loans	0.08	0.90	1.17	-0.81	-1.09
	(0.00)	(0.02)	(0.01)	(0.02)	(0.01)
Last Rate (bps over LIBOR)	319.72	340.73	304.81	-21.01	14.91
	(1.13)	(1.55)	(0.38)	(1.92)	(1.20)
# of SBIR Loans	0.42	0.65	1.20	-0.23	-0.77
	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)

Table 3.2: **Comparing startups receiving super-large VC finance to treated startups and within-industry startups- continued**

This table displays summary statistics comparing investment firm, treated firm, and other firm averages. Standard errors (in parentheses) are displayed below means. Investment firms are those invested in by super-large VC funds. The treated group is the set of competitor firms selected by the matching algorithm. The other firms are firms from the industries of the investment firms, which were present at the median super-large VC investment in that industry. Investment and treated averages in columns (1) and (2) are taken directly over firms. Other firm averages in column (3) are computed as weighted averages of industry averages. Industry weights are the proportion of investment firms from that particular industry. Columns (4) and (5) are the differences between columns (1) - (2) and (1) - (3), respectively. Panel A measures averages using data up to the super-large investment date. Panel B measures averages using all available data.

	Investment Firms	Treated Firms	Same Industry Firms	Investment - Treated	Investment - Same Industry
	(1)	(2)	(3)	(4)	(5)
Panel B: Variables measured at most recently available data					
All Finance Received to-Date ($M)	102.84	30.06	20.91	72.78	81.93
	(11.74)	(1.60)	(0.78)	(11.85)	(11.77)
VC Finance Received to-Date ($M)	82.32	16.34	4.73	65.98	77.59
	(4.07)	(0.60)	(0.14)	(4.12)	(4.08)
# of VC Investments	3.53	1.66	0.73	1.86	2.80
	(0.02)	(0.02)	(0.01)	(0.03)	(0.02)
Quarterly Prob. of VC Finance	0.12	0.05	0.02	0.07	0.10
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Avg. Quarterly VC Finance ($M)	2.72	0.52	0.13	2.20	2.59
	(0.10)	(0.02)	(0.00)	(0.10)	(0.10)
# of Bank Loans	0.84	1.74	2.16	-0.90	-1.31
	(0.01)	(0.02)	(0.01)	(0.03)	(0.02)
Last Rate (bps over LIBOR)	416.91	370.02	369.62	46.89	47.29
	(1.67)	(1.70)	(0.55)	(2.38)	(1.76)
# of SBIR Loans	1.48	2.41	2.77	-0.93	-1.29
	(0.03)	(0.06)	(0.02)	(0.06)	(0.04)
Year Founded	2008	2008	2003	0.38	4.75
	(0.08)	(0.08)	(0.03)	(0.11)	(0.08)
Public	0.05	0.02	0.03	0.03	0.02
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Merged/Acquired	0.40	0.37	0.43	0.03	-0.04
	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
Out of Business	0.09	0.12	0.10	-0.03	-0.01
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
US Based	0.84	0.86	0.41	-0.01	0.44
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
California Based	0.47	0.46	0.07	0.00	0.39
	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)

Table 3.3: **Comparing treated startups to control startups and outside-industry startups**

This table displays summary statistics comparing treated firm, control firm, and other firm averages. Standard errors (in parentheses) are displayed below means. The treated group is the set of competitor firms selected by the matching algorithm. Control firms are the set of comparison firms selected by the matching algorithm. Other firms are firms from the sectors but outside industries of the treated firms which were present at the median super-large VC investment of their sector. Treated and control averages in columns (1) and (2) are taken directly over firms. Other firm averages in column (3) are computed as weighted averages of sector averages. Weights are determined by sectors and non-industries proportional to treated firm sectors and industries. Columns (4) and (5) are the differences between columns (1) - (2) and (1) - (3), respectively. Panel A measures averages using data up to the super-large investment date. Panel B measures averages using all available data.

	Treated Firms	Control Firms	Outside Industry Firms	Treated - Control	Treated - Outside Industry
	(1)	(2)	(3)	(4)	(5)
Panel A: Variables measured at time of super-large VC investment					
All Finance Received to-Date ($M)	11.36	6.87	8.34	4.49	3.02
	(0.57)	(0.52)	(0.12)	(0.77)	(0.58)
VC Finance Received to-Date ($M)	7.35	2.60	0.88	4.75	6.47
	(0.27)	(0.13)	(0.01)	(0.30)	(0.27)
# of VC Investments	0.92	0.47	0.24	0.45	0.68
	(0.01)	(0.01)	(0.00)	(0.02)	(0.01)
Quarterly Prob. of VC Finance	0.06	0.03	0.01	0.03	0.04
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Avg. Quarterly VC Finance ($M)	0.42	0.15	0.04	0.27	0.37
	(0.02)	(0.01)	(0.00)	(0.02)	(0.02)
# of Bank Loans	0.90	0.97	1.39	-0.08	-0.49
	(0.02)	(0.02)	(0.00)	(0.03)	(0.02)
Last Rate (bps over LIBOR)	340.73	383.57	332.34	-42.84	8.39
	(1.55)	(1.82)	(0.13)	(2.39)	(1.55)
# of SBIR Loans	0.65	0.78	1.37	-0.13	-0.72
	(0.02)	(0.03)	(0.00)	(0.04)	(0.02)

Table 3.3: **Comparing treated startups to control startups and outside-industry startups- continued**

This table displays summary statistics comparing treated firm, control firm, and other firm averages. Standard errors (in parentheses) are displayed below means. The treated group is the set of competitor firms selected by the matching algorithm. Control firms are the set of comparison firms selected by the matching algorithm. Other firms are firms from the sectors but outside industries of the treated firms which were present at the median super-large VC investment of their sector. Treated and control averages in columns (1) and (2) are taken directly over firms. Other firm averages in column (3) are computed as weighted averages of sector averages. Weights are determined by sectors and non-industries proportional to treated firm sectors and industries. Columns (4) and (5) are the differences between columns (1) - (2) and (1) - (3), respectively. Panel A measures averages using data up to the super-large investment date. Panel B measures averages using all available data.

	Treated Firms	Control Firms	Outside Industry Firms	Treated - Control	Treated - Outside Industry
	(1)	(2)	(3)	(4)	(5)
Panel B: Variables measured at most recently available data					
All Finance Received to-Date ($M)	30.06	22.69	22.82	7.37	7.24
	(1.60)	(1.51)	(0.23)	(2.21)	(1.62)
VC Finance Received to-Date ($M)	16.34	7.68	3.37	8.65	12.97
	(0.60)	(0.34)	(0.04)	(0.69)	(0.60)
# of VC Investments	1.66	1.06	0.56	0.60	1.10
	(0.02)	(0.02)	(0.00)	(0.03)	(0.02)
Quarterly Prob. of VC Finance	0.05	0.03	0.02	0.02	0.04
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Avg. Quarterly VC Finance ($M)	0.52	0.20	0.10	0.31	0.42
	(0.02)	(0.01)	(0.00)	(0.02)	(0.02)
# of Bank Loans	1.74	1.97	2.48	-0.23	-0.74
	(0.02)	(0.03)	(0.00)	(0.04)	(0.03)
Last Rate (bps over LIBOR)	370.02	383.94	374.47	-13.92	-4.45
	(1.70)	(1.65)	(0.14)	(2.37)	(1.70)
# of SBIR Loans	2.41	3.14	3.26	-0.73	-0.85
	(0.06)	(0.07)	(0.01)	(0.09)	(0.06)
Year Founded	2008	2007	2002	0.91	5.28
	(0.08)	(0.08)	(0.01)	(0.12)	(0.08)
Public	0.02	0.02	0.02	0.01	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Merged/Acquired	0.37	0.35	0.47	0.01	-0.10
	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
Out of Business	0.12	0.08	0.09	0.04	0.03
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
US Based	0.86	0.82	0.41	0.04	0.44
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
California Based	0.46	0.34	0.07	0.12	0.40
	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)

Table 3.4: **Main results: effect of super-large investment on competitor startup VC financing outcomes**

This table presents changes in treated firm VC finance after the super-large VC investment event. Observations are at the firm-quarter level. $\mathbb{1}\{\text{VC Finance}\}$ is an indicator for the startup firm receiving VC finance in the observation quarter. VC Finance ($M) is the amount of VC finance (in millions) received by the startup firm in observation quarter, winsorized at the 1% level. Treat is an indicator for the startup firm being a competitor of a super-large VC's investment, Post is an indicator for the observation quarter being after the super-large VC investment in the startup firm's competitor. Standard errors (in parentheses) are clustered at firm level. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

Dep.Var.	$\mathbb{1}\{$**VC Finance**$\}$		**VC Finance ($M)**	
	(1)	(2)	(3)	(4)
Post × Treat	-0.012***	-0.008***	-0.051***	-0.023***
	(0.001)	(0.001)	(0.006)	(0.006)
Treat	0.030***		0.155***	
	(0.001)		(0.005)	
Post	-0.005***	-0.010***	-0.014***	-0.068***
	(0.001)	(0.001)	(0.004)	(0.006)
Firm FE	NO	YES	NO	YES
Time FE	NO	YES	NO	YES
No. Observations	359,769	359,769	359,769	359,769
adj. R-squared	0.004	0.001	0.005	0.001

Table 3.5: **Main results: dynamic DID specification**

This table presents the results of regressions of VC finance on treated indicators interacted with event-time (e) indicators. Observations are at the firm-quarter level. $\mathbb{1}\{\text{VC Finance}\}$ is an indicator for the startup firm receiving VC finance in the observation quarter. VC Finance ($M) is the amount of VC finance (in millions) received by the startup firm in the observation quarter, winsorized at the 1% level. Treat is an indicator for the startup firm being a competitor of a super-large VC's investment, Post is an indicator for the observation quarter being after the super-large VC investment in the startup firm's competitor. All coefficients are interpreted as relative to Treated $\times\mathbb{1}\{e = -1\}$ Standard errors (in parentheses) are clustered at firm level. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

Dep. Var.	$\mathbb{1}\{$**VC Finance**$\}$		**VC Finance ($M)**	
	(1)	(2)	(3)	(4)
Treated $\times\mathbb{1}\{e = -6\}$	0.027***	-0.014***	0.127***	-0.111***
	(0.003)	(0.004)	(0.014)	(0.020)
Treated $\times\mathbb{1}\{e = -5\}$	0.021***	-0.020***	0.105***	-0.131***
	(0.003)	(0.004)	(0.014)	(0.020)
Treated $\times\mathbb{1}\{e = -4\}$	0.027***	-0.014***	0.133***	-0.102***
	(0.003	(0.004)	(0.014)	(0.020)
Treated $\times\mathbb{1}\{e = -3\}$	0.028***	-0.012**	0.162***	-0.071***
	(0.003)	(0.004)	(0.015)	(0.021)
Treated $\times\mathbb{1}\{e = -2\}$	0.035***	-0.006	0.187***	-0.045*
	(0.003)	(0.004)	(0.015)	(0.022)
Treated $\times\mathbb{1}\{e = 0\}$	0.008***	-0.033***	0.051***	-0.184***
	(0.002)	(0.004)	(0.012)	(0.019)
Treated $\times\mathbb{1}\{e = 1\}$	0.022***	-0.021***	0.109***	-0.125***
	(0.003)	(0.004)	(0.012)	(0.020)
Treated $\times\mathbb{1}\{e = 2\}$	0.023***	-0.020***	0.119***	-0.114***
	(0.0024)	(0.004)	(0.013)	(0.020)
Treated $\times\mathbb{1}\{e = 3\}$	0.023***	-0.020***	0.125***	-0.108***
	(0.002)	(0.004)	(0.013)	(0.020)
Treated $\times\mathbb{1}\{e = 4\}$	0.016***	-0.027***	0.090***	-0.144***
	(0.002)	(0.004)	(0.012)	(0.020)
Treated $\times\mathbb{1}\{e = 5\}$	0.019***	-0.023***	0.098***	-0.137***
	(0.002)	(0.004)	(0.0126)	(0.020)
Treated $\times\mathbb{1}\{e = 6\}$	0.017***	-0.025***	0.104***	-0.132***
	(0.002)	(0.004)	(0.0130)	(0.020)
Firm FE	NO	YES	NO	YES
Time FE	NO	YES	NO	YES
No. Observations	359,769	359,769	359,769	359,769
adj. R-squared	0.006	0.001	0.005	0.002

Table 3.6: **Small-VC investments as placebo events**

This table presents changes in treated firm VC finance after a small VC investment event as a placebo test. Observations are at the firm-quarter level. $\mathbb{1}\{\text{VC Finance}\}$ is an indicator for the startup firm receiving VC finance in the observation quarter. VC Finance ($M) is the amount of VC finance (in millions) received by the startup firm in the observation quarter, winsorized at the 1% level. Treat is an indicator for the startup firm being a competitor of a super-large VC's investment, Post is an indicator for the observation quarter being after the super-large VC investment in the startup firm's competitor. Standard errors (in parentheses) are clustered at firm level. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

Dep. Var.	$\mathbb{1}\{$**VC Finance**$\}$		**VC Finance ($M)**	
	(1)	(2)	(3)	(4)
Post × Treat	-0.002	-0.002	-0.005	-0.006*
	(0.001)	(0.001)	(0.003)	(0.003)
Treat	-0.004***		-0.009***	
	(0.001)		(0.002)	
Post	0.001	0.002	0.013***	0.004
	(0.001)	(0.001)	(0.002)	(0.003)
Firm FE	NO	YES	NO	YES
Time FE	NO	YES	NO	YES
No. Observations	407,166	407,166	407,166	407,166
adj. R-squared	0.000	0.000	0.000	0.001

Table 3.7: **Effect of super-large VC investment on competitor startup loans**

This table presents changes in competitor bank and SBIR finance after the super-large VC investment event. Observations are at the firm-quarter level. $\mathbb{1}\{\text{SBIR}\}$ is an indicator for the startup firm receiving SBIR finance in the observation quarter. $\mathbb{1}\{\text{Bank}\}$ is an indicator for the startup firm receiving bank finance in the observation quarter. Bank Rate is the last available bank interest rate for the startup firmin the observation quarter. Treat is an indicator for the startup firm being a competitor of a super-large VC's investment, Post is an indicator for the observation quarter being after the super-large VC investment in the startup firm's competitor. Standard errors (in parentheses) are clustered at firm level. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

Dep. Var.	**$\mathbb{1}\{$SBIR$\}$**		**$\mathbb{1}\{$Bank$\}$**		**Bank Rate**	
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treat	-0.006	-0.005	0.017	0.020*	-10.740	11.420
	(0.008)	(0.008)	(0.009)	(0.009)	(20.400)	(13.700)
Treat	-0.006		-0.012		-30.970	
	(0.008)		(0.008)		(30.170)	
Post	0.017**	0.015*	-0.018**	-0.029**	8.878	-9.148
	(0.006)	(0.008)	(0.007)	(0.010)	(13.220)	(8.384)
Firm FE	NO	YES	NO	YES	NO	YES
Time FE	NO	YES	NO	YES	NO	YES
No. Observations	15,842	15,842	5,752	5,752	1,914	1,914
adj. R-squared	0.001	0.022	0.001	0.005	0.015	0.054

Table 3.8: **Effect of super-large VC investment on in-network competitor startup finance likelihood from in-network VCs**

This table presents results of triple-difference regressions along dimensions of time, competitor group, and whether firm has prior backing from a well-networked VC firm (see Section 3.5). Observations are at the firm-quarter level. The dependent variable is $\mathbb{1}\{\text{VC Finance}\}$, an indicator for the startup firm receiving VC finance in the observation quarter. Network is an indicator for the startup firm having prior VC backers with a previous relationship with the associated super-large VC. Treat is an indicator for the startup firm being a competitor of a super-large VC's investment, Post is an indicator for the observation quarter being after the super-large VC investment in the startup firm's competitor. Columns (3) and (4) only run a two-difference interaction regression, using only treated observations. Standard errors (in parentheses) are clustered at firm level. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	$\mathbb{1}\{\textbf{VC Finance}\}$			
Model	Triple Difference		Using Treated Firms Only	
	(1)	(2)	(3)	(4)
Post × Treated × Network	-0.001	-0.005		
	(0.009)	(0.009)		
Post × Treated	-0.010***	-0.006***		
	(0.001)	(0.001)		
Post × Network	-0.050***	-0.053***	-0.053***	-0.058***
	(0.008)	(0.008)	(0.004)	(0.004)
Treated × Network	-0.026***			
	(0.006)			
Treated	0.027***			
	(0.001)			
Network	0.113***		0.088***	
	(0.005)		(0.003)	
Post	-0.003***	-0.008***	-0.012***	-0.014***
	(0.001)	(0.001)	(0.001)	(0.002)
Firm FE	NO	YES	NO	YES
Time FE	NO	YES	NO	YES
No. Observations	359,769	359,769	197,684	197,684
adj. R-squared	0.011	0.002	0.009	0.002

Table 3.9: **Effect of super-large VC investment on in-network competitor startup VC finance**

This table presents results of triple-difference regressions along dimensions of time, competitor group, and whether firm has prior backing from a well-networked VC firm (see Section 3.5). Observations are at the firm-quarter level. The dependent variable is VC Finance ($M) is the amount (in millions) of VC finance received by the startup firm in the observation quarter. Network is an indicator for the startup firm having prior VC backers with a previous relationship with the associated super-large VC. Treat is an indicator for the startup firmbeing a competitor of a super-large VC's investment, Post is an indicator for the observation quarter being after the super-large VC investment in the startup firm's competitor. Columns (3) and (4) only run a two-difference interaction regression, using only treated observations. Standard errors (in parentheses) are clustered at firm level. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	VC Finance ($M)			
Model	Triple Difference		Using Treated Firms Only	
	(1)	(2)	(3)	(4)
Post × Treated × Network	-0.088	-0.128**		
	(0.048)	(0.049)		
Post × Treated	-0.034***	-0.008		
	(0.006)	(0.006)		
Post × Network	-0.132**	-0.135**	-0.231***	-0.263***
	(0.041)	(0.042)	(0.025)	(0.026)
Treated × Network	0.007			
	(0.033)			
Treated	0.132***			
	(0.005)			
Network	0.481***		0.491***	
	(0.029)		(0.017)	
Post	-0.007*	-0.064***	-0.032***	-0.087***
	(0.004)	(0.006)	(0.005)	(0.009)
Firm FE	NO	YES	NO	YES
Time FE	NO	YES	NO	YES
No. Observations	359,769	359,769	197,684	197,684
adj. R-squared	0.013	0.002	0.011	0.002

Table 3.10: **Effect of super-large VC investment on in-network competitor startup loan outcomes**

This table presents results of triple-difference regressions along dimensions of time, competitor group, and whether firm has prior backing from a well-networked VC firm (see Section 3.5). Dependent variables are measures of SBIR and bank finance. Observations are at the firm-quarter level. $\mathbb{1}\{\text{SBIR}\}$ is an indicator for the startup firm receiving SBIR finance in the observation quarter. $\mathbb{1}\{\text{Bank}\}$ is an indicator for the startup firm receiving bank finance in the observation quarter. Network is an indicator for the startup firm having prior VC backers with a previous relationship with the associated super-large VC. Treat is an indicator for the startup firm being a competitor of a super-large VC's investment, Post is an indicator for the observation quarter being after the super-large VC investment in the startup firm's competitor. Columns (2) and (4) only run a two-difference interaction regression, using only treated observations. Standard errors (in parentheses) are clustered at firm level. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

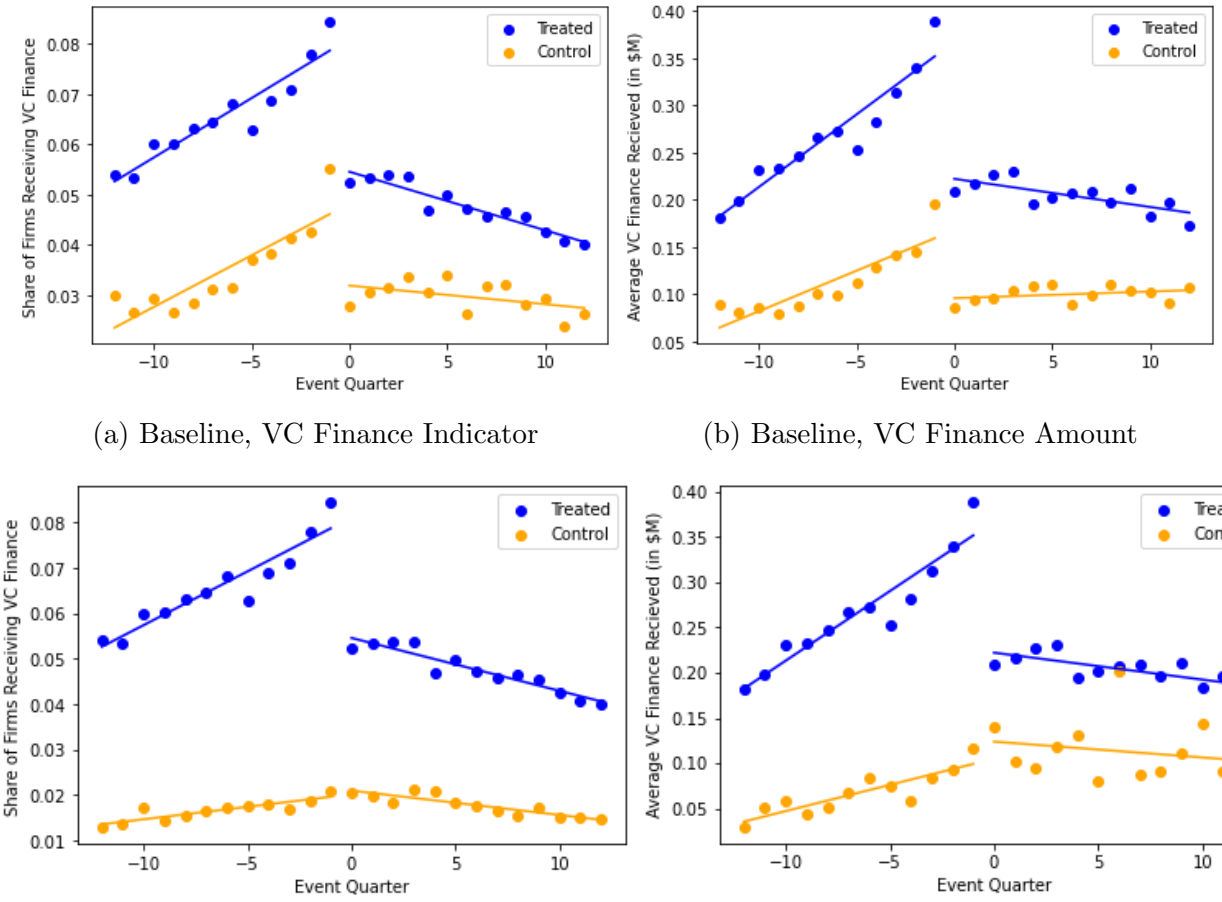
Dep.Var.	$\mathbb{1}\{\textbf{SBIR}\}$		$\mathbb{1}\{\textbf{Bank}\}$	
	(1)	(2)	(3)	(4)
Post × Treated × Network	-0.009		0.028	
	(0.036)		(0.054)	
Post × Treated	-0.005		0.020*	
	(0.008)		(0.009)	
Post × Network	0.018	0.010	-0.039	-0.010
	(0.031)	(0.019)	(0.053)	(0.009)
Post	0.014	0.017*	-0.028**	0.003
	(0.008)	(0.008)	(0.010)	(0.010)
Firm FE	NO	YES	NO	YES
Time FE	NO	YES	NO	YES
No. Observations	15,842	9,109	5,752	2,966
adj. R-squared	0.022	0.019	0.005	0.010

Table 3.11: **Exit outcomes by treatment groups**

Statistics related to the exit outcomes of the three groups of startups in the data are presented in this table. The three treatment groups are: control startups, treated startups, and startups that received super-large VC investment (investment group). Outcomes are measured as of the exit event, being the first observable VC exit from the startup which comes after the final VC investment in the startup. Exit value is assumed to be 0 for all firms that went out of business. The outcome in column (2) is the ratio of the exit value to total VC finance raised prior to exit. This variable is winsorized at the 99% level to mitigate the effect of outliers. The outcomes in columns (3)-(5) are indicator variables for the type of exit, being respectively: the startup going public in an IPO, being acquired, or going out of business. Averages of these variables are reported, with standard errors in parentheses beneath the means.

	N	Value / Total VC $	$\mathbb{1}\{\text{IPO}\}$	$\mathbb{1}\{\text{Acquired}\}$	$\mathbb{1}\{\text{Out of business}\}$
	(1)	(2)	(3)	(4)	(5)
Control Group	14,955	3.153	0.013	0.251	0.079
		(8.193)	(0.113)	(0.434)	(0.269)
Treated Group	14,970	3.311	0.032	0.359	0.086
		(7.982)	(0.175)	(0.480)	(0.280)
Investment Group	14,970	3.027	0.107	0.495	0.063
		(6.173)	(0.309)	(0.500)	(0.243)

3.8 Figures



(a) Baseline, VC Finance Indicator

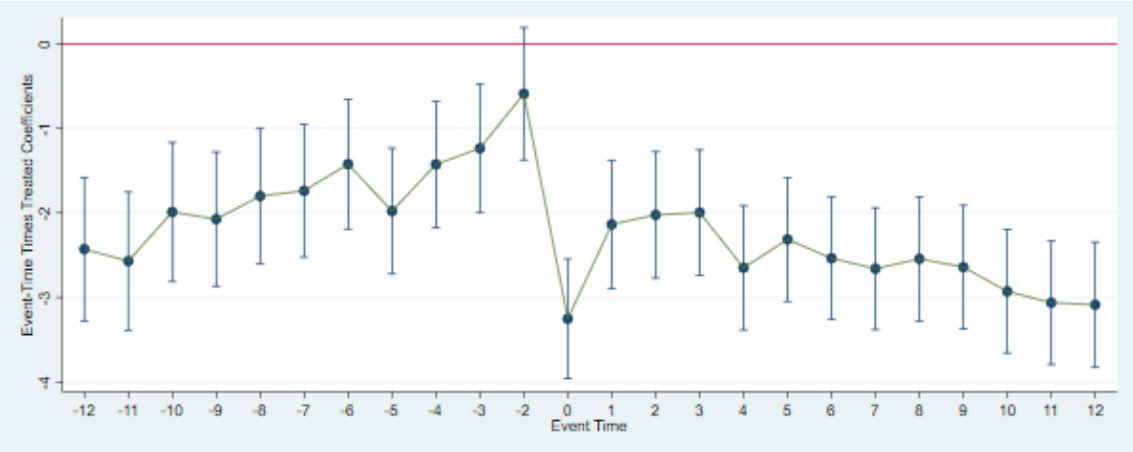
(b) Baseline, VC Finance Amount

(c) Alternate Control, VC Finance Indicator

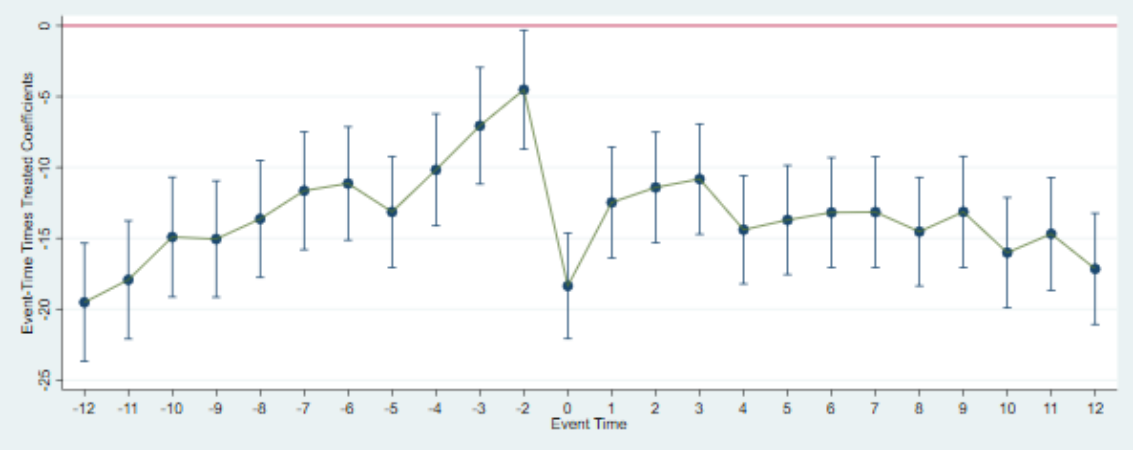
(d) Alternate Control, VC Finance Amount

Figure 3.1: **Trends in VC finance outcomes for treated firms over event time**

These charts present averages of VC finance outcome variables for treated firms taken at quarters relative to the super-large VC investment for treated and control groups. The control group for Panels (a) and (b) is the baseline control group. The control group for Panels (a) and (b) is the control group constructed matching only on founding year. The outcome variable for Panels (a) and (c) is an indicator for the startup firm receiving VC finance in the observation quarter, and the outcome variable for Panels (b) and (d) is the amount of VC finance ($M) that the startup received in the observation quarter.



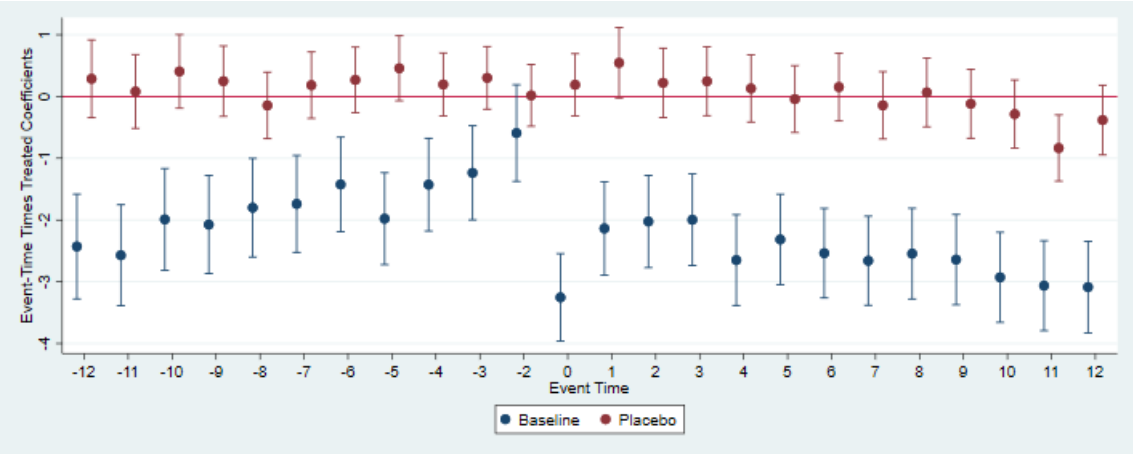
(a) Dependent Variable: VC Finance Indicator



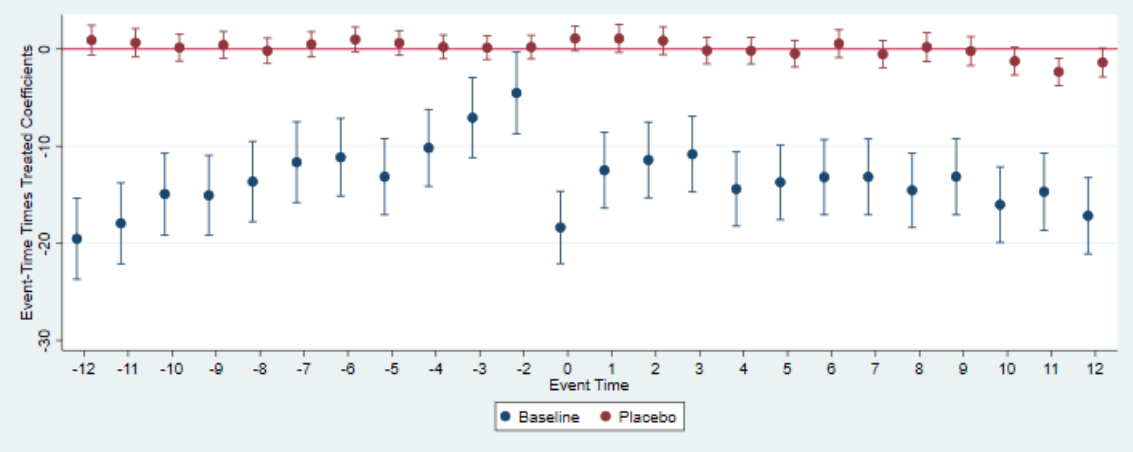
(b) Dependent Variable: VC Finance Amount

Figure 3.2: **Dyanamic DID Coefficients**

These charts show the estimates of coefficients from Equation 3.2. The X-axis charts the coefficients, with their 95% confidence intervals. Coefficients should be interpreted as an effect of treatment in a given event time, relative to the comparison period: the quarter before treatment. The LHS variable used in the estimates from Panel (a) is an indicator for the startup firm receiving VC finance in the observation quarter. The LHS variable used in the estimates from Panel (b) is the amount of VC finance ($M) that the startup received in the observation quarter.



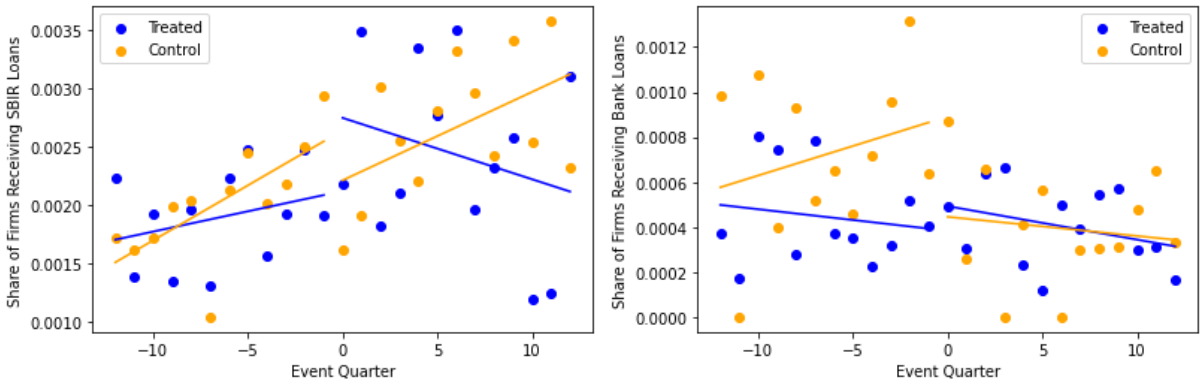
(a) Dep. Var.: VC Finance Indicator



(b) Dep. Var.: VC Finance Amount

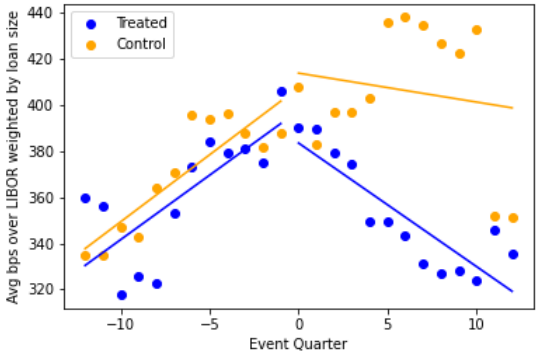
Figure 3.3: **Small VC placebo treatment, dynamic DID coefficients**

These charts show the estimates of coefficients from Equation 3.2, using a placebo for treatment events. The placebo events are investments made by small VCs into a startup firm's industry. The X-axis charts the coefficients, with their 95% confidence intervals. Coefficients should be interpreted as an effect of placebo treatment in a given event time, relative to the comparison period: the quarter before placebo treatment. The LHS variable used in the estimates from Panel (a) is an indicator for the startup firm receiving VC finance in the observation quarter. The LHS variable used in the estimates from Panel (b) is the amount of VC finance ($M) that the startup received in the observation quarter.



(a) SBIR Indicator

(b) Bank Finance Indicator



(c) Interest Rates

Figure 3.4: **Trends in loan outcomes for treated startup firms**

These charts present averages of loan finance outcome variables for treated startup firms taken at quarters relative to the super-large VC investment for treated and control groups. The outcome variable for Panel (a) is an indicator for the startup firm receiving finance through an SBIR loan in the observation quarter. The outcome variable for Panel (b) is an indicator for the startup firm receiving finance through an bank loan in the observation quarter. The outcome variable for Panel (c) is last observable bank loan rate for startups with prior, observable bank loans.

APPENDIX A

Supplementary Material for Chapter 1

A.1 Variable Definitions

Table 1, Panel A

- Vintage: Year of fund inception.
- Closing Year: Last year in which the fund makes an observable investment.
- Size ($M): Size of the fund (in $ millions).
- Fund Number: Number of the fund in the PE firm's sequence of funds.
- $\mathbb{1}\{\text{CA}\}$: Indicator for the fund's PE firm having headquarters in California.
- $\mathbb{1}\{\text{NY}\}$: Indicator for the fund's PE firm having headquarters in New York.
- $\mathbb{1}\{\text{MA}\}$: Indicator for the fund's PE firm having headquarters in Massachusetts.
- # Investments: Number of all investments made over the life of the fund.
- Avg Deal Size ($M): Average size of deals in which the fund collaborated (in $ millions).
- # LPs: Number of LPs investors in the fund.
- # Co-investments: Number of unique co-investments deals made with fund LPs over the life of the fund.

- $\mathbb{1}\{$Co-investment$\}$: Indicator variable for the fund making at least one co-investment deal.
- Treated: Indicator variable for the fund having at least one bank LP prior to the Volcker Rule.

Table 1, Panel B

- Year Founded: Year listed for the LP's founding.
- # PE funds: Number of buyout funds in which the LP makes investments over its lifetime.
- # Funds: Number of any type of private fund in which the LP makes investments over its lifetime.
- Fund Size ($M): AUM of the LP.
- $\mathbb{1}\{$Corporate Pension$\}$: Indicator variable for the LP being a corporate pension fund.
- $\mathbb{1}\{$Endowment$\}$: Indicator variable for the LP being a endowment fund.
- $\mathbb{1}\{$Foundation$\}$: Indicator variable for the LP being a private foundation.
- $\mathbb{1}\{$Fund of Funds$\}$: Indicator variable for the LP being a fund of funds.
- $\mathbb{1}\{$High-net-worth investor$\}$: Indicator variable for the LP being a high-net-worth individual.
- $\mathbb{1}\{$Insurance Company$\}$: Indicator variable for the LP being an insurance company.
- $\mathbb{1}\{$Public Pension Fund$\}$: Indicator variable for the LP being a public pension fund.
- $\mathbb{1}\{$Other Type$\}$: Indicator variable for the LP being any other type.
- $\mathbb{1}\{$CA$\}$: Indicator variable for the LP having headquarters in California.

- $\mathbb{1}\{\text{NY}\}$: Indicator variable for the LP having headquarters in New York.
- $\mathbb{1}\{\text{MA}\}$: Indicator variable for the LP having headquarters in Massachusetts.
- # Co-investments: Number of unique co-investment deals that the LP participates in over its lifetime.
- $\mathbb{1}\{\text{Co-investment}\}$: Indicator variable for the LP participating in at least one co-investment deal in its lifetime.
- % Treated: Fraction of all LP funds that had at least one bank LP.
- $\mathbb{1}\{\text{Treated}\}$: Indicator variable for the LP having an investment in at least one treated fund.

Table 1, Panel C

- Size ($M): Total size of the co-investment ($ millions).
- Year: Year in which the co-investment was made.
- $\mathbb{1}\{\text{Parallel Fund}\}$: Indicator variable for the co-investment being made by LP investors through a parallel fund.
- $\mathbb{1}\{\text{In Volcker Sample}\}$: Indicator variable the co-investment deal having any LP-fund investors from the main sample.

Tables 2 & 3:

- Size Ratio: Ratio of the size of the LP in AUM to the size of committed capital in the fund.
- FracCommitted: Ratio of the LP's capital commitment to the sum of observable capital commitments in the fund.

- Other funds: Number of other active PE funds in which the LP was an investor at the vintage of the fund.
- ReturnRatio: Ratio of the LP's average TVPI on all past PE funds to the average TVPI of all past funds managed by the fund's PE firm.
- NetworkInvestments: Number of syndicate partners of the fund in which the LP was an investor.
- AgeDiff: Age of the LP - Age of the fund's PE firm.

Table 4:

- $\mathbb{1}\{\text{Next Fund}\}$: Indicator variable for the LP investing in the next fund opened by the PE firm.
- $\mathbb{1}\{\text{Any Fund} < 5 \text{ yr}\}$: Indicator variable for the LP investing in any of the future funds opened by the PE firm within 5 years of the fund.
- $\mathbb{1}\{\text{Any Future Fund}\}$: Indicator variable for the LP investing in any future fund opened by the PE firm.

Table 6:

- DryPowder: Indicator variable for the fund having above-median proportions of dry powder remaining at the time of the shock.

Table 7:

- # Secondary Sales: Number of secondary sales of a PE fund stake made by the LP over its lifetime.
- $\mathbb{1}\{\text{Secondary Sale}\}$: Indicator variable for the LP making at least one secondary sale of a PE fund stake over the LP's lifetime.

Table 8:

- # Secondary Sales: Number of secondary sales of LP stakes made over the life of the fund.
- $\mathbb{1}\{\text{Secondary Sale}\}$: Indicator variable for at least one LP stake in the fund being sold on the secondary market over the life of the fund.

A.2 LP-level sample construction

I begin by considering all funds from the main sample. To avoid over-weighting states with virtually no buyout activity, I first rank states by total PE deal volume, and drop all states from the bottom of the distribution whose cumulative volume constitutes less than 10% of all PE activity in the US. I am left with 236 funds active as of 2013 in 32 states. I then sort states into high- and low-exposure categories based on whether the state has above- or below-median percentage of LP capital originating from bank LPs over the years 2009-2013. I take all non-bank LPs of the remaining funds (1,004 LPs), and calculate the proportion of the LP's funds that come from high-bank-exposure states. I refer to the resulting proportion as $Exposure_j$, where j indexes LP's. I then include an observation for each LP for every quarter between 2009-2018 where the LP had investments in at least one active PE fund.

A.3 Tables

Table A.1: **LP level DID**

This table displays results from the DID specification intended to test the change co-investment activity for LPs exposed to the Volcker rule. Data is at the LP-year level over the years 2009-2018. The dependent variable is $\mathbb{1}\{\text{Co-investment}\}$, which is an indicator variable for a co-investment deal between the fund and the LP in a given year. Exposure is the fraction of the LP's PE funds that are headquartered in states dependent on bank capital. For details on how this is defined, see appendix section A.2. Post is an indicator for a given year being 2014 or later. The strictest specification includes both LP and year-level fixed effects. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	$\mathbb{1}$**{Co-investment}**	
	(1)	(2)
Exposure × Post	0.057*	0.063*
	(0.033)	(0.033)
Exposure	0.021	0.018
	(0.023)	(0.023)
Post	0.005	
	(0.007)	
Year FE	NO	YES
R-squared	0.000	0.000
No. observations	42,164	42,164

A.4 Figures

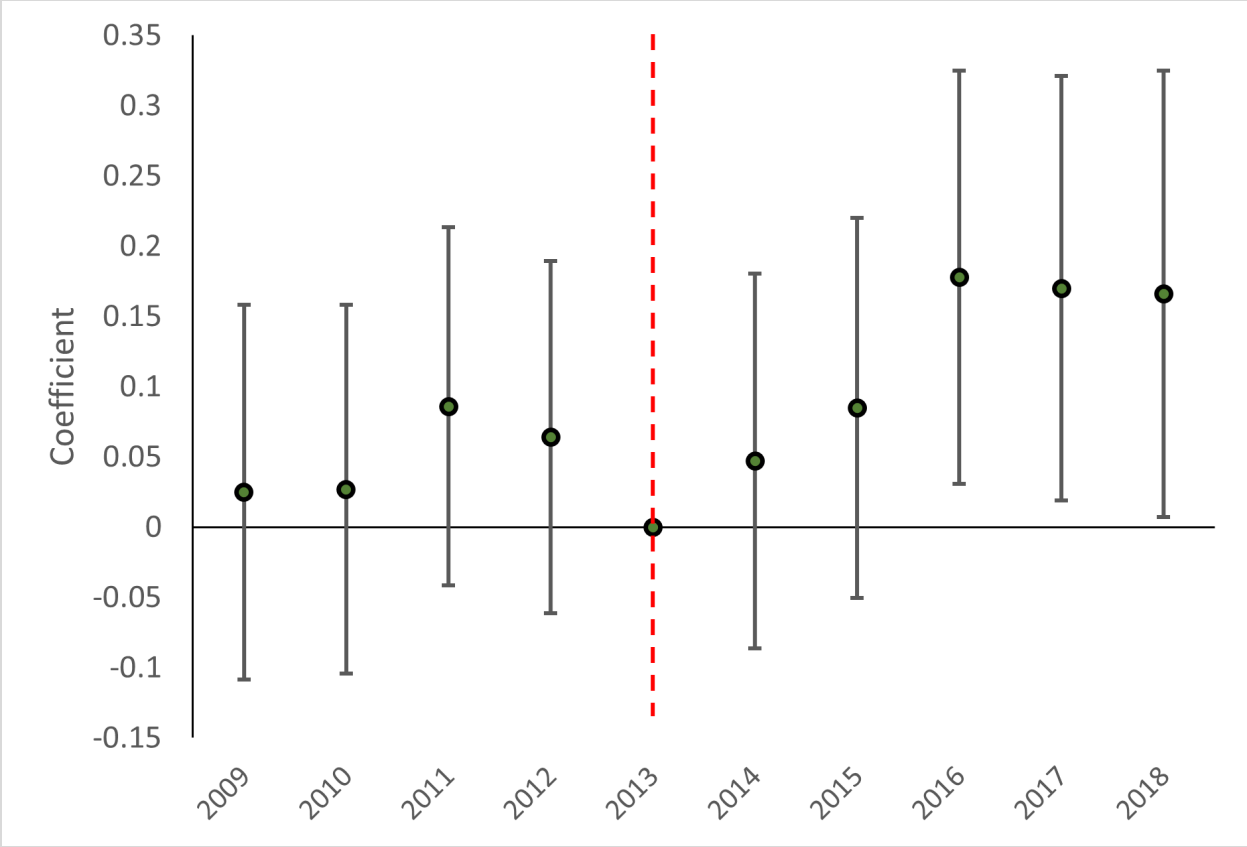


Figure A.1: **Coefficients from dynamic DID- LP level**

This graph charts the coefficients from the dynamic DID regression specified in equation 1.5. Data is at the LP-year level over the period 2009-2018. 95% confidence intervals are also displayed. The coefficient in each year represents the coefficient on an interaction term indicating the degree of treatment multiplied by indicator variables for a given year. The dependent variable is an indicator for the LP participating in a co-investment deal in the given year.

APPENDIX B

Supplementary Material for Chapter 2

B.1 Sample Construction

B.1.1 Identifying Underwriters and Their IPOs

1. We start with a set of entities in PitchBook that underwrote an IPO from the years 2001 to 2016. The total number of underwriters is 2,107.

   (a) We begin the search in the year 2001, since our final sample will run from 2006 to 2016. This is to allow us to construct the time-varying measures that require a five-year look-back window for sample observations in 2006-2010 using underwriter/IPO data from 2001 to 2005.

2. We keep only US-based underwriters, and we require that each underwriter has at least one IPO for a startup backed by at least one VC firm in the PitchBook data within the past five years. After applying this step, we are left with a set of 252 underwriters.

3. We identify 2,324 IPOs that these underwriters were involved with from 2001 to 2016.

4. The average underwriter was involved with 24.2 IPOs over the sample period.

B.1.2 Identifying Mergers & Acquisitions

1. Using the set of underwriters identified in B.1.1, we collect information on all mergers/acquisitions where an IPO underwriter was listed as a target in PitchBook. This yields a base sample of 121 mergers/acquisitions.

2. We hand-inspect this list of M&A deals to ensure a change in effective ownership. An example of a deal omitted at this stage is Lazard Middle Markets being "acquired" by Lazard.

3. After this step, we are left with 79 unique underwriter M&A targets.

4. For an underwriter acquisition to appear in the sample, the underwriter needs to have worked on an IPO in the 5 years leading up to its acquisition, and at least one such IPO must also be backed by at least one VC firm.

5. Due to this, our sample of mergers & acquisitions shrinks to 47 unique deals.

6. The average (median) underwriter acquisition happened in 2009 (2010), with the mode year being 2008 with 7 deals.

B.1.3 Identifying Bankruptcies

1. To identify underwriter bankruptcies, we use a similar process to the one we used to identify mergers in B.1.2.

2. We are left with five bankruptcies of unique underwriters: two in 2010, two in 2012, and one in 2016.

B.1.4 Identifying Venture Capital Firms

1. We identify our set of treated and control VC firms using the set of IPOs underwritten by the 252 unique underwriters in our sample from 2006 to 2016.

2. For inclusion, the VC firm must have backed at least one startup that went public in one of these IPOs.

3. We are left with a sample of 1,255 VC firms.

4. The average (median) VC firm was founded in the year 1996 (1998) and made investments in 62 companies by the end of our sample, 53.5% of which the VC firm had exited (the company either closed, was acquired, or went public) by the end of our sample.

B.1.5 Constructing Final Sample and Main Variables

Our data contains an observation for each VC firm (i) and quarter (t) pair for our 1,255 VC firms over 2006-2016. We wish to consider only VC firm quarters where the VC firm has observable, recent interactions with an underwriter. Thus, we drop observations it where VC firm i does not have an IPO underwritten by one of the 252 underwriters over the last five years (i.e., period $t-20$ to t). This produces an unbalanced panel with 26,343 observations. The primary dependent variable is IPOs_{it}, the number of portfolio companies of VC firm i that go public in quarter t.

B.1.5.1 Main Independent Variables

In constructing our primary independent variable, we aim to develop a measure of the impact of underwriter closures on a VC firm. Intuitively, we would expect that if a VC firm has many underwriting partners, it should be less impacted by the loss of any given underwriting relationship. Conversely, a VC firm with few relationships should suffer more from any lost relationship. To capture this intuition, we create a measure at the VC firm-quarter level by calculating the fraction of the VC firm's recent IPO underwriting partners that have closed over the last five years. This variable, which we call $\mathit{FracClosed}_{it}$, is given by:

$$\text{FracClosed}_{it} \equiv \frac{\text{NumClosures}_{it}}{\text{NumPartners}_{it}}$$

Where:

- $\text{NumPartners}_{it} \equiv$ Number of unique institutions that have underwritten an IPO for VC firm i over the past five years (i.e., from $t-20$ to t).
- $\text{NumClosed}_{it} \equiv$ Number of unique institutions that have underwritten an IPO for VC firm i over the past five years (i.e., from $t-20$ to t), that went bankrupt or were acquired over the past five years.

In an average quarter from the sample, the typical venture capital (VC) firm has recently collaborated with 1.8 unique underwriting firms on IPOs, out of which 0.4 have recently closed down. From the 1,255 VC firms sampled, 651 of them, or 51.9%, have experienced the closure of at least one of their recent underwriters. The overall average of *FracClosed* is 0.07.

B.1.5.2 Other Independent Variables

Definitions for the other control variables are as follows:

- USDummy_i/$\text{CaliforniaDummy}_i \equiv$ Dummy variables for VC i being headquartered in the US and California, respectively.
- $\text{VCAge}_{it} \equiv$ Age of VC firm i by quarter t.
- $\text{MostRecentFundSize}_{it} \equiv$ Size of the most recent fund of VC firm i as of quarter t.
- $\text{NumInvestments}_{it} \equiv$ Total number of startup investments made by VC firm i over the period $t-20$ to t.

- AvgInvestmentSize$_{it}$≡ Average size of startup investments made by VC firm i over the period $t-20$ to t.
- RegionVCSupply$_{it}$≡ Sum of all funding from venture-backed deals in the state where VC firm i is located in quarter t.
- Quarter t fixed effects.

Certain variables exhibit significant skew over the cross-section of VC firms. To account for this, we take the natural log of $NumPartners_{it}$, $MostRecentFundSize_{it}$, $AvgInvestmentSize_{it}$, $RegionVCSupply_{it}$, and $VCAge_{it}$ before including these variables in regressions. However, our results are robust to including these variables without first taking natural logs.

B.1.6 Tables 2.1 and 2.2 Variable Definitions

1. # Investments$_{it}$≡ Number of startup investments made by VC firm i in period t.
2. DealSize$_{it}$≡ Average size of all startup investments made by VC firm i in period t.
3. % Healthcare$_{it}$≡ % of VC firm i's period t investments that were healthcare startups.
4. % IT$_{it}$≡ % of VC firm i's period t investments that were tech startups.
5. % California$_{it}$≡ % of VC firm i's period t investments that were startups from California.
6. % US$_{it}$≡ % of VC firm i's period t investments that were startups from the US.
7. # Acquisitions$_{it}$≡ Number of startups backed by VC firm i that were acquired in period t.
8. # IPOs$_{it}$≡ Number of startups backed by VC firm i that went public in period t.
9. Founding Year$_i$≡ Year of founding for VC firm i.

10. $\mathbb{1}(\text{HQ=CA})_i \equiv$ Dummy variable for VC firm i being headquartered in California.

11. $\mathbb{1}(\text{HQ=US})_i \equiv$ Dummy variable for VC firm i being headquartered in the US.

12. $\text{AUM}_i \equiv$ Last known value (as of 2022) for assets under management for VC firm i ($ Millions).

13. $\#\ \text{Underwriters}_{it} \equiv$ Number of underwriters that VC firm i partnered with from the period $t-20$ to t.

14. $\text{VC Firm Age}_{it} \equiv$ Age of VC firm i as of period t.

15. $\text{FundAge}_{it} \equiv$ Average age of all open VC funds associated with VC firm i as of period t.

16. $\mathbb{1}(\text{New Fund})_{it} \equiv$ Dummy variable for VC firm i raising a new fund in quarter t.

17. $\mathbb{1}(\text{Lost Underwriter})_{it} \equiv$ Dummy variable for VC firm i having any recent underwriter partners close over the last five years (over the period $t-20$ to t).

B.2 Job title categorization

In this section, we describe specifically how we categorized the job titles from the CoreSignal data used in Table 2.7. We used keywords on each experience entry to make the categorization. We took each job title, removed whitespace, punctuation, and numerals, and converted to lowercase. We then did a keyword search along two dimensions: seniority and IPO-adjacent terms.

B.2.1 Seniority keywords

The first keyword search was to get a rough determination of the seniority of the job title, whether senior management, middle management, or lower level employees. The keywords we used for the three levels are listed below. We first identified all titles containing the senior keywords, then from the remaining titles we identified those with middle management keywords, and then from the remaining titles we identified those with lower-level employee keywords. We categorized all other titles into a "none" category. Panel B of Table 2.7 calculates the proportion of target-bank employees who go on to have experience at the acquirer bank for each of these four groups.

- **Senior**: ceo, cfo, coo, president, director, head, chief
- **Middle**: manager, lead, supervisor, vice president, assistant director
- **Lower**: analyst, associate, intern, assistant

B.2.2 IPO-adjacent keywords

The intention behind our second keyword search was to identify job titles containing keywords or phrases common to employees that work on IPO underwriting at investment banks. Many such job titles refer to "equity" or "capital" markets, so we begin by identifying all titles containing these keywords. Another useful term that often arises in the job titles of bankers

working on IPOs is a "coverage industry." Just as bankers may be assigned to a product group (e.g. equity or debt markets), they may also be assigned to work any type of product within a specific bank-defined industry (such as telecom or energy).[1] IPO underwriting teams will often contain representatives from both of these groups. For example, in underwriting a tech IPO, there may be a team member who exclusively works in equity capital markets (product), as well as those that work on a variety of banking services for tech firms (industry). To capture this second group, we make a further categorization of a job title based on whether the presence of one of these industry keywords below appears in their job title. Panel C of Table 2.7 calculates the proportion of target-bank employees who go on to have experience at the acquirer bank for the group of employees with at least one of these keywords in their title. It also presents this proportion for the remaining employees at the target bank.

- **Equity markets**: equity, capital
- **IPO industry designations**: tmt, telecom, technology, energy, consumer, retail, healthcare, financial institutions, industrials, real estate, sponsors, media, natural resources, gaming, lodging, financial sponsors

[1] See here for a discussion on and examples of industry groups.

B.3 Supplementary Tables

Table B1: **Details on underwriter closures**

This table shows the closures of the different underwriters in our sample by closure date and type. We also enumerate the number of VC firms and startups potentially affected by these closures in the two rightmost columns. We do this by first counting all VC investment firms which backed a startup whose IPO was underwritten by the investment bank. Then, we count the number of all startups backed by these VC firms.

Underwriter Name	Closure Date	Closure Type	#VC Firms	#Startups
U.S. Bancorp	2001-02-27	Merger/Acquisition	7	242
First Union National Bank	2001-09-01	Merger/Acquisition	8	619
Soundview Technology Group	2004-01-16	Merger/Acquisition	18	1,513
Harrisdirect	2005-10-06	Merger/Acquisition	7	1,037
Houlihan Lokey	2005-10-31	Merger/Acquisition	1	272
Adams Harkness & Hill Financial	2006-01-03	Merger/Acquisition	18	2,062
Lebenthal Holdings	2007-01-01	Merger/Acquisition	25	1,695
Ryan, Beck & Co.	2007-02-28	Merger/Acquisition	9	1,228
Blaylock Van	2007-04-02	Merger/Acquisition	34	2,628
Putnam Lovell	2007-06-21	Merger/Acquisition	3	368
C.E. Unterberg, Towbin Holdings	2007-07-16	Merger/Acquisition	18	2,774
A. G. Edwards	2007-10-01	Merger/Acquisition	3	307
ViewTrade Securities	2007-11-29	Merger/Acquisition	5	505
Bear Stearns	2008-03-16	Merger/Acquisition	96	9,317
Hilliard Lyons Business Owner Solutions	2008-03-31	Merger/Acquisition	3	90
Punk, Ziegel & Company	2008-05-05	Merger/Acquisition	14	1,548
Ferris, Baker Watts	2008-06-20	Merger/Acquisition	1	98
Merrill Lynch	2008-09-14	Merger/Acquisition	116	9,294
Lehman Brothers	2008-09-17	Merger/Acquisition	188	17,789
Wachovia Bank	2008-10-03	Merger/Acquisition	70	7,899
First Southwest	2009-01-02	Merger/Acquisition	10	1,744
Salomon Brothers	2009-01-13	Merger/Acquisition	7	350
Pacific Growth Equities	2009-02-27	Merger/Acquisition	104	11,386
Utendahl Capital Partners	2010-01-12	Merger/Acquisition	7	1793
Pali Capital	2010-04-01	Bankruptcy	4	82
Thomas Weisel Partners Group	2010-07-01	Merger/Acquisition	304	28,457
GunnAllen Financial	2010-11-01	Bankruptcy	11	1,038
Howe Barnes Hoefer & Arnett	2011-04-01	Merger/Acquisition	4	716
Morgan Keegan	2012-04-02	Merger/Acquisition	60	7,373
ThinkEquity Partners	2012-10-17	Bankruptcy	125	16,923
Advanced Equities	2012-11-12	Bankruptcy	8	491
Caris & Company	2012-12-17	Merger/Acquisition	18	3,180
Dahlman Rose	2013-02-01	Merger/Acquisition	1	1
Keefe, Bruyette & Woods	2013-02-15	Merger/Acquisition	10	1,379
Jackson Securities	2013-03-06	Merger/Acquisition	1	201
Knight Capital Group	2013-07-02	Merger/Acquisition	1	457
Crowell, Weedon & Co.	2013-08-06	Merger/Acquisition	6	1,118
Gleacher and Company	2014-03-13	Merger/Acquisition	57	10,204
J.P. Turner & Company	2014-06-13	Merger/Acquisition	4	126
McAdams Wright Ragen	2014-07-02	Merger/Acquisition	29	6,316
KBCM Technology Group	2014-09-03	Merger/Acquisition	308	39,025
Solebury Capital Group	2014-09-30	Merger/Acquisition	15	2,881
MLV & Co	2015-09-01	Merger/Acquisition	3	126
Concept Capital Market	2015-09-01	Merger/Acquisition	10	3,630
Edelman Financial Services	2015-10-12	Merger/Acquisition	3	83
Simmons Energy	2016-02-29	Merger/Acquisition	14	2,051
Arlington Asset Investment	2016-06-14	Merger/Acquisition	49	7,416
Sterne Agee Group	2016-07-01	Merger/Acquisition	1	7
CRT Capital Group	2016-08-01	Bankruptcy	1	29
National Holdings	2016-09-30	Merger/Acquisition	18	1,971
Tudor, Pickering, Holt & Company	2016-12-01	Merger/Acquisition	3	75
Muriel Siebert & Co.	2016-12-16	Merger/Acquisition	30	8,159

Table B2: **Association between underwriter closure and IPOs (OLS/FE regression)**

This table presents results testing the association between closures of a VC firm's underwriters and the IPO frequency of startups in the VC firm's portfolio. We present the coefficients from OLS/fixed effect regressions at the VC firm–quarter level, with standard errors in parentheses. Standard errors are clustered at the VC firm level. The sample includes 1,255 VC firms that have had recent interactions (in the past five years) with at least one underwriter through an IPO. The dependent variable is the natural log of the number of IPOs backed by the VC firm in a given quarter plus one. The primary independent variable, *FracClosed*, is the percentage of a VC firm's recent underwriters that have closed in the previous 20 quarters. Additional independent variables are defined in the appendix. Note that the number of observations may vary across columns due to missing information in the *RegionVCSupply* variable for some VC firms. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	log(# IPOs+1)			
	(1)	(2)	(3)	(4)
FracClosed	-0.193***	-0.172***	-0.178***	-0.168***
	(0.014)	(0.012)	(0.014)	(0.014)
NumPartners	0.109***	0.110***	0.118***	0.095***
	(0.008)	(0.008)	(0.008)	(0.007)
USDummy			0.029***	0.040***
			(0.009)	(0.013)
CaliforniaDummy			0.009	0.008
			(0.008)	(0.008)
RegionVCSupply			0.001	-0.001
			(0.002)	(0.002)
VCAge				-0.006
				(0.005)
MostRecentFundSize				0.006***
				(0.002)
NumInvestments				0.006
				(0.004)
AvgInvestmentSize				0.008***
				(0.002)
Quarter Fixed Effects	NO	YES	YES	YES
R-squared	0.065	0.108	0.124	0.134
No. observations	26,343	26,343	21,319	21,269

Table B3: **Association between underwriter closure and VC firm IPOs (including VC firm fixed effects)**

This table presents results from testing the association between closures of a VC firm's underwriters and the IPO frequency of startups in the VC firm's portfolio, including VC firm fixed effects. We present the coefficients from OLS/fixed effect regressions at the VC firm–quarter level, with standard errors in parentheses. Standard errors are clustered at the VC firm level. The sample includes 1,255 VC firms that have had recent interactions (in the past five years) with at least one underwriter through an IPO. The dependent variable is an indicator for the IPO of any startup backed by the VC in the quarter. The primary independent variable, *FracClosed*, is the percentage of a VC firm's recent underwriters that have closed in the previous 20 quarters. Additional independent variables are defined in the appendix. Note that the number of observations may vary across columns due to missing information in the *RegionVCSupply* variable for some VC firms. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	$\mathbb{1}$**{IPOs}**			
	(1)	(2)	(3)	(4)
FracClosed	-0.254***	-0.218***	-0.331***	-0.300***
	(0.017)	(0.017)	(0.027)	(0.026)
NumPartners	0.124***	0.109***	0.092***	0.112***
	(0.008)	(0.007)	(0.009)	(0.009)
RegionVCSupply		0.002	0.002	-0.007
		(0.001)	(0.005)	(0.005)
VCAge		-0.014**	-0.119***	-0.275***
		(0.006)	(0.015)	(0.036)
MostRecentFundSize		0.006***	0.005	0.008
		(0.002)	(0.007)	(0.006)
NumInvestments		0.006	-0.010	0.006
		(0.005)	(0.010)	(0.009)
AvgInvestmentSize		0.012***	0.012***	0.011**
		(0.003)	(0.005)	(0.005)
Quarter Fixed Effects	NO	YES	NO	YES
VC Fixed Effects	NO	NO	YES	YES
R-squared	0.055	0.120	0.145	0.195
No. observations	26,343	21,269	21,269	21,269

Table B4: **Relationship between a startup's VC firm's underwriters and IPOs (continuous treatment)**

This table presents results testing the association between an underwriter's closure and the exit outcomes for startups associated with the venture capital (VC) firm that subsequently lost an underwriting relationship. We present coefficients from OLS regressions run at the portfolio firm–VC firm level, with standard errors in parentheses. Standard errors are clustered at the VC firm level. The sample comprises 13,254 startups. This set of VC firms is identical to those in earlier tests. The dependent variable is an indicator for the portfolio firm going public by the second quarter of 2021. The independent variable of interest is *FracClosed*, which represents the fraction of all unique underwriters recently associated with the VC firm that closed over the life of the startup. In particular, the life of the startup encompasses the years from the VC firm's first investment date in the startup and the exit date. We include VC firm and industry-year fixed effects. The other independent variables are indicators for startups headquartered in California, the number of unique VC firms that have invested in the startup, and the number of unique underwriters with whom the VC firms had recently (in the previous five years) interacted. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	$\mathbb{1}${**IPO**}			
	(1)	(2)	(3)	(4)
FracClosed	-0.330***	-0.139***	-0.204***	-0.130***
	(0.046)	(0.037)	(0.033)	(0.034)
CADummy		-0.054***	-0.059***	-0.052***
		(0.005)	(0.005)	(0.005)
NumVCs		0.038***	0.039***	0.038***
		(0.001)	(0.001)	(0.001)
NumUnderwriters		-0.001***	0.000***	-0.001***
		(0.000)	(0.000)	(0.000)
VC Fixed Effects	NO	YES	NO	YES
Industry-Year Fixed Effects	NO	NO	YES	YES
R-squared	0.004	0.278	0.234	0.326
No. observations	30,267	30,267	30,267	30,267

Table B5: **Relationship between a startup's VC firm's underwriters and IPOs**

This table presents results testing the association between the number of unique underwriter relationships of a startup's VC investors and the IPO probability We present coefficients from OLS regressions run at the portfolio firm level, with standard errors in parentheses. Standard errors are estimated robust to heteroscedasticity. The sample comprises 13,340 startups. This set of VC firms is identical to those in earlier tests. The dependent variable is an indicator for the portfolio firm going public by the second quarter of 2021. The primary independent variable is *NumUnderwriters*, which is the natural log of the number of unique underwriters with whom the startup's VC backers had recently (in the previous five years) interacted at the time of investment. We include VC industry-vintage year fixed effects. The other independent variables are indicators for startups headquartered in California and the natural log of the number of unique VC firms that have invested in the startup. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	$\mathbb{1}${**IPO**}		
	(1)	(2)	(3)
NumUnderwriters	0.021***	0.010***	0.008***
	(0.002)	(0.003)	(0.002)
CADummy		-0.076***	-0.058***
		(0.006)	(0.005)
NumVCs		0.099***	0.084***
		(0.005)	(0.005)
Industry-Year Fixed Effects	NO	NO	YES
R-squared	0.006	0.046	0.149
No. observations	13,340	13,340	13,340

Table B6: **Test for association between the closure of Lehman Brothers/Bear Stearns and IPOs (shortening the sample period to 2006–2012)**

This table presents results from our analysis of the Lehman Brothers and Bear Stearns closures as a psuedo-random shock to a VC firm's ability to take portfolio companies public. We present coefficients from Poisson regressions run at the VC firm–quarter level, with standard errors in parentheses. Standard errors are clustered at the VC firm level. The sample includes 855 VC firms that enter the sample when we can observe their recent interactions with at least one underwriter. The dependent variable is the number of IPOs backed by the VC firm in that quarter. The primary independent variable, *CrisisBankFlag*, is an indicator for whether the VC firm was associated with Lehman Brothers or Bear Stearns. A VC is associated if it backed a firm that went public, and Lehman Brothers or Bear Stearns was the underwriter in the five years before 2008. The other independent variables are defined in the appendix. The number of observations varies across columns because some VC firms are missing information for the *RegionVCSupply* variable. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	# IPOs			
	(1)	(2)	(3)	(4)
CrisisBankFlag	-0.702***	-0.262**	-0.368***	-0.348***
	(0.104)	(0.108)	(0.108)	(0.105)
NumPartners	1.076***	1.043***	1.145***	0.870***
	(0.055)	(0.056)	(0.059)	(0.056)
USDummy			0.050	0.428
			(0.293)	(0.342)
CaliforniaDummy			0.307***	0.306***
			(0.098)	(0.086)
RegionVCSupply			0.073	-0.244
			(0.195)	(0.168)
VCAge				-0.627***
				(0.165)
MostRecentFundSize				0.029
				(0.115)
NumInvestments				0.068
				(0.153)
AvgInvestmentSize				1.247***
				(0.238)
Quarter Fixed Effects	NO	YES	YES	YES
Pseudo R-squared	0.06	0.14	0.17	0.18
No. observations	14,503	14,503	11,860	11,833

Table B7: **Test for association between the closure of Lehman Brothers and IPOs (OLS/FE regression)**

This table presents results from our analysis of the Lehman Brothers and Bear Stearns closures as a psuedo-random shock to a VC firm's ability to take portfolio companies public. We present coefficients from OLS/fixed effect regressions run at the VC firm–quarter level, with standard errors in parentheses. Standard errors are clustered at the VC firm level. The sample includes 1,255 VC firms that enter the sample when we can observe their recent interactions with at least one underwriter via an IPO between 2006 and 2016. The dependent variable is the logged number of IPOs backed by the VC firm in that quarter, plus one. The primary independent variable, *CrisisBankFlag*, is an indicator for whether the VC firm was associated with Lehman Brothers or Bear Stearns. A VC firm is associated if it backed a firm that went public, and Lehman Brothers or Bear Stearns was the underwriter in the five years before 2008. The other independent variables are defined in the appendix. The number of observations varies across columns because some VC firms are missing information for the *RegionVCSupply* variable. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	log(# IPOs+1)			
	(1)	(2)	(3)	(4)
CrisisBankFlag	-0.076***	-0.044***	-0.051***	-0.049***
	(0.007)	(0.007)	(0.008)	(0.007)
NumPartners	0.115***	0.113***	0.122***	0.098***
	(0.008)	(0.008)	(0.009)	(0.007)
USDummy			0.024***	0.035***
			(0.008)	(0.012)
CaliforniaDummy			0.010	0.009
			(0.008)	(0.008)
RegionVCSupply			0.001	-0.001
			(0.002)	(0.002)
VCAge				-0.008*
				(0.005)
MostRecentFundSize				0.005***
				(0.002)
NumInvestments				0.006
				(0.004)
AvgInvestmentSize				0.009***
				(0.002)
Quarter Fixed Effects	NO	YES	YES	YES
R-squared	0.061	0.103	0.120	0.130
No. observations	26,343	26,343	21,319	21,269

Table B8: **Test for association between the closure of Lehman Brothers and IPOs (OLS/FE regression, shortening the sample period to 2006–2012)**

This table presents results from our analysis of the Lehman Brothers and Bear Stearns closures as a psuedo-random shock to a VC firm's ability to take portfolio companies public. We present coefficients from OLS/fixed effect regressions run at the VC firm–quarter level, with standard errors in parentheses. Standard errors are clustered at the VC firm level. The sample includes 855 VC firms that enter the sample when we can observe their recent interactions with at least one underwriter via an IPO between 2006 and 2012. The dependent variable is the logged number of IPOs backed by the VC firm in that quarter, plus one. The primary independent variable, *CrisisBankFlag*, is an indicator for whether the VC firm was associated with Lehman Brothers or Bear Stearns. A VC firm is associated if it backed a firm that went public, and Lehman Brothers or Bear Stearns was the underwriter in the five years before its closure in 2008. The other independent variables are defined in the appendix. The number of observations varies across columns because some VC firms are missing information for the *RegionVCSupply* variable. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	log(# IPOs+1)			
	(1)	(2)	(3)	(4)
CrisisBankFlag	-0.064***	-0.035***	-0.045***	-0.044***
	(0.007)	(0.007)	(0.007)	(0.007)
NumPartners	0.100***	0.096***	0.105***	0.088***
	(0.008)	(0.008)	(0.009)	(0.008)
USDummy			-0.001	0.026**
			(0.012)	(0.013)
CaliforniaDummy			0.028***	0.027***
			(0.008)	(0.008)
RegionVCSupply			-0.000	-0.003
			(0.002)	(0.002)
VCAge				-0.013**
				(0.006)
MostRecentFundSize				0.004*
				(0.002)
NumInvestments				0.010**
				(0.005)
AvgInvestmentSize				0.007**
				(0.003)
Quarter Fixed Effects	NO	YES	YES	YES
R-squared	0.047	0.095	0.109	0.120
No. observations	14,503	14,503	11,860	11,833

Table B9: **Association between closure of Lehman Brothers and Bear Stearns and startup IPOs**

This table presents results testing the association between closures of large banks in the financial crisis and the exit outcomes of the associated VC firm's startup investments. We present coefficients from OLS regressions run at the portfolio firm–VC firm level, with standard errors in parentheses. Standard errors are clustered at the VC firm level. We consider only startups whose first VC firm's investment came before an observable interaction between the VC firm and an underwriter. This leaves us with 10,049 portfolio firms and 1,073 VC firm investors. The dependent variable is an indicator for the portfolio firm's eventually going public. The independent variable of interest is *CrisisBankFlag*, an indicator taking a value of 1 if the VC firm was associated with Lehman Brothers or Bear Stearns, and VC had not exited its investment in the startup before 2008. We include VC firm and industry-year fixed effects. The other independent variables are indicators for startups headquartered in California, the number of unique VC firms that have invested in the startup, and the number of unique underwriters with which those VC firms had recently (in the previous five years) interacted. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	$\mathbb{1}${**IPO**}			
	(1)	(2)	(3)	(4)
CrisisBankFlag	0.042***	-0.028*	0.031***	-0.034**
	(0.006)	(0.016)	(0.006)	(0.015)
CADummy		-0.059***	-0.057***	-0.057***
		(0.011)	(0.010)	(0.011)
NumVCs		0.039***	0.036***	0.038***
		(0.003)	(0.003)	(0.003)
NumUnderwriters		-0.001***	0.000	-0.001***
		(0.000)	(0.000)	(0.000)
VC Fixed Effects	NO	YES	NO	YES
Industry-Year Fixed Effects	NO	NO	YES	YES
R-squared	0.003	0.228	0.161	0.262
No. observations	22,416	22,416	22,416	22,416

Table B10: **Association between fund age, lost underwriting relationships, and IPOs (Poisson regression)**

This table presents the results of a Poisson regression analysis examining whether the relationship between the number of IPOs in the VC firm's portfolio and the loss of underwriting partners to closures or acquisitions depends on the fund's age at the time that the underwriting relationship was lost. The dependent variable is the number of IPOs of startups backed by the VC firm in a given quarter, and the main independent variable is the interaction term *FracClosed* × *RFA*. *RFA* is the natural log of the age of VC firm i's most recent fund. The unit of observation is the VC firm quarter, with standard errors (in parentheses) clustered at the VC firm level. The number of observations is slightly smaller than the original sample because some VC firms are missing fund vintage year data. We define all other independent variables in the appendix. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	# IPOs			
	(1)	(2)	(3)	(4)
FracClosed × RFA	-0.207	-0.658*	-0.682*	-0.493
	(0.396)	(0.357)	(0.373)	(0.406)
FracClosed	-3.327***	-2.279***	-2.242***	-2.982***
	(0.650)	(0.589)	(0.587)	(0.635)
RFA	-0.216***	-0.204***	-0.075*	-0.099**
	(0.035)	(0.034)	(0.040)	(0.040)
NumPartners	0.971***	0.996***	0.755***	0.868***
	(0.046)	(0.050)	(0.050)	(0.047)
RegionVCSupply				0.052
				(0.231)
USDummy			-0.523***	0.619***
			(0.101)	(0.219)
CaliforniaDummy			0.031	-0.002
			(0.061)	(0.097)
VCAge			0.175	0.122
			(0.179)	(0.170)
MostRecentFundSize			0.036	0.073
			(0.112)	(0.111)
NumInvestments			0.026	-0.386**
			(0.222)	(0.161)
AvgInvestmentSize			1.332***	1.793***
			(0.292)	(0.243)
Quarter Fixed Effects	NO	YES	YES	YES
Pseudo R-squared	0.10	0.16	0.18	0.20
No. observations	18,776	18,776	18,776	15,513

Table B11: **Relationship between a startup's IPOs and the loss of VC underwriting relationships interacted with VC fund age**

This table presents results testing the association between closures of a startup's VC firm's underwriters and the startup's exit outcome. We present coefficients from OLS regressions run at the portfolio firm–VC firm level, with standard errors in parentheses. Standard errors are clustered at the VC firm level. We consider only startups whose first VC firm's investment came before an observable interaction between the VC firm and an underwriter. This set of VC firms is identical to those in earlier tests. We dropped startups that lacked eventual exit/bankruptcy data, and VC firms lacking fund age data, leaving 10,424 portfolio firms associated with 776 VC firms. The dependent variable is an indicator for the portfolio firm's eventually going public. *Treated* is an indicator variable taking a value of 1 if, over the life of the startup, at least one of the underwriters associated with the investing VC firm closes. In particular, the life of the firm encompasses the years from the VC firm's first investment date in the startup and the exit date. We include VC firm and industry-year fixed effects. The baseline regression is augmented by an interaction term, *log(FundAge)*, the natural log of the age of the fund at the average time of the portfolio firm–VC firm pair's underwriter closure. The other independent variables are indicators for startups headquartered in California, the number of unique VC firms that have invested in the startup, and the number of unique underwriters with which those VC firms had recently (in the previous five years) interacted. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	$\mathbb{1}${**IPO**}			
	(1)	(2)	(3)	(4)
Treated × log(FundAge)	0.013	-0.008**	-0.013***	-0.010**
	(0.012)	(0.004)	(0.004)	(0.004)
Treated	-0.045*	-0.021	-0.002	-0.013
	(0.024)	(0.015)	(0.013)	(0.014)
Log(FundAge)		-0.008**	-0.013***	-0.010**
		(0.004)	(0.004)	(0.004)
CADummy		-0.054***	-0.052***	-0.050***
		(0.007)	(0.007)	(0.007)
NumVCs		0.039***	0.039***	0.039***
		(0.002)	(0.002)	(0.002)
NumUnderwriters		-0.001***	0.001***	-0.001***
		(0.000)	(0.000)	(0.000)
VC Fixed Effects	NO	YES	NO	YES
Industry-Year Fixed Effects	NO	NO	YES	YES
R-squared	0.001	0.254	0.228	0.305
No. observations	20,718	20,718	20,718	20,718

Table B12: **Association between VC size, lost underwriting relationships, and IPOs (Poisson regression)**

This table presents results testing whether the association between the closure of a VC firm's underwriters and VC IPO frequency is stronger for larger VC firms. We present the coefficients from Poisson regressions at the VC firm–quarter level, with standard errors in parentheses. Standard errors are clustered at the VC firm level. The sample includes 570 VC firms from the main sample who had non-missing investment data prior to the first closure of a connected IPO underwriter. The dependent variable is the number of IPOs backed by the VC firm in a given quarter. *FracClosed*, is the percentage of a VC firm's recent underwriters that have closed in the previous 20 quarters. We interact *FracClosed* with *VCSize*, the natural log of the number of startup investments the VC firm had made prior to the first observable closed/acquired underwriter, plus one. Additional independent variables are defined in the appendix. Note that the number of observations may vary across columns due to missing information in the control variables for some VC firms. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	# IPOs			
	(1)	(2)	(3)	(4)
FracClosed × VCSize	0.634**	0.585**	0.575**	0.723**
	(0.278)	(0.251)	(0.253)	(0.283)
FracClosed	-6.137***	-5.506***	-5.456***	-6.506***
	(1.004)	(0.920)	(0.940)	(1.063)
VCSize	0.160***	0.148***	0.169***	0.129***
	(0.036)	(0.035)	(0.044)	(0.046)
NumPartners	0.918***	0.986***	1.004***	1.101***
	(0.047)	(0.051)	(0.053)	(0.053)
RegionVCSupply				0.343
				(0.289)
USDummy			-0.517***	0.424
			(0.121)	(0.260)
CaliforniaDummy			0.129*	0.005
			(0.070)	(0.117)
VCAge			-0.129	-0.155
			(0.234)	(0.227)
Quarter Fixed Effects	NO	YES	YES	YES
Pseudo R-squared	0.12	0.18	0.19	0.20
No. observations	16,885	16,885	16,869	14,242

Table B13: **Association between underwriting frequency, lost underwriting relationships, and IPOs (Poisson regression)**

This table examines how the deal frequency of an underwriter impacts its relationship VC firm's portfolio companies when the underwriter closes. In this specification, the main independent variable is an interaction of *FracClosed* with the variable *HighFrequency*, which is the percentage of the VC firm's underwriters who were in the top quintile of IPO underwriters in terms of the number of deals underwritten. The dependent variable is the number of IPO exits for the VC firm in that quarter. We include quarter fixed effects and other controls, which we define in the appendix. Standard errors are clustered at the VC firm level. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	# IPOs		
	(1)	(2)	(3)
FracClosed × HighFrequency	-1.941	-2.469**	-3.603***
	(1.268)	(1.120)	(1.052)
HighFrequency	-0.049	-0.023	-0.166
	(0.098)	(0.113)	(0.109)
FracClosed	-2.623***	-1.878***	-1.801***
	(0.685)	(0.581)	(0.598)
NumPartners	1.029***	1.057***	0.833***
	(0.044)	(0.047)	(0.045)
USDummy			0.630***
			(0.181)
CaliforniaDummy			0.046
			(0.088)
RegionVCSupply			-0.011
			(0.191)
VCAge			-0.272**
			(0.134)
MostRecentFundSize			0.159*
			(0.096)
NumInvestments			-0.013
			(0.114)
AvgInvestmentSize			1.102***
			(0.149)
Quarter Fixed Effects	NO	YES	YES
Pseudo R-squared	0.09	0.15	0.19
No. observations	26,343	26,343	21,269

Table B14: **Association between underwriter IPO deal size, lost underwriting relationships, and IPOs (Poisson Regression)**

This table examines how the IPO deal-size preferences of an underwriter impacts its relationship VC firm's portfolio companies when the underwriter closes. The main independent variable is an interaction of *FracClosed* with the variable *SmallSize*, which is the percentage of the VC firm's underwriters that were in the bottom quintile of underwriters in average IPO size. The dependent variable is the number of IPO exits for the VC firm in that quarter. We include quarter fixed effects and other controls, which we define in the appendix. Standard errors are clustered at the VC firm level. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	# IPOs		
	(1)	(2)	(3)
FracClosed × SmallSize	-3.244	-6.333**	-6.970*
	(2.731)	(2.987)	(3.788)
SmallSize	0.432**	0.430**	0.551***
	(0.176)	(0.173)	(0.209)
FracClosed	-3.486***	-2.926***	-3.345***
	(0.333)	(0.306)	(0.313)
NumPartners	1.040***	1.069***	0.866***
	(0.044)	(0.048)	(0.045)
USDummy			0.672***
			(0.182)
CaliforniaDummy			0.042
			(0.088)
RegionVCSupply			-0.017
			(0.191)
VCAge			-0.270**
			(0.132)
MostRecentFundSize			0.158
			(0.096)
NumInvestments			-0.034
			(0.113)
AvgInvestmentSize			1.118***
			(0.150)
Quarter Fixed Effects	NO	YES	YES
Pseudo R-squared	0.09	0.15	0.19
No. observations	26,343	26,343	21,269

Table B15: **Impact of underwriter closure on VC fund performance (extensive margin treatment)**

This table presents the results of a regression analysis examining the relationship between the performance of a VC fund, as measured by the natural log of the fund's TVPI, and the closure of underwriting partners associated with the investment firm managing the fund. An underwriting partner is an underwriter that previously collaborated on an IPO with the VC investment firm managing the fund. The unit of observation in the analysis is a VC fund, with data consisting of 954 funds associated with 296 VC firms. The dependent variable is the natural log of the last reported TVPI, and the independent variable is *Treated*, an indicator for at least one of the VC firm's recent underwriters closing during the lifetime of the fund. The lifetime of the fund is defined as the time period between the fund's vintage year and the year we observe the last reported TVPI. The analysis controls for the total number of underwriting partners the VC firm had on previous IPOs as of the fund's vintage year, and also includes VC firm and vintage year fixed effects. The data includes all funds with documented relationships with at least one underwriter, and excludes funds with missing return data. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

	log(TVPI)			
	(1)	(2)	(3)	(4)
Treated	-0.008	-0.055	0.093	-0.154*
	(0.048)	(0.073)	(0.087)	(0.086)
NumUnderwriters		0.008***	0.007	0.000
		(0.002)	(0.006)	(0.005)
Vintage Year Fixed Effects	NO	YES	NO	YES
VC Fixed Effects	NO	NO	YES	YES
R-squared	0.000	0.242	0.457	0.599
No. observations	954	954	954	954

Table B16: **VCs most exposed to Silicon Valley Bank closure**

This table lists the top 20 VC firms most reliant on Silicon Valley Bank (SVB) for IPO underwriting over the period 2000 to 2020. % SVB Underwriter is the percentage of all VC IPOs over the period that were underwritten by SVB. Also listed are the total numbers of startups the VC has taken public over the period. A VC firm is only included in this tabulation if they have taken at least 10 startups public between 2000 and 2020.

VC Firm Name	% SVB Underwriter	Total IPOs
Biobrit	40%	15
Bay City Capital	38%	32
Lightstone Ventures	36%	11
Osage University Partners	32%	22
Nextech Invest	31%	16
Aisling Capital	31%	42
Flagship Pioneering	29%	41
Brookside Capital	29%	21
Sands Capital	29%	14
Third Rock Ventures	28%	25
New Leaf Venture	28%	43
The Column Group	27%	11
Quaker Partners	27%	11
Sabby Management	27%	11
HBM Partners	27%	48
MPM Capital	26%	68
WRF Capital	25%	12
Altitude Life Science Ventures	25%	12
Decheng Capital	25%	12
Aperture Venture Partners	25%	12
Sofinnova Investments	25%	61

APPENDIX C

Supplementary Material for Chapter 3

Table C1: **Less restrictive control group matching process**

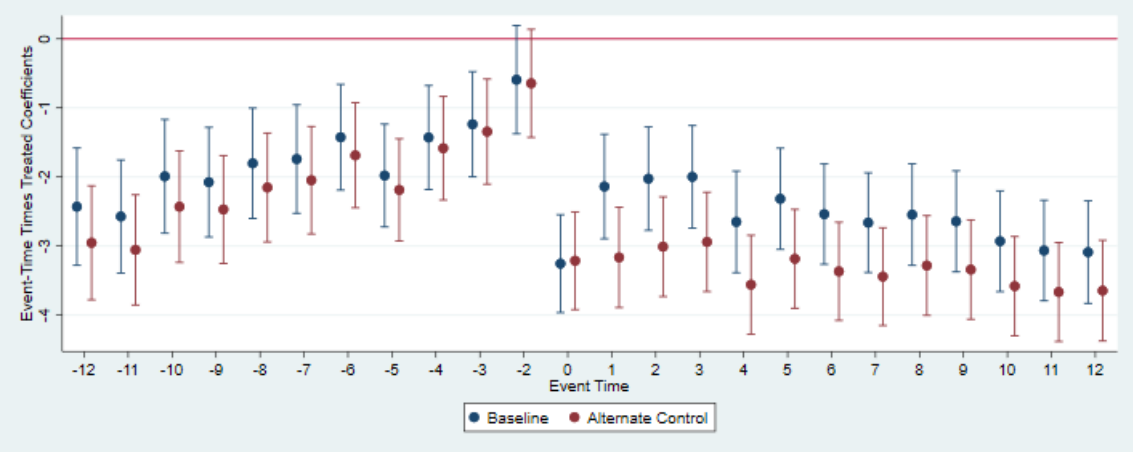
This table presents changes in treated firm VC finance after the super-large VC investment event using a similar matching strategy as baseline, but matching only along founding year. Observations are at the firm-quarter level. $\mathbb{1}\{\text{VC Finance}\}$ is an indicator for the startup firm receiving VC finance in the observation quarter. VC Finance ($M) is the amount (in millions) of VC finance received by the startup firm in the observation quarter, winsorized at the 1% level. Treat is an indicator for the startup firm being a competitor of a super-large VC's investment, Post is an indicator for the observation quarter being after the super-large VC event. Standard errors (in parentheses) are clustered at firm level. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

Dep.Var.	$\mathbb{1}$**{VC Finance}**		**VC Finance ($M)**	
	(1)	(2)	(3)	(4)
Post × Treat	-0.0184***	-0.0146***	-0.0592***	-0.0389***
	(0.00109)	(0.00107)	(0.00365)	(0.00353)
Treat	0.0485***		0.166***	
	(0.000960)		(0.00322)	
Post	0.000481	-0.000875	0.00800***	-0.0134***
	(0.000542)	(0.000953)	(0.00151)	(0.00302)
Firm FE	NO	YES	NO	YES
Time FE	NO	YES	NO	YES
No. Observations	452,935	452,935	452,935	452,935
adj. R-squared	0.013	0.001	0.013	0.001

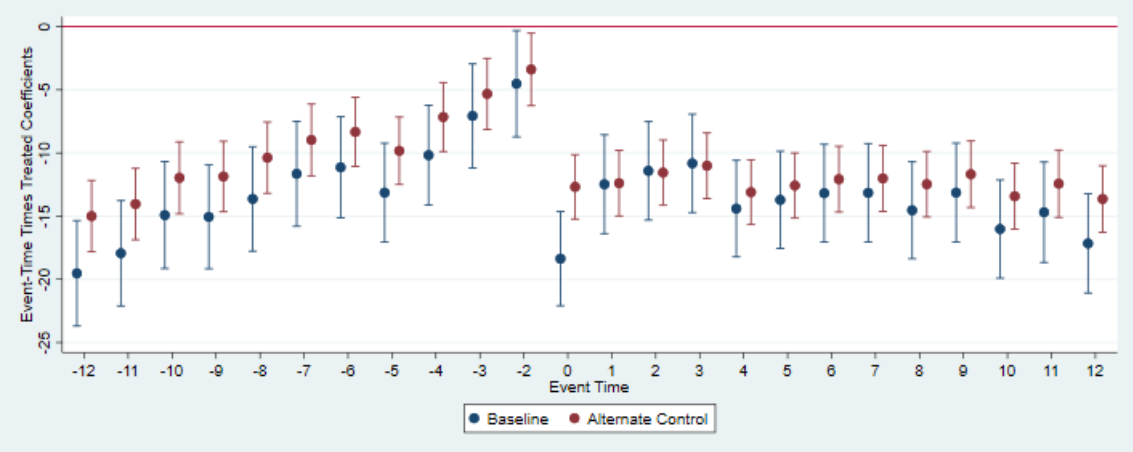
Table C2: **Varying the treatment window**

This table presents changes in treated firm VC finance after the super-large VC investment event varying the event time window used. The columns correspond to an event time window (T) of ± 10, 8, 6, 4, 2, and 1 quarter(s) about the event period, as opposed to 12, used throughout the body of chapter 3. Observations are at the firm-quarter level. The dependent variable for panel A is $\mathbb{1}\{\text{VC Finance}\}$, an indicator for the startup firm receiving VC finance in the observation quarter. The dependent variable for panel B is VC Finance ($M), the amount (in millions) of VC finance received by the startup firm in the observation quarter, winsorized at the 1% level. Treat is an indicator for the startup firm being a competitor of a super-large VC's investment, Post is an indicator for the observation quarter being after the super-large VC event. Standard errors (in parentheses) are clustered at firm level. $^{***}p < 0.01$ denotes significance at the 1% level, $^{**}p < 0.05$ denotes significance at the 5% level, and $^{*}p < 0.10$ denotes significance at the 10% level.

Panel A: Dependent variable: $\mathbb{1}\{$*VC Finance*$\}$						
	$T = 10$	$T = 8$	$T = 6$	$T = 4$	$T = 2$	$T = 1$
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treat	-0.0292***	-0.0330***	-0.0336***	-0.0398***	-0.0345*	-0.0252
	(0.00624)	(0.00673)	(0.00758)	(0.00943)	(0.0136)	(0.0185)
Post	-0.0643***	-0.0599***	-0.0525***	-0.0264*	0.0463**	0.174***
	(0.00670)	(0.00754)	(0.00875)	(0.0107)	(0.0146)	(0.0202)
Firm FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
No. Observations	315,529	266,481	212,257	152,710	87,706	53,441
adj. R-squared	0.001	0.001	0.001	0.001	0.002	0.006
Panel B: Dependent variable: VC Finance ($M)						
	$T = 10$	$T = 8$	$T = 6$	$T = 4$	$T = 2$	$T = 1$
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treat	-0.0292***	-0.0330***	-0.0336***	-0.0398***	-0.0345*	-0.0252
	(0.00624)	(0.00673)	(0.00758)	(0.00943)	(0.0136)	(0.0185)
Post	-0.0643***	-0.0599***	-0.0525***	-0.0264*	0.0463**	0.174***
	(0.00670)	(0.00754)	(0.00875)	(0.0107)	(0.0146)	(0.0202)
Firm FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
No. Observations	315,529	266,481	212,257	152,710	87,706	53,441
adj. R-squared	0.001	0.001	0.001	0.001	0.002	0.006



(a) Alternate Control Group: VC Finance Indicator



(b) Alternate Control Group: VC Finance Amount

Figure C1: **Alternate treatment group, dynamic DID coefficients**

These charts show the estimates of coefficients from Equation 3.2, using the less restrictive match to construct the control group (matching only on founding year). The X-axis charts the coefficients, with their 95% confidence intervals. The blue dots chart coefficients using the baseline control group. The red dots chart coefficients using the alternate control group. Coefficients should be interpreted as an effect of treatment in a given event time, relative to the comparison period: the quarter before placebo treatment. The LHS variable used in the estimates from Panel (a) is an indicator for the startup firm receiving VC finance in the observation quarter. The LHS variable used in the estimates from Panel (b) is the amount of VC finance ($M) that the startup received in the observation quarter.

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