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THREE ESSAYS IN EMPIRICAL CORPORATE FINANCE

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FACULTÉ DES HAUTES ÉTUDES COMMERCIALES

DÉPARTEMENT DE FINANCE

**THREE ESSAYS IN EMPIRICAL
CORPORATE FINANCE**

THÈSE DE DOCTORAT

présentée à la

Faculté des Hautes Études Commerciales
de l'Université de Lausanne

pour l'obtention du grade de
Docteur ès Sciences Économiques, mention « Finance »

par

Rustam ABUZOV

Directeur de thèse
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Jury

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Sans se prononcer sur les opinions de l'auteur, la Faculté des Hautes Etudes Commerciales de l'Université de Lausanne autorise l'impression de la thèse de Monsieur Rustam ABUZOV, titulaire d'un bachelor en économie et d'un master en finance de l'École des hautes études en sciences économiques de Moscou, en vue de l'obtention du grade de docteur ès Sciences économiques, mention « finance ».

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Summary

In Chapter 1, I study the effect of limited attention on resource allocation by venture capitalists. Using engagement in the IPO process as a measure of distraction, I document that investments made by distracted venture capitalists into new portfolio companies tend to underperform in terms of their future financial success. Such companies are 7% less likely to go public or become acquired, and also exhibit lower exit multiples. The adverse effect of the attention constraints is present only in the vicinity of the distracting IPO and manifests itself both for individual partners and venture capital funds. Overall, the evidence indicates that the scarcity of attention hypothesis holds in the context of deal sourcing and screening in venture capital, highlighting the presence of skill in the company selection process.

In Chapter 2, I document that the private information disclosure shapes the choice of limited partners by venture capital firms. Following the court rulings in 2002-2003, public pensions and public university endowments disclose historical fund-level performance information. The response of venture capital firms to the disclosure was heterogeneous. Top-tier firms that tend to have oversubscribed funds exclude public institutions from their new funds compared to less successful and younger firms. Subsequently, domestic nonpublic institutional investors and foreign limited partners experience an increase in their capital commitments to top-tier venture capital firms. I find no evidence suggesting that the changes are driven by the unwillingness to disclose poor post-dotcom bubble returns. The reversal of the trend highlights the uncertainty over the scope of regulation as the primary reason behind the capital reallocations.

In Chapter 3, together with Christoph Herpfer and Roberto Steri we investigate the link between competition in credit markets and an important non-price term in lending, financial covenants. Using an exhaustive dataset covering the U.S. market for leveraged loans, we exploit a regulatory action as a plausibly exogenous reduction in the ability of regulated banks to offer loan contracts with lax covenant protection. As regulated banks demand relatively more covenants, borrowers switch to unregulated lenders, or shadow banks. As a consequence, the market share of regulated banks declined by roughly $30bn, to the advantage of the shadow banking system. This shift is not driven by a general drop in loan supply or change in other lending terms by regulated banks. Our results suggest the necessity to internalize the effects of non-price competition between the regulated and the non-regulated sectors in regulatory decision making.

Acknowledgements

Contents

Chapter 1

1 The Impact of Venture Capital Screening

1.1 Introduction

Both academics and practitioners have long recognized the vital contribution of venture capital to the economy in terms of innovation and job creation. As financial intermediaries, venture capitalists (VCs) perform the central function of the venture capital market – resource allocation. The role of VCs is to fund promising young companies with the highest potential and to create value by supporting and monitoring their investments. While venture capitalists are engaged with multiple startups simultaneously, VCs' attention is not unbounded. Busy VCs often find themselves balancing their time and effort between different startups within their portfolio.[1] In my paper, I exploit this conflict to document the presence of skill in the capital allocation decisions of VCs.

Although the literature shows that VCs rank deal sourcing and screening as the most important factors in terms of the final success, surprisingly little is known about the efficiency of their investment choices.[2] VCs typically analyze hundreds of startups to fund a few, but are they able to identify young companies which are poised to succeed? The high level of risk and uncertainty is inherent in new ventures, and a startup's success is difficult to predict. It could be the case that the stellar performance of renowned venture capital firms is built on better post-investment monitoring (Bernstein et al., 2016), networks (Hochberg et al., 2007), and reputation (Atanasov et al., 2012), combined with initial luck in the company selection. Furthermore, Nanda et al. (2020) argue that performance persistence in venture capital (VC) stems from VC firms' initial success, rather than their ability to select and nurture portfolio companies. A major challenge in empirically identifying the effect of VCs' abilities on the

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[1]Startup founders and VC partners are wary of this problem, as some VCs might sit on 20 different boards simultaneously ("Start-ups grumble about directors too busy to help", *Wall Street Journal*, 2010, by Scott Austin).

[2]Gompers et al. (2020) show that three core VC activities – screening, post-investment monitoring and deal flow – are ranked as the most important factors contributing to the final success of an investment by 49%, 27% and 23% of VCs respectively.

choice of portfolio companies is that the level of effort exerted in the assessment of investment opportunities is not observable. Studying educational and career profiles of VCs could be one way to proxy for their skills, however many other unobservable factors might be playing a role in the company selection, thereby contributing to final outcomes.

To overcome this identification challenge and to isolate the impact of screening, I exploit shocks to individual VC's screening capacity in the form of the initial public offering (IPO) engagement. While the process of going public entails long-term planning and preparation, an individual venture capitalist has only a limited ability to influence the exact timing of her IPO engagement. First, the decision to go public is made by a board of directors, including VCs, founders, and independent directors. Second, the specific quarter of the year when a company goes public is largely determined by market conditions.[3] Due to these factors, the active phase of the process is likely to cause a shock to the involved parties' attention shortly before the event, since the ability to arrange the process in advance is limited. In line with Kaplan and Strömberg (2001), who note that time, not capital, is the scarcest commodity in venture capital, I argue that the involvement of VC partners with existing portfolio companies in the IPO process impairs VCs' ability to efficiently source and screen potential startups to invest in.

I start by constructing the granular data based on VC investments, public offerings, board memberships, and the employment history of venture capitalists. The resulting dataset allows to introduce the measure of distraction defined at the individual partner level, as I observe investment decisions and IPO engagements of a particular venture capitalist over time. To provide the validity for the measure of distraction, I show that VCs are less likely to make investments around IPOs. However, if new investments do occur concurrently with the distraction events, they tend to perform worse. I find that such companies have a 7% lower chance of going public or being acquired and exhibit 16% lower exit multiples. I reinforce the

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[3]A survey by Gompers et al. (2016) indicates that capital market conditions are considered as the most significant factor for exit timing, being an important concern for 96.9% of the private equity investors in their sample. The Harvard Business Case by Katz and Sahlman (1999) provides evidence that even though the CEO of Amazon.com Jeff Bezos had planned the IPO in advance, market conditions played a central role regarding the exact timing.

causal interpretation of this finding by estimating a sequence of tests. The tests indicate that the results hold during both hot and cool market conditions, and are not driven by time-variant industry level characteristics as well as IPO waves. The estimations at the VC fund level produce qualitatively similar results. In all, these findings indicate that deal origination and screening are relevant channels of generating returns in the context of venture capital.

While the baseline results show that investments made simultaneously with IPOs exhibit lower performance, there is no obvious reason why this negative impact should be attributable to the screening and deal sourcing channels. For example, the successful exit with high returns might encourage VCs to invest in riskier projects. The fund fee structure is another concern: IPOs might coincide with the end of the fund's investment period, when the management fee base typically switches from committed capital to invested capital (Robinson and Sensoy, 2013). This VC industry convention could push partners to invest in a faster and less considerate manner in order to avoid a drop in the fee base. To study these hypotheses, I estimate placebo tests around the distraction event, and find that the negative effect is present only one quarter before and one quarter after the distracting IPO, likely because of the lag between the investment decision and the final close.[4] Controlling for the time since VC fund inception, which is another way to address the switch in the fee base, does not influence the results. At the same time, an increase in risk-taking would have had a more long-lasting impact, plus such a change should not systematically lead to underperforming investments due to the positive relationship between risk and return.[5]

An alternative confounding factor is that positive market or industry-wide shocks might potentially push companies to go public and VCs to make worse investment decisions, due to lower discount rates and a possible influx of low quality entrepreneurs seeking funding (Rhodes-Kropf et al., 2005). I include industry, time, and industry $\times$ time fixed effects, which help ensure that the results are not driven by changes in investment opportunities. To further corroborate the distraction channel, I exploit sources of heterogeneity in the distraction

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[4]The average investment takes 83 days to close (Gompers et al., 2020).

[5]Relatedly, Tian and Wang (2014) find that firms backed by VCs with higher failure tolerance are more innovative.

intensity. If IPO-induced busyness does indeed matter, I expect the effect to be stronger for IPOs with higher exit multiples and VCs without prior IPO experience. The evidence confirms this heterogeneity – the impact of distraction on future investment outcomes is more pronounced for larger public offerings, likely because VCs get more distracted when potential payoffs are higher. At the same time, the results are weaker for VCs that have had IPOs in the past. Importantly, the detrimental impact of distraction is not present for acquisition-induced busyness, as this type of exit requires less time and effort.[6]

Finally, since the sample contains many VC partners that have never invested during the IPO process, one might be worried about the comparability of the control group in the estimations. To address this concern, I restrict the sample to VC partners that made at least one investment during the IPO engagement, that is, I require the same VCs to be present both in the treatment and control groups. The baseline results hold in this restricted sample, which provides further support to the main findings.

Related literature. This paper is at the intersection of two lines of research: venture capital and the limited attention of economic agents. First, I contribute to the growing empirical literature on the diversity of ways through which venture capitalists can generate returns for their investors. Bernstein et al. (2016) establish the importance of the VC monitoring channel for portfolio company outcomes. Using a structural model, Sørensen (2007) finds that deal sourcing and company selection jointly contribute more than monitoring activities in terms of value added. In contrast, using short-term "within VC partner" jumps in screening costs induced by the IPO engagement, I establish the significant contribution of company selection to final outcomes, which does not entail structural assumptions for identification.

Other papers describe how VCs create value in practice. Megginson and Weiss (1991) show that VCs, as recurring players in the IPO market, build strong relationships with leading underwriters and auditors. Relatedly, the authors find that the presence of VCs in offering

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[6]Venture capitalist William H. Draper III points out that "cashing out through a merger with a larger public company as we did with Torrent and Skype is far less complicated <...> than going through the IPO or public offering route" (Draper, 2012). Additionally, Table A.2 in the Appendix provides anecdotal evidence regarding the involvement of venture capitalists in different stages of the IPO process.

companies lowers the level of underpricing and the amount of underwriter compensation. Kaplan and Strömberg (2001) outline the value of company selection and provide evidence that venture capitalists devote a lot of time to screening, performing an in-depth analysis of potential portfolio companies. Confirming the importance of screening further, a recent survey of venture capitalists by Gompers et al. (2020) reveals that VCs rank screening first for value creation among core VC activities, surpassing deal sourcing and post-investment monitoring. At the same time, Nanda et al. (2020) argue that the stylized fact of performance persistence at the fund level is attributable not to the ability of VCs to pick winners, but rather to the initial success of the VC firm. I contribute to this literature by highlighting the presence of skill in deal sourcing and screening, though the interconnectedness of these two pre-investment activities does not allow me to disentangle one from the other.

Second, this paper contributes to the literature on behavioral finance discussed in detail by Baker and Wurgler (2012), and in particular studying the limited attention of economic agents and its implications for investment outcomes and corporate actions. Kacperczyk et al. (2014) and Ben-Rephael et al. (2017) study the attention allocation of institutional and individual investors and its impact on trading performance. Lu et al. (2016) provide similar evidence for hedge funds' performance, while Kempf et al. (2017) investigate the implications of limited attention of institutional investors on corporate actions. Another large strand of the literature has looked at busy boards of directors, for example, Falato et al. (2014), Hauser (2018), and Ljungqvist and Raff (2018). These studies indicate that busyness is value-destructive, confirming that attention is a limited and scarce resource. Interestingly, in the sample of VC-backed IPOs Field et al. (2013) provide the opposite evidence – busy boards of VC-backed companies are value-creating, due to network connections and experience. Such evidence is in line with the studies documenting the positive impact of VCs' past IPO experience on the performance of their new portfolio companies. I add to the literature on distracted economic agents by showing that earlier results on the negative impact of busyness hold in the context of company screening in the VC market.

The remainder of the paper is organized as follows. Section 2 describes the data, the variables, and the empirical strategy. Section 3 presents the main results. Section 4 provides additional analyses and robustness tests, which further support the validity of the screening and deal sourcing channels. Section 5 concludes.

1.2   Data and Methodology

1.2.1   Data

Data on VC investments come from the Thomson Reuters VentureXpert database, one of the most comprehensive data sources available for research on venture capital. I start with the sample of all investments from VentureXpert made in companies in the United States founded between 1990 and 2017. I drop follow-on participation and consider only new investments.[7] To determine exit outcomes, I subsequently merge data with the SDC Platinum database on acquisitions and IPOs. I assign investments to individual partners based on their board memberships and VC firm affiliations. I obtain information on boards of directors from the BoardEx database, which contains data on board seats and the employment history of directors in both public and private companies. Importantly, the data do not exhibit a strong bias toward public firms, as over a third of all directors in this database have only private firm affiliations. I match portfolio company and venture capital firm names from VentureXpert with company names from BoardEx combining exact and fuzzy name-matching algorithms.[8] I consider the wide range of partner level roles inside venture capital firms ("General Partner", "Partner", "Founding Partner", "Associate Partner", "Managing Partner", "Investment Partner" etc.), and also roles such as "Vice President" and "Principal".[9] For observable starting and ending dates of board memberships, I require IPO and investment events to fall between board

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[7]VCs are likely to face significantly lower level of information asymmetry in case of follow-on participation. Restricting the sample only to new investments allows to keep the degree of information asymmetry between VCs and entrepreneurs in the sample relatively homogeneous.

[8]Appendix A.2 describes the matching algorithms in greater detail.

[9]Based on BoardEx and CapitalIQ, VC firm employees with any of these listed roles might take board seats in portfolio companies. Relatedly, Metrick and Yasuda (2011) point out that the screening process inside the VC fund is usually handled by general partners and principals.

appointment and termination dates. The resulting sample consists of 19,367 investments, 9,049 portfolio companies, 3,655 VC funds, 1,599 VC firms, and 5,963 individual partners. The numbers of matched observations within each dimension are comparable to the corresponding numbers in Ewens and Rhodes-Kropf (2015). My final sample includes 2,097 exit events via public offering or trade sale which corresponds to 23% of all companies.

Two key measures of success in this study are IPOs and acquisitions. I create an indicator variable equal to one if a particular company goes public or gets acquired before the end of the sample period. I require companies to be founded prior to the end of 2012, which allows companies founded at the end of the sample five years to exit.[10] For the second measure of success I focus on exit multiples, defined as the ratio of the company value at exit to the total amount of invested capital. I do not make specific assumptions regarding exit multiples of defunct companies or those remaining private, setting the recovery rates in such cases equal to zero. Studying exit multiples helps alleviate the concern that some acquisitions might be disguised failures, as VCs could decide to sell poorly performing startups.

Table 1.1 presents descriptive statistics at the partner, investment, and company level respectively.

One percent of all investments was made during the quarter when another company, already existing in a portfolio of the same VC partner, went public ("busy" investment). This observation is likely to be an outcome of various frictions like competition for startups, time pressure etc., that keep VCs investing during the periods of increased screening costs. The average VC partner made $5.5 mln worth investments in a given quarter. Companies received $52 mln of VC funding on average and had a median of three investors per financing round. The sample mostly consists of first-round investments. 23% of all companies went public or became acquired. The median startup had two venture capitalists in its board of directors.

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[10]The results are robust to allowing investments ten years to exit.

Table 1.1
Summary Statistics
This table reports summary statistics for the variables underlying the analysis of the impact of VC partner distraction on investment outcomes. All variables are defined in Table A.1. The sample is a quarterly panel of venture capital investments from 1990 through 2017.

Panel A. Partner-level statistics (Partner-quarter level)						
	Obs.	Mean	Median	Std. dev.	Min.	Max.
"Busy" investments	19,367	0.01	0.00	0.12	0.00	1.00
Portfolio size, #	19,367	1.13	1.00	0.37	1.00	4.00
Amount invested, $ mln	19,367	5.46	3.00	12.82	0.00	501.72
Panel B. Investment-level statistics						
	Obs.	Mean	Median	Std. dev.	Min.	Max.
Syndicate size	19,367	3.74	3.00	2.49	1.00	24.00
Round number	19,367	2.24	1.00	1.95	1.00	22.00
Amount raised, $ mln	19,367	1.74	1.70	1.01	0.00	4.62
Panel C. Company-level statistics (conditional on being founded prior to 12/31/2012)						
	Obs.	Mean	Median	Std. dev.	Min.	Max.
IPO/Acquisition	9,049	0.23	0.00	0.42	0.00	1.00
Exit multiple	9,049	1.82	0.00	10.91	0.00	525.67
Total funding, $ mln	9,049	52.06	27.56	164.98	0.00	10,862.61
Age (quarters)	9,049	3.82	2.75	3.38	0.00	24.25
# VCs in a board	9,049	2.14	2.00	1.45	1.00	14.00

1.2.2 Distraction events

Time is a scarce resource in the VC market, where attention-grabbing events happen with regularity. Since VCs' workload is not directly observable, to analyze the economic impact of VCs' distraction one needs to choose an event which (1) arguably has a strong effect on busyness, (2) is observable, and (3) plausibly exogenous, for example, driven by factors that cannot be influenced by VC partners. An attractive candidate for such an event are IPOs. Megginson and Weiss (1991) highlight that VCs take an active participation in the process of going public in their portfolio companies, starting from searching for underwriters to the final exit at the end of the lockup period. This kind of active involvement is not surprising given that IPOs are the most profitable type of exit in the VC market, which drives payoffs to both limited (LP) and general partners (GP). Therefore, careful navigation through the process is key to ensure high exit multiples. An active engagement in the IPO process is likely to

significantly reduce the amount of time and effort that VCs can exert on screening, which in turn impairs their ability to choose promising companies with high growth potential. Possible channels are insufficient due diligence, misvaluation of the potential market, the technology and the management team, etc. One crucial advantage that IPOs as the distraction events offer is that while IPOs are often preceded by the long period of preparation, the exact timing of an IPO is largely influenced by plausibly exogenous capital market conditions. In addition, the active phase of the preparation to go public needs to be carried out shortly before the event, causing a shock to VC's attention.[11] This property of the IPO process helps restrain the measurement error in the independent variable and the associated attenuation bias.

Timing of the screening process. One crucial challenge in relation to studying the screening process is that its timing is not observable until the actual investment date. To overcome this issue, I allocate known investment days across four 90-day windows around the IPO date. Due to the lag between the decision to invest and the actual investment, I estimate the baseline regressions for the first 90-day window after the IPO date, which results in 331 investments made by distracted VC partners. This approach allows to capture investment decisions made before the offering date, as the average time to close an investment is about 83 days (Gompers et al., 2020), while the active phase of the IPO process usually takes three to five months prior to the offering date.[12] Placebo tests support the validity of this assumption.

I refer to the investments made in new portfolio companies during the two 90-day windows around the IPO as "busy" investments. I use the remaining two 90-day intervals in the vicinity of the IPO for placebo tests. The following figure demonstrates this concept taking the general partner of Kleiner Perkins Caufield & Byers, LP. ("General Partner X"), as an example illustrating the terminology I use.

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[11]The composition of general partners within a particular venture capital fund is defined by a limited partnership agreement written at the fund inception. This institutional feature puts a restriction on the ability to quickly and easily adjust the size of a venture capital team by hiring additional general partners with comparable skills and expertise. Appendix A.3 presents excerpts from a typical limited partnership agreement governing issues around time commitments and fund composition.

[12]According to the IPO Roadmap by BVCA (British Private Equity & Venture Capital Association). Various IPO roadmaps from industry professionals report a similar time frame.

GP "X" of Kleiner Perkins Caufield & Byers, LP
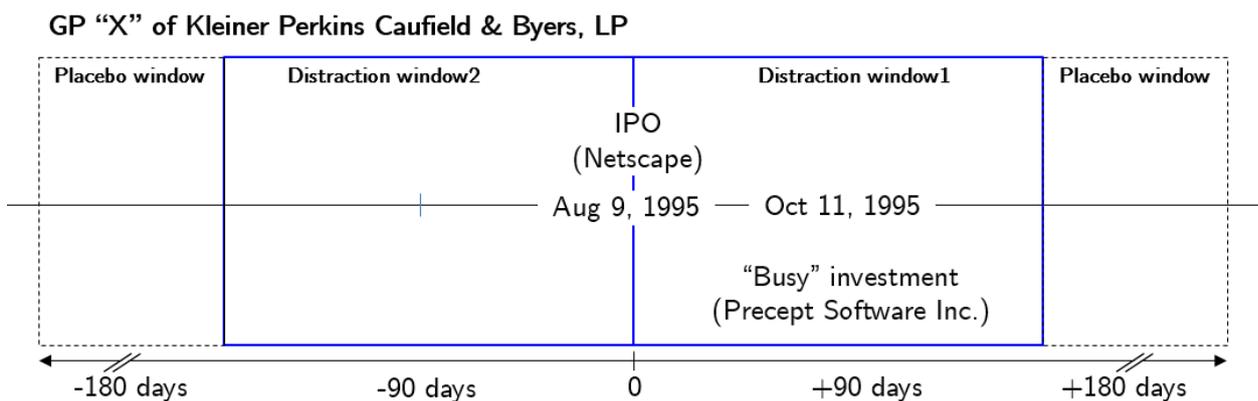
The figure considers the case of two investments made by Kleiner Perkins Caufield & Byers, LP. According to BoardEx and CapitalIQ biography databases, "General Partner X" had board seats in two portfolio companies under consideration – Netscape Corp. and Precept Software Inc. In my analysis, I treat the IPO of Netscape Corp. as an attention-grabbing event for the VC partner, while I refer to the new investment made in Precept Software Inc. as a "busy" investment. Investments that occur during the placebo windows are used for robustness checks. I use this terminology throughout the paper.

The frequency of "busy" investments is an interesting question on its own. The incidence of such investments mostly comes from general partners that manage several funds, or companies included in a portfolio at a later stage, so they become listed firms before the end of a VC fund's investment period. Expectedly, the number of "busy" investments is small, although they do occur throughout the sample period. Figure 1.1 reports the distribution of "busy" investments over time, along with the total number of investments.

A substantial fraction of "busy" investments happened between 1995 and 2000. A potential explanation for the decreasing trend in the number of investments made by distracted VCs after 2000 is the growth in the number of VC partners over time and the decline in the number of IPOs, which are used to identify the distraction windows.[13] The average percentage of "busy" investments observed each year is clustered around 1-2% of the total annual volume of VC

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[13]Metrick and Yasuda (2011) provide evidence showing that the number of VC principals doubled between 1995 and 2005.

investments, with a maximum of almost 6% in 1999. Given the amount of effort and time the

IPO process might take, this number appears to be expectedly low.

Figure 1.1 The time trend of "busy" investments
This figure plots the annual dynamics in the number of investments conditional on partners
taking a board seat. The red line represents the total number of investments, the histogram
represents the fraction of "busy" investments in the total number of investments. Data come
from the Thomson Reuters VentureXpert database merged with BoardEx.
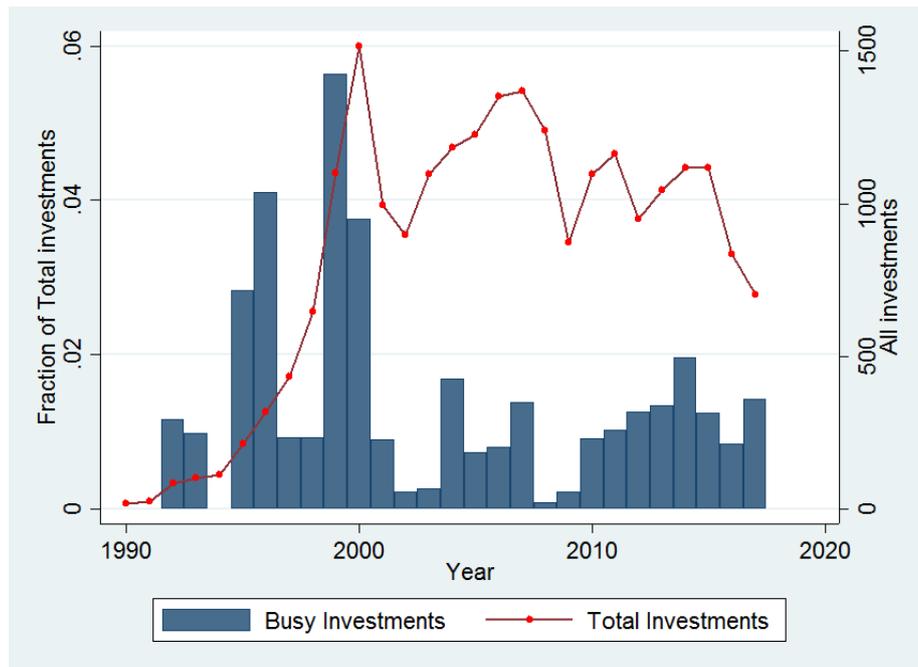
1.2.3   Empirical methodology

I consider the following linear model as my baseline specification

$$Y_{ijt} = \beta_1 BusyPartner_{jt} + \beta_2 X_{jt} + \beta_3 Z_{ijt} + \delta_j + \gamma_k + \phi_l + \eta_t + \varepsilon_{ijt} \tag{1}$$

where i denotes the entrepreneurial firm, j the VC partner, t the quarter of the investment,

k and l index industry of operation and state of location of entrepreneurial firms. Y is the

outcome variable of interest, *BusyPartner* is an indicator variable showing whether venture

partner j is busy in a quarter t, X_{jt} is a vector of time-variant VC partner level controls, which

include the invested amount and the number of portfolio companies measured at the quarter

frequency, Z_{ijt} is a vector of investment level control variables, which is composed of the

syndicate size, the round number, the company age and the amount raised, δ_j and η_t are the VC partner and investment quarter fixed effects, γ_k and ϕ_l are the portfolio company's industry and state fixed effects respectively, and ε is an error term. I consider two types of outcome variables: an indicator variable for going public or being acquired, and exit multiples. All variables are defined in Table A.1 in the Appendix. In all specifications standard errors are double-clustered at the VC partner and the investment quarter level and are robust to heteroskedasticity. The coefficient of interest is β_1, which measures the impact of VC's distraction on investment outcomes.

VC partner fixed effects are the essential element of the identification strategy in this paper, as they allow me to study investment decisions made by the same VC partner over time, meaning that any time-invariant unobservable VC partner characteristics cannot influence the findings. To the extent that a VC partner's ability to anticipate the future IPO engagement and new investment rounds is a time-invariant characteristic, this concern is also taken care of by the inclusion of VC partner fixed effects. Additionally, all regressions include the portfolio company's industry and state of location fixed effects, which allow to make sure that the results are not influenced by time-invariant differences among industries and states. Robustness tests also contain industry $\times$ time fixed effects, which alleviate the issue that industry level fluctuations over time, for example, shocks to investment opportunities, might drive the baseline results. The latter concern is particularly important, since both the decision to go public and to deploy capital might be influenced by market or industry-wide factors: for example, hot market conditions make IPOs attractive for companies timing the market, and the associated high-growth expectations create the inflow of lower-quality startups. To further corroborate the validity of the results, I exploit heterogeneity in the treatment intensity. I find that the negative effect of distraction is particularly pronounced for distracting IPOs with higher exit multiples. This evidence allows to argue that the results are likely to be attributable to the distraction channel.

While IPO engagement can be a good measure for VC partner's distraction, the effect might also have other origins. The alternative story could be that VC partners involved in an

upcoming IPO might be willing to take more risk in their new investments because of having a successful exit. Another identification concern is the management fee calculation rules: at the end of the investment period the fee base switches from the committed to the invested capital, which might push VCs to make faster and less considerate investment decisions. To show that these alternative explanations are not likely to be an underlying mechanism in my setting, I (1) estimate placebo tests around the IPO date, and (2) control for the time since VC fund inception. The estimations mitigate the presence of these two channels: the results are significant only in the vicinity of the distracting IPO, while changes in risk-taking and the fee base would have had a more long-lasting impact. Controlling for the time since VC fund inception does not particularly influence the findings.

Company screening in its broad sense includes a range of activities, varying from deal sourcing and deal negotiation to contracting. One might hypothesize that distracted VCs are less likely to have a board representation, which subsequently leads to lower post-investment monitoring incentives. I do not find systematic patterns in relation to the probability of taking a board seat, as well as preferences of busy VCs regarding the type of investment security. The lack of data on VC's deal flow, the design of term sheets and investor agreements limits the ability to disentangle different dimensions of company selection. While the data scarcity reduces the possibility to pin down the exact second-order mechanisms, this limitation does not diminish the relevance of the screening channel being a route through which the impact of busyness gets transmitted into investment outcomes.

1.3 Empirical results

1.3.1 Financial success

I begin by examining financial outcomes of new investments, namely the likelihood of going public or being acquired and the log (plus one) of exit multiples. In each case I present specifications based solely on the variable of interest as well as controlling for investment and VC partner level characteristics.

Columns 1-3 of Table 1.2 indicate that the likelihood of exiting via IPO or acquisition is 7% lower for investments made by distracted VCs. Exit multiples tend to be 16% lower for such portfolio companies. The effect is economically meaningful, as the unconditional probability of exit in the sample is 23%, and the average exit multiple is 1.82.

Table 1.2
Distracted VC partners and performance of new investments
This table presents estimates of the effect of VC partner distraction on financial outcomes of new investments. All variables are defined in Table A.1. The sample is a quarterly panel of venture capital investments from 1990 through 2017. Parentheses contain standard errors clustered by VC partner and quarter and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	IPO/Acquisition			ln(exit multiple+1)		
	(1)	(2)	(3)	(4)	(5)	(6)
Busy Partner	−0.076***	−0.078***	−0.073***	−0.163***	−0.163***	−0.159***
	(0.024)	(0.024)	(0.025)	(0.051)	(0.052)	(0.052)
Round characteristics						
ln(Syndicate size)			0.064***			0.074***
			(0.011)			(0.022)
ln(Round number)			0.046***			0.031**
			(0.008)			(0.014)
ln(Company age)			−0.014			−0.003
			(0.009)			(0.018)
ln(Amount raised)			0.035***			0.069***
			(0.006)			(0.012)
Partner characteristics						
ln(Portfolio size)			0.025			0.009
			(0.026)			(0.042)
ln(Amount invested)			−0.003**			−0.010***
			(0.002)			(0.003)
Partner FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	No	Yes	Yes	No	Yes	Yes
State FE	No	Yes	Yes	No	Yes	Yes
Observations	16,815	16,815	16,815	16,815	16,815	16,815
R2	0.35	0.36	0.37	0.35	0.35	0.36

These results reveal that investments made concurrently with the distraction events seem to create less value for their investors. Control variables for round characteristics have a reasonable sign and magnitude: the chances of exit increase with the size of the syndicate, the

round number, and the amount raised from investors. The effect of the portfolio size and the amount invested on the financial success is not particularly pronounced.

To corroborate the finding, I replicate the analysis at the VC fund and VC firm level. Table 1.12 reveals that the baseline regressions estimated at the VC fund level produce both qualitatively and quantitatively similar results. At the same time Columns 2 and 4 show that the impact of distraction is not observable in the sample of VC firms, because this level of aggregation does not distinguish well between busy and non-busy partners working inside the same VC firm.

Table 1.3

Dynamic effect of VC partner distraction on financial outcomes

This table studies the dynamic effect of VC partner distraction on the probability of exit for new investments made 180 days before and after the distracting IPO. All variables are defined in Table A.1. The sample is a quarterly panel of venture capital investments from 1990 through 2017. Parentheses contain standard errors clustered by VC partner and quarter and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	IPO/Acquisition		ln(exit multiple+1)	
	(1)	(2)	(3)	(4)
Between [-180; -90 days)	0.046	0.044	0.021	0.017
	(0.029)	(0.027)	(0.047)	(0.045)
Between [-90; 0 days)	-0.060**	-0.058**	-0.136***	-0.131***
	(0.023)	(0.023)	(0.047)	(0.046)
Between [0; 90 days)	-0.079***	-0.074***	-0.162***	-0.158***
	(0.024)	(0.025)	(0.051)	(0.051)
Between [90; 180 days)	0.011	0.012	-0.074	-0.074
	(0.029)	(0.029)	(0.051)	(0.051)
Controls	No	Yes	No	Yes
VC Partner FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	16,815	16,815	16,815	16,815
R2	0.36	0.37	0.35	0.36

I further study the robustness of the findings and consider the effect of VC partner's busyness in dynamics – I compare the baseline results based on four 90-day intervals around

the offering date. Table 1.3 reveals the results for the probability of new portfolio companies becoming listed or being acquired as well as exit multiples.

Table 1.3 reports that investments made within [-180; -90 days) and [90, 180 days) windows are not statistically different from investments made any other time. The estimations in Columns 1 and 2 indicate that companies added to the portfolio within 90 days prior to the offering date exhibit significantly negative performance in terms of the likelihood of exit, while the effect becomes more pronounced after the IPO date. This pattern confirms the initial hypothesis about the lag between the observable investment date and the start of the screening process. As Gompers et al. (2020) note, it takes roughly a quarter to close the investment, meaning that the first post-IPO quarter captures a significant portion of the "busy" investment decisions. Since IPO engagement lasts on average three to five months, a fraction of "busy" investments occurs in the quarter immediately preceding the IPO, which explains the presence of the impact also prior to the event.

Analogously, Columns 3 and 4 show that in case of exit multiples the negative impact of busyness manifests itself only two 90-day windows around the IPO. The effect is still more pronounced for the first post-IPO quarter, likely because this window captures the higher number of "busy" investments vis-à-vis the previous quarter. This finding is consistent with the assumption about the timing of the selection process and IPO engagement.

Figure 1.2 compares the regression coefficients across all four windows. The coefficient estimates follow similar trends for both measures of success, with the significance clustering around the IPO. This pattern is consistent with the time lag between the company selection and the day we observe the investment in the data.

I limit post-IPO windows to the first two quarters in order to avoid the potential influence of another event – the end of the lockup period, which usually lasts 180 days after the initial offering and might affect VCs' behavior in different ways. Lerner et al. (2012) describe the distribution of cash flows among limited and general partners as an extremely complex process, which requires dealing with a number of competing interests. Furthermore, VCs and limited partners might decide to use cash flows to invest, possibly in a faster and less

considerate manner. Therefore, the post-IPO cash allocation process might play out regarding the investment decisions made after the end of the lockup period via various channels.

1.3.2 Treatment intensity

Figure 1.2 Dynamic effect of VC partner distraction on financial outcomes
This figure plots the regression coefficients from Table 1.3. The dots represent the coefficient estimates from the regressions on the likelihood of success and the exit multiples, along with 90% confidence intervals. The horizontal axis indicates the distance (in days) of the investment from the relevant IPO, aggregated to four 90-day bins. Day 0 is the day of an IPO. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.
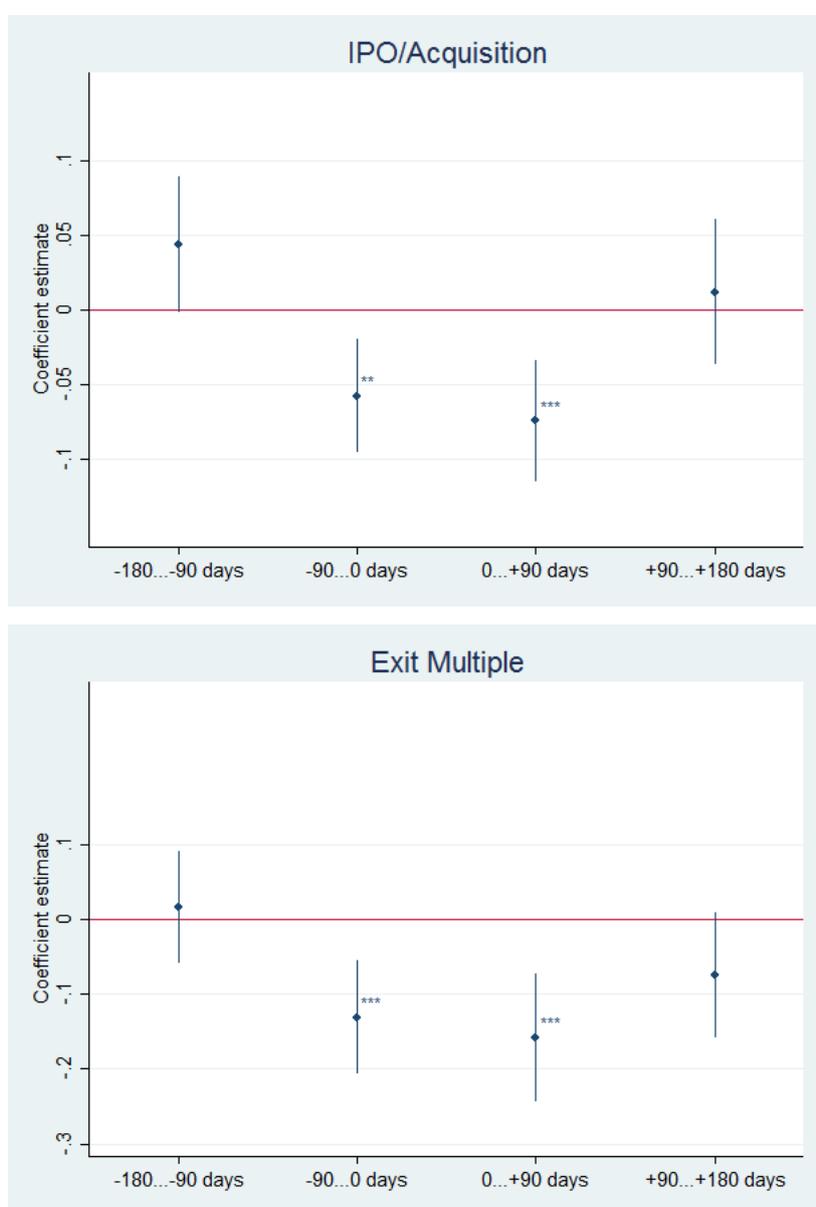
If the results are indeed attributable to the distraction mechanism, one would expect the effect to be stronger in case of higher treatment intensity. One way to capture this type of heterogeneity is to compare distracting IPOs based on their relative significance. Arguably, VCs are likely to spend more time on IPOs promising potentially higher exit multiples compared to public offerings with lower payoff.

To test this hypothesis, I interact *Busy Partner* with two indicator variables, specifying cases of high and low treatment intensity. I consider the distraction intensity to be high if the distracting IPO has an above-median exit multiple or market capitalization based on the offering price. Table 1.4 contains the baseline specification exploiting variation in IPO size as a source of heterogeneity in the treatment intensity.

Table 1.4
Treatment intensity: large versus small IPOs
This table reestimates Table 1.2 by separating the indicator of distraction into two variables. *"Large"* is an indicator variable equal to one if the distracting IPO has above-median exit multiple or size. *"Small"* is an indicator variable equal to one if the distracting IPO has below-median exit multiple or size. All variables are defined in Table A.1. The sample is a quarterly panel of venture capital investments from 1990 through 2017. Parentheses contain standard errors clustered by VC partner and quarter and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	IPO/Acquisition		ln(exit multiple+1)	
	IPO multiple	IPO size	IPO multiple	IPO size
	(1)	(2)	(3)	(4)
Busy × Large IPO	-0.080***	-0.104***	-0.203***	-0.224***
	(0.030)	(0.028)	(0.070)	(0.064)
Busy × Small IPO	-0.061	-0.019	-0.106*	-0.057
	(0.039)	(0.039)	(0.059)	(0.076)
Controls	Yes	Yes	Yes	Yes
Partner FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	16,815	16,815	16,815	16,815
R2	0.36	0.37	0.35	0.36

The estimations suggest that the negative relationship between busyness and financial success is much more pronounced for distracting IPOs with higher exit multiples and size. In case of exit multiples, the coefficient is almost two times smaller for VC partners involved in public offerings of lower magnitude.

Venture capitalists also differ from each other in terms of their past IPO experience. Seasoned VCs might be less prone to the negative impact of distraction because of their expertise and/or improved deal flow. Therefore, IPOs are likely to be more detrimental for partners that have no prior IPO experience. I test this hypothesis by defining cases of high and low distraction based on prior IPO involvement. I assign high distraction intensity to exits when VCs did not have IPOs in the past. Table 1.5 reports the comparative impact of two types of cases.

Table 1.5
Treatment intensity: past IPO experience
This table reestimates Table 1.2 by separating the indicator of distraction into two variables. "0 IPO before" is an indicator variable equal to one if the VC partner had no past IPO experience. " 1 IPO before" is an indicator variable equal to one if the VC partner had one or more IPOs in the past. All variables are defined in Table A.1. The sample is a quarterly panel of venture capital investments from 1990 through 2017. Parentheses contain standard errors clustered by VC partner and quarter and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	IPO/Acquisition		ln(exit multiple+1)	
	(1)	(2)	(3)	(4)
Busy x = 0 IPO before	-0.132***	-0.127***	-0.191***	-0.185***
	(0.032)	(0.033)	(0.052)	(0.053)
Busy $\geq$ 1 IPOs before	-0.028	-0.023	-0.136	-0.134
	(0.037)	(0.039)	(0.082)	(0.083)
Controls	No	Yes	No	Yes
Partner FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	16,815	16,815	16,815	16,815
R2	0.36	0.37	0.35	0.36

The results indicate that the coefficients of interest are much more pronounced for less seasoned VC partners. VCs with prior IPO experience might have knowledge and skills to mitigate the negative effect of busyness and navigate through the process more efficiently. This result additionally suggests that the channel behind the results seems to be distraction, which impairs VCs' abilities to originate and screen investment opportunities.

1.3.3 Instrumental variables approach

In this subsection I exploit the heterogeneity linked to the IPO experience further by building on the earlier evidence on the persistence in exit styles of individual VCs (Ewens and Rhodes-Kropf, 2015). Past IPO engagements are likely to lead to a new IPO engagement, which allows to use the prior IPO experience as an instrumental variable for current busyness. As prior success could alter the quality of the deal flow, in these regressions I directly control for this channel via the inclusion of very granular VC firm x Time FEs, which take care of the variation in deal flow at the VC firm level (Nanda et al., 2020). Table 1.6 contains the results. In Columns 1-2, I reestimate my baseline regressions including VC firm x Time fixed effects, to show that the results continue to hold after controlling for time-variant VC firm level characteristics. Then, I use the number of IPOs that a particular venture capitalist had in the past to predict his or her future IPO engagement. Columns 3-4 indicate that the relevance restriction is satisfied and this outcome is consistent with the existing empirical evidence on the persistence in individual exit styles. Columns 5-6 contain the second stage estimates which reveal that the relationship between busyness and the quality of concurrent investments remains negative after using the instrumented values for the independent variables. Regarding both exit porbabilities and exit multiples, the local average treatment effects (LATE) are somewhat larger than the coefficients in the OLS version presented in Column 2. This observation could be explained by the tendency of the IV approach to inflate the coefficients of interest compared to the corresponding uninstrumented estimates (Jiang, 2017).

Table 1.6

Distracted VC partners and performance of new investments: instrumental variables approach

This table presents estimates of the effect of VC partner distraction on performance of new investments, using instrumental variables approach. All variables are defined in Table A.1. The sample is a quarterly panel of venture capital investments from 1990 through 2017. Parentheses contain standard errors clustered by VC partner and quarter and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	IPO/Acq.	Exit multiple	Busy Partner		IPO/Acq.	Exit multiple
	OLS		IV 1st stage		IV 2nd stage	
	(1)	(2)	(3)	(4)	(5)	(6)
Busy Partner	-0.062**	-0.148**				
	(0.028)	(0.067)				
Log(# Prior IPOs)			0.098***	0.098***		
			(0.025)	(0.026)		
$\widehat{BusyPartner}$					-1.032**	-2.178***
					(0.450)	(0.736)
Controls	Yes	Yes	No	Yes	Yes	Yes
Partner FE	Yes	Yes	Yes	Yes	Yes	Yes
VC Firm x Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,466	13,466	5,575	5,575	5,575	5,575
R2	0.57	0.56	0.47	0.47	0.47	0.45
Wald F					14.05	14.05

1.3.4 Alternative explanations

If new investment opportunities are predictable and VCs have a large influence over the decision to go public, venture capitalists could theoretically time their IPO engagement when they expect projects of lower quality to arrive. This possibility gives rise to endogeneity concerns between the IPO engagement and the screening intensity. However, due to differences in timelines, the decision to go public is likely to be made when the following quarter's deal flow is not observable, that is, the IPO decision is plausibly exogenous regarding the quality of deal flow in one or two quarters. Table 1.3 additionally shows that the quality of new investments recovers in three quarters after the start of the IPO engagement, which is both (1) harder to predict for forward-looking VCs and (2) makes it difficult to rationalize why VCs knowingly make

worse investments when they can wait for one to two quarters. To reinforce these arguments further, I perform two types of tests. First, I focus on IPO decisions driven by favorable market conditions. Similar to Bernstein (2015), I use fluctuations in the NASDAQ Composite Index measured over the two months prior to the IPO filing date.[14] I then split distracting IPOs into two variables based on the above (*Hot Market*) and below-median (*Cool Market*) 60-day NASDAQ returns.

Table 1.7
(In)Sensitivity to pre-IPO market fluctuations
This table reestimates Table 1.2 by separating the indicator of distraction into two variables. *"Hot Market" ("Cool Market")* is an indicator variable equal to one if the filing date of the distracting IPO is preceded by above-median (below-median) NASDAQ fluctuations. Two-month windows are used to measure NASDAQ fluctuations. All variables are defined in Table A.1. The sample is a quarterly panel of venture capital investments from 1990 through 2017. Parentheses contain standard errors clustered by VC partner and quarter and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	IPO/Acquisition		ln(exit multiple+1)	
	(1)	(2)	(3)	(4)
Busy x Hot market	-0.075**	-0.071*	-0.169**	-0.164**
	(0.037)	(0.038)	(0.068)	(0.067)
Busy x Cool market	-0.087**	-0.081*	-0.177**	-0.173**
	(0.042)	(0.042)	(0.073)	(0.074)
Controls	No	Yes	No	Yes
Partner FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	16,809	16,809	16,809	16,809
R2	0.36	0.37	0.35	0.36

Table 1.7 indicates that the findings continue to hold during the *Hot Market* conditions, that is, when the decision to go public was plausibly exogenous in relation to the deal flow quality, and hence the screening intensity. This observation helps alleviate the issue that the result might potentially be driven by VCs that base their IPO decisions on the observation of a worse deal flow quality in upcoming quarters. Importantly, the ability of VCs to force the

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[14]The IPO filing date is based on the original Form S-1 filing date with SEC Edgar system.

startup to go public is also limited – the final decision is to be made by the board of directors, including a CEO.[15]

The time variation of investment and exit opportunities is another concern. Positive market or industry-wide shocks might push companies to go public and investors to make worse investment decisions, due to higher valuations, lower discount rates and a possible inflow of low quality entrepreneurs.

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[15]Anecdotes indicate that CEOs can play an important role in deciding to go public or not. M. Svane (founder and CEO of Zendesk) remembers that "I realized that it would come down to me making a decision". And despite doubts about the market, he "wanted to get the IPO done and move on" (Svane, 2014).

Table 1.8
(In)Sensitivity to time-variant industry characteristics and IPO waves
This table contains reestimations of Table 1.2 controlling for industry-wide fluctuations and IPO waves. All variables are defined in Table A.1. Panel A estimates the effect of VC partner distraction on financial outcomes of new investments with industry × time fixed effects. Panel B directly controls for the total number of IPOs in the market minus distracting IPOs for a given partner. The sample is a quarterly panel of venture capital investments from 1990 through 2017. Parentheses contain standard errors clustered by VC partner and quarter and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Panel A: Industry × Time FEs			
	IPO/Acquisition		ln(exit multiple+1)	
	(1)	(2)	(3)	(4)
Busy Partner	-0.063**	-0.057**	-0.139***	-0.135***
	(0.024)	(0.025)	(0.047)	(0.046)
Controls	No	Yes	No	Yes
Partner FE	Yes	Yes	Yes	Yes
Industry × Time FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	16,769	16,769	16,769	16,769
R2	0.40	0.41	0.39	0.40

	Panel B: Controlling for the Total Number of IPOs in the market			
	IPO/Acquisition		ln(exit multiple+1)	
	(1)	(2)	(3)	(4)
Busy Partner	-0.078**	-0.075**	-0.208***	-0.205***
	(0.034)	(0.034)	(0.078)	(0.076)
Number of IPOs in the market	-0.007	-0.028	-0.565	-0.579
(minus distracting IPOs by partner)	(0.293)	(0.281)	(0.654)	(0.632)
Controls	No	Yes	No	Yes
Partner FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	16,815	16,815	16,815	16,815
R2	0.36	0.37	0.35	0.35

To make sure that the results are not confounded by these factors, I reestimate the baseline regression including industry × time fixed effects (Panel A). Additionally, I directly control for the number of IPOs minus the number of IPOs in the distraction window of a given VC partner.

The coefficients of interest in Panel A of Table 1.8 are less pronounced compared to the baseline estimations, but they do remain statistically significant and economically meaningful across all specifications, which helps ensure that the impact of industry-wide fluctuations is limited. This result is also in line with Panel B, which shows that the total number of IPOs has only a mild impact on the exit prospects of new investments made in the same quarter.[16]

1.3.5 Investment activity around IPOs

Given the time and effort-consuming nature of the pre-IPO process, the measure of VCs' distraction I use in this paper is based on the IPO engagement: I consider a particular VC partner as being distracted if his or her portfolio company goes public. Do VCs respond to IPO-induced distraction? One might expect that a profit-maximizing rational venture capitalist, who solves a simple problem of effort allocation between an IPO and a new investment, will employ a disproportionately high fraction of resources toward the former, since the IPO is practically a guaranteed exit event compared to exit prospects of a new startup. As a consequence, VCs might react to the attention shock by choosing not to invest during the distraction period. I check this possibility by extending the sample to include quarters without investments during the investment lifespan of each VC partner. The exact investment dates are known for the quarters with investment, but not observable for the quarters when VCs did not invest. For that reason I conduct this analysis at the quarterly level, rather than 90-day intervals.

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[16]Table A.4 indicates that the result are similar when IPO waves are defined at the industry level.

Table 1.9
VCs' investment activity around IPOs
This table presents estimates of the effect of VC partner distraction on the probability of making a new investment. The dependent variable is equal to one if the VC partner makes an investment in a given quarter or the number of new investments in a given quarter. The regressions in this table focus on quarters instead of 90-day intervals, since the absence of investments could be defined only for the quarterly data. Columns 1-2 report the coefficients of the linear probability model, Columns 3-4 reports the coefficients of the Poisson regression. The sample is a quarterly panel of venture capital investments from 1990 through 2017 extended to include quarters with no investments within the investment lifespan of each venture capitalist. Parentheses contain standard errors clustered by VC partner and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Prob(Investment)		ln(Number of investments+1)	
	(1)	(2)	(3)	(4)
-2 quarters		0.00002		-0.00041
		(0.01175)		(0.00755)
-1 quarter		0.00016		-0.00164
		(0.01158)		(0.00736)
IPO quarter	-0.02200*	-0.02213**	-0.01659**	-0.01671**
	(0.01126)	(0.01129)	(0.00719)	(0.00720)
+1 quarter		-0.01662		-0.01208*
		(0.01090)		(0.00698)
+2 quarters		0.00235		0.00172
		(0.01194)		(0.00767)
Partner FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Observations	116,546	116,546	116,546	116,546
R2	0.09	0.09	0.09	0.09

The linear probability models in Columns 1-2 of Table 1.9 indicate that VCs are about 2.2% less likely to invest into a new startup during a quarter coinciding with an IPO. The Poisson regressions in Columns 3-4 report both quantitatively and qualitatively similar results. Across all estimations, the finding is valid only in the vicinity of the distracting event. Such an investment pattern lines up with the hypothesis that busy VCs allocate their attention toward the IPO at the expense of a new investment, which provides validity to the measure of distraction exploited in this paper. While distracted VCs are less likely to invest, new investments do occur during the IPO engagement. The existence of various frictions as the competition for startups,

time pressure to invest, or, possibly, psychological factors might limit VCs ability to completely suspend their investment activity during the period of increased screening costs.[17]

1.4 Additional analysis and robustness tests

In the additional analysis I conduct supplementary tests indicating the robustness of my baseline results. First, I revisit the concern regarding changes in the management fee structure by directly controlling for the time since VC fund inception. Second, I estimate tests to understand if acquisition-related busyness also has a detrimental effect on the investment outcomes. I further estimate and compare the results at VC partner, VC fund, and VC firm levels. Finally, I provide a few extensions to the previous analysis and estimate additional robustness tests.

1.4.1 "Ever treated" VCs

If only certain types of VCs tend to invest during the IPO process, one might raise a concern regarding the comparability of the treatment and control groups. To alleviate this issue, I limit the sample to VC partners that made at least one investment during the time of their engagement in the IPO process – "ever treated" VCs. Therefore, the control group entirely consists of VCs that made a new investment during the distraction period at some point in time. This approach is similar to that employed by Bertrand and Mullainathan (2003), who form the control group focusing solely on "eventually treated" companies in the context of the staggered adoption of takeover laws.

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[17]The specifics of the interaction between limited and general partners is another possibility, as Maurin et al. (2019) point out that GPs might inefficiently accelerate investments to ensure that LPs respect their funding commitment.

Table 1.10
"Ever treated" VCs
This table presents estimates of the effect of VC partner distraction on financial outcomes of new investments by restricting the sample to the VCs that made at least one "busy" investment ("ever treated" VCs). All variables are defined in Table A.1. The sample is a quarterly panel of venture capital investments from 1990 through 2017. Parentheses contain standard errors clustered by VC partner and quarter and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	IPO/Acquisition		ln(exit multiple+1)	
	(1)	(2)	(3)	(4)
Busy Partner	-0.077***	-0.072***	-0.120**	-0.120**
	(0.022)	(0.024)	(0.055)	(0.056)
Controls	No	Yes	No	Yes
VC Partner FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	3,279	3,279	2,053	2,053
R2	0.34	0.35	0.38	0.39

Table 1.10 indicates that the main result holds when I require the same VCs to be present both in the treatment and control group. The coefficients of interest have a smaller magnitude compared to the baseline results, which might be due to the fact that I significantly lose power by imposing a very strict limitation on the sample size.

1.4.2 Time since VC fund inception

As financial intermediaries, VC firms typically charge a 2% management fee as the compensation for their services (Metrick and Yasuda, 2011). The base for the fee calculation is the committed capital, which gets replaced by the invested capital after the investment period or some other pre-determined point in time. This feature of the fee structure might create incentives for VCs to make more investments, possibly in lower quality startups, in order to maintain the base. To additionally ensure that the previous findings are not driven by this specificity in the fee calculation, I add the time since VC fund inception as the control variable to my baseline specification. To proxy for the time since inception, I compute the

difference between the investment quarter and the quarter of the first investment made by a particular fund.

Table 1.11

Distracted VC partners and performance of new investments: controlling for the time since VC fund inception

This table presents estimates of the effect of VC partner distraction on performance of new investments, taking into account the time since VC fund inception (in quarters) for each investment. All variables are defined in Table A.1. The sample is a quarterly panel of venture capital investments from 1990 through 2017. Parentheses contain standard errors clustered by VC partner and quarter and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	IPO/Acquisition		ln(exit multiple+1)	
	(1)	(2)	(3)	(4)
Busy Partner	-0.072***	-0.067**	-0.162***	-0.156***
	(0.025)	(0.026)	(0.053)	(0.053)
ln(Time since inception)	-0.006*	-0.007**	-0.016**	-0.017**
	(0.003)	(0.003)	(0.006)	(0.006)
Controls	No	Yes	No	Yes
VC Partner FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	15,169	15,169	15,169	15,169
R2	0.37	0.38	0.35	0.36

As Table 1.11 shows, the main results hold for both performance measures when the time since fund inception is taken into account. The sign of this new control variable is negative in all cases, which could be explained by the switch in the fee base. Additionally, the empirical evidence shows that the performance of an existing VC fund is often regarded as a next fund raising device (Chakraborty and Ewens, 2018). This consideration might potentially push VC partners to spend more time and effort on screening and deal flow in the beginning of the VC fund's life.

1.4.3   VC partners, VC funds, and VC firms

I study the influence of the distraction events on the performance of portfolio companies at the VC partner level which offers several advantages over analyzing VC firms. VC firms often manage multiple active funds with several partners involved, while only one partner is usually engaged with a particular portfolio company. For that reason, the analysis at the VC firm level will overstate the amount of "busy" investments, since a new investment made by a non-distracted fund or partner will be mistakenly treated as being a "busy" one.

Table 1.12
VC fund and VC firm level analyses
This table replicates the results in Table 1.2 at the VC fund (firm) level and presents estimates of the effect of VC fund (firm) distraction on financial outcomes of new investments. All variables are defined in Table A.1. The sample is a quarterly panel of lead venture capital investments from 1990 through 2017. Parentheses contain standard errors clustered by lead VC fund (firm) and quarter and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	IPO/Acquisition		ln(exit multiple+1)	
	VC fund	VC firm	VC fund	VC firm
	(1)	(2)	(3)	(4)
Busy VC fund	-0.051***		-0.156***	
	(0.018)		(0.043)	
Busy VC firm		-0.019		-0.013
		(0.012)		(0.024)
Controls	Yes	Yes	Yes	Yes
VC fund FE	Yes	No	Yes	No
VC firm FE	No	Yes	No	Yes
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	20,662	14,665	20,662	14,665
R2	0.34	0.25	0.31	0.22

The analysis at the VC fund level still misspecifies "busy" investments, since several partners could manage the same fund, or the same partner might manage several funds. In addition, the lead VC fund is not necessarily the one that takes a board seat in an entrepreneurial firm, which has potential implications for the definition of distraction.

Therefore, the performance of the analysis at the individual partner level plausibly offers higher accuracy compared to studying VC firms and funds.

Table 1.12 reports the estimations based on VC firms and funds, which confirm that the effect is not observable at the VC firm level, while it continues to hold at the VC fund level. Evidently, the analysis at the VC fund level does not presumably lead to substantial measurement error in comparison to the individual partner level – VC partners work closely with each other within a VC fund. Such an active collaboration is dictated by the design of a typical limited partnership agreement (LPA), specifying the remuneration of VCs at the fund level. Even if the same partner is sometimes involved in two or more funds simultaneously, LPAs often limit the overlap between investment periods of adjacent funds managed by the same partners, putting restrictions on fundraising by VCs to avoid the potential misalignment of interests.[18]

1.4.4 Extensions and miscellaneous robustness checks

Pre- and post-dotcom bubble. Since about a third of all "busy" investments happened during the dotcom bubble, I split the sample and reestimate the baseline regressions for the periods before and after the year 2000. Table A.3 indicates that the results hold for both periods. The regressions show that the magnitude of the effect is higher before 2000, particularly regarding exit multiples. This observation might stem from abnormally high valuations specific to the dotcom bubble period.

Acquisition-based busyness. Another natural candidate for an attention-grabbing event is acquisitions. First, I redefine distraction based on acquisitions – VCs are now distracted when one of their portfolio companies is about to get acquired. This approach results in 278 new "busy" investments made by the same VC partner during the first 90-day post-transaction interval, which I use to reestimate the baseline regressions.[19] Table A.5 reveals that

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[18]Gompers and Lerner (1996) report that 84.4% of LPAs contained such a covenant between 1988 and 1992. Typically, such restrictions last until the end of the investment period of a given fund.

[19]The number of "busy" investments is lower compared to the previous analysis due to the fact that the board membership data on acquired companies is not as comprehensive as on companies that went public.

acquisition-based busyness does not seem to have a significant impact on the financial performance of new investments. The intensity of distraction induced by this type of event is plausibly lower compared to public offerings, as exit multiples are more moderate and the execution time frame is shorter.

The contracting channel. While the data does not allow to separate screening and deal sourcing, the sample still gives the opportunity to study certain aspects of the contracting channel. One might hypothesize that busy VC partners could potentially choose not to take a board seat in a new company, which implies less post-investment monitoring and lower exit prospects. Since my sample is entirely based on board memberships, the investments that did not lead to a board seat are not directly observable. I use the following approach to pin down cases without board membership. First, based on the baseline sample I identify "venture capital fund-partner" pairs with only one partner. Next, I make an assumption that all investments made by that particular fund were also made by that specific VC partner. For such partners, I assume that each investment, which was left unmatched after merging VentureXpert with BoardEx, is a case without a board representation. Table A.6 contains the estimations on the baseline sample extended to include investments which did not imply a board seat for a VC partner. Columns 1-2 indicate that busyness does not seem to significantly influence the probability of taking a board seat by a VC partner. At the same time, Columns 3-4 show that the baseline results remain significant in the extended sample. This test suggests that the impact of distraction might have a limited impact on the contracting behavior of VCs, though further analysis of various VC contract provisions is necessary to bolster this finding.

Incidence of "busy" investments. If all VCs involved in the IPO process postponed their investments, I would not have sufficient power to pin down the impact of the company selection. Evidently, the existence of various frictions in the VC industry could cause partners to make investments during periods of high screening costs. Potential reasons include, but are not limited to, the acquisition of valuable network connections, competition for new startups,

commitment to invest in a time-limited fashion, physiological aspects, etc. I test the first possibility by studying whether "busy" investments are associated with establishing direct links to top VC firms. Table A.8 shows that VC partners do not seem to gain new connections with top VC firms, when they make a "busy" investment, as the coefficients of interest are insignificant and not distinct from zero.

Further, I explore the existence of patterns in relation to the occurrence of "busy" investments. I estimate regressions for the incidence of "busy" investments made by distracted VCs as a dependent variable. Round characteristics are the same as in the baseline regressions, while partner characteristics such as the portfolio size and the amount invested are now aggregated not to the partner-quarter, but to the partner level. By changing the level of the analysis one can make comparisons between different partners over time and study the sensitivity of the "busy" investment incidence to the overall partner level variables. Because partner characteristics are now time-invariant, regressions do not include VC partner level fixed effects. Table A.7 reports the results. The estimations suggest that the most important characteristic is the total amount of capital invested by a VC partner into portfolio companies, which is positively correlated with the incidence of "busy" investments. This observation adds to the evidence on decreasing returns to scale in private equity and venture capital, documented by Kaplan and Schoar (2005). VC partners with larger portfolios are more likely to be involved in the IPO process and have a higher number of successful exits, leading to the persistence of performance both at the fund and partner level. However, the higher level of deployable capital might also increase the probability of making "busy" investments, which tend to underperform as the previous analysis suggests.

Investments within the same industry and state. Different industries are likely to differ from one another in terms of the screening and deal sourcing intensity, which to some extent might alter VCs' investing behavior around the IPO. VCs could also choose to invest into startups located closer to their VC firms. To study the potential presence of these channels, I estimate regressions including VC partner $\times$ industry $\times$ state fixed effects, which allow to track

investment decisions made by the same VC partner in the same industry and state. Table A.9 shows that the results hold after imposing this granular layer of fixed effects. The magnitude of the coefficients continues to be comparable to the main findings, suggesting that the key result does not seem to be contaminated by VCs investing in different industries and states conditional on their workload.

1.5   Conclusion

This paper studies the efficiency of the capital allocation decisions in the context of the VC market. Using the IPO involvement as a short-term shock to the VCs' ability to search and screen investment opportunities, I find that distracted VCs make investment decisions that underperform in the future. Such companies have a lower likelihood of going public or being acquired and exhibit lower exit multiples. The results hold both for individual partners and venture capital funds. Thereby, the *ex-post* performance of portfolio companies depends not only on VC post-investment monitoring, but also on the capabilities of VCs to choose startups with *ex-ante* higher chances to succeed. This finding indirectly hints at the presence of skill in VCs' ability to source and screen investment opportunities.

To further validate the deal origination and screening channels, I study heterogeneity in the treatment intensity. I find that the results are stronger when distracting IPOs have larger exit multiples and size, likely because VCs spend more time and energy working with their portfolio companies when potential payoffs are higher. The underperformance is also weaker for VCs with prior IPO experience, suggesting that seasoned VCs might have knowledge and skills to navigate the process more effectively. In addition, the negative impact of busyness does not hold when I redefine distraction based on acquisitions. Relatedly, placebo tests indicate that the pattern is present only in the vicinity of the distracting IPO for both measures of success. In all, these findings suggest that the effect is attributable to screening and deal flow and not likely to come from other channels, such as the change in VC partner's attitude toward risk-taking, higher expectations during hot markets, or the specificity of the management fee structure.

The ongoing movement of institutional investors to create their own direct investment programs adds practical relevance to the results presented in this paper.[20] The evidence tells investors that skills play an important role in generating VC returns. Therefore, the question of attracting the top talent in the right amount is one of the relevant dimensions to consider while deciding to go ahead with the direct investment program.

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[20]One example is the California Public Employees' Retirement System (CalPERS), the largest pension fund in the United States ("Calpers Gives New Private-Equity Plan Tentative Thumbs-Up", *Wall Street Journal*, 2019, by Chris Cumming).

Chapter 2

2   The Value of Privacy and the Choice of Limited Partners by Venture Capitalists

2.1   Introduction

As institutional investors increase their capital commitments to private equity, so does the effort of the public to regulate this industry which is known to be protective regarding any information related to its operations. Over the last two decades, public institutions have been forced to disclose returns of the funds underlying their portfolios as well as the fund management fees. While more standardized and detailed fee disclosures are under discussion, we still know relatively little about the impact of the prior regulatory moves on private equity firms and their investors.[21] In this paper, I aim to bridge this gap and document the effect of mandatory fund-level return disclosure on the capital allocation decisions in the context of venture capital partnerships.

The inferences in this paper are predicated on a series of court rulings which took place in late 2002 and early 2003. These rulings mandated public pensions and pubic endowments (collectively, public LPs) to disclose returns of the funds underlying their portfolio to the public. The important characteristic of the movement for increased transparency was the uncertainty regarding the scope of the disclosure. The attitude of venture capital (VC) firms to the disclosure of their historical performance was heterogenous. Some VC firms, like Sequoia Capital, have openly asked their public LPs to leave their funds and led the movement against any disclosure. At the same time, another VC firm, MPM Capital LP, took advantage of the situation by raising the largest fund in its history.[22] A number of important considerations

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[21]"Investors Urge Private-Equity Industry to Improve Transparency", *Wall Street Journal,* 2020, by Simon Clark.

[22]"MPM Nails Year's Biggest Fund" *Venture Capital Journal,* February 1, 2003

might underlie VC firm decisions, ranging from the desire to conceal inferior performance to maintaining the notion of exclusivity behind VC investing business.

While the general response of VC firms to the new disclosure requirements was heterogenous, top-tier VC firms are arguably more sensitive to the regulation. LPs are primarily capital providers, with public LPs often accounting for the largest stakes in VC funds. Such a prevalence of public LPs creates an environment where VC firms might find it hard to exclude public LPs from the pool of their investors. The most successful VC firms are less prone to this situation as they often have oversubscribed funds with many potential limited partners wishing to invest. Therefore, prominent VC firms are less dependent on public LPs and consequently more likely to forgo commitments from public LPs during the period of uncertainty surrounding the movement for an increased disclosure. The evidence I find in this paper confirms this hypothesis and reveals that primarily only top-tier VC firms responded to the performance disclosure measures by banning public LPs from their current and future funds. The results also show that not the return disclosure per se, but rather the uncertainty regarding the scope of the disclosure is the primary reason behind the changes.

The long-lasting movement for performance disclosure dramatically accelerated shortly after the Enron's bankruptcy, culminating in the end of 2002 when a few of the largest public pensions and endowments were forced to disclose the performance of their funds based on the state-level Freedom of Information Acts (FOIAs). This setting serves as a quasi-natural event which allows to use the difference-in-differences framework with the treated group being public pension funds and public university endowments. Since other types of limited partners as private pension plans, foundations, and private endowments are not subject to compliance with the FOIAs and court rulings, I use them as a control group. The results remain qualitatively similar when I consider foreign LPs as a control group. The recurring feature of capital commitments to venture capital funds allows to exploit limited partner fixed effects, that is, any unobservable time-invariant characteristic of institutional investors cannot drive the results. I find that following the disclosure requirements, public LPs are almost 20% less likely to invest in top-tier VCs relative to the control group. The decline reverses one year

after the event as the court rulings clarify that the disclosure will not impact the sensitive portfolio company level information.

The substantial drop in commitments from public LPs is likely to disturb the existing links between VC firms and their investors, and cause disbalances in capital allocations. I find that following the court rulings top-tier VC firms increase capital commitments originating not only from other nonpublic LPs located in the United States, but also from foreign LPs. On the supply side of the market, public LPs are likely to allocate more capital to lower ranked VC firms, though I do not find evidence that public LPs shift toward investing into first-time funds and funds of funds. Pre-regulation returns are significantly higher for top-tier VC firms, which rules out the concern that the results might potentially be driven by the unwillingness of some VCs to disclose poor dotcom bubble returns that could be associated with a lack of skills.

Related literature This paper relates to the growing literature studying limited partners and their relationships with venture capital firms. Lerner et al. (2007) provide the first large-scale evidence on the investment choices made by limited partners. Sensoy et al. (2014) explore the performance of limited partners further and show that there is no persistence in returns after 1998. More recently, Cavagnaro et al. (2019) disentangle between skill and luck in limited partner investments, documenting the heterogeneity in skill among institutional investors, while Brown et al. (2020) study if limited partners can time their exposure to private equity. These papers analyze capital commitments as a choice of limited partners, pointing out that the limited access to some VC funds stems from long-term relationships. I add to these papers by studying additional friction in the market in the form of limited accessibility to the most successful venture capital funds. I show that matching between LPs and VC firms is of two-sided nature. The regulatory environment surrounding institutional investors is a relevant dimension influencing the choice of LPs by VC partnerships. In addition, I also find that while endowments are often considered as a single class of investors, public university endowments might be quite different from their private counterparts due to a different degree of exposure to the FOIAs.

Another strand of the literature focuses on specific types of institutional investors. Hochberg and Rauh (2012), Andonov et al. (2018) study the particular aspects of investment decisions made by public pensions, Binfare et al. (2019) explores university endowments. I contribute to these literature by emphasizing the relevance of the regulatory framework that LPs operate in, and by showing that the differential exposure to FOIAs is an important dimension of segregation among institutional investors.

Finally, my paper adds to a very small but growing literature on the fee and performance disclosure by investment managers across various asset classes. Kronlund et al. (2019) study 401(k) retirement plans, Badoer et al. (2019) explore mutual fund disclosures. I add to this literature by documenting the impact of performance disclosure on capital allocation decisions in the context of the venture capital industry.

The remainder of the paper is organized as follows. Section 2 discusses the institutional background and describes the data, the variables, and the empirical strategy. Section 3 presents the main results. Section 4 conducts additional analysis and robustness tests, which provide further support for the key findings. Section 5 concludes.

2.2   Data and Methodology

2.2.1   Institutional background

Public pensions started to receive requests to disclose their data on private investments in the late 1990s. This movement became harder to resist after the Enron scandal in 2001, which created a general call for increased transparency. In October 2002 under the pressure of journalists and local newspapers the University of Texas Investment Management Company (UTIMCO) made its fund-level returns available to the public. This event triggered a chain of lawsuits, most notably against the California State Teachers' Retirement System (CalSTRS), California Public Employees' Retirement System (CalPERS), the University of California, and the University of Michigan calling for the disclosure of returns, fees and portfolio

company valuations. The above-mentioned public pensions and public endowments ended up disclosing their data on fund-level returns in late 2002-early 2003.

The important feature of the lawsuits calling for the increased disclosure was the uncertainty regarding its scope. The plaintiffs were typically journalists who sought the comprehensive disclosure of public pension portfolios based on state-level open information acts, or Freedom of Information Acts (FOIAs) enacted by each state over the previous decades. The object of disclosure was not only fund returns, but also identities of other investors in the funds, identities of underlying portfolio companies, the size of equity stakes in startups, etc. The opposition from public LPs, as well as precedents created by the first court rulings helped resolve the uncertainty regarding the scope of the disclosure. At the end of 2003, the venture capital industry had the understanding that the disclosure will be limited to historical fund-level returns. I use this institutional insight in the empirical part of the paper.

The response of venture capital firms was heterogeneous. Top-tier VC funds like Sequoia Capital, openly opposed the movement, and made public announcements expelling the University of Michigan and the University of California from its recently closed funds. Such an opposition from Sequoia and other firms had an effect, as the state of Michigan amended its FOIA act in 2004 allowing the University of Michigan to keep its records private. This development also led some other states to reconsider their attitude toward return disclosure, which is similar to D'Acunto et al. (2020) who show that a few large cases cause peer industry participants to reassess the potential consequences of certain types of actions. Another outcome of VC firms' opposition was the creation of a limited reporting approach, where VC firms do not report their returns to the limited partners upon mutual consent stated in the limited partnership agreement. While not every LP has accepted this approach, it has allowed others to maintain their access to top VC funds, like the University of California, which has continued investing with Sequoia though the University has not disclosed Sequioa's returns due to the absence of the information since 2003.

Other, primarily younger, funds, took advantage of the situation. One example is MPM Bioventures III of Boston-based MPM Capital LP, which ended up raising the largest funds of

the year in 2002 with 900 mln raised. The fund did not exclude CalPERS from its investors, saying that the firm was not concerned about IRR release, but would keep a close eye on the development of the situation regarding potential portfolio company disclosures.

2.2.2 Data

My primary sources of data are Preqin and Thomson Reuters' SDC Platinum database. To study the impact of the disclosure on capital allocations, I first construct a sample of institutional investor investments based on Preqin. Preqin contains information on the links between investment funds and their limited partners. The database also reports the amount of capital commitments, though that part of the data is far less comprehensive due to the proprietary nature of this type of information. I limit my main sample to the venture capital partnerships and limited partners located in the United States. In the analysis of potential spillover effects, I extend this sample to include funds of funds as well as foreign investors. Since Preqin does not distinguish between private and public endowments, I manually search and segregate endowments, and add the latter category in the treated group. I drop secondaries, buyout and turnaround funds and consider the four-year period around the event, focusing on the period from 2000q4 through to 2004q3. My final sample includes 700 limited partners, which made 3,046 capital commitments in 397 VC firms, or 468 VC funds. I further aggregate the resulting sample to a LP-year level.

The key measures capturing the change in the access to top-tier VC firms are the occurrence and frequency of investments to top-tier VC firms. Table 2.1 presents descriptive statistics separately for the treated and control group as well as the distribution of capital commitments by the type of LP. The sample contains 270 LP-year observations of the treated group, including public pension funds and public university endowments. The control group contains 1,025 LP-year observations, mostly consisting of private pensions, foundations, and funds of funds.

Table 2.1
Summary Statistics
Panel A and Panel B report summary statistics for the variables underlying the analysis of the impact of the disclosure on commitments to top-tier VC firms. Panel C shows the distribution of commitments by the limited partner type. All variables are defined in Table B.1. The sample is an annual panel of venture capital commitments from 2000q4 through 2004q3.

Panel A. Treated group (Public pension funds and Public endowment plans)						
	Obs.	Mean	Median	Std. dev.	Min.	Max.
Any top-tier VC commitments	270	0.3	0.00	0.46	0.00	1.00
# of top-tier VC commitments	270	0.51	0.00	0.99	0.00	5.00
Experience (years)	270	13.28	16.00	7.92	1.00	33.00
# of investments, quarterly	270	3.08	2.00	3.6	1.00	33.00
Commitments, quarterly	270	59.68	15.00	129.41	0.00	1000.82

Panel B. Control group						
	Obs.	Mean	Median	Std. dev.	Min.	Max.
Any top-tier VC commitments	1025	0.25	0.00	0.43	0.00	1.00
# of top-tier VC commitments	1025	0.34	0.00	0.74	0.00	7.00
Experience (years)	1025	8.03	6.00	6.66	1.00	35.00
# of investments, quarterly	1025	2.16	1.00	2.45	1.00	26.00
Commitments, quarterly	1025	3.3	0.00	14.62	0.00	183.00

Panel C. Number of commitments by the type of LPs		
	Investments	Percentage
Public Pension Fund	747	25%
Private Sector Pension Fund	627	21%
Foundation	478	16%
Private Equity Fund of Funds Manager	405	14%
Insurance Company	273	9%
Private Endowment Plan	108	4%
Public Endowment Plan	84	3%
Other	324	11%
Total	**3,046**	**100%**

Panel A and B indicate that treated LPs make more and larger capital commitments, and have higher investment experience. Public LPs are also more likely to invest with top-tier VC firms, though both groups are quite comparable in terms of their exposure to at least one top-tier VC firm. As Panel C shows, pension funds account for almost a half of capital commitments made during the period under consideration. Investments by private and public endowments constitute similar fractions of total commitments, representing 4% and 3% respectively.

As discussed earlier, top-tier VC firms are most likely to be affected by the disclosure requirement from LPs. Following Nahata (2008) and Atanasov et al. (2012), I use the IPOs of portfolio companies to identify top-tier VC firms. I produce a ranking of VC firms based on a simple count of IPOs that investment firms participated in as investors over the period between 1995 and 2002. I consider the top-two percentile of venture capital firms as top-tier VC firms. To corroborate the robustness of the results, I additionally consider top-one, top-five and top-ten percentiles of firms, which indicate that the impact of the regulation is less pronounced as we move further down the list of VC firms.

Table 2.2
List of Top-Tier VC Firms
This table presents the list of top-tier VC firms ranked by historical performance. The top-tier firm is defined as a firm in two percentile of IPOs distribution. Performance is based on the number of portfolio companies' IPOs that a VC firm was engaged in as an investor between 1995 and 2002.

#	Venture Capital Firm
1	Kleiner Perkins Caufield & Byers LLC
2	New Enterprise Associates Inc
3	Sequoia Capital Operations LLC
4	TA Associates Management LP
5	Oak Investment Partners
6	Warburg Pincus LLC
7	Mayfield Fund
8	Accel Partners & Co Inc
9	Greylock Partners LLC
10	Venrock Inc
11	Summit Partners LP
12	HarbourVest Partners LLC
13	Opus Capital
14	Institutional Venture Partners
15	Bessemer Venture Partners
16	U.S. Venture Partners
17	Norwest Venture Partners
18	Advent International Corp
19	Sutter Hill Ventures

Table 2.2 lists the top-two percentile of VC firms based on the number of IPOs. The list includes many prominent VC firms. Funds raised by these firms are typically larger compared to funds of other VC firms and have a higher subscription ratio.

Figure 2.1 The time trend of capital commitments by Public and Nonpulic LPs
This figure plots the annual dynamics in the number of top-tier VC firm capital commitments in the breakdown by public versus nonpublic LPs. The red line represents the total number of capital commitments, the histogram represents the fraction of top-tier VC firm capital commitments in the total number of investments. Data come from Preqin.
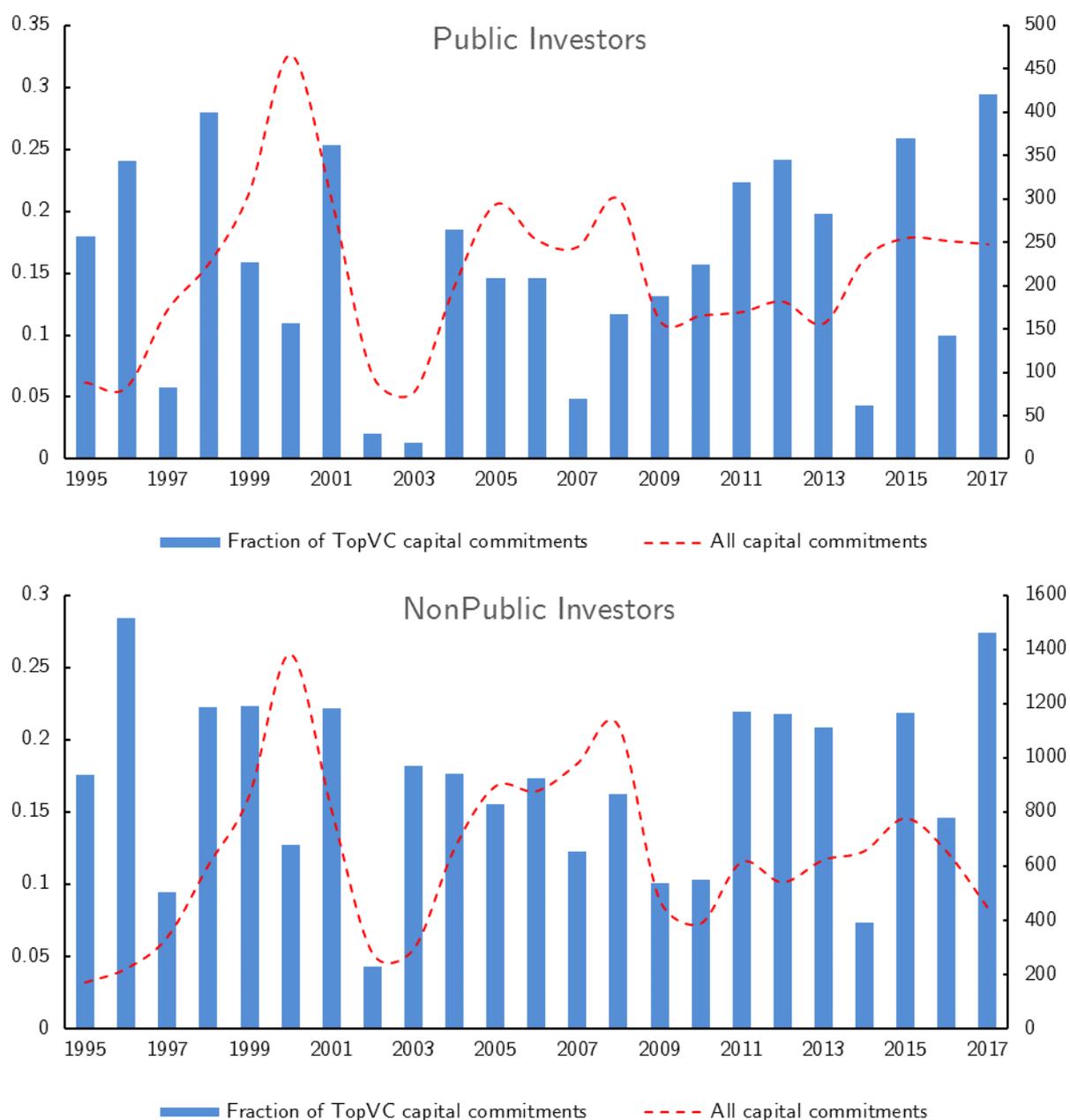
Figure 2.1 shows the time trend in capital commitments made by public versus nonpublic LPs. The fraction of top-tier VC investments by public LPs has a higher variation for public investors compared to nonpublic investors. Most notably, the investments in top-tier VC firms by nonpublic LPs quickly recover after the dotcom bubble, while it takes one additional year for public LPs. This observation is plausibly connected with the disclosure requirements as the next sections suggest.

2.2.3 Empirical methodology

I study the impact of the performance disclosure on capital allocation in a difference-in-differences setting. This approach allows to compare the investment behavior of public LPs to the investment behavior of a control group.

My baseline regression model is the following:

$$Y_{i,t} = \beta_0 + \beta_1 PostTreated_i + \beta_2 X_{it} + \eta_t + \delta_i + \varepsilon_{i.t} \tag{2}$$

where $Y_{i,t}$ is the outcome variable of interest in period t for limited partner i, *Post* is an indicator equal to one for 2002q3-2004q2 and zero for 2000q3-2002q2, *Treated* is a dummy indicating whether LP i belongs to the treatment or the control group, X_i is a vector of limited partner-level controls, which include the total amount of known capital commitments, experience and total number of investments at an annual frequency, δ_i and η_t are LP and period fixed effects, and $\varepsilon_{i,t}$ is an error term. The outcome variables of interest are the occurrence and frequency of capital commitments in top-tier VC firms. The choice of these dependent variables addresses two goals. First, studying investment links instead of commitments allows to mitigate the limitation of the data on capital commitments. Second, these variables are also likely to be more informative than commitments because the court ruling by its very nature plausibly affects investment occurrence and not investment size. All variables are defined in Table B.1 in the Appendix. I take logs (plus one) of all control variables. In all specifications standard errors are clustered at the limited partner level and are

robust to heteroskedasticity. The coefficient of interest is β_1, which measures the change in the outcome variable following the performance disclosure for public pensions and public university endowments relative to other types of limited partners.

A crucial assumption for the DID regression to be valid is that both treatment and control groups follow parallel trends in the absence of the treatment event. Figure 2.2 depicts the fraction of LPs investing in top VC firms in the breakdown by treatment and control groups.

Figure 2.2 Capital commitments to top-tier VC firms: Public vs Private LPs
This figure plots the probability of making an investment into at least one top-tier VC firm among treatment and control groups around the event (2002q4). Public pensions and public endowment plans are the treatment group, all remaining domestic institutional investors are the control group. The horizontal axis indicates the timeline aggregated to four-quarter periods around the event.
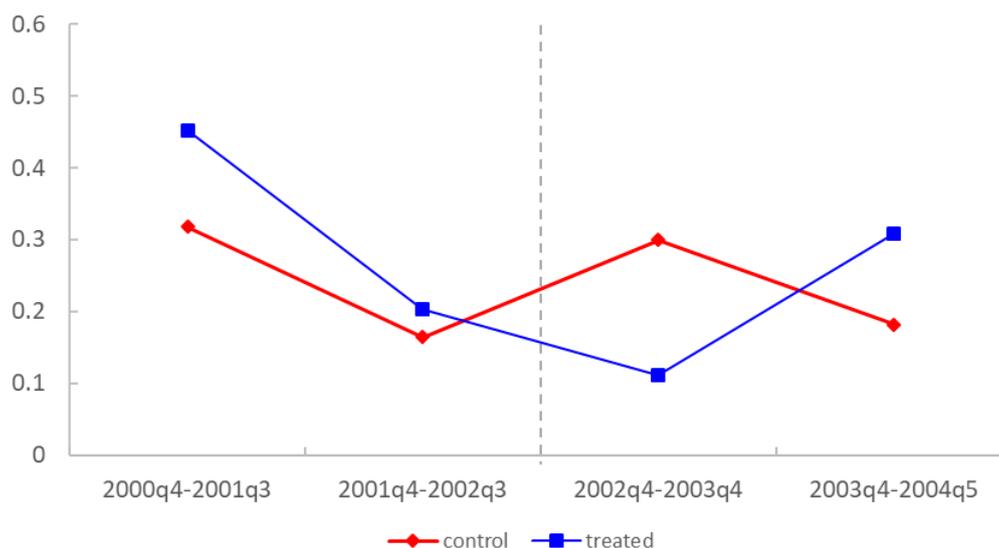
The figure shows that two groups follow downward sloping parallel trends before the event, which is in line with the general trend in VC capital commitments after the dotcom bubble. Public LPs invested significantly more in top-tier VCs in 2002q3-2002q2, with about 50% of them having invested in at least one top-tier VC fund. However, public LP commitments dramatically drop to almost 10% during the first year after the event, while commitments from other LPs do not practically change. Public LP commitments rebound to some extent one year after, with 30% of public pensions and public university endowments making at least one top-tier VC firm investment.

2.3 Empirical results

2.3.1 Commitments to venture capital funds

I start with the DID estimation of the effect of the performance disclosure on the capital commitments of LPs in VC funds. Table 2.3 reports the results of the DID regression for the occurrence and frequency of investments in top-tier VC firms.

Table 2.3
Effect of the Disclosure on Top-Tier VC Investments
This table presents estimates of the effect of the disclosure on commitments to top-tier VC firms. All variables are defined in Table B.1. The sample is an annual panel of venture capital commitments from 2000q4 through 2003q3. Parentheses contain standard errors clustered by limited partner and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Any TopVC Commitments		# of TopVC commitments	
	(1)	(2)	(3)	(4)
Post x Treated	-0.253***	-0.188**	-0.254***	-0.185***
	(0.093)	(0.083)	(0.084)	(0.068)
Experience		0.481***		0.558***
		(0.165)		(0.132)
# of investments		0.219***		0.358***
		(0.058)		(0.048)
Total commitments		0.060***		0.053**
		-0.023		-0.022
Limited Partner FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Obs	621	621	621	621
R2	0.57	0.61	0.62	0.71

Column 1 indicates that following the disclosure, public pensions and public endowments are 25 percentage points less likely to invest in top-tier VC firms. The effect remains strong after controlling for observable LP level characteristics like experience, the number and amount of investments. Poisson regressions in Columns 3 and 4 show that the drop in the number of investments in top VC firms has a similar magnitude.

These baseline results indicate that public LPs are indeed less likely to invest in top-tier VC firms compared to other institutional investors after the court-mandated disclosure

requirements. The result is meaningful in terms of its economic magnitude, suggesting that

top-tier VCs reacted to the performance disclosure by banning public LPs from their funds.

2.3.2 The effect in dynamics

Figure 2.2 previously revealed the reversal in the effect one year after the event. Table 2.4

provides a quantitative assessment of the results in dynamics, indicating the absence of the

differential impact of the treatment for the second year of the post-treatment period.

Table 2.4
Effect of the Disclosure in Dynamics
This table studies the dynamic effect of the disclosure on commitments to top-tier VC firms. *Post year 1 (2)* is an indicator variable equal to one for the first (second) four-quarter period after the event. All remaining variables are defined in Table B.1. The sample is an annual panel of venture capital commitments from 2000q4 through 2004q3. Parentheses contain standard errors clustered by limited partner and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Any TopVC Commitments		# of TopVC commitments	
	(1)	(2)	(3)	(4)
Post year 1 x Treated	-0.277***	-0.230***	-0.275***	-0.217***
	(0.089)	(0.080)	(0.080)	(0.065)
Post year 2 x Treated	-0.012	0.023	-0.062	-0.015
	(0.072)	(0.071)	(0.067)	(0.062)
Experience		0.268**		0.339***
		(0.105)		(0.090)
# of investments		0.228***		0.318***
		(0.045)		(0.040)
Total commitments		0.044**		0.045**
		(0.019)		(0.018)
Limited Partner FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Obs	920	920	920	920
R2	0.55	0.59	0.59	0.66

The reversal of the trend one year after the event suggests that the drop in commitments

from public LPs was due to uncertainty regarding the scope of disclosure. As courts were

consistent in restraining the disclosure requirements to the past returns, VC firms gained

confidence in understanding that the movement for broader disclosure would be limited by historical performance and would not include other types of information.

2.3.3 Availability of return data and return characteristics of top-tier VC firms

While studying historical returns of top-tier VC firms would offer many insights regarding the economic consequences of the regulation for public LPs, the data on returns remains scarce. This shortcoming of the data is especially true for the subsample of returns of top-tier VCs that made substantial efforts to restrict the disclosure of their data after 2002. Therefore, instead I look at return information only before 2002, and complement this analysis by exploiting IRR datapoints missing from the Preqin database. Table 2.5 below indicates that top-tier VC firms are more likely to avoid disclosure of their return information after the disclosure requirements, which provides further robustness to the channel of (1) switching to nonpublic LPs and (2) limiting the transfer of information between general and limited partners in case of public LPs. Columns 5-6 in Table 2.5 indicate that the funds raised by the affected top-tier VC funds in fact outperformed other funds before the court rulings took place. This observation implies that the results are not driven by unwillingness of VC funds to disclose poor returns after the dotcom bubble.

Table 2.5
Availability of return information and return characteristics of VC funds before and after the regulation.
This table studies the availability of return information and return characteristics of top-tier VC firms. Columns 1-2 contain estimates for the availability of return information on the sample of VC funds raised between 1992 and 2002, Columns 3-4 contain estimates for the availability of return information on the sample of VC funds raised between 2002 and 2012, Columns 5-6 contain estimates for the return characteristics on the sample of VC funds raised between 1992 and 2002. Parentheses contain standard errors clustered by VC fund type and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Missing IRR information				IRR	
	1992-2002		2002-2012		1992-2002	
	(1)	(2)	(3)	(4)	(5)	(6)
TopTier VC firm	0.000	0.019	0.265**	0.265***	24.612***	16.064***
	(0.075)	(0.069)	(0.081)	(0.071)	-4.142	-3.692
log(Size)	-0.107***	-0.123***	-0.131***	-0.134***	-8.031***	0.289
	(0.008)	(0.006)	(0.009)	(0.009)	(2.065)	(1.589)
log(Fund number)	-0.147**	-0.142**	-0.077***	-0.077***	7.375	5.290
	(0.058)	(0.058)	(0.022)	(0.023)	(4.803)	(3.779)
Type FE	No	Yes	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs	942	940	1,360	1,359	282	282
R2	0.15	0.17	0.15	0.16	0.07	0.15

2.3.4 Spillover effects

The regulation drove a wedge into existing links between limited partners and VC funds, which is likely to cause a reallocation of capital on both the supply and demand side of the market. In this section I study the spillover effects generated by the suspension of capital flows between the affected parties.

Foreign Institutional Investors After the divorce with public LPs, VC firms still need to raise money. Disclosure-averse VC firms that face high demand for their funds are likely to choose investors that are not public and thus not subject to the state-level FOIAs. Indeed, the baseline results already show that the public LPs were crowded out by investors in the control group. To provide additional robustness to the main results, I swap the control group of non-

public domestic LPs to foreign LPs. While many foreign LPs are public, they do not have to comply with the FOIAs of the United States.

Consistent with the previous findings, Table 2.6 shows that following the court decisions, top-tier VC firms raise more capital from investors located overseas.

Table 2.6
Spillover Effects: Foreign LPs
This table reestimates Table 2.3, changing the control group to foreign limited partners. All variables are defined in Table B.1. The sample is an annual panel of venture capital commitments from 2000q4 through 2003q3. Parentheses contain standard errors clustered by limited partner and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Any TopVC Commitments		# of TopVC commitments	
	(1)	(2)	(3)	(4)
Post x Treated	-0.264***	-0.179**	-0.306***	-0.179**
	(0.089)	(0.083)	(0.082)	(0.069)
Experience		0.286*		0.371***
		(0.148)		(0.127)
# of investments		0.233***		0.391***
		-0.085		-0.072
Total commitments		0.023		0.029
		(0.031)		(0.029)
Limited Partner FE	Yes	Yes	Yes	Yes
Time FE	No	Yes	No	Yes
Obs	328	328	328	328
R2	0.66	0.70	0.69	0.78

The magnitude of the coefficients is similar to that in the baseline regressions. The estimates indicate that after the event foreign LPs, primarily those located in the UK and Switzerland, get better access to top-tier VC firms.

Flows to young funds and funds of funds   As public pensions and public endowments now have more capital to allocate, they are likely to be searching for other investment opportunities. The main results have already revealed that they make more capital commitments to lower ranked VC firms as they are considerably less sensitive to the disclosure of their return data. In addition, public LPs might also increase allocations to

younger VC funds, which is a less disclosure-averse subsegment of firms within the control group. Another alternative are funds of funds as this type of asset class reports the performance of underlying funds on the aggregated level which does not lead to the disclosure of information on portfolio companies.

Table 2.7
Spillover Effects: Funds of Funds and First-Time VC Funds
This table reestimates Table 2.3 by changing top-tier VC firms to funds of funds and first-time VC funds. All variables are defined in Table B.1. The sample is an annual panel of funds of funds and first-time VC funds commitments from 2000q4 through 2003q3. Parentheses contain standard errors clustered by limited partner and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Any TopVC Commitments		# of TopVC commitments	
	Funds of funds	1st time VC funds	Funds of funds	1st time VC funds
	(1)	(2)	(3)	(4)
Post x Treated	0.022	-0.023	0.007	-0.026
	(0.066)	(0.094)	(0.056)	(0.067)
Experience	0.067	-0.236	-0.045	-0.189*
	(0.109)	(0.154)	(0.090)	(0.109)
# of investments	0.229***	0.168***	0.312***	0.148***
	(0.049)	(0.059)	(0.043)	(0.051)
Total commitments	0.036	-0.007	0.032	-0.015
	(0.028)	(0.034)	(0.023)	(0.025)
Limited Partner FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Obs	989	621	989	621
R2	0.73	0.55	0.74	0.60

Table 2.7 does not indicate that public pension and public endowments increase capital commitments to funds of funds or first-time VC funds. This evidence suggests that the effect comes from VC firms that already have an investment track-record, but do not have a superior performance to attract capital commitments from public LPs earlier.

2.4 Additional analysis and robustness tests

In this section I conduct supplementary tests indicating the robustness of my baseline results. First, I revisit the classification of funds into top-tier VC firms. Next, I limit my sample to only

those VC firms that are originally exposed to public LPs, and estimate placebo tests by shifting the timeframe by one year backward for both definitions of the control group. Finally, I study the demand for funds raised by top-tier VC firms, measured by size and subscription ratio.

2.4.1 Sensitivity to historical performance

To provide the robustness for the heterogeneous response of VC firms, I extend the group of affected VC firms to the top 5 and 10 percentile of firms ranked by the number of IPOs.

Table 2.8 indicates that the results get weaker and disappear altogether as I move further down the ranking of VC firms to include firms from percentiles 5 and 10 in the IPO distribution. Less successful funds seem to be much less sensitive to performance disclosure requirements compared to top-tier VC firms, as they typically have a weaker subscription base for their funds. By excluding public LPs, lower-ranked VC firms face a risk of a significant drop in the size of their future funds. The untabulated regressions based on the top 1 percentile of firms indicate that the results continue to hold for a smaller sample of top-perfroming VC partnerships.

2.4.2 VC firms ever exposed to public LPs

An alternative confounding factor could be that the treated and control groups have historically invested in different types of VC funds. Investments in these funds might be driven by factors other than the disclosure of performance measures.

Table 2.8
Sensitivity to Different Top-Tier Category Thresholds

This table presents estimates of the effect of the disclosure on commitments to different top-tier categories. All variables are defined in Table B.1. The sample is an annual panel of venture capital commitments from 2000q4 through 2003q3. Parentheses contain standard errors clustered by limited partner and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A. Top 5 percentile

	Any TopVC Commitments		# of TopVC commitments	
	(1)	(2)	(3)	(4)
Post x Treated	-0.128	-0.076	-0.168*	-0.109
	(0.098)	(0.093)	(0.092)	(0.079)
Experience		0.412**		0.581***
		(0.166)		(0.143)
# of investments		0.208***		0.448***
		(0.064)		(0.052)
Total commitments		0.043		0.025
		(0.029)		(0.023)
Limited Partner FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Obs	621	621	621	621
R2	0.58	0.61	0.65	0.74

Panel B. Top 10 percentile

	Any TopVC Commitments		# of TopVC commitments	
	(1)	(2)	(3)	(4)
Post x Treated	0.010	0.042	-0.091	-0.050
	(0.091)	(0.089)	(0.092)	(0.077)
Experience		0.172		0.386**
		(0.176)		(0.150)
# of investments		0.294***		0.585***
		(0.063)		(0.052)
Total commitments		0.040		0.016
		(0.030)		(0.025)
Limited Partner FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Obs	621	621	621	621
R2	0.58	0.62	0.67	0.77

To alleviate this concern, I reestimate Table 2.3 by limiting the sample to VC firms that had exposure to public LPs before the event.

Table 2.9
Top-Tier VC funds Ever Exposed to Public LPs
This table reestimates Table 2.3 by limiting the sample to top-tier VC funds that were ever exposed to public LPs. All variables are defined in Table B.1. The sample is an annual panel of venture capital commitments from 2000q4 through 2003q3. Parentheses contain standard errors clustered by limited partner and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Any TopVC Commitments		# of TopVC commitments	
	(1)	(2)	(3)	(4)
Post x Treated	-0.372***	-0.259***	-0.340***	-0.210***
	(0.105)	(0.094)	(0.094)	(0.076)
Experience		0.406*		0.526***
		(0.221)		(0.175)
# of investments		0.246***		0.395***
		(0.063)		(0.056)
Total commitments		0.093***		0.089***
		(0.025)		(0.023)
Limited Partner FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Obs	521	521	521	521
R2	0.58	0.63	0.64	0.73

Table 2.9 shows that the results are even stronger after eliminating funds with no prior links to public LPs, as disclosure-averse VC firms with existing connections with public investors shift to other types of limited partners. This observation not only mitigates the concern about inherently different types of VC funds, but also reinforces the main argument that performance disclosure by public LPs is the key driver behind the changes.

2.4.3 Demand for VC funds raised by top-tier VC firms

To see if top-tier VC firms have higher demand for their funds, I estimate the panel regressions with fund size and subscription ratio as dependent variables.

Table 2.10

Investor demand for funds raised by TopVC firms

This table studies the investor demand for funds raised by TopVC firms. All variables are defined in Table B.1. The sample is an annual panel of venture capital commitments from 1995 through 2015. Parentheses contain standard errors clustered by limited partner and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Fund Size		Subscription ratio	
	(1)	(2)	(3)	(4)
Treated	1.356***	0.600***	0.090***	0.086**
	(0.107)	(0.150)	(0.023)	(0.028)
log(fund number)		0.771***		0.005
		(0.075)		(0.011)
Type FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Obs	2,835	2,835	1,561	1,561
R2	0.19	0.30	0.05	0.05

Table 2.10 shows that funds raised by top-tier VC firms are larger in size and have a better subscription ratio. Therefore top-tier VC firms are able to select their investors and exclude public LPs without causing significant harm to their fundraising process.

I also estimate similar regressions around the event to see if top-tier VC firms adjust their fundraising behavior.

Table 2.11

Effect of Disclosure on Size and Demand for Public LPs

This table presents estimates of the effect of the disclosure on size and subscription ratio of top-tier VC firms. All variables are defined in Table B.1. The sample is an annual panel of venture capital commitments from 2000q4 through 2003q3. Parentheses contain standard errors clustered by limited partner and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Fund Size		Final Size to Target ratio	
	(1)	(2)	(3)	(4)
Post year 1 x Treated	-0.644***	-0.624***	-0.443***	-0.438***
	(0.087)	(0.089)	(0.031)	(0.030)
Post year 2 x Treated		0.947*		0.204
		(0.450)		(0.199)
log(fund number)	0.990***	0.973***	0.051*	0.047
	(0.061)	(0.060)	(0.024)	(0.026)
Fund Type FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Obs	435	435	435	435
R2	0.32	0.33	0.06	0.07

Consistent with the main results, Table 2.11 reveals that top-tier VC funds raise lower funds and have a lower subscription ratio following the mandatory disclosure, but this trend reverses one year later as the uncertainty around the scope of the disclosure gets resolved.

2.4.4 Placebo tests

To corroborate the robustness further, I reestimate Table 2.3 by switching from the linear probability model to probit regressions.[23] I also substitute the number of top-tier VC firm commitments with the fraction of top-tier VC commitments.

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[23]Unlike the linear probability model, probit regressions do not include LP fixed effects due to convergence issues.

Table 2.12
Robustness Tests: Alternative Specifications
This table reestimates Table 2.3 by changing the specification from the linear probability model to probit regressions (Columns 1-2), and from the number of top-tier VC commitments to the fraction of top-tier VC commitments. Columns 1-2 contain marginal effects from probit regressions. All variables are defined in Table B.1. The sample is an annual panel of venture capital commitments from 2000q4 through 2003q3. Parentheses contain standard errors clustered by limited partner and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Any TopVC commitments		Fraction of TopVC commitments	
	(1)	(2)	(3)	(4)
Post x Treated	-0.224***	-0.253***	-0.193***	-0.185***
	(0.085)	(0.081)	(0.054)	(0.068)
Experience		0.059***		0.558***
		(0.019)		(0.132)
# of investments		0.288***		0.358***
		(0.027)		(0.048)
Total commitments		-0.011		0.053**
		(0.010)		(0.022)
Limited Partner FE	No	No	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Obs	979	979	621	621
R2	0.03	0.18	0.53	0.71

Table 2.12 shows that the results continue to hold under alternative specifications and are robust to changes in the definition of the dependent variable.

As an additional robustness check I shift backward both the sample timeframe and the date of the event by one year compared to the baseline sample dates.

Table 2.13
Placebo Tests: Time of Treatment
This table reestimates Table 2.3 by moving the treatment event backwards by one year. All variables are defined in Table B.1. The sample is an annual panel of venture capital commitments from 1999q4 through 2002q3. Parentheses contain standard errors clustered by limited partner and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Any TopVC Commitments		# of TopVC commitments	
	Domestic LPs	Foreign LPs	Domestic LPs	Foreign LPs
	(1)	(2)	(3)	(4)
Post x Treated	-0.061	0.065	-0.093	-0.064
	(0.066)	(0.083)	(0.062)	(0.080)
Experience	-0.040	0.214	0.039	0.153
	(0.140)	(0.141)	(0.122)	(0.136)
# of investments	0.180***	0.163***	0.321***	0.281***
	(0.055)	(0.060)	(0.052)	(0.067)
Total commitments	0.021	0.013	-0.004	-0.010
	(0.022)	(0.025)	(0.019)	(0.023)
Limited Partner FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Obs	850	440	850	440
R2	0.66	0.75	0.73	0.77

Columns 1 and 2 of Table 2.13 indicate that public LPs are not significantly different from other domestic LPs or foreign LPs in terms of their investments in top-tier VC firms. Poisson regressions in Columns 3-4 point to a similar conclusion. This test helps mitigate the possibility that the findings above are due to the sample selection issues or other unobservable trends in the venture capital industry.

2.5   Conclusion

I study the impact of performance disclosure on the capital allocation decisions in the context of venture capital. I find that the movement to disclose performance led many top-tier VC funds to exclude public institutions from the list of their investors. The tests suggest that the result is driven by the uncertainty over the scope of the disclosure, as the impact of the court rulings

vanishes a year later. Overall, the results indicate that the regulatory framework surrounding LPs matters for VC firms whose business model largely relies on private information.

To compensate for the loss of capital, top-tier VC firms resort to nonpublic domestic institutional investors and foreign LPs. Pre-regulation returns are significantly higher for top-tier VC firms, which rules out the possibility that the observed outcomes might potentially be driven by the unwillingness of some VCs to disclose poor returns in the aftermath of the dotcom bubble.

The results are important given that the movement for increased transparency is on the rise. I estimated only part of the possible channels that might have been affected. While the disclosure has impacted the occurrence and frequency of investments into top-tier VC firms in the short-term, it might also have some longer-term consequences. For example, on the positive side, some VC firms might choose to offer lower fees to public LPs. Since public pensions disclose net-of-fee returns, this approach will to some extent boost their disclosed IRRs, which is especially attractive for young VC firms and those with poor performance history. The academic research might be able to address this question in the future, as the data on limited partnership fees are becoming more available to scholars.

Chapter 3

3   Do Banks Compete on Non-Price Terms? Evidence from Loan Covenants

3.1   Introduction

Competition among financial institutions in credit markets has been receiving significant attention by academics and regulators in recent years. Traditional banks increasingly compete with a "shadow banking" system of non-depository institutions. A deep understanding of the competitive dynamics of credit markets and the interplay between shadow banks and traditional lenders is essential for an effective macroprudential regulation (Adrian and Shin, 2009).[24]

The existing literature mainly focuses on price competition in credit markets, i.e. it assumes that lenders in the regulated and shadow banking sector compete primarily on interest rates. However, credit products can markedly differ with respect to non-price terms. A particularly relevant non-price term is the presence of loan covenants, i.e. contractual obligations that impact the ability of lenders to monitor borrowers by acting as "trip wires" that signal a deterioration in performance (Dichev and Skinner, 2002; Chava et al., 2019b). Competition on non-price terms received substantially less attention in the literature, despite the importance of loan covenants for borrowers (Chava and Roberts, 2008; Chava et al., 2019a) and in transmitting bank health to the real economy (Chodorow-Reich and Falato, 2018).

In this paper, we take a step toward filling this gap. We address the question to what degree non-price terms, namely loan covenants, are an economically relevant dimension of competition among lenders. Recently, regulators specifically put the waning use of covenants by banks under the spotlight and introduced provisions to mitigate excessive bank risk taking.

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[24]Recent studies link credit market competition to bank risk-taking (Boyd and De Nicolo, 2005), monetary policy transmission (Wang et al., 2018), and financial stability (Corbae and D'Erasmo, 2019).

These provisions limited the ability to offer loans without covenant protections for regulated lenders, but not for shadow banks.

We use this setting to focus on two questions. Do borrowers attach value to covenant-lite loan structures, allowing shadow banks to offer covenant-lite loans and gain market share at the expense of regulated banks? Or do the provisions promoting the use of covenants result in loans with stronger covenant protection? Which of these scenarios prevails is ultimately an empirical question.

There are two major identification challenges to answering these questions. First, variation in observed covenant protection in loan contracts is endogenous to the match between borrowers and lenders. For example, a standard regression of lender market share on measures of covenant protection in loan contracts may capture other effects than competition because of a number of observable and unobservable factors affecting both the demand and the supply of differentiated credit products. More optimistic lenders may choose to supply both more credit and extend loans with covenant-lite structures. Although some observables, such as interest rates and loan maturity, can be controlled for, the design of the covenant structure in a loan contract is plausibly based on several unobservable factors. Second, reverse causality concerns may arise. Lenders that have been successful to increase market share may trade off their level of covenant protection with expected monitoring and renegotiation costs (e.g. Chodorow-Reich and Falato, 2018; Berlin et al., 2018).

We therefore exploit a plausibly exogenous shock to the ability of lenders to offer covenant-lite loans: the Leveraged Lending Clarification ("the Clarification") on November 4th, 2014 by regulatory bodies in the United States. The Clarification targets the $1.2 trillion Leveraged Loan segment of the syndicated loan markets, and promotes improved underwriting standards in this high-risk segment. Specifically, the Clarification points to both qualitative and quantitative measures of covenant protection ("few or weak covenants", Clarification 11). We exploit the Clarification as a regulatory shock affecting loan non-price terms, namely lenders' ability to offer covenant-lite products. The scope of the Clarification,

which targets regulated banking and financial institutions, but not the shadow banks, offers a natural choice of a treatment and control group.

While the Clarification provides a sharp, direct shock to the ability of regulated banks to offer covenant-lite loan structures, regulated banks and non-banking institutions are not comparable on neither observable nor unobservable dimensions. For once, the two types of lenders generally lend to different borrowers. Thus, estimates from a standard difference-in-differences analysis would suffer from a selection bias by comparing the behavior of different kinds of borrowers. To deal with this challenge, we take advantage of the fact that lenders originate several loans in a given quarter, as well as borrowers repeatedly taking loans during our sample period. This structure allows to saturate our empirical specifications with borrower and lender $\times$ time fixed effects, effectively drawing inference comparing the same borrower over time, and comparing loans within the same lender at a certain time, along the lines of Khwaja and Mian (2008), Jiménez et al. (2012) and Jiménez et al. (2014). To the extent that fixed effects absorb loan-level confounding factors, differences between the treatment and the control groups can be plausibly attributed to the Clarification.

Using a comprehensive dataset for U.S. issued leveraged loans, the S&P's Leveraged Commentary and Data (LCD) database, our analysis offers three main results. First, borrowers value covenant-lite loan structures. The Clarification was effective and affected the lending standards of treated banks by reducing their probability of offering covenant-lite loans by roughly 20% relative to unaffected lenders. As a consequence, we estimate that borrowers which originally borrowed from regulated banks have a 35% higher probability of switching to non-bank lenders as a response to a covenant-lite offer. Second, regulated lenders that require tighter covenants structures lose approximately 1.2% market share, or roughly $30 bln of lending, to the advantage of the non-banking institutions.[25] These effects are particularly concentrated in cases where the existing banking relationship is weak and hence provides little incentives for borrowers to stay with their lender. Effects are also stronger for more affected

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[25]The result is less pronounced when a particular borrower and lender pair has a history of strong lending relationship, which is consistent with the existing literature.

lender-borrower pairs, either because lenders have shown a higher propensity to issue covenant-lite loans prior to the Clarification (supply effects), or because a borrower's last loan prior to the guidance had been covenant-lite (demand effects). Third, competition on non-price terms has relevant aggregate effects. We find that the relation between lower covenant protection and bank market share holds not just in our setting, but more broadly in the larger syndicated loan market.

We then consider alternative explanations for our results. We find that our results are not driven by changes in loan terms other than covenant packages, or a change in the borrower pool served by treated lenders. We further show that there is no evidence of a general reduction in loan supply of regulated banks, either in absolute nor relative terms.

As a final exercise, we analyze the performance of covenant-lite loans. Consistent with recent literature emphasizing specialization between lenders and "split control rights" (Berlin et al., 2018), we find that borrowers with only covenant-lite loans are associated with a 15% higher probability of bankruptcy. This higher rate of bankruptcy for borrowers in the absence of covenants, coupled with our finding that relaxing covenants allows lenders to gain market share, provides the missing link to the question raised in Fahlenbrach et al. (2017) why fast loan growth leads to bad bank performance.

Our results establish that lenders compete on non-price terms and can gain market share through laxer covenant protection. Our findings indicate the necessity to "internalize the externalities that are generated in the shadow banking system" (Adrian and Shin, 2009) and account for competition on non-price terms between the regulated and the non-regulated sectors in regulatory decision making. Lax lending standards lead to increased bankruptcies and the buildup of risks in the economy.

Our first contribution to the literature consists in making progress toward addressing identification challenges to establish the importance of competition on non-price terms in credit markets. Considering credit products as differentiated products can be informative for the regulatory debate and the design of effective regulations. Although our results do not substitute a quantitative equilibrium analysis, they relate to the literature that studies banking

competition and its impact for regulatory design. Recent contributions include Boyd and De Nicolo (2005), Moreira and Savov (2017), Wang et al. (2018), and Corbae and D'Erasmo (2019). With respect to these studies, we emphasize the economic importance of incorporating competition of non-price terms in quantitative studies, and the need to consider the externalities imposed by the presence of a non-regulated shadow banking system.

Second, our paper relates to studies that investigate the secular decline in covenant protection in syndicated loan agreements over the last decade. Some studies link the decline in the use of loan covenants to more efficient financial contracting (Becker and Ivashina, 2016; Berlin et al., 2018; Griffin et al., 2020). Other studies emphasize the influx of capital supply by shadow bank institutions and the change in the regulatory environment (Irani et al., 2020; Paligorova and Santos, 2019; Ivashina and Vallee, 2020). Our setting allows us to obtain causal evidence linking the design of loan covenants and bank market share in an important segment of the syndicated loan market. It documents the empirical relevance of loan covenant design in gaining market share for lenders, and highlights difficulties in regulations stemming from the competition between regulated and shadow banks.

Finally, our identification strategy is based on the large existing literature that investigates the importance of relationship lending. Recent seminal contributions include Degryse and Van Cayseele (2000), Degryse and Ongena (2005), Ivashina and Scharfstein (2010), Ivashina and Kovner (2011) and Chodorow-Reich (2013). We contribute to this extensive literature by exploiting an arguably exogenous source of variation in loan covenants and study competition between regulated and shadow banks on non-price terms.

3.2 Data and Background

3.2.1 Institutional Background: the Leveraged Lending Clarification

To investigate the effect of loan covenants on competition in credit markets, we exploit the introduction of a clarification ("the Clarification") of the regulatory guidelines on leveraged lending for U.S. borrowers on November 7, 2014. The Clarification was issued by the three

largest regulators of the United States banking sector, namely the Office of the Comptroller of the Currency (OCC), the Board of Governors of the Federal Reserve System (Board) and the Federal Deposit Insurance Corporation (FDIC) and, collectively with the OCC and the Board, "the Agencies".

In our empirical analyses, we exploit the fact that the Clarification targets regulated banking and financial institutions, but not the shadow banking system. Precisely, the Clarification applies to "national banks, federal savings associations, and federal branches and agencies supervised by the OCC; state member banks, bank holding companies, savings and loan holding companies, and all other institutions for which the Federal Reserve is the primary federal supervisor; and state nonmember banks, foreign banks having an insured branch, state savings associations, and all other institutions for which the FDIC is the primary federal supervisor."

The process that led to the introduction of the Clarification began on March 22, 2013. In response to increasingly lax lending standards, especially in the segment of high-risk leveraged lending, the Agencies initially issued a Leveraged Lending Guidance ("the Guidance"). Broadly speaking, the Guidance called for improved underwriting standards of leveraged loans. To this end, the Guidance formalized the definition of leveraged lending (as in Table A.1 in the Appendix) and specified minimum loan underwriting standards.

Importantly, the Guidance did not explicitly demand more financial covenants in lending agreements and there was considerable uncertainty among market participants as to the scope of its applicability. For this reason, compliance was low initially and the Agencies issued the Clarification in November 2014. While the Clarification points to both qualitative and quantitative measures of covenant protection ("few or weak covenants", Clarification 11), it does not single out covenants as the only non-price loan dimension that the regulators will pay close attention to. However, both the Guidance and Clarification were perceived by industry participants as predominantly addressing the waning use of covenants in loan agreements[26].

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[26]News articles and industry professionals discussing the Clarification typically refer to covenants, e.g. "Feds Win Fight Over Risky-Looking Loans", *Wall Street Journal*, 2015, by Tracy Ryan; Credit Suisse Loans Draw Fed Scrutiny, *Wall Street Journal*, 2015, by Gillian Tan, Tracy Ryan.

In addition, the regulators themselves specifically put a lot of emphasis on covenants while discussing and analyzing the impact of the Clarification in their communication. For example, 2015 Shared National Credits (SNC) Review points out that "The most frequently cited underwriting deficiencies identified during the 2015 SNC review were minimal or no loan covenants, liberal repayment terms, repayment dependent on refinancing, and inadequate collateral valuations. The weak underwriting structures were in part attributable to aggressive competition and market liquidity." These observations from the media and official press releases, combined with our initial evidence on the impact of the Clarification, underscore the covenant channel as the primary mechanism affecting the dynamics of lending after the regulation.[27]

To the extent the Clarification is not a completely anticipated, we interpret it as a plausibly exogenous event concerning loan covenants for our analysis. Our interpretation is reinforced by additional events taking place in the third and fourth quarter of 2014. Specifically, whenever the Agencies have concerns regarding a bank's risk, they must formally express them in direct letters to banks called "matters requiring immediate attention" ("MRIA"). Failure to promptly respond to MRIA letters may be subject to formal enforcement actions, which "include cease and desist orders, formal written agreements under U.S. federal law, and Prompt Corrective Action Directives" (Webb, 2016). In July 2014, Credit Suisse received a MRIA letter expressing regulatory concerns about its underwriting standards and, as Webb (2016) describes, other banks also received MRIA letters regarding their leveraged lending operations in the third quarter of 2014.

The timing of events entails an important caveat, which drives our choice of treatment period. An alternative interpretation of the MRIA letter directed to Credit Suisse is that is serves as a message to the entire industry that banks should take the 2013 Guidance more seriously and change their lending procedures accordingly. Only in the following quarter, the agencies later formalized their concerns in the Clarification in November 2014. In our main analysis,

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[27]To mitigate the impact of other channels, we always control for variables that proxy other loan non-price terms mentioned in the 2015 SNC Review, such as maturity and the absence of collateral.

we define the treatment period conservatively starting from the fourth quarter of 2014. We provide a robustness exercise excluding the third quarter of 2014 from our sample in Appendix Table C.9.

Two recent papers also analyze the effect of the Leveraged Lending Guidance and Clarification on aggregate changes in the syndicated lending. Using supervisory data, Schenck and Shi (2017) show that non-bank participation in the leveraged loan market broadly increased after the initial Guidance. Kim et al. (2018) document that the Clarification led regulated banks to reduce the number of leveraged loans. Our paper is the first to demonstrate that the observed reduction of lending by regulated institutions is attributable to the loan covenant channel, which highlights loan non-price terms as an important dimension of competition among lenders.[28]

3.2.2 Data Sources

Our primary source of loan data is S&P's Leveraged Commentary and Data (LCD) database, which is a comprehensive source of U.S. issued leveraged loans. Unlike the widely used LPC DealScan, LCD is a data source with a peculiar focus on the leveraged segment of the syndicated loan market. Because the covenant-lite phenomenon is exclusive to the leveraged segment, LCD is suitable for this study. Our sample period begins with LCD coverage in 2000 and ends with loans originated in 2018. We aggregate observations at the quarterly frequency.

We restrict our analyses to lead arrangers, since the Clarification emphasizes that rules primarily apply to lenders that *originate* loans, even if they do not subsequently hold any stake in that loan. Lead arrangers play the most important role in the loan origination, hold the largest stake in a loan and bear most of the risks associated with the lending process. Eventual loan securitization or sale in the secondary loan market does not exempt loans from the Clarification. Because the Clarification applies only to commercial banks, it is key to our analysis to accurately classify lenders as banks and non-banks to determine the treatment

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[28]In Section 4 we implement an additional robustness test using the Guidance instead of the Clarification to define our treatment period. Consistent with Kim et al. (2018), we find that regulated lenders reacted to the Clarification, while the Guidance had less impact.

status. We manually identify treated banks based on the list of commercial banks from the Federal Reserve, and also the list of FDIC-insured banking institutions.

There are two key reasons why LCD is a critical resource for our analysis. First, as Becker and Ivashina (2016) and Billett et al. (2016) discuss, Dealscan has a poor reporting quality in leveraged lending segment, which results in a widespread misclassification of covenant-lite loans. Both LCD and Dealscan link the definition of leveraged loans to loan rating and interest rate over LIBOR. However, only LCD allows to actually verify the classification of loans, as this database reports both loan rating and interest rate. Covenant-lite loans are defined in LCD as those "that have bond-like financial incurrence covenants rather than traditional maintenance covenants that are normally part and parcel of a loan agreement".[29] Second, LCD indicates only one lead arranger for a given loan, instead of providing a list of co-agents. This effectively allows to assign each loan to a unique lender and unambiguously determine the treatment status in the Clarification.

In robustness analyses at the lender level, we use Thomson Reuters' LPC DealScan loan database, which historically has a significantly longer time horizon (coverage is comprehensive since 1996). In these analyses, we aggregate data to the package level and consider only loans that contain information on covenants, following Berlin et al. (2018). In these tests, we identify leveraged loans in this sample as those with the primary purpose being "Acquisition line", "Takeover", "Merger", "LBO", "MBO", "SBO", "Dividend Recap", and "Stock Buyback". This definition corresponds to the first feature of leveraged loans outlined in the Clarification (see Table A.1) and we additionally require loans to have an all-in-drawn spread over LIBOR of more than 150 basis points, which is another widely used filter in the empirical literature on leveraged lending. We link borrower characteristics to Dealscan data via the Compustat linking file provided by Michael Roberts and described in Chava and Roberts (2008).

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[29]Maintenance covenants are more restrictive than incurrence covenants because they require the borrower to meet a requirement every quarter not to be in violation. Incurrence tests, instead, simply require that the borrower is in compliance when it takes a specific action (e.g. paying a dividend, issuing new debt).

3.2.3 Summary Statistics

Table C.2 presents descriptive statistics from the LCD dataset from the 2012-2018 period, i.e. the main time window around the Clarification we consider in the analyses. Panel A refers to the entire sample, Panel B contrasts banks and non-banks, Panel C contrasts loans that are covenant-lite with the remaining loans, and Panel D refers to observed frequencies of switching lenders from banks to non-banks and vice versa. Panel A shows that the average leveraged loan has a size is $640 mln , bears an interest rate of 4.08% and has a maturity of almost 6 years. Only 2% of loans are secured by collateral, while approximately 96% of borrowers have an available rating at time of loan origination. 28% of lending relationships are new, that is, borrower never borrowed from the same lender since the beginning of our sample. Roughly one half of loans are covenant-lite, and the vast majority of lenders has an existing banking relationship (with at least one bank) at the time of loan origination.

Panel B shows that nonbanks, compared to banks, have on average smaller deals, charge higher interest rates, and lend more to non-rated borrowers. This suggests that the shadow banking system likely picks up riskier borrowers in the leveraged lending market. Perhaps not surprisingly, borrowing relationships with nonbanks tend to be new more often, namely 38% of the times versus 27% for regulated banks, suggesting their increasing involvement in the leveraged loan segment. Finally, only 37% of loans originated by nonbank lead arrangers involve a borrower with a previous banking relationship, while the figure raises to 96% for bank-originated loans. Banks are significantly more active than nonbanks in the 2012-2018 period in the leveraged loan segment, as the number of observations for the two groups suggests. Overall, Panel B suggests that the behavior of our treated and control lenders is not comparable in the leveraged loan market along a number of different dimensions. This underscores the importance of saturating the model with lender× quarter fixed effects to deal with selection bias.

The statistics in Panel C highlight that covenant-lite loans are on average larger, bear higher interest rates, have longer maturities, less likely to have collateral and tend to involve rated

borrowers more often. Table C.15 adds to this, by reporting the Top15 lead agents in the covenant-lite segment. After the Clarification, prominent nonbank lenders, such as Jefferies Finance and General Electric Capital Corp. increase their participation, in contrast to banks, which in most cases decrease their covenant-lite part of leveraged lending.

Finally, Panel D reports transition frequencies of borrowers from banks to non-banks and vice versa, both before and after the Clarification. The two leftmost matrices indicate that the chance to switch from a bank to a non-bank lender increases from 3.3% to 5.1%, while the probability to switch from non-bank lender to a bank drops from 47.3% to 35.8%. The comparison of frequencies in the rightmost panel shows that all changes are statistically significant at 1% level. The descriptive statistics in Panel D possibly suggest an effect of the Clarification on lender-borrower relationships. The in-depth investigation of this effect is at the core of the following empirical analyses.

3.3 Empirical Strategy and Results

3.3.1 Loan Covenants and Lender Choice

Empirical Strategy This section presents our main analysis at the loan level. Methodologically, our identification strategy relies on a differences-in-differences (DID) framework, combined with an instrumental variable (IV) approach. This approach is often referred to as "Instrumented DID" (see, for example, Duflo (2001)).

Relationship Lenders and Treated Loans. We identify a loan in our dataset with a borrower index b, a lender index l and a quarter index t. The tuple (b,t) generally suffices to identify a loan in our sample uniquely, besides the cases in which the same borrower issues two leveraged loans in same quarter with different lead arrangers. Because these occurrences are rare (fewer than 2% of loans) and potentially the result of data errors, we exclude them from our main analyses.[30]

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[30]In un-tabulated results, we verify that our empirical strategy are virtually unaffected by including these observations.

The main idea behind our analysis is that firms which seek a loan after the clarification receive loan offers from regulated institutions that including relatively more covenants than the offers from non bank institutions. As a result, these borrowers migrate to the non bank sector. A key challenge in implementing this analysis is that loan offers are inherently unobservable. Our solution to this challenge builds on the large consensus in the literature about the importance and stickiness of banking relationships, which imply a cost to borrowers who switch lenders (Chodorow-Reich, 2013). We therefore proxy for the unobservable offer extended to borrower b with the average loan terms that are being made by their last lender in the quarter that .

Formally, for each loan b at time t, we define the *relationship lender* as the last lender from which borrower b was linked in our sample before quarter t. Similar to Chodorow-Reich (2013), we only consider the most recent loan to determine relationship status.[31] We then define treated loans as those whose borrower have a regulated bank as a relationship lender ($BankBorrower_{b,t} = 1$), in contrast to control loans in which the relationship lender is a non-banking institution ($BankBorrower_{b,t} = 0$). Observe that, unlike in classic DID, our treatment indicator is time varying. Because the same borrower b at different points in time can have a different relationship lender, our unit of treatment is the loan (b,t). We then define $Post_t$ as an indicator equal to one for all quarters from 2014Q4 to 2018Q4, and zero from 2012Q1 to 2014Q3. With our sample spanning the seven calendar years from 2012 to 2018, this splits our sample into two pieces of roughly three years before and four years after the guidance, respectively.[32]

First Stage. The dependent variable in our first stage specification, $NonCovliteOffer_{b,l,t}$, estimates the likelihood that borrower b receives a non-covenant-lite loan offer from its relationship lender despite accepting a loan offer from lender l in quarter t. A key challenge in

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[31]Syndicated loans often exhibit a structure in which a large, regulated bank performs the role of lead arranger and the syndicate consists of a mix of regulated banks and non banks. Importantly, the Clarification applied to loans even if there was just a single regulated bank as lead arranger and all participants were non regulated banks. We therefore assign treatment based on the lead arranger being a regulated bank only. While in theory a loan could be arranged by a non bank and feature regulated banks as participants, the general structure of syndicates implies that loans arranged by non banks rarely feature regulated lenders in subordinated roles.

[32]In robustness tests presented in Appendix Table C.3 we show that results are robust to changing this time window to a symmetric 1, 2, 3, or 4 year window, respectively.

our setup is that we only observe *realized* loan terms, whereas our ideal variable of interest would be the unobservable *offered* loan terms. We therefore define $NonCovliteOffer_{b,l,t}$ as the fraction of non-covenant-lite loans from the relationship lender of loan (b,t) to all other borrowers it lends to in the same quarter t that are not covenant-lite.

Critically, borrowers and lenders in our sample are heterogeneous. First, borrower b's relationship lender likely customizes offers accounting for the identity of available borrowers in quarter t. Second, different lenders likely pursue different strategies with respect to their supply of credit and covenant-light structures, especially in light of the non-comparability between treated and non-treated lenders, as the evidence in Table C.2 suggests. As discussed later, the structure of the data allows to saturate the model with borrower and lender $\times$ time fixed effects, similar to Khwaja and Mian (2008), Jiménez et al. (2012) and Jiménez et al. (2014). To the extent fixed effects absorb borrower and lender heterogeneity, the instrumented $NonCovliteOffer_{b,l,t}$ can be included along with them in the second-stage specification to gauge the likelihood of obtaining a non-covenant-lite for any loan.

The baseline regression model for the first stage is:

$$NonCovliteOffer_{b,l,t} = \beta_0 + \beta_1 BankBorrower_{b,t} \times Post_t + \beta_2 X_{b,l,t} + \delta_b + \beta_3 BankBorrower_{b,t} + \eta_{l,t} + \varepsilon_{b,l,t},$$

(3)

where $X_{b,l,t}$ is a vector of loan-level controls, including loan size, interest rate, maturity, and indicators for collateralized loans, rated borrowers, and covenant-lite loans. In all specifications, we include fixed effects for loan purposes[33], δ_b denotes borrower fixed effects, $\eta_{l,t}$ denotes lender $\times$ quarter fixed effects, and $\varepsilon_{b,l,t}$ is an error term. Observe that the term $Post_t$ is absorbed by $\eta_{l,t}$.[34] In all specifications standard errors are clustered at the borrower level and are robust to heteroskedasticity.[35]

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[33]LCD includes the following loan purposes: M&A/Acquisition, M&A/LBO, Recap/Dividend, Corp Purpose, Recap/Stock Repurchase, DIP, Project Financing, M&A/Merger, Exit Financing, Recap/General Recap, M&A/Spinoff, Recap/IPO, Expansion/covenant-lite, Recap/Equity Infusion, M&A/MBO, M&A/Leveraged Buildup, Recap/IPO/IDS, and Working Capital/Inventory/Receivables.

[34]Table 3.7 in the Appendix additionally indicates that the Clarification does not seem to have any significant impact on our control variables.

[35]As an alternative, we verify that results are robust to clustering the lender level.

The key identifying assumption is that lending relationships for large U.S. borrowers are fairly stable over time, and borrowers likely approach their old relationship banks when shopping for a new loan. If the Clarification is effective, regulated banks are more likely to offer non-covenant-lite loans after the event. Thus, we expect a positive loading on $BankBorrower_{b,t} \times Post_t$.

Second Stage. In the second stage we estimate the effect of competition on non-price terms, namely loan covenants, for the probability of leaving the relationship lender for a new lender. We define the dependent variable *New Lender*$_{b,l,t}$ as an indicator equal to one if the borrower never borrowed from the same lender l from the beginning of our sample. Our main variable of interest is the instrumented *NonCovliteOffer*$_{b,l,t}$ from the first stage. If regulated lenders require more covenants after the Clarification and borrowers value covenant-lite loans, borrowers are more likely to switch to a different lender. What causes the switch in lender is not the covenant structure offered by the current lender, but rather the tighter covenant structure offered by the previous lender.

We estimate the following baseline regression model in the second stage:

$$NewLender_{b,l,t} = \gamma_0 + \gamma_1 \widehat{NonCovliteOffer}_{b,l,t} + \gamma_2 X_{b,l,t} + \delta_b + \eta_{l,t} + \varepsilon_{b,l,t}, \qquad (4)$$

where $\widehat{NonCovliteOffer}_{b,l,t}$ denotes the instrumented *NonCovliteOffer*$_{b,l,t}$ from the first stage and $\varepsilon_{b,l,t}$ is an error term. Figure 3.1 provides a graphical illustration of our empirical strategy.

Figure 3.1 Identification strategy and bank relationships

This figure provides a graphical illustration of the identification strategy based on relationship banking and described in the text. In time period t^-, borrower b takes out a loan with relationship lender l_R. At time period t, we estimate the unobservable offer lender l_R makes to borrower b from observing loans between l_R and *other* borrowers. Our instrumented variable is *NonCovlite Offer*$_{b,l,t}$, the fraction of non-covenant-lite loans arranged by lender l_R at t. If the Clarification forces treated lenders to offer less covenant-lite loans, borrowers switch to novel non-bank lenders such as lender l.
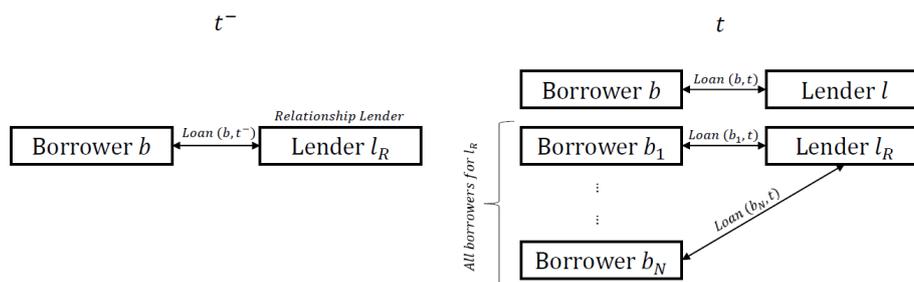
Initial Evidence: Clarification and the Probability to Switch between Lenders Table 3.1 reports the simple DID estimation for the probability to switch between lenders. After the Clarification, borrowers are likely to switch from their existing lender to a new one.

Table 3.1
Probability to Switch Lenders After the Clarification

The table presents difference-in-differences estimates of how the Clarification (2014Q4) affected the chance of switching between lenders. All variables are defined in Appendix Table C.1. The sample contains all loans to non-financial borrowers in the years 2012 to 2018. Commercial US banks are the treated group, non-bank financial institutions are the control group. Controls include loan size, interest rate, maturity, and indicators for collateralized loans, rated borrowers, covenant-lite loans, and borrower characteristics as revenue and profit. t-statistics clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

	New Lender			
	(1)	(2)	(3)	(4)
Post × Bank Borrower	0.182**	0.165**	0.220**	0.350***
	(0.086)	(0.081)	(0.087)	(0.113)
Bank Borrower	-0.138*	-0.130*	-0.143*	-0.265***
	(0.078)	(0.074)	(0.074)	(0.080)
Borrower FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No
Lender FE	No	No	Yes	No
Lender × Time FE	No	No	No	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes
Rating FE	No	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
Obs	4460	4460	4460	4460
R2	0.51	0.53	0.56	0.65

Table C.13 adds additional granularity to the dependent variable. Column 2 shows that the switch happens primarily to lenders offering covenant-lite structures. Nonbank lenders account for a significant fraction of this migration (Column 3), that often offer covlite loans (Column 4).

Table C.16 provides placebo analysis, indicating that the results on switching do not hold prior to the Clarification.

First-Stage Results: Effect of Treatment on Covenant-Lite Loans Table 3.2 reports the results of the first stage regression. Column (1) shows that the coefficient of $BankBorrower_b \times Post_t$ is positive and statistically significant at the one percent level, with a point estimate of 0.170. Columns (2) and (3) introduce loan level control variables and add time-invariant lender

fixed effects. Finally, Column (4) presents our most stringent specification, in which we saturate the model with lender $\times$ quarter fixed effects. The coefficient becomes larger, with a point estimate of 0.192. Economically, this coefficient indicates that borrowers with an existing relationship with banks have roughly a 20% higher probability of obtaining a non-covenant-lite loans from their relationship lenders after the Clarification in comparison to the control group.

From the leftmost to the rightmost column, the specifications in the tables apply a progressive saturation strategy to assess the severity of selection bias on unobservables, as in Altonji et al. (2005) and Oster (2019). The coefficient of the main explanatory variable remains fairly stable across specifications, while the R^2 increases from 74% to 88%. As in Oster (2019), to the extent selection on the added covariates is informative about selection on residual unobserved heterogeneity, a stable coefficient along with an increase in the R^2 indicates that the bias based on omitted unobservables is limited.

Overall, the table shows that borrowers in a relationship with treated banks are less likely to obtain covenant-lite loan offers. We interpret this result as the Clarification affecting the lending standards of treated banks and reducing their supply of covenant-lite loans.

Table 3.2

First Stage: Clarification and Probability of Getting a Non-Covlite Loan Offer from Relationship Lender

The table presents difference-in-differences estimates of how the Clarification (2014Q4) affected the chance of being offered a covenant-lite loan. All variables are defined in Appendix Table C.1. The sample contains all loans to non-financial borrowers in the years 2012 to 2018. Commercial US banks are the treated group, non-bank financial institutions are the control group. $NonCovliteOffer_{b,l,t}$ estimates the likelihood that borrower b's relationship lender would have offered a non-covenant-lite loan to b measured as the fraction of covenant-lite loans given out by the borrower b's relationship lender to all other borrowers (if any) at time t. Controls include loan size, interest rate, maturity, and indicators for collateralized loans, rated borrowers, covenant-lite loans, and borrower characteristics as revenue and profit. t-statistics clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

	NonCovlite offer			
	(1)	(2)	(3)	(4)
Post × Bank Borrower	0.170***	0.164***	0.158***	0.192***
	(0.025)	(0.024)	(0.024)	(0.031)
Bank Borrower	-0.185***	-0.181***	-0.178***	-0.217***
	(0.022)	(0.021)	(0.021)	(0.021)
Borrower FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No
Lender FE	No	No	Yes	No
Lender × Time FE	No	No	No	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
Obs	4460	4460	4460	4460
R^2	0.74	0.74	0.75	0.88

Figure 3.2 refers to the dynamics of the treatment effect before and after the Clarification. The figure plots the coefficients on a set of indicators for each quarter in the sample in place of $Post_t$. The estimates are from the most stringent specification (column 4) of Table 3.2. The graph shows that, prior to the Clarification, borrowers that had previously borrowed from regulated banks were *more* likely to obtain a covenant-lite loan offer on their next loan. After the clarification, all coefficient estimates for these borrowers are either zero or negative. The

figure suggests that the drop in offered covenant-lite loans for borrowers of regulated lenders coincided with the Clarification.

Figure 3.2 Treatment effect over time
This figure provides a graphical illustration the treatment dynamics in our first-stage regression. The graph plots the dynamic development of treatment coefficients from the most stringent specification (column 4) of Table 3.2 in which $Post_t$ is decomposed into a set of indicator variables for each quarter in the sample. Vertical lines present 90% confidence intervals.
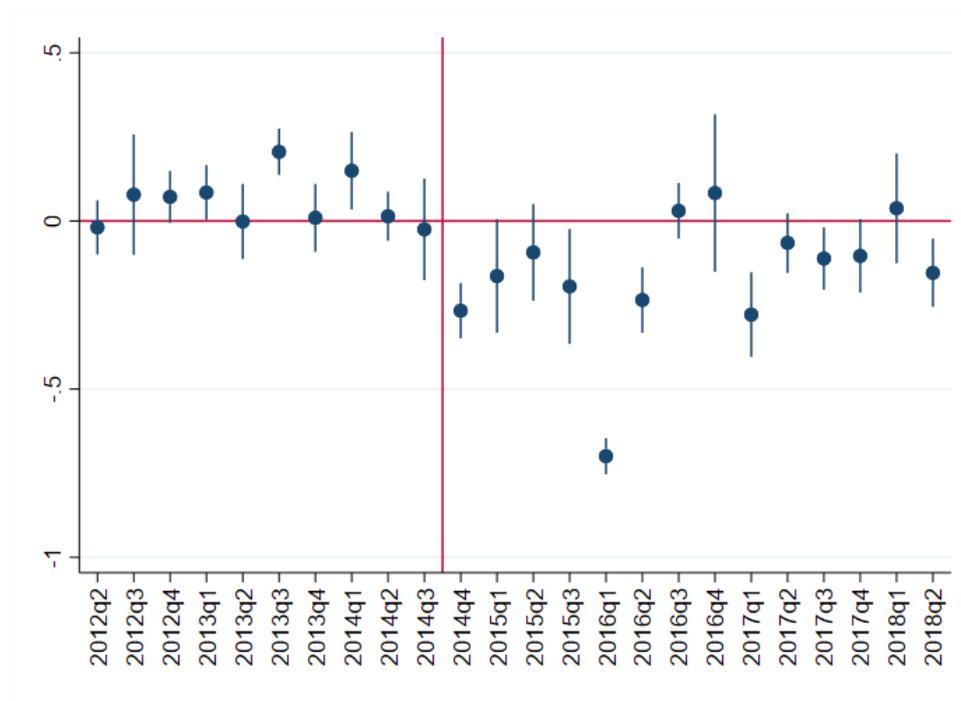
Second-Stage Results: Covenant-Lite Loans and New Lending Relationships Table 3.3 reports the results of second stage regression. Columns (1) to (4) in Table 3.3 include the same controls as Columns (1) to (4) in Table 3.2. The coefficient estimate on $Non\widehat{CovliteOffer}_{b,l,t}$ is positive and statistically significant across all specifications, with point estimates ranging from 1.076 to 1.825. In our baseline and most stringent specification, reported in Column (4), the estimated effect is the highest. Its point estimates is roughly 1.8, statistically significant at the one percent level. For ease of exposition, we also report first stage Kleibergen-Paap rk Wald F statistics in this table. The F value is 37.45 in our most stringent specification, which is

significantly above the critical Stock and Yogo level of 16.4 in this specification, and alleviates concerns of a weak instrument issue.

The economic magnitude of our estimated treatment effect is sizeable. In the most stringent specification of Column (4), the second stage local average treatment effect (LATE) is roughly a 35% (0.192*1.825) chance of switching to the non-bank sector as a response to a covenant-lite offer induced by the Clarification. On the aggregate level, we find that about 50 borrowers switched from traditional to shadow banks as a result of the reform. In correspondence of the mean (median) loan size of treated firms, this represents more than $30bn ($20bn) of lending that migrated into the shadow banking sector.

Table 3.3
Second Stage: Instrumented Non-Covenant-Lite Loan Offers and Switching Lenders
The table presents second-stage 2SLS estimates of $NewLender_{b,l,t}$, an indicator equal to one if the borrower never borrowed from the same lender l since the beginning of our sample, on the instrumented $\widehat{NonCovLiteOffer}_{b,l,t}$ from the first stage. All variables are defined in Appendix Table C.1. The sample contains all loans to nonfinancial borrowers in the years 2012 to 2018. Commercial US banks are the treated group, nonbank financial institutions are the control group. Controls include loan size, interest rate, maturity, and indicators for collateralized loans, rated borrowers, covenant-lite loans, and borrower characteristics as revenue and profit. t-statistics clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

	New Lender			
	(1)	(2)	(3)	(4)
NonCovlite Offer	1.076**	1.009**	1.388***	1.825***
	(0.473)	(0.467)	(0.518)	(0.625)
Bank Borrower	0.060	0.052	0.103	0.131
	(0.062)	(0.059)	(0.065)	(0.095)
Borrower FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No
Lender FE	No	No	Yes	No
Lender × Time FE	No	No	No	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes
Rating FE	No	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
Obs	4460	4460	4460	4460
Kleibergen-Paap rk Wald F	45.87	45.90	42.91	37.45

Overall, the results of Table 3.3 highlight the economic importance of competition on loan covenants in determining lending choices. Although banking relationships are sticky, borrowers appear to value covenant-lite loans and to be willing to seek funding from other lenders as a result of the Clarification. Table C.14 additionally shows that the results continue to hold for borrowers switching to lenders offering covenant-lite loans, nonbanks, and nonbanks offering covenant-lite loans.

Cross-Sectional Analyses

Strength of Lender-Borrower Relationships Several studies suggest the strength of the borrower's relationship with their relationship lender may affect competition on non-price terms. Lender ascertain soft information on borrowers over the relationship. This learning process may benefit borrowers via higher credit availability and lower loan prices. At the same time, the informational advantage of the relationship lender may induce adverse selection and limits the borrower's ability to switch to a new lender, leading to "informational capture" (Sharpe, 1990; von Thadden, 2004). Both these channels imply that borrowers with stronger relationships face higher switching costs, reducing the sensitivity to the increase in loan covenants after the Clarification.

Alternatively, borrowers in strong relationships might be able to obtain more favorable non-price terms ex ante. For example, Prilmeier (2017) shows that banks tend to lower their covenant demands over the course of lending relationships. If borrowers with stronger lending relationships are more likely to have received covenant-lite loans prior to the Clarification, they are more affected by the increase in covenants, and react more strongly to the Clarification.

To test how relationship strength affects our baseline results, we interact our instrument with the number of previous interactions a given borrower had with their previous lender prior to each loan.

Table 3.4
Cross-Sectional Effects: Relationship Intensity
The table presents second-stage 2SLS estimates of $NewLender_{b,l,t}$, an indicator equal to one if the borrower never borrowed from the same lender l since the beginning of our sample, on the instrumented $\widehat{NonCovLiteOffer}_{b,l,t}$ from the first stage. The variable *Number of Interactions* measures the relationship strength between the borrower and its previous lender as the number of loans taken out by the borrower from its previous lender, All variables are defined in Appendix Table C.1. The sample contains all loans to nonfinancial borrowers in the years 2012 to 2018. Commercial US banks are the treated group, non-bank financial institutions are the control group. Controls include loan size, interest rate, maturity, and indicators for collateralized loans, rated borrowers, covenant-lite loans, and borrower characteristics as revenue and profit. t-statistics clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

	New Lender			
	(1)	(2)	(3)	(4)
NonCovlite Offer	1.399***	1.354***	1.467***	1.960***
	(0.421)	(0.426)	(0.468)	(0.491)
NonCovlite Offer × Number of Interactions	-0.276***	-0.262***	-0.260***	-0.228***
	(0.065)	(0.061)	(0.061)	(0.062)
Bank Borrower	0.083	0.076	0.098	0.120
	(0.055)	(0.054)	(0.060)	(0.081)
Number of Interactions	0.030	0.026	0.031	0.019
	(0.033)	(0.031)	(0.030)	(0.030)
Borrower FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No
Lender FE	No	No	Yes	No
Lender × Time FE	No	No	No	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes
Rating FE	No	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
Obs	4460	4460	4460	4460
Kleibergen-Paap rk Wald F	19.69	20.18	18.87	17.12

Table 3.4 reports results on the differential impact of relationship strength in the second-stage regressions. The impact of the Clarification observed in Table 3.3 is concentrated in borrower-lender pairs that did not have a record of strong lending relationships. In all specifications, the coefficient on $\widehat{NonCovliteOffer}_{b,l,t}$ is larger than its

counterpart in Table 3.3. The most stringent specification in Column 4 the coefficient on $\widehat{NonCovliteOffer}_{b,l,t}$ rises to 1.96. However, the presence of repeated interaction between a specific borrower and previous lender decreases this effect by roughly 0.23 per interaction. A one standard deviation (2.6) increase in the number of higher previous interactions attenuates the propensity to switch to a new lender in reaction to a covenant-lite loan offer by about one third compared to the unconditional value. The results in Table 3.4 suggest that transactional borrowers with low costs of switching lenders reacted the most to the Clarification and sought funding from the shadow banking sector.

In additional tests, presented in Table C.5 and Table C.6 in the Appendix, we consider alternative measures of relationship strength, respectively an indicator variable equal to one if a borrower had taken out more than two loans from a particular lender in the past, and the duration of the relationship. The results in both table are qualitatively consistent with the estimates in Table 3.4.

Historical Presence in the Covenant-Lite Segment The Clarification's impact likely depends on the historical presence in the covenant-lite segment of both lenders and borrowers. Borrowers without covenant-lite loans prior to the Clarification likely are either not interested in, or do not qualify for, covenant-lite loan structures. For these borrowers, the reduced ability of regulated banks to offer covenant-lite loans should be less important, and they should react less to the threat of not being able to obtain covenant-lite loans from their relationship lenders. Similarly, lenders that rarely offered covenant-lite loan structures prior to the Clarification were less limited by the regulation.

To test these conjectures, in Column 1 of Table 3.5, we interact $\widehat{NonCovliteOffer}_{b,l,t}$ with the indicator variable $LastLoanCovLite_{b,t}$, which is equal to one if the last loan by borrower b in quarter t was covenant-lite. In Column 2, we replace $LastLoanCovLite_{b,t}$ with a measure of the lender's propensity to offer covenant-lite contracts, namely the indicator $HighCovLiteShare_{rl,t}$.

The latter is equal to one for relationship lenders rl that extended an above-median fraction of Covenant-Lite loans in the five years prior to the Clarification.[36]

In Column 1, the coefficient on the instrumented variable $Non\widehat{Covlite}Offer_{b,l,t}$ is 1.445, statistically significant at the five percent level. The interaction term with $LastLoanCovLite_{b,t}$ has a coefficient of roughly 0.68, statistically significant at the five percent level. Thus, borrowers that are more dependent on covenant-lite loans before the clarification are about 50% more likely to switch lenders in reaction to receiving a covenant-lite loan offer after the Clarification.

In Column 2, $Non\widehat{Covlite}Offer_{b,l,t}$ has a statistically significant coefficient equal to 1.962. The interaction with $HighCovLiteShare_{l,t}$ has a coefficient of approximately 2.5, statistically significant at the five percent level. These results suggest that relationship lenders that, before the Clarification, were extending more covenant-lite loans are more likely to lose borrowers seeking funding without covenant protections.

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[36]Note that, for ease of exposition, Table 3.5 displays just the interaction terms. The un-interacted indicator for $LastLoanCovLite_{b,t}$ is absorbed by the borrower fixed effect.

Table 3.5

Historical Presence in the Covenant Lite Segment

The table presents second-stage 2SLS estimates of $NewLender_{b,l,t}$, an indicator equal to one if the borrower never borrowed from the same lender l since the beginning of our sample, on the instrumented $\widehat{NonCovLiteOffer}_{b,l,t}$ from the first stage. Column 1 investigates the cross sectional impact for borrowers who's last loan prior to the clarification was covenant lite, by interacting the *NonCovLite Offer* with the indicator *Last loan pre-Clarification Covlite*. Column 2 investigates the cross sectional impact for lenders who extended an above median fraction of Covenant Lite loans in the 5 years prior to the Clarification by interacting the *NonCovLite Offer* with the indicator *High CovLite share pre-Clarification*. All variables are defined in Appendix Table C.1. The sample contains all loans to nonfinancial borrowers in the years 2012 to 2018. Commercial US banks are the treated group, non-bank financial institutions are the control group. Controls include loan size, interest rate, maturity, and indicators for collateralized loans, rated borrowers, covenant-lite loans, and borrower characteristics as revenue and profit. t-statistics clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

	New Lender	
	(1) Covlite Demand	(2) Covlite Supply
NonCovlite Offer	1.445**	1.926***
	(0.621)	(0.686)
NonCovlite Offer×Last loan pre-Clarification Covlite	0.680**	
	(0.320)	
NonCovlite Offer×High Covlite share pre-Clarification		2.533**
		(1.260)
Bank Borrower	0.144	0.091
	(0.098)	(0.086)
Borrower FE	Yes	Yes
Lender × Time FE	Yes	Yes
Loan Purpose FE	Yes	Yes
Rating FE	Yes	Yes
Controls and un-interacted terms	Yes	Yes
Obs	4460	4460
Kleibergen-Paap rk Wald F	17.56	11.42

Jointly, these results suggest that on the demand side, borrowers that are more dependent on covenant-lite loans are more likely to switch lenders as a response to the Clarification. On the supply side, lenders with a high historical presence in the covenant-lite segment are more likely to experience an outflow of borrowers to the advantage of other lenders.

3.3.2 Loan Covenants and Bank Level Market Share

In this section, we switch our focus to the lender level and test whether the restrictions to issuing covenant-lite loans that regulated lenders face after the Clarification translate into a loss of market share to the advantage of the shadow banking sector. Notice that the outcome does not mechanically follow from the analysis of the previous section, neither quantitatively nor qualitatively. For example, if banks offer fewer covenant-lite loans to their existing borrowers, but at the same time specifically cater demand by borrowers previously linked to non-banks and offer then more covenant-lite loans, their aggregate share in the covenant-lite segment may not decrease.

The empirical strategy to estimate the effect of loan covenants on bank market share is similar to the one at the loan level in our main analysis, and also relies on the combination of a DID setting with an instrumental variable (IV) approach. The key difference is that we aggregate data at the lender-quarter level to analyze the effect of the Clarification on lenders' market share.

Our regression model for the first-stage is:

$$CovLiteLending_{l,t} = b_0 + b_1 BankBorrower_l \times Post_t + b_2 X_{l,t} + \delta_l + \eta_t + \varepsilon_{l,t}, \quad (5)$$

where $CovLiteLending_{l,t}$ is the value-weighted fraction of covenant-lite loans for lender l in quarter t, $BankBorrower_l$ is an indicator taking the value one if a lender l belongs to the treatment group, $X_{l,t}$ is a vector of lender-level controls, including the weighted average interest rate, loan maturity, and fraction of collateralized loans, δ_l and η_t are lender and time fixed effects, and $\varepsilon_{l,t}$ is an error term. Observe that our specification includes lender and year-quarter fixed effects to account for time invariant lender level heterogeneity as well as time varying market conditions. Since these data are now aggregated at the lender quarter level, we cannot control for loan specific variables and borrower fixed effects any more.

Our second-stage model estimates:

$$MarketShare_{l,t} = c_0 + c_1 \widehat{CovLiteLending}_{l,t} + c_2 X_{l,t} + \delta_l + \eta_t + \varepsilon_{l,t}, \tag{6}$$

where $MarketShare_{l,t}$ is the ratio of loans originated by lender l in quarter t to the total amount of loans syndicated in the market during the same quarter, and $\widehat{CovLiteLending}_{l,t}$ is the instrumented fraction of covenant-lite loans from the first stage.

Table 3.6 presents the estimation results for both the first-stage (Column 1) and for the second-stage regression (Column 2) described above. Column 1 presents the results from the first stage regression, the coefficient on the interaction term $BankBorrower_l \times Post_t$ is -0.145 and statistically significant at the 1% level. Thus, after the Clarification, regulated banks reduced the fraction of covenant-lite loans among their newly issued loans by roughly 15%. Controls include the average interest rate charged by each lender in the quarter, as well as the average maturity and collateralization. The regression absorbs a substantial share of the variation in the dependent variable, namely 62%. Overall, these estimates provide a bank level equivalent to the results in Table 3.2 and confirm that the Clarification was effective in influencing the lending standards of regulated banks in the leveraged loan segment.

The second column of Table 3.6 presents results from the second stage estimates. The coefficient on the instrumented variable, $\widehat{Covlite\ Lending}$ is 8.349, which implies that a bank that switched from issuing all of its loans with covenants to issuing all of them without covenants would gain 8.3% of the total market share. Evaluating this coefficient at sample means, we find that a one standard deviation increase in the propensity to offer covenant-lite loans (about 33%) increases market share by about 2.9%.[37] This is an economically very large effect that corresponds to about half of a standard deviation in bank market share (6%). Of course our estimate here reflects the local average treatment effect uncovered by the instrumental variable approach.

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[37]Presented estimates are based on bank-quarter level data.

Thus, competition between banks and non-banks appears to results in a larger share of the leveraged loan segment being captured by non-banking institutions. This result is consistent with Schenck and Shi (2017), who show that non-bank participation in the leveraged loan market recently increased. In contrast Schenck and Shi (2017), however, we find that the Clarification, rather than the Guidance, was triggering the competitive pressure on non-price terms from nonbanks. The economic magnitude of the coefficient estimate is large. A one standard deviation increase in the instrumented fraction of covenant-lite loans is associated with approximately a 2% increase in market share.

Overall, the results in Table 3.6 suggest that borrowers' preference for covenant-lite loans translates into borrowers seeking funding from non-banks, which gain market share at the expense of traditional regulated lenders.

Table 3.6
Effect of Covenant-Lite Lending on Bank-Level Market Share
The table presents first-stage difference-in-difference estimates (Columns (1) and (2)) and second-stage 2SLS estimates (Columns (3) and (4)) of the effect of the Leverage Lending Clarification (2014Q4) on the fraction of covenant-lite loans originated by a lender in a given quarter, and on the lender market share. All variables are defined in Appendix Table C.1. The sample contains all loans to nonfinancial borrowers in the years 2012 to 2018. Commercial US banks are the treated group, nonbank financial institutions are the control group. Controls include value-weighted interest rate, maturity, loan rating, and indicators for collateralized loans. t-statistics clustered by lender and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

	IV 1st Stage	IV 2nd Stage	DID
	Covlite Lending	Market Share	Market Share
	(1)	(2)	(3)
Post x Bank Borrower	-0.145***		-1.210*
	(0.050)		(0.636)
Covlite lending		8.349*	
		(4.938)	
Lender FE	Yes	Yes	Yes
Year-quarter FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Obs	626	626	626
R2	0.62		0.82
Kleibergen-Paap rk Wald F		8.45	

3.4 Additional Analyses and Robustness

3.4.1 Parallel Trends

A key assumption for the DID estimation in our first stage estimation to be valid is that the treatment and control groups exhibit parallel trends before the Clarification. In Figure 3.3, we plot the value-weighted fraction of covenant-lite loans in total lending on a yearly basis from 2010 to 2018.

The graph shows that the two groups appear to exhibit parallel trends prior to the Clarification. In the immediate post-treatment period the fraction of covenant-lite loans of treated lenders decrease, while for control group they exhibit an increasing trend. This suggests that non-banks are relatively similar to banking institutions with respect to the fraction of covenant-lite lending prior to the Clarification. Importantly, after the treatment, the fraction of covlite lending for both groups continues to follow parallel trends. We do not observe a steep decline in the number of covlite loans underwritten by banks during the year immediately after the Clarification. This pattern is in line with the data reported in the 2016 SNC Review, which shows only a modest decrease in the level of special mention and classified loan commitments.

This weakening of the effect is in line with the shift in the enforcement of the Clarification.[38]

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[38]The change in the attitude toward the enforceability of the regulation started in 2017 under the new administration, with the Government Accountability Office (GAO) issuing an opinion that the Guidance needs to be approved by the Congress before rules could take effect, "Auditor's Decision Deals Blow to U.S. Crackdown on Leveraged Loans", *Wall Street Journal*, 2017, Tracy Ryan.

Figure 3.3 Trends in the fraction of covenant lite lending among treated and control lenders

This figure plots average fraction of covenant lite lending among treated and control groups around the introduction of the Clarification (2014Q4). Commercial banks are the treated group (blue line). The control group includes non-bank institutions unaffected by the Clarification (red line).
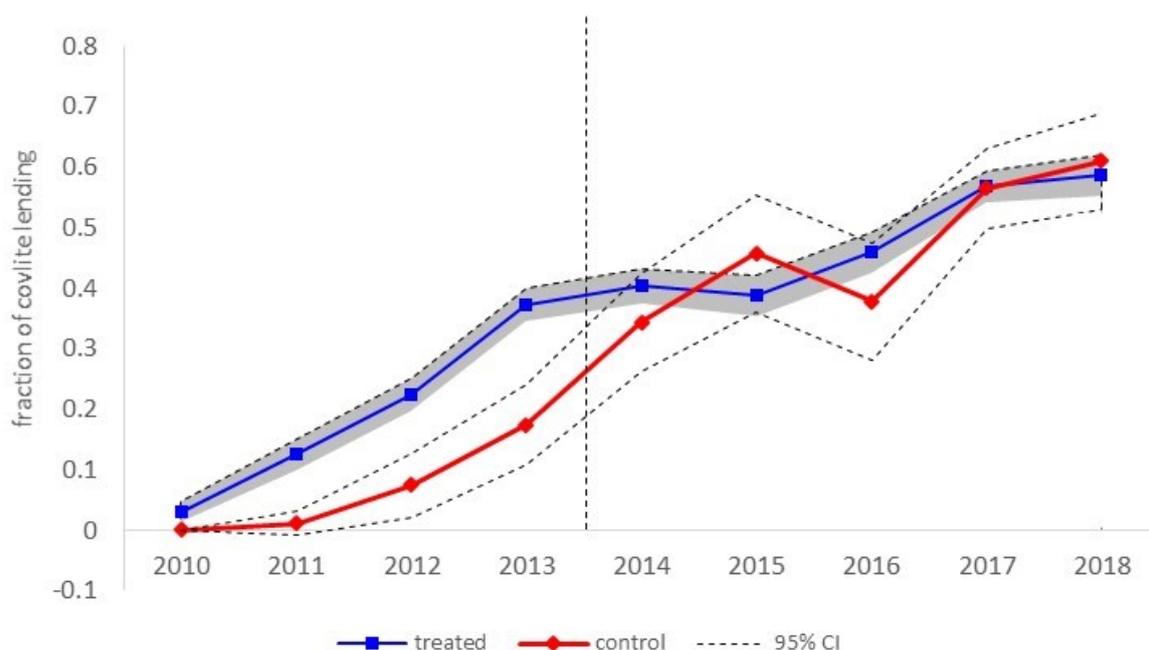
3.4.2 Alternative Channels

The first-stage regression results indicate that our instrumental variable satisfies the relevance condition. To satisfy the exclusion restriction, the Clarification needs to influence the outcome variables exclusively through the covenant channel.

The Clarification refers not only to covenants, but also considers other aspects of loan contract design, namely the capacity of borrowers to repay loans, the sustainability of their enterprise value, borrower leverage, and their ability to reduce leverage based on cash flow projections within a reasonable period of time. In addition, on the supply side, banks might have reduced their lending in the leveraged loan sector, regardless of the presence of covenants in loan agreements. The challenge to the exclusion restriction is hence that the Clarification impacted the lender choice of borrowers, and the market share of banks, through

those other dimensions, rather than loan covenants. This section explores empirically whether there was a contemporaneous change in lending along those dimensions. To empirically test this question, we estimate regressions analogous to our first stage tests in Table 3.2, using as outcome variables different measures of borrower quality and credit supply.

Other Loan Characteristics   We start by looking at other observable loan characteristics such as interest rate, loan maturity, collateral, and deal size. Table 3.7 indicates that covenant-lightness was the loan dimension most sensitive to the regulation. While loans became more expensive after the regulation, and more likely to have collateral, both these changes are not significant.

Table 3.7
Effect of the Clarification (2014Q4) on Other Loan Terms
This table reports DID estimates of the effect of the Leverage Lending Clarification (2014Q4) on key loan characteristics as interest rate, maturity, collateral, and deal size of loans originated by a given lender in a given quarter. All variables are defined in Table A.2. The sample contains all loans to nonfinancial borrowers in the years 2012 to 2018. Commercial US banks are the treated group, nonbank financial institutions are the control group. Parentheses contain t-statistics clustered by borrower and quarter and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

	NonCovlite	Interest	Maturity	Collateral	Deal Size
	(1)	(2)	(3)	(4)	(5)
Post× Bank Borrower	0.192***	0.131	0.015	0.101	0.125
	(0.031)	(0.199)	(0.023)	(0.205)	(0.153)
Bank Borrower	-0.217***	-0.034	-0.014	-0.046	-0.177*
	(0.021)	(0.158)	(0.017)	(0.142)	(0.106)
Borrower FE	Yes	Yes	Yes	Yes	Yes
Lender × Time FE	Yes	Yes	Yes	Yes	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes	Yes
Obs	4460	4460	4460	4460	4460
R^2	0.88	0.73	0.49	0.66	0.73

Indeed, the effect of the regulation on interest rates is rather non-trivial. Banks might have pushed interest rates higher, because regulated institutions find making covlite loan more costly after the Clarification. However, we also observe that banks increase the supply of noncovlite

loans relative to total loans in their portfolios which reduces average interest rates. This trade-off happens because loans with covenants are empirically associated with lower interest rates as they increase the contractual completeness and provide lenders state-contingent control rights (Matvos, 2013). Table C.4 in the Appendix provides further robustness to this result by showing that there was no significant change in interest rates for different event windows.

Borrower Rating Around the Clarification   The first challenge to the exclusion restriction is that lenders outright stopped extending loans to high risk borrowers altogether, shifting lending to higher quality borrowers. Figure 3.4 provides a first inspection of borrowers' risk profile before and after the introduction of the Clarification. The figure shows that the average credit rating of bank borrowers is stable around rating B between 2010 and 2017. Thus, Figure 3.4 indicates that the risk profile of bank borrowers remains virtually unchanged around the Clarification.

Figure 3.4 Dynamics of borrower credit ratings in the leveraged loan market
The figure depicts the evolution of the average borrower's credit rating in leveraged loan agreements originated to non-financial US borrowers. The blue line represents the average credit rating across bank borrowers and the area between the dashed lines represents the maximum and minimum credit rating across non-bank borrowers. Data on leveraged loans come from Thomson Reuters LPC database.
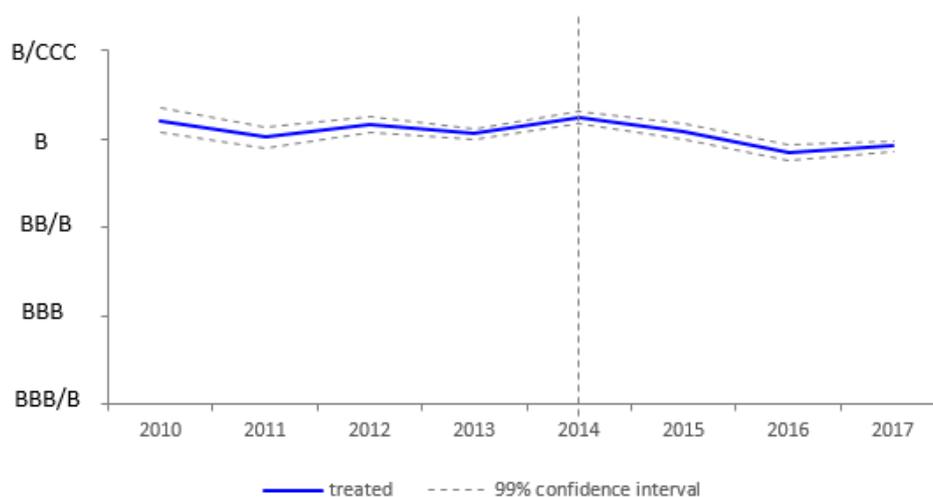
To corroborate this intuition, in Table 3.8 we estimate regressions in which we consider measures of borrower quality as dependent variables. Specifically, the dependent variable in Column 1 is the borrower's numerical credit rating, encoded such that lowest values corresponds to high ratings. The dependent variable in Column 2 is an indicator equal to one if borrower has a non-investment grade rating. In Column 1, the estimated coefficient on *Post × BankBorrower* is -0.040, with a standard error of 0.034. Similarly, in Column 2, the estimated coefficient on the borrower having an investment-grade rating is close to zero. These results do not show any indication that the Clarification lead to a shift of lending by regulated institution towards higher quality borrowers.

Table 3.8
Bank Borrowers' Risk Profile
The table studies the credit risk profiles of borrowers post-Clarification. The dependent variable in Column 1 is the numerical credit rating, where 1 corresponds to the best rating of AAA. The dependent variable in Column 2 is an indicator *1(Investment Grade)* which is equal to one if the borrower has a investment grade rating. The sample period is 2012 through 2018. Variables are aggregated to the lender-quarter level. Parentheses contain t-statistics clustered by borrower and quarter, and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Credit Rating (continuous)	1(Investment Grade)
	(1)	(2)
Post× Bank Borrower	-0.040	-0.000
	(0.034)	(0.002)
Bank Borrower	0.006	-0.000
	(0.026)	(0.001)
Borrower FE	Yes	Yes
Lender × Time FE	Yes	Yes
Loan Purpose FE	Yes	Yes
Controls	Yes	Yes
Obs	3874	3874
R^2	0.87	0.99

Table 3.9
Alternative Channels
The table reports results on the impact of the Clarification on lending via alternative channels. Panel A explores changes in Debt to EBITDA ratios of borrowers, both in terms of the continuous ratios (Column 1) as well as an indicator for the most highly leveraged borrowers with Debt to EBITDA larger than 6, *1(Debt to EBITDA>6)* (column 2). Panel B reports the leveraged lending dynamics of regulated banks. The dependent variable in Column 1 is the log of leveraged lending defined at bank and quarter level. The dependent variable in Column 2 is the fraction of leveraged lending (LCD) to the total lending (Dealscan). All variables are defined in Table A.2. The sample period is 2012 through 2018. Variables are aggregated to the lender-quarter level. Parentheses contain t-statistics clustered by borrower and quarter (Panel A) or clustered by lender (Panel B), and robust to heteroskedasticity. *, ** and *** indicate statistical significance at 10%, 5%, and 1% level, respectively.

	Panel A: Debt to EBITDA ratio	
	Debt To EBITDA	1(Debt To EBITDA>6)
	(1)	(2)
Post× Treated	0.234	-0.292
	(0.219)	(0.327)
Treated	-0.131	-0.344**
	(0.119)	(0.145)
Controls	Yes	Yes
Borrower FE	Yes	Yes
Lender × Time FE	Yes	Yes
Loan Purpose FE	Yes	Yes
Obs	1444	1454
R2	0.89	0.86

	Panel B: Leveraged Lending Dynamics of Regulated Banks	
	Leveraged supply (total)	Leveraged Loans to Total Lending (%)
	(1)	(2)
Post	-0.039	0.567
	(0.037)	(0.641)
Lender FE	Yes	Yes
Controls	Yes	Yes
Obs	498	406
R^2	0.80	0.14

Borrower Repayment Capacity Around the Clarification Panel A of Table 3.9 considers explores changes in the Debt-to-EBITDA ratios of borrowers. Column 1 considers the

borrower's Debt-to-EBITDA ratio as the dependent variable, while in Column 2 the dependent variable is an indicator variable equal to one if the Debt-to-EBITDA ratio is larger than 6. We choose a threshold of 6, as this is explicitly mentioned in the Clarification as a "red flag" to evaluate borrowers' repayment capacity. In both columns the coefficient are economically very small, and not statistically different from zero. In particular, in Column 1, the point estimates of *Post* × *BankBorrower* is 0.234, which would suggest that regulated lenders extended credit to borrowers with higher debt to EBITDA, and hence lower debt repayment ability than before. In Column 2, the coefficient on BankBorrower × Post is -0.292, suggesting regulated banks were slightly less likely to extend credit to the most financially stressed borrowers, although the coefficient is not statistically significant. The coefficient of *BankBorrower* in Column 2 is negative and statistically significant at the one percent level. This indicates that, regardless of the clarification, regulated banks are less likely to lend to borrowers with extremely high Debt-to-EBITDA ratios. Altogether, these results again provide no support to the potential challenge to the exclusion restriction that regulated lenders shifted funding away from borrowers with low ability to repay their debt claims around the Clarification.

Loan Supply Around the Clarification   Finally, in Panel B of Table 3.9, we turn our attention to overall loan supply, and in particular to variations in the presence of regulated banks in the leveraged lending segment around the Clarification. The potential challenge to the exclusion restriction from loan supply is that regulated banks reduced their *absolute* level of leveraged lending, leading to a *relative* decline in market share compared to shadow banks. To test this conjecture, we limit our sample to regulated lenders and aggregate data on the lender-quarter level. The dependent variable in Column 1 is the (log) amount of leveraged lending for bank b in quarter t. The dependent variable in Column 2 is the fraction of leveraged lending from the LCD data to total lending in the syndicated loan market from Dealscan. The resulting ratio, *Leveraged Loans to Total Lending*, captures the loan supply in the leveraged segment relative to the overall loan supply of lenders. These two outcome

variables capture the absolute and relative loan supply in the leveraged lending segment, respectively. If regualted lenders were to ration leveraged lending per se in reaction to the Clarification, the coefficient estimates for both of these outcomes should be negative.

However, the estimates form the lender-level regressions indicate that, in both columns, the coefficient on *Post* is not statistically different from zero. In particular, the coefficient on total loan supply is -0.039, with a standard error of 0.037. The coefficient on the fraction of leveraged lending is 0.567, with a standard error of 0.641. These economically small, and statistically insignificant estimates provide no indication that regulated lenders rationed credit supply to leveraged borrowers following the Clarification. These results are in line with the 2016 SNC Review, which shows only a minor decrease in the number of non-pass loans (Figure C.2 in the Appendix).

3.4.3 Split Control Rights

Berlin et al. (2018) show that covenant-lite loans are often originated as a part of lending packages that include other borrowing facilities, especially revolving credit lines, with enhanced covenant protection. This effectively creates loans with split control rights that attribute bargaining power to revolving lenders. Because, from a borrower perspective, these loan packages may be perceived as equally intrusive than other non-covenant-light loans, it is conceivable their presence in the sample affects our baseline results.

In this section we investigate cross-sectional differences in loans with respect to the presence of split control rights through revolving facilities. Table C.7 reports second-stage results for a restricted sample from which we exclude all loans with an attached revolving facility. All coefficients on $\widehat{NonCovliteOffer}_{b,l,t}$ are positive and statistically significant at the conventional levels. In the most stringent specification of Column 4, the coefficient on $\widehat{NonCovliteOffer}_{b,l,t}$ is substantially higher than its baseline value in Table 3.3, with a point estimate around 2.7, statistically significant at the one percent level.[39] The comparison with

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[39]In the first stage specification, the corresponding coefficient on $BankBorrower_{b,t} \times Post_t$ is 0.175, also statistically significant at the one percent level.

Table 3.3 shows that, although the presence of loans with attached revolving facilities lowers the economic magnitude of the coefficient, our results do not appear to be driven by such lending packages.

In all, the results in Table C.7 suggest that the effect of the Clarification on non-price competition on covenants is stronger among loans without an attached revolving facility, consistent with the evidence in Berlin et al. (2018).

Covenant-Lite Loans and Loan Performance   Financial covenants help lenders take control contingent on borrower's financial health to protect loans and avoid their further deterioration. These control rights allow lenders to substantially impact business decisions of borrowers such as investments, and increase long run borrower performance (Chava and Roberts, 2008).

Accordingly, the absence of meaningful covenants could lead to worse loan performance. We empirically test whether covenant-lite lending is correlated with higher probabilities of default in the panel regressions of Table C.8.[40] In these loan level regressions, the dependent variable is an indicator equal to one if the borrower eventually defaults on a loan. We obtain default events from LCD, and are defined as filing for bankruptcy, downgrading to rating level "D", or missing an interest payment.

We begin by investigating if borrowers that do not have taken out any loans with financial covenants are more likely to subsequently default. In Column 1, *Borrower has covenant-lite loans only* is an indicator equal to one if the borrower has only active covenant-lite loans in the quarter of loan issuance. We include the same control variables as in Table 3.3, with the exception of borrower fixed effects, since bankruptcy is a borrower level event and there is not enough variation left in the data after including them. The estimated coefficient is 0.15, statistically significant at the one percent level. This effect is economically very large, corresponding to three times the unconditional sample average of defaulting loans of 5%. Thus, borrowers with only covenant-lite loans have a substantially higher probability of

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[40]Unfortunately, there is not enough variation in these default events to estimate the locally identified IV regressions from the main analysis.

bankruptcy, consistent with the lack of control rights keeping lenders from intervening when borrower performance deteriorates.

In the second column, *Borrower has both loans with and without covenants* is an indicator equal to one if the borrower has both at least one active covenant-lite loan and one active non-covenant-lite loan. The estimated coefficient is negative and around -0.01 and it is not statistically significant. This result on borrowers with mixed loan portfolios is consistent echoes the evidence in Berlin et al. (2018), who show that oftentimes covenant-lite loans coexist with revolving credit lines that have an enhanced covenant protection. In these cases of "split control rights", indeed, the presence of covenant-lite loans is not associated with long-run loan performance.

The results that banks can gain market share by offering loans with lax covenant protection, and the result that these loans with lax covenant are associated with higher rates of default, potentially provide an important mssing link to the question raised in Fahlenbrach et al. (2017) why fast loan growth is followed by poor bank performance. Importantly, Fahlenbrach et al. (2017) find no deterioration in the objective quality of borrowers for fast growing banks, similar to our findings presented earlier. Importantly, Fahlenbrach et al. (2017) do not consider the possible deterioration of lending standards along the dimension of non-price terms, and hence our findings can provide a potential explanation why neither investors nor analysts foresee the deteriorating performance.

3.5 Aggregate Trends and External Validity

In this final section, we investigate whether our locally identified results form the leveraged lending market and the regulatory Clarification extend more broadly to the loan market as a whole. We test whether banks in general can poach borrowers from competitors and gain market share by offering lax covenant packages. We estimate panel regressions of quarterly bank market shares on the average number of covenants offered based on the longest and broadest available sample of syndicated loans, including all U.S. syndicated loans from LPC

Dealscan covering 1995 to 2017. If lax covenant protections allow banks to gain market share, the coefficient estimate on the average number of covenants should be negative, i.e. banks lose market share if they offer loans with more stringent covenant restrictions, and gain market share when offering lax covenant protections.

Table 3.10 reports the estimation results. We saturate our models with both lender and year-quarter fixed effects, to account for time invariant lender characteristics, such as specialization in a certain industry, and time varying loan market conditions. Effectively, the resulting coefficient captures variations in the level of covenant protection offered within the same lender, and compared to other lenders at the same point in time. The coefficient estimate on the average number of covenants in Column 1 is 0.37%, meaning a bank that relaxes lending standards by reducing the average number of covenants by 1 standard deviation gains about 0.5% of total market share, an economically sizable effect.

In Column 2 we add additional controls for average loan- and borrower characteristics, effectively horse racing the ability of banks to gain market share through lax covenant packaged against the ability to do so through other loan determinants. We find that, as we sequentially add controls for average loan loan maturity, interest rate, and borrower credit rating, the effect of loan covenants on market share stays economically stable, and statistically robust. Interestingly, the estimated coefficient on the average rating of borrowers in Column 2 is negative, which suggests that banks that gain market share tend to lend to borrowers that are of *higher observable credit quality*, but with *laxer contracted lending standards*. This finding is again consistent with lax lending standards providing the missing link in Fahlenbrach et al. (2017) as to why fast loan growth is followed by poor bank performance, yet without a deterioration in observable borrower quality.

Table 3.10

External validity: Number of Covenants, Bank Market Share, and Decision to Switch Lenders

The table presents bank-quarter level regressions of the relationship between loan characteristics, bank market share, and the borrower's decision to switch lenders. The dependent variable in Columns 1-2 is the bank's total market share (in percentages) in a given quarter, the dependent variable in Columns 3-4 is 1(Loan from new lenders) that takes the value of 1 for loans which are the first between a borrower and a lender. The explanatory variables in Columns 1-2 are bank-quarter averages of the number of covenants, average loan maturity, average interest rate, and average borrower rating. The explanatory variables in Columns 3-4 are various loan and borrower characteristics, such as the number of covenants in the loan contract, loan maturity, loan interest rate, and the borrower's rating. These regressions therefore present a comparison of loans made by new lenders compared to those in existing lending relationships. All variables are defined in Table A.2. The sample period is 1995 through 2017 and includes all loans to non-financial borrowers from LPC DealScan. Variables are aggregated to the lender-quarter level. Parentheses contain t-statistics clustered by lender and quarter and robust to heteroskedasticity. *, ** and *** indicate statistical significance at 10%, 5%, and 1% level, respectively.

	Market Share		1(Loan from new lenders)	
	(1)	(2)	(3)	(4)
Number of covenants	-0.373***	-0.419**	-0.023***	-0.022**
	(-3.988)	(-2.147)	(-4.368)	(-2.485)
Maturity		0.197		0.002***
		(1.518)		(2.709)
Interest		-0.115		0.010
		(-1.182)		(1.206)
Rating		-0.084***		0.002
		(-3.178)		(0.315)
Lender FE	Yes	Yes	Yes	Yes
Year-quarter FE	Yes	Yes	Yes	Yes
Obs	3,823	2,775	29,846	17,298
R^2	0.67	0.68	0.38	0.36

Finally, similar to Ioannidou and Ongena (2010), Columns 3 and 4 Table 3.10 inspect loan conditions after a borrower's decision to switch lenders. These tests provide micro level analogues to the bank level results from Columns 1-2. If banks poach borrowers by offering lax covenant terms, the first loan after a lender acquires a new borrowing relationship should exhibit lower than normal covenant protections.

To test this conjecture, in Columns 3 and 4 we estimate loan level panel regressions. The dependent variable in these tests is an indicator equal to one if a loan is originated as a borrower's first loan after switching lenders. In all specifications, the coefficient on the number of covenants is negative an statistically significant at the one percent level, with stable point estimates between -0.022 and -0.026. These coefficient estimates imply that loans featuring more covenant protections are significantly less likely to be first loans in a new lending relationship. Put differently, the first loan after poaching a new borrwoer exhibits significantly lower covenant protections.

The results show that borrowers' decision to switch lenders is consistently associated with loans that feature weaker covenant protections, even after accounting for other dimensions of the loan contract and borrower quality.

Taken together, the results in this section provide evidence that the locally identified findings from the Leveraged Lending Clarification, which link bank market share to the design of attractive (and lax) financial covenant packages, reflect a broader pattern of competition among lenders on non-price terms. Importantly, these results imply that banks growing their market share do not extend credit to objectively worse borrowers, consistent with the findings in Fahlenbrach et al. (2017). Instead, they build up dark matter of risk by relaxing contractual safeguards, a lack of which is associated with more subsequent defaults.

3.6   Conclusion

Using the Interagency Clarification on Leveraged Lending as a quasi-experimental setting, we show that loan covenants are an important dimension of lender competition. We exploit a comprehensive dataset, the S&P's Leveraged Commentary and Data (LCD) database, to take steps towards the identification of the importance of non-price competition in the data.

Following regulatory recommendations, regulated banking institutions slowed the issuance of covenant lite loans in comparison to non-banks. As a result, banks subsequently lost market share to the advantage of non-bank institutions. In the aggregate, we find that the enhanced

covenant protection of bank loans was crowded out by an increased participation of non-bank institutions in leveraged lending. These effects are concentrated in borrowers with low switching costs, and instances where borrowers and lenders demonstrate a previous affinity to covenant lite loan structures. While we find no deterioration in observable borrower quality, the absence of safeguards in the form of covenants leads to an increase in the incidence of defaults.

While our identification strategy is limited to the $1tn leveraged loan subsegment of the U.S. lending market, we provide reduced form, large sample, evidence that banks gain market share by offering loans with fewer covenants. In this way, our results provide a plausible explanations as to why fast loan growth is followed by bad bank performance (Fahlenbrach et al., 2017), and more importantly, why neither bank insiders nor external observers anticipate this lack of performance. While fast growing banks do not take on more risk based on observable borrower characteristics, they relax contractual safeguards that spring into action when borrower performance deteriorates, building up a dark matter of not easily observable risk.

Our results also serve as a clarion call to account for competition on non-price terms between the regulated and the non-regulated sectors in regulatory decision making and "internalize the externalities that are generated in the shadow banking system" (Adrian and Shin (2009)). Thus, while the Clarification reasonably addressed a lack of covenant protections in the regulated banking sector, a portion of risky loans migrated outside the regulatory environment. On one hand, shifting risks to the non-banking sector is not necessarily undesirable ("waterbed effect"). For example, some studies document that the probabilities of bankruptcy filing by non-bank borrowers are comparable to those by bank borrowers after controlling for borrower characteristics. On the other hand, there is evidence suggesting that excessive shadow lending makes the financial system more fragile. We leave the estimation of the welfare impact of the increase in shadow lending, one of the main and plausibly most important consequences of the Clarification, to future research.

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A   Appendix to Chapter 1

Figure A.1 VCs' investment activity around the quarter with an IPO

This figure plots the coefficient estimates from Column 2 of Table 1.9. Each point represents the coefficient of the linear probability model in which the dependent variable is equal to one if the VC partner makes an investment in a given quarter, and zero otherwise. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

A.1 Variable definitions
Table A.1
Variable definitions

Variable	Definition
Panel A. Partner and fund-level variables	
Acquisition-busy VC partner	Equals one if a particular company, existing in a VC partner's portfolio, gets acquired
Amount invested, $ mln	Total amount of investments into new portfolio companies a VC partner made in a given quarter
"Busy" investment	Equals one if busy VC partner invests in a new company during the distraction window
Busy VC partner	Equals one if a particular company, existing in VC partner's portfolio, goes public
Portfolio size	Number of new portfolio companies in which a VC partner invested in a given quarter
Time since inception	VC fund's age since its first investment (quarters)
VCs	VC firm employees in the capacity of a general partner, principal, or vice-president
Panel B. Investment-level variables	
Amount raised, $ mln	Total amount of VC funding raised by a particular company in a given financing round
Round number	The consecutive number of an investment round as defined by VentureXpert
Syndicate size	Total number of VC funds participating in a given investment round
Panel C. Company-level variables (conditional on being founded before 12/31/2012)	
Age	Company age at the date of an investment round (quarters)
IPO/Acquisition	Equals one if the company goes public or gets acquired Equals zero otherwise
Exit multiple	Equals the exit value over the total capital invested (total funding)
Exit value, $ mln	Equals the transaction value as reported by SDC Platinum for acquired companies, and market capitalization for companies that went public Equals zero otherwise
Industry	Industry of company's operation based on Venture Economics Industry Group classification, which has 6 categories: Communications and Media, Computer Related, Medical/Health/Life Science, Biotechnology, Semiconductors/Other Elect., Non-High-Technology
State	State of company's location as defined by VentureXpert
Total funding, $ mln	Total amount of VC funding raised by a particular company across all investment rounds

A.2 Matching VentureXpert to BoardEx

In order to study the impact of VCs' busyness at the individual partner level I match VentureXpert (retrieved via Thomson Reuters Eikon) to BoardEx (retrieved via WRDS). The basic name-matching algorithm that I use is similar to those implemented in previous papers: I match names step-by-step, with unmatched names rolling over to the next stage.

1. I merge two datasets based on original company names.

2. I clean company names from legal suffixes, prefixes, punctuations, leading and trailing blanks and transfer to lower case letters. Based on resulting stem names, datasets are matched again.

3. For the remaining unmatched company names I implement the fuzzy-matching technique based on the N-grams algorithm in Python. I require the match quality to be equal or higher than 0.9 on a scale from 0 to 1, and the first three characters to be identical for each matching pair.

Using this algorithm I create "portfolio company-to-company-to-director" and "venture capital firm-to-firm-to-partner" matched triplets. Based on "portfolio company-to-venture capital firm" combinations from VentureXpert, I match triplets to each other to form "portfolio company-to partner" pairs. Finally, I obtain IPO dates, investment dates, and dates when director took and left a board seat. For observable starting and ending dates of board memberships, I require IPO and investment events to fall between board appointment and termination dates.

I consider the following roles at venture capital firms: "General Partner", "Partner", "Founding Partner", "Associate Partner", "Partner Emeritus", "Operating Partner", "Venture Partner", "Managing Partner", "Regional Managing Partner", "Investment Partner", "Partner/Chief Marketing Officer", "Chairman/Partner", "Senior Partner", "Principal", "Vice President", "Managing General Partner", "MD", "Partner Chairman (Executive)", "Chairman/General Partner", "Chairman/Managing Partner", "Partner/MD", "Partner/CFO", "Partner/Regional MD", "Chief Operating Partner", "Development Partner", "General Partner/Regional Chairman".

A.3   Excerpts from a typical Limited Partnership Agreement

On time commitment

The General Partner hereby agrees to use its best efforts in furtherance of the purposes and objectives of the Partnership and to devote to such purposes and objectives such of its time as shall be necessary for the management of the affairs of the Partnership. During the Commitment Period, each of the members of the General Partner will devote substantially all of his business time to the affairs of the Partnership.

On venture capital fund composition

It is not contemplated that any additional general partners will be admitted to the Partnership. A person may be admitted to the Partnership as a general partner only with the written consent of the General Partner and Two-Thirds in Interest of the Limited Partners. Any such person so admitted as a general partner shall be liable for all the obligations of the Partnership arising before its admission as though it had been a general partner when such obligations were incurred. In the event of the addition of a general partner, the participation of such person in the management of the Partnership and the interest of such person in the Partnership's Operating Income and Loss and Investment Gain and Loss must be approved by the General Partner and Two-Thirds in Interest of the Limited Partners at the time of such person's admission.

A.4 IPO Engagement Process

Table A.2
The IPO engagement
This table provides anecdotal evidence regarding the participation of venture capitalists in the IPO process.

Stage of the IPO process	Venture Capitalist	Venture Capital Firm	Startup Company	Source
Choice of the underwriters	John Doerr	Kleiner Perkins Caufield & Byers	Amazon.com, Inc.	HBS Case # 9-899-003
IPO road show	John Doerr	Kleiner Perkins Caufield & Byers	Amazon.com, Inc.	LINK
IPO pricing	Michael Volpi	Index Ventures	Elastic, Inc.	LINK
IPO opening event	Michael Volpi	Index Ventures	Elastic, Inc.	LINK

A.5 Additional Tests

Table A.3
Pre- and post-dotcom bubble
This table studies the robustness of the main results over time and splits the sample into two subsamples – before and after December 31, 2000. All variables are defined in Table A.1. The sample is a quarterly panel of venture capital investments from 1990 through 2017. Parentheses contain standard errors clustered by VC partner and quarter and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	IPO/Acquisition		ln(exit multiple+1)	
	pre-2000	post-2000	pre-2000	post-2000
	(1)	(2)	(3)	(4)
Busy Partner	-0.104***	-0.083***	-0.274***	-0.154***
	(0.038)	(0.030)	(0.100)	(0.042)
Controls	Yes	Yes	Yes	Yes
VC Partner FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	2,994	12,916	2,994	12,916
R2	0.43	0.33	0.45	0.29

Table A.4

Controlling for the total number of IPOs by industry and time

This table contains reestimations of Table 1.2 controlling for IPO waves defined at the industry level. All variables are defined in Table A.1. The regressions directly control for the total number of IPOs in the industry minus distracting IPOs for a given partner. The sample is a quarterly panel of venture capital investments from 1990 through 2017. Parentheses contain standard errors clustered by VC partner and quarter and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	IPO/Acquisition		ln(exit multiple+1)	
	(1)	(2)	(3)	(4)
Busy Partner	-0.081**	-0.081**	-0.243***	-0.243***
	(0.032)	(0.031)	(0.082)	(0.081)
Number of IPOs by industry	-0.016	-0.034	-0.331	-0.346
(minus distracting IPOs by partner)	(0.104)	(0.101)	(0.212)	(0.210)
Controls	No	Yes	No	Yes
Partner FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	16,815	16,815	16,815	16,815
R2	0.36	0.37	0.35	0.35

Table A.5
Acquisition-busy VC partners and performance of new investments
This table presents estimates of the effect of VC partner acquisition-based distraction on financial outcomes of new investments. All variables are defined in Table A.1. The sample is a quarterly panel of venture capital investments from 1990 through 2017. Parentheses contain standard errors clustered by VC partner and quarter and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	IPO/Acquisition		ln(exit multiple+1)	
	(1)	(2)	(3)	(4)
Busy Partner (M&A)	-0.029	-0.032	-0.038	-0.040
	(0.028)	(0.028)	(0.055)	(0.053)
Controls	No	Yes	No	Yes
VC Partner FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	17,183	17,178	16,787	16,787
R2	0.36	0.37	0.35	0.36

Table A.6

Probability of taking a board seat and performance on the extended sample

This table presents estimates of the effect of VC partner distraction on the probability of taking a board seat and financial outcomes of new investments. The baseline sample is extended to include investments which did not imply a board seat for a VC partner. To figure out the absence of board membership, I first identify "venture capital fund-partner" pairs with only one partner. Next, I make an assumption that all investments made by that particular fund were also made by that specific VC partner. All variables are defined in Table A.1. The sample is a quarterly panel of venture capital investments from 1990 through 2017. Parentheses contain standard errors clustered by VC partner and quarter and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	=1 if VC takes a board seat		IPO/Acq.	ln(exit multiple)
	(1)	(2)	(3)	(4)
Busy Partner	-0.007	-0.008	-0.071***	-0.162***
	(0.009)	(0.009)	(0.024)	(0.050)
Controls	No	Yes	No	Yes
VC Partner FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	18,079	18,079	18,079	18,079
R2	0.69	0.69	0.36	0.35

Table A.7
Are "busy" investments different compared to other investments?

This table studies the determinants of "busy" investments. All variables are defined in Table A.1. The sample is a quarterly panel of venture capital investments from 1990 through 2017. Parentheses contain standard errors clustered by VC partner and quarter and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	"Busy" Investment					
	(1)	(2)	(3)	(4)	(5)	(6)
Partner characteristics						
ln(Portfolio size)	0.007***	0.002**	0.002**	0.002**	0.002**	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
ln(Amount invested)		0.016***	0.016***	0.016***	0.016***	0.017***
		(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Round characteristics						
ln(Syndicate size)			-0.001	-0.000	-0.000	-0.000
			(0.003)	(0.003)	(0.003)	(0.003)
ln(Round number)				-0.001	-0.001	-0.001
				(0.002)	(0.002)	(0.002)
ln(Company age)					-0.002	-0.002
					(0.002)	(0.002)
ln(Amount raised)						0.002*
						(0.001)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs	16,815	16,815	16,815	16,815	16,815	16,815
R2	0.03	0.04	0.04	0.04	0.04	0.04

Table A.8
Incidence of "busy" investments and probability of network acquisition

This table presents estimates of the relationship between the incidence of "busy" investments and the probability of network acquisition. All variables are defined in Table A.1. The sample is a quarterly panel of venture capital investments from 1990 through 2017. Parentheses contain standard errors clustered by VC partner and quarter and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	Syndication with			
	TOP10 VC firm by # of IPOs		TOP10 VC firm by # of investments	
	(1)	(2)	(3)	(4)
Busy Partner	-0.016	-0.016	-0.011	-0.011
	(0.013)	(0.014)	(0.020)	(0.020)
Controls	No	Yes	No	Yes
VC Partner FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	9,612	9,612	9,612	9,612
R2	0.26	0.26	0.27	0.27

Table A.9

Distracted VC partners and investments within the same industry and state

This table reestimates Table 1.2 by including Partner × Industry × State FEs, which allow to track investment decisions made by the same partner in the same industry and the same state. All variables are defined in Table A.1. The sample is a quarterly panel of venture capital investments from 1990 through 2017. Parentheses contain standard errors clustered by VC partner and quarter and robust to heteroskedasticity. *, ** and *** indicate statistical significance at the 10%, 5%, and 1% level, respectively.

	IPO/Acquisition		ln(exit multiple+1)	
	(1)	(2)	(3)	(4)
Busy Partner	-0.086**	-0.081**	-0.164**	-0.163**
	(0.039)	(0.040)	(0.071)	(0.071)
Controls	No	Yes	No	Yes
Partner × Industry × State FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	8,623	8,623	8,623	8,623
R2	0.46	0.47	0.45	0.46

B   Appendix to Chapter 2

B.1   Variable definitions

Table B.1
Variable definitions

Variable	Definition
# of TopVC commitments	The number of TopVC commitments made by a specific limited partner in a given year
# of investments	The number of capital commitments made by a specific limited partner in a given year
Any TopVC commitments	Indicator equal to one if a specific limited partner made at least one capital commitment to a top-tier VC firm in a given year
Experience	The number of years between a specific capital commitment and the first recorded capital commitment by the same limited partner in Preqin
Fraction of TopVC commitments	The number of TopVC commitments divided by the number of total VC commitments made by a specific limited partner in a given year
Fund number	The sequential number of a fund raised within a particular firm
Post	Indicator equal to one for the years following the event (2002q4)
Subscription ratio	The final fund size divided by the target fund size
Total commitments	The aggregate amount of capital commitments made by a specific limited partner in a given year
Treated	Indicator equal to one for public pensions and public endowment plans

C Appendix to Chapter 3

C.1 Variable definitions

Table C.1
Variable definitions

Variable	Definition
Collateral	Average fraction of the deals secured with collateral and originated by lender in a given quarter weighted by the amount of loan.
$CovLiteLending_{l,t}$	Dollar amount of covlite loans originated by lender / Total dollar amount of covlite loans originated in the loan market in a given quarter
Debt to EBITDA	Borrower debt divided by EBITDA.
1(Debt to EBITDA>6)	Indicator if borrower debt divided by EBITDA exceeds 6, a level of particular attention in the Leveraged Lending Clarification.
Interest	Average all-in-drawn spread of the deals originated by lender in a given quarter weighted by the amount of loan.
Market share	Dollar amount of loans originated by lender / Total dollar amount of loans originated in the loan market in a given quarter
Maturity	Average maturity of the deals originated by lender in a given quarter weighted by the amount of loan.
$NonCovlite\ Offer_{b,t}$	the likelihood that borrower b's relationship lender would have offered a non-covenant-lite loan to b measured as the fraction of non-covenant-lite loans given out by the borrower b's relationship lender to all other borrowers (if any) at time t.
Number of interactions	the number of loan agreements between borrower and lender prior to the Clarification
Post	Indicator equal to one in the one year following the issuance of the Clarification and zero in the two years prior to the Clarification.
Relationship	Indicator equal to one if a specific borrower have taken at least two loans in the past from a particular lender and zero otherwise.
Relationship duration	the number of years between two loan agreements in a given borrower and lender pair
Bank Borrower	Indicator equal to one for borrowers who's last loan was issued by US and foreign banks supervised by Federal Reserve, OCC, and FDIC and zero in all other cases.

C.2 Excerpts from the Leveraged Lending Guidance (2013): Leveraged Loans

#	Definitions of leveraged loan
1	Proceeds used for buyouts, acquisitions, or capital distributions.
2	Transactions where the borrower's Total Debt divided by EBITDA (earnings before interest, taxes, depreciation, and amortization) or Senior Debt divided by EBITDA exceed 4.0X EBITDA or 3.0X EBITDA, respectively, or other defined levels appropriate to the industry or sector.
3	A borrower recognized in the debt markets as a highly leveraged firm, which is characterized by a high debt-to-net-worth ratio.
4	Transactions when the borrower's post-financing leverage, as measured by its leverage ratios (for example, debt-to-assets, debt-to-net-worth, debt-to-cash flow, or other similar standards common to particular industries or sectors), significantly exceeds industry norms or historical levels.

C.3   Trends in the syndicated loan market

Figure C.1 Dynamics of the number of covenants in the syndicated loan market
This figure plots dynamics of the number of covenants across all syndicated loan agreements originated to nonfinancial US borrowers. The blue line represents simple average number of covenants across all loans, the red line represents the total commitments to the market. Data on covenants are from Thomson Reuters LPC DealScan database. Data on total amount of loan commitments comes from Shared National Credit Program governed by the Board of Governors of the Federal Reserve System, the Federal Deposit Insurance Corporation, and the Office of the Comptroller of the Currency.
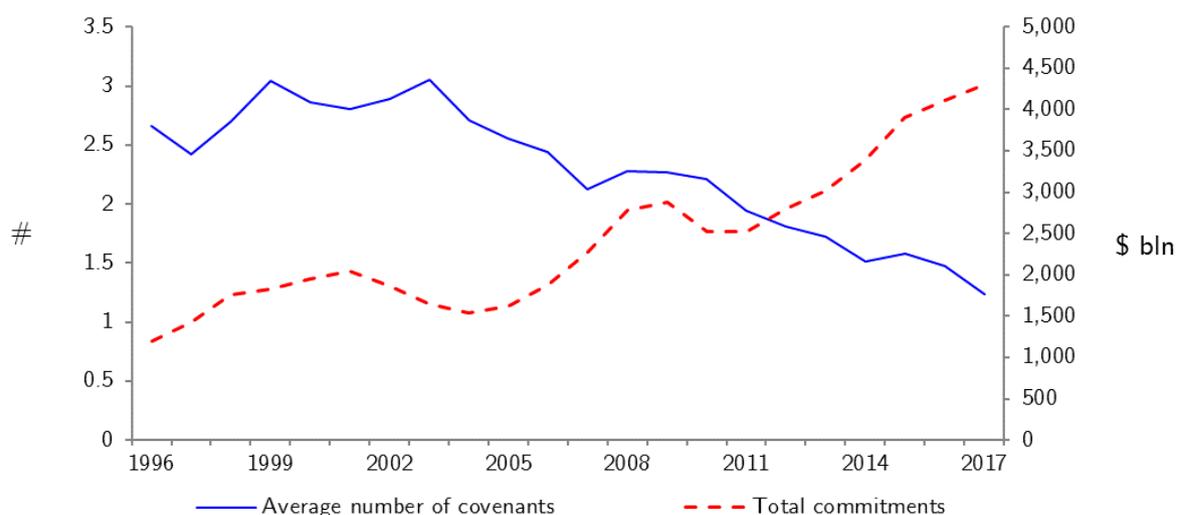
Figure C.2 Dynamics of the volume and fraction of non-pass loans
This figure plots dynamics of the volume and fraction of non-pass loans across all syndicated loan agreements originated to nonfinancial US borrowers. A non-pass loan is any loan that is rated special mention, substandard, or doubtful. The green line represents the fraction of non-pass loans across all loans, the red line represents the total non-pass loan commitments to the market. Data comes from Shared National Credit Program governed by the Board of Governors of the Federal Reserve System, the Federal Deposit Insurance Corporation, and the Office of the Comptroller of the Currency.
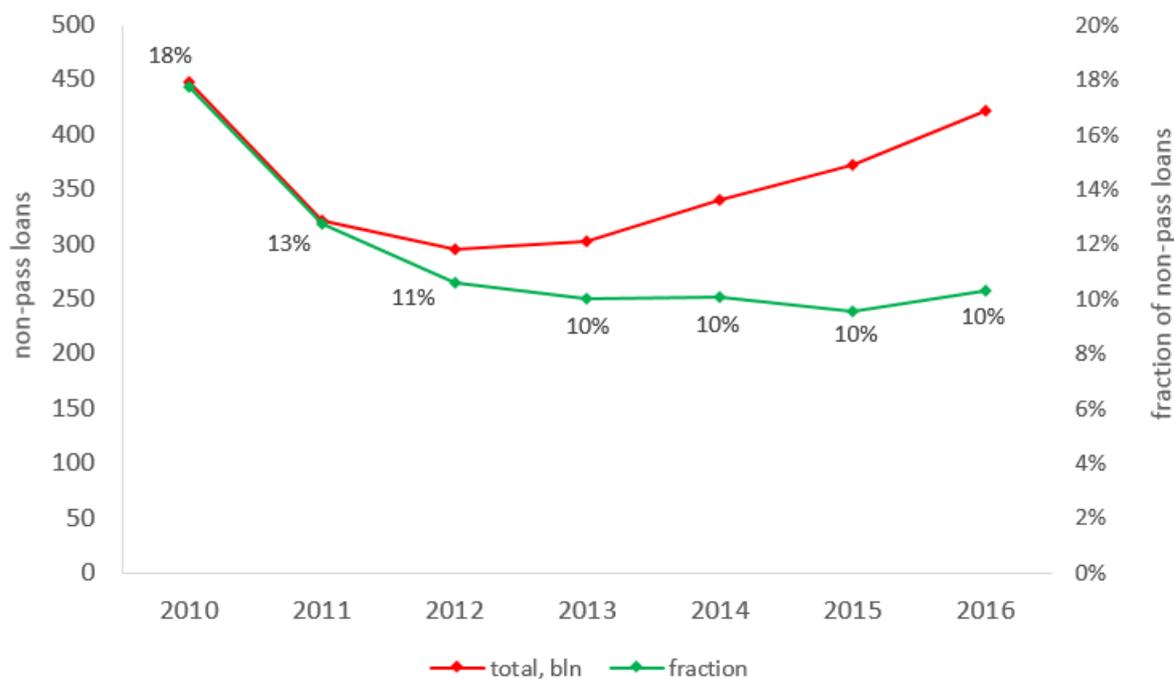
C.4 Data description

Table C.2

Summary Statistics

The table reports descriptive statistics for our sample of leveraged loans from the LCD dataset. The sample period is from 2012 to 2018. Panel A refers to the full sample, Panel B contrasts bank and nonbank institutions, Panel C contrasts the covenant-lite and the remaining loans. Panel D reports frequency of transitions from bank to non-bank lenders and vice versa along with a test on differences between the periods before and after the clarification (test statistics are in parentheses). Deal Size denotes the size of the loan in USD bln. Interest is the loan interest rate; Maturity the loan maturity in years; Collateral is an indicator for the loan being asset backed; Rated in an indicator for the loan being rated; New Lender is an indicator for new lenders. CovLite is an indicator variable for covenant-lite loans in LCD. Bank Relationship is an indicator for the borrower having a banking relationship at the time of loan origination. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

Panel A: Full Sample

	Mean	St. Dev.	Min	Max	NObs
DealSize ($ bln)	0.64	0.77	0.01	12.35	4460
Interest	4.08	1.81	0.33	15.00	4460
Collateral	0.02	0.14	0.00	1.00	4460
Maturity	5.82	1.21	0.50	10.00	4460
Rated	0.96	0.20	0.00	1.00	4460
New Lender	0.28	0.45	0.00	1.00	4460
CovLite	0.56	0.50	0.00	1.00	4460
Bank Relationship	0.90	0.30	0.00	1.00	4460

Panel B: Banks vs NonBanks

	Banks					NonBanks				
	Mean	St. Dev.	Min	Max	NObs	Mean	St. Dev.	Min	Max	NObs
DealSize ($ bln)	0.67	0.80	0.01	12.35	4039	0.35	0.41	0.01	2.40	421
Interest	4.00	1.78	0.33	15.00	4039	4.86	1.87	2.25	12.00	421
Collateral	0.02	0.14	0.00	1.00	4039	0.01	0.11	0.00	1.00	421
Maturity	5.81	1.20	0.70	10.00	4039	5.87	1.23	0.50	8.50	421
Rated	0.96	0.19	0.00	1.00	4039	0.93	0.25	0.00	1.00	421
New Lender	0.27	0.44	0.00	1.00	4039	0.38	0.49	0.00	1.00	421
CovLite	0.56	0.50	0.00	1.00	4039	0.58	0.49	0.00	1.00	421
Bank Relationship	0.96	0.20	0.00	1.00	4039	0.37	0.48	0.00	1.00	421

Panel C: Covenant-Lite vs Non-Covenant-Lite

	Covenant-Lite					Non-Covenant-Lite				
	Mean	St. Dev.	Min	Max	NObs	Mean	St. Dev.	Min	Max	NObs
DealSize ($ bln)	0.71	0.75	0.01	7.60	2515	0.55	0.79	0.01	12.35	1945
Interest	4.10	1.73	1.75	12.00	2515	4.06	1.90	0.33	15.00	1945
Collateral	0.00	0.00	0.00	0.00	2515	0.05	0.21	0.00	1.00	1945
Maturity	6.18	1.14	0.70	8.50	2515	5.36	1.13	0.50	10.00	1945
Rated	0.98	0.13	0.00	1.00	2515	0.92	0.26	0.00	1.00	1945
New Lender	0.26	0.44	0.00	1.00	2515	0.30	0.46	0.00	1.00	1945
Bank Relationship	0.90	0.30	0.00	1.00	2515	0.90	0.29	0.00	1.00	1945

Panel D. Frequency of Transitions

Before the Clarification			After the Clarification			Changes in Transition Frequencies		
From	To: Bank	Non-bank	From	To: Bank	Non-bank	From	To: Bank	Non-bank
Bank	96.7%	3.3%	Bank	94.9%	5.1%	Bank		1.9%*** (-4.38)
Non-bank	47.3%	52.7%	Non-bank	35.8%	64.2%	Non-bank	-11.5%*** (3.40)	

C.5   Additional Tests

Table C.3
Varying Event Windows (Second Stage)
The table presents estimates of the effect of the Leverage Lending Clarification (2014Q4) on borrowers switching lenders as a result of increased inclusion of financial covenants. We estimate the most complete specification from our main analysis (Table 3.3, Column 4) using a 1, 2, 3, and 4 year event window. All variables are defined in Appendix Table C.1. The sample contains all loans to nonfinancial borrowers in the years 2010 to 2018. Commercial US banks are the treated group, nonbank financial institutions are the control group. Controls include value-weighted interest rate, maturity, and indicators for collateralized loans. t-statistics clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at 10%, 5%, and 1% level, respectively.

	New Lender			
	(1)	(2)	(3)	(4)
NonCovlite Offer	3.076***	1.337*	1.338**	1.715***
	(0.960)	(0.718)	(0.621)	(0.630)
Bank Borrower	0.297	0.073	0.109	0.119
	(0.241)	(0.145)	(0.101)	(0.094)
Borrower FE	Yes	Yes	Yes	Yes
Lender × Time FE	Yes	Yes	Yes	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Obs	1023	2288	3731	4885
First stage Kleibergen-Paap F	28.41	31.49	32.89	36.44

Table C.4

The impact of the regulation on interest rates

The table presents estimates of the effect of the Leverage Lending Clarification (2014Q4) on interest rates. We estimate the baseline specification using a 1, 2, 3, and 4 year event window. All variables are defined in Appendix. The sample contains all loans to nonfinancial borrowers in the years 2010 to 2018. Commercial US banks are the treated group, nonbank financial institutions are the control group. t-statistics clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at the ten, five and one percent level respectively.

	Interest rate			
	(1)	(2)	(3)	(4)
Post × Bank Borrower	0.176	0.412	0.138	0.193
	(0.704)	(0.329)	(0.251)	(0.222)
Bank Borrower	-0.119	-0.223	0.016	-0.014
	(0.532)	(0.191)	(0.171)	(0.157)
Borrower FE	Yes	Yes	Yes	Yes
Lender × Time FE	Yes	Yes	Yes	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes
Obs	1023	2288	3731	4885
R2	0.68	0.67	0.66	0.65

Table C.5
Cross-Sectional Effects on Relationship Intensity: Alternative Measure
The table presents second-stage 2SLS estimates of the cross sectional impact of the Clarification (2014Q4) on borrowers based on the cost of switching to new lenders. The switching cost is proxied by an indicator variable taking a value of 1 if a certain borrower and lender pair had more than two previous relationships, and 0 otherwise. All variables are defined in Appendix Table C.1. The sample contains all loans to nonfinancial borrowers in the years 2012 to 2018. Commercial US banks are the treated group, non-bank financial institutions are the control group. Controls include value-weighted interest rate, maturity, and indicators for collateralized loans. t-statistics clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at 10%, 5%, and 1% level, respectively.

	New Lender			
	(1)	(2)	(3)	(4)
NonCovlite Offer	1.484***	1.468***	1.824***	2.031***
	(0.437)	(0.441)	(0.497)	(0.623)
NonCovlite Offer × Relationship	-1.176***	-1.199***	-1.262***	-1.061***
	(0.373)	(0.362)	(0.367)	(0.333)
Bank Borrower	0.099	0.087	0.133**	0.152*
	(0.060)	(0.059)	(0.066)	(0.090)
Relationship	0.429**	0.461**	0.519**	0.397**
	(0.213)	(0.208)	(0.212)	(0.192)
Borrower FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No
Lender FE	No	No	Yes	No
Lender × Time FE	No	No	No	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes
Rating FE	No	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
Obs	4460	4460	4460	4460
First stage Kleibergen-Paap F	22.28	22.32	20.46	18.94

Table C.6
Cross-Sectional Effects on Relationship Intensity: Duration
The table presents second-stage 2SLS estimates of the cross sectional impact of the Clarification (2014Q4) on borrowers based on the cost of switching to new lenders, proxied by relationship duration. All variables are defined in Appendix Table C.1. The sample contains all loans to nonfinancial borrowers in the years 2012 to 2018. Commercial US banks are the treated group, non-bank financial institutions are the control group. Controls include loan size, interest rate, maturity, and indicators for collateralized loans, rated borrowers, covenant-lite loans, and borrower characteristics as revenue and profit. t-statistics clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at 10%, 5%, and 1% level, respectively.

	New Lender			
	(1)	(2)	(3)	(4)
NonCovlite Offer	1.289***	1.248***	1.366***	1.826***
	(0.423)	(0.426)	(0.464)	(0.532)
NonCovlite Offer × Relationship Duration	-0.030***	-0.028***	-0.029***	-0.017*
	(0.008)	(0.008)	(0.008)	(0.010)
Bank Borrower	0.101*	0.094*	0.112*	0.120
	(0.058)	(0.057)	(0.061)	(0.085)
Relationship Duration	-0.012***	-0.012***	-0.011***	-0.017***
	(0.004)	(0.004)	(0.004)	(0.005)
Borrower FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No
Lender FE	No	No	Yes	No
Lender × Time FE	No	No	No	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes
Rating FE	No	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
Obs	4460	4460	4460	4460
First stage Kleibergen-Paap F	21.27	21.43	19.65	17.73

Table C.7
Robustness: Excluding Loans with Split Control Rights

The table presents the same second-stage 2SLS estimates of the impact of the Clarification (2014Q4) as in Table 3.3, excluding all loans which have an associated revolving facility that features covenants, i.e. cases of "split control rights" (Berlin et al., 2018). All variables are defined in Appendix Table C.1. The sample contains all loans to nonfinancial borrowers in the years 2012 to 2018. Commercial US banks are the treated group, non-bank financial institutions are the control group. Controls include loan size, interest rate, maturity, and indicators for collateralized loans, rated borrowers, covenant-lite loans, and borrower characteristics as revenue and profit. t-statistics clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at 10%, 5%, and 1% level, respectively.

	New Lender			
	(1)	(2)	(3)	(4)
NonCovlite Offer	1.244*	1.181*	1.633*	2.713***
	(0.643)	(0.657)	(0.839)	(0.876)
Bank Borrower	0.098	0.085	0.158	0.153
	(0.085)	(0.083)	(0.107)	(0.139)
Borrower FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No
Lender FE	No	No	Yes	No
Lender × Time FE	No	No	No	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes
Rating FE	No	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
Obs	3366	3366	3366	3366
Kleibergen-Paap rk Wald F	28.61	26.77	20.40	17.30

Table C.8
Probability of Default of Covenant-Lite Loans
The table reports loan level OLS estimates of default on a loan-level indicator variable equal to one in case of borrower default during the life of the loan. Default events are provided by LCD, and include bankruptcy filing, downgrade of credit rating to level "D", missed interest payment, but exclude distressed security exchanges. "Covlite loans only" is an indicator equal to one if the borrower has only active covenant-lite loans in a given quarter. "Covlite and NonCovlite loans" is an indicator equal to one if the borrower has at least one active covenant-lite loan and one active non-covenant-lite loan in a given quarter. All variables are defined in Table A.2. The sample contains all loans to nonfinancial borrowers in the years 2012 to 2020. Controls include loan size, interest rate, maturity, and indicators for collateralized loans, rated borrowers, covenant-lite loans, and borrower characteristics as revenue and profit. Parentheses contain t-statistics clustered by borrower and quarter and robust to heteroskedasticity. *, ** and *** indicate statistical significance at 10%, 5%, and 1% level, respectively.

	Default	
	(1)	(2)
Borrower has covenant lite loans only	0.150***	
	(0.027)	
Borrower has both loans with and without covenants		-0.013
		(0.008)
Time FE	Yes	Yes
Lender × Time FE	Yes	Yes
Loan Purpose FE	Yes	Yes
Rating FE	Yes	Yes
Controls	Yes	Yes
Obs	12601	12601
R^2	0.18	0.17

Table C.9
Robustness: Exclusion of 2014q3
The table presents second-stage 2SLS estimates of $NewLender_{b,l,t}$, an indicator equal to one if the borrower never borrowed from the same lender since the beginning of our sample, on the instrumented $\widehat{NonCovliteOffer}_{b,l,t}$ from the first stage. All variables are defined in Appendix Table C.1. The sample contains all loans to nonfinancial borrowers in the years 2012 to 2018, while leaving the 3rd quarter of 2013 out of the sample (due to the issuance of the MIRA letter to Credit Suisse and other banks). Commercial US banks are the treated group, nonbank financial institutions are the control group. Controls include loan size, interest rate, maturity, and indicators for collateralized loans, rated borrowers, covenant-lite loans, and borrower characteristics as revenue and profit. t-statistics clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at 10%, 5%, and 1% level, respectively.

	New Lender			
	(1)	(2)	(3)	(4)
NonCovlite Offer	1.118**	1.097**	1.372**	1.729***
	(0.499)	(0.497)	(0.541)	(0.613)
Bank Borrower	0.072	0.065	0.116*	0.125
	(0.067)	(0.064)	(0.070)	(0.095)
Borrower FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No
Lender FE	No	No	Yes	No
Lender $\times$ Time FE	No	No	No	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes
Rating FE	No	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
Obs	4301	4301	4301	4300
First stage Kleibergen-Paap F	40.90	41.25	40.06	38.59

Table C.10
Robustness: Controls Interacted with Post Variable
The table presents second-stage 2SLS estimates of $NewLender_{b,l,t}$, an indicator equal to one if the borrower never borrowed from the same lender since the beginning of our sample, on the instrumented $\widehat{NonCovLiteOffer}_{b,l,t}$ from the first stage. All variables are defined in Appendix Table C.1 and interacted with Post indicator variable. The sample contains all loans to nonfinancial borrowers in the years 2012 to 2018. Commercial US banks are the treated group, nonbank financial institutions are the control group. Controls include loan size, interest rate, maturity, and indicators for collateralized loans, rated borrowers, covenant-lite loans, and borrower characteristics as revenue and profit. t-statistics clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at 10%, 5%, and 1% level, respectively.

	New Lender			
	(1)	(2)	(3)	(4)
NonCovlite Offer	1.036**	1.005**	1.384***	1.841***
	(0.465)	(0.466)	(0.517)	(0.628)
Bank Borrower	0.059	0.053	0.104	0.134
	(0.062)	(0.059)	(0.065)	(0.095)
Borrower FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No
Lender FE	No	No	Yes	No
Lender × Time FE	No	No	No	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes
Rating FE	No	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
Obs	4460	4460	4460	4460
First stage Kleibergen-Paap F	46.09	45.85	42.95	37.17

Table C.11
Placebo: Effect of the *Initial Guidance* on Covenant-Lite Lending, First Stage
The table presents difference-in-differences estimates of how the Leverage Lending Guidance (2013Q2) rather than the Clarification (2014Q4) affected the chance of being offered a covenant-lite loan. All variables are defined in Appendix Table C.1. The sample contains all loans to nonfinancial borrowers in the years 2012 to 2018. Commercial US banks are the treated group, nonbank financial institutions are the control group. $NonCovliteOffer_{b,l,t}$ estimates the likelihood that borrower b's relationship lender would have offered a non-covenant-lite loan to b measured as the fraction of covenant-lite loans given out by the borrower b's relationship lender to all other borrowers (if any) at time t. Controls include loan size, interest rate, maturity, and indicators for collateralized loans, rated borrowers, covenant-lite loans, and borrower characteristics as revenue and profit. t-statistics clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at 10%, 5%, and 1% level, respectively.

	NonCovlite loans			
	(1)	(2)	(3)	(4)
Post× Bank Borrower Guidance	0.022	0.015	0.017	-0.015
	(0.038)	(0.036)	(0.036)	(0.024)
Post× Bank Borrower	0.165***	0.159***	0.152***	0.199***
	(0.048)	(0.032)	(0.032)	(0.032)
Bank Borrower	-0.200***	-0.190***	-0.188***	-0.208***
	(0.027)	(0.027)	(0.028)	(0.022)
Borrower FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No
Lender FE	No	No	Yes	No
Lender × Time FE	No	No	No	Yes
Purpose FE	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
Obs	4460	4460	4460	4460
R^2	0.74	0.74	0.75	0.88

Table C.12
Placebo: Effect of the *Initial Guidance* on Covenant-Lite Lending, Second Stage
The table presents second-stage 2SLS estimates of how the Leverage Lending Guidance (2013Q2) rather than the Clarification (2014Q4) affected the probability to switch lenders. All variables are defined in Appendix Table C.1. The sample contains all loans to nonfinancial borrowers in the years 2012 to 2018. Commercial US banks are the treated group, nonbank financial institutions are the control group. $NonCovliteOffer_{b,l,t}$ estimates the likelihood that borrower b's relationship lender would have offered a non-covenant-lite loan to b measured as the fraction of covenant-lite loans given out by the borrower b's relationship lender to all other borrowers (if any) at time t. Controls include loan size, interest rate, maturity, and indicators for collateralized loans, rated borrowers, covenant-lite loans, and borrower characteristics as revenue and profit. t-statistics clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at 10%, 5%, and 1% level, respectively.

	New Lender			
	(1)	(2)	(3)	(4)
NonCovlite Offer	0.637	0.541	0.669	1.712
	(0.907)	(0.960)	(1.057)	(1.248)
Bank Borrower	0.028	0.016	0.048	0.122
	(0.085)	(0.086)	(0.092)	(0.143)
Borrower FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No
Lender FE	No	No	Yes	No
Lender $\times$ Time FE	No	No	No	Yes
Purpose FE	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes
Obs	4460	4460	4460	4460
First stage Kleibergen-Paap F	20.88	16.97	14.46	11.46

Table C.13

Probability to Switch Lenders After the Clarification: Different Lender Types

The table presents difference-in-differences estimates of how the Clarification (2014Q4) affected the chance of switching between lenders. Column 1 analyzes switching to any new lender Any, Column 2 - to lender with covlite loan (C), Column 3 - to nonbank (nB), Column 4 - to nonbank with covlite loan (CnB), Column 5 - to bank with covlite loan (nCB). All variables are defined in Appendix Table C.1. The sample contains all loans to non-financial borrowers in the years 2012 to 2018. Commercial US banks are the treated group, non-bank financial institutions are the control group. Controls include loan size, interest rate, maturity, and indicators for collateralized loans, rated borrowers, covenant-lite loans, and borrower characteristics as revenue and profit. t-statistics clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at 10%, 5%, and 1% level, respectively.

	New Lender				
	(1)	(2)	(3)	(4)	(5)
	Any	C	nB	CnB	nCB
Post × Bank Borrower	0.350***	0.224***	0.180***	0.163***	0.109
	(0.113)	(0.080)	(0.059)	(0.037)	(0.806)
Bank Borrower	-0.265***	-0.061	0.166***	-0.008	-0.378***
	(0.080)	(0.042)	(0.046)	(0.011)	(0.061)
Borrower FE	Yes	Yes	Yes	Yes	Yes
Lender × Time FE	Yes	Yes	Yes	Yes	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Obs	4460	4460	4460	4460	4460
R2	0.65	0.47	0.87	0.68	0.68

Table C.14
Second Stage: Different Lender Types

The table presents second-stage 2SLS estimates of $NewLender_{b,l,t}$, an indicator equal to one if the borrower never borrowed from the same lender l since the beginning of our sample, on the instrumented $\widehat{NonCovLiteOffer}_{b,l,t}$ from the first stage. Column 1 analyzes switching to any new lender Any, Column 2 - to lender with covlite loan (C), Column 3 - to nonbank (nB), Column 4 - to nonbank with covlite loan (CnB), Column 5 - to bank with non-covlite loan (nCB). All variables are defined in Appendix Table C.1. The sample contains all loans to nonfinancial borrowers in the years 2012 to 2018. Commercial US banks are the treated group, nonbank financial institutions are the control group. Controls include loan size, interest rate, maturity, and indicators for collateralized loans, rated borrowers, covenant-lite loans, and borrower characteristics as revenue and profit. t-statistics clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at 10%, 5%, and 1% level, respectively.

	New Lender				
	(1)	(2)	(3)	(4)	(5)
	Any	C	nB	CnB	nCB
NonCovlite Offer	1.825***	1.658***	0.939**	0.998***	0.566
	(0.625)	(0.488)	(0.377)	(0.305)	(0.408)
Bank Borrower	0.131	0.160*	0.370***	0.254***	-0.255***
	(0.095)	(0.082)	(0.058)	(0.054)	(0.059)
Borrower FE	Yes	Yes	Yes	Yes	Yes
Lender $\times$ Time FE	Yes	Yes	Yes	Yes	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Obs	4460	4460	4460	4460	4460
Kleibergen-Paap rk Wald F	37.45	37.45	37.45	37.45	37.45

Table C.15
Top-15 Lead Agents in Covenant-lite Loan Market
The table presents league tables for Top-15 Lead Agents in Covenant-lite Loan Market before (Panel A) and after (Panel B) the Clarification (2014Q4). The indicators are calculated for unsecured covenant-lite lending segment.

Panel A. Before the Clarification (2012q4-2014q3) | Panel B. After the Clarification (2014q4-2016q3)

#	Lead Agent	N	%	mln	#	Lead Agent	N	%	mln
1	Credit Suisse	191	18.82	109.9	1	Credit Suisse	108	15.77	68.3
2	Bank of America	168	16.55	113.5	2	Bank of America	83	12.12	54.1
3	JP Morgan Chase	113	11.13	101.4	3	JP Morgan Chase	82	11.97	81.9
4	Deutsche Bank	102	10.05	71.6	4	Goldman Sachs	56	8.18	29.4
5	Morgan Stanley	79	7.78	37.8	5	Morgan Stanley	54	7.88	38.4
6	Barclays Bank	68	6.7	42.5	6	Deutsche Bank	51	7.45	34.7
7	Goldman Sachs	60	5.91	31.7	7	**Jefferies Finance**	45	6.57	18.2
8	Citigroup	59	5.81	44.2	8	Citigroup	43	6.28	45.7
9	UBS AG	44	4.33	17.5	9	Barclays Bank	39	5.69	30.6
10	**Jefferies Finance**	43	4.24	15.8	10	UBS AG	32	4.67	9.1
11	Royal Bank of Canada	40	3.94	9.6	11	Royal Bank of Canada	24	3.5	8.3
12	Wells Fargo	16	1.58	7.1	12	**General Electric Capital Corp**	15	2.19	4.5
13	**General Electric Capital Corp.**	8	0.79	2.7	13	Wells Fargo	11	1.61	4.0
14	SunTrust Bank	7	0.69	1.6	14	**Antares Leveraged Capital Corp**	9	1.31	1.9
15	BNP Paribas Group	3	0.3	0.9	15	**Macquarie Capital**	6	0.88	1.1

Table C.16
Placebo: Probability to Switch Lenders Before the Clarification
The table presents difference-in-differences estimates of how the Clarification (2014Q4) affected the chance of switching between lenders. All variables are defined in Appendix Table C.1. The sample contains all loans to non-financial borrowers in the years 2010 to 2014. Commercial US banks are the treated group, non-bank financial institutions are the control group. Controls include loan size, interest rate, maturity, and indicators for collateralized loans, rated borrowers, covenant-lite loans, and borrower characteristics as revenue and profit. t-statistics clustered by borrower and robust to heteroskedasticity are in parentheses. *, ** and *** indicate statistical significance at 10%, 5%, and 1% level, respectively.

	New Lender			
	(1)	(2)	(3)	(4)
Bank Borrower × 2010q4	0.058			
	(0.362)			
Bank Borrower × 2011q4		0.220		
		(0.168)		
Bank Borrower × 2012q4			0.011	
			(0.170)	
Bank Borrower × 2013q4				-0.126
				(0.211)
Bank Borrower	-0.314	-0.434***	-0.265*	-0.237**
	(0.354)	(0.148)	(0.137)	(0.113)
Borrower FE	Yes	Yes	Yes	Yes
Lender × Time FE	Yes	Yes	Yes	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Obs	2402	2402	2402	2402
R2	0.70	0.70	0.70	0.70