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Disciplining delegated monitors: When venture capitalists fail to prevent fraud by their IPO firms ☆

Xuan Tian [a,b], Gregory F. Udell [a], Xiaoyun Yu [a,*]

[a]Department of Finance, Kelley School of Business, Indiana University, USA
[a]PBC School of Finance, Tsinghua University, China

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A B S T R A C T

Information-based theories of financial intermediation focus on delegated monitoring. However, there is little evidence on how markets discipline intermediaries who fail at this function. We exploit the direct link between corporate fraud and monitoring failure and examine how a venture capital (VC) firm's reputation is affected when it fails to prevent fraud in its portfolio companies. We find that reputation-damaged VCs interact differently in the future with their limited partners, other VCs, and IPO underwriters because they are perceived as ineffective monitors. In addition, VCs that fail to prevent fraud experience greater difficulty in taking future portfolio companies public.

1. Introduction

Modern information-based theories of financial intermediation emphasize information production and delegated monitoring as the raison d'être for financial intermediaries (e.g., Diamond, 1984; Boyd and Prescott, 1986). Substantially unanswered in the literature, however, is how markets discipline financial intermediaries that fail as delegated monitors.

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* Corresponding author.
E-mail addresses: tianx@indiana.edu, tianx@pbcsf.tsinghua.edu.cn (X. Tian), gudell@indiana.edu (G.F. Udell), xiyu@indiana.edu (X. Yu).

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Addressing this question is challenging because it requires a research design that causally links monitoring failure to market discipline. We surmount this challenge by examining an event that unambiguously identifies a breakdown in monitoring in a market where the consequences to a financial intermediary are measurable on multiple dimensions; the event is initial public offering (IPO) fraud, and the market is venture capital (VC). Specifically, we explore the future impact on VC firms when the portfolio companies they took public are revealed to have committed accounting fraud before or around the IPO, thus exposing their VCs as ineffective monitors.

To evaluate the economic consequences faced by these VCs, we use a sample of VC-backed companies that went public from 1995 to 2005 and were detected to have committed accounting-related fraud. We measure detected fraud using lawsuits alleging financial misreporting that occurred before or within two years after the IPO. We find evidence that the market does indeed discipline VCs for failing to prevent IPO fraud, as reputation-damaged VCs appear to suffer in future interactions with limited partners (LPs), other VCs, and IPO underwriters. First, reputation-damaged VCs experience a significant decline in subsequent fundraising from LPs and are forced into investing in fewer industries and companies that are geographically closer. Furthermore, they are more likely in the future to syndicate with VCs with weaker reputations, join smaller syndicates, and work with lower ranked underwriters. Second, reputation-damaged VCs are less likely to harvest subsequent portfolio investments through the most attractive exit—the IPO. Instead, they tend to exit via a less attractive means—mergers and acquisitions (M&As).

Lastly, we find the severity of market discipline depends on the extent of the interaction between a VC and its fraudulent portfolio companies. Reputational damage due to monitoring failure is much more pronounced among VCs that have the most extensive involvement with fraudulent companies, that is, those that lead the financing syndicate and invest in the portfolio companies at an early stage. By contrast, the reputational damage is mostly negligible if the VC exits from the company long before the fraud. Similarly, monitoring failure causes more significant damage to VCs with a higher reputation. In addition, the market appears to punish VCs less when general business conditions are more conducive to fraud, that is, during periods that can be considered fraud waves.

Our research setting offers several important advantages. First, the essence of monitoring is to mitigate agency problems between insiders and outsiders. Insiders have an incentive to misrepresent their quality and conduct. Fraud is, in effect, a strong form of misrepresentation. While we cannot observe a financial intermediary's monitoring effort directly, we can see the outcomes of some of the most egregious monitoring failures in the VC market: portfolio companies becoming defendants in securities fraud lawsuits after going public. Unlike in other settings, the VC is almost solely responsible for monitoring the company prior to its IPO. Therefore, by analyzing accounting-related fraud under these circumstances, our paper contributes to the literature on financial intermediation by exploring the extent to which failure in a core activity—monitoring—affects reputation. Second, the VC market offers a rich opportunity to examine a wide variety of channels through which the market can discipline a financial intermediary that fails to monitor. Moreover, it allows us to measure the intensity of monitoring effort by looking at the stage at which the VC gets involved with its portfolio companies.

Equally important, information production and discipline matter enormously in the intermediated market because this market provides funding to firms that are the most opaque–small and mid-sized enterprises (SMEs). Unfortunately, the market is challenging to analyze because of a paucity of panel data linking monitors to firms receiving funding. However, the U.S. VC market is a striking exception: it has data that is ideal for our purpose because VC firms very much act as delegated monitors and the start-up companies they invest in are highly opaque due to their small size and lack of tangible assets. Thus the monitoring intensity in the VC market is likely greater than in any other intermediated market.[1] In addition, the VC market is substantially unregulated and thus not "polluted" (as an economic experiment) by government regulation and government guarantees.

Our paper nests in a broad literature that has analyzed disciplining performance in a variety of settings ranging from investment banking to the managerial labor market. The methods vary with the markets. They include punishing less reputable investment bankers with lower spreads and valuations in future IPOs (Fernando et al., 2012) and lower fees and higher yields in future bond underwritings (Fang, 2005) and punishing poorly performing CEOs with lower pay (Garvey and Milbourn, 2006) and termination (Jenter and Lewellen, 2014). One strand of this literature has focused on discipline in an industry where participants clearly act as delegated monitors—commercial banking. Studies find that banks experience a decline in market value and difficulty in syndicating future loans when their borrowers suffer financial distress (Dahiya et al., 2003; Gopalan et al., 2011) or turn out to be fraudulent (Lin and Paravisini, 2010).

We depart from this literature by identifying a specific event that is directly linked to monitoring—and not confounded by other functions performed by financial intermediaries. In addition, we examine an industry setting (i.e., the VC market) which is free from the confounding effects of intense government regulation and government guarantees. By studying how failed VC monitors interact with other financial intermediaries in subsequent deals, the VC market allows us to explore a much broader range of effects beyond just access to future funding. Finally, monitoring in the VC market is an especially

[1] Research on VC monitoring intensity indicates that VCs visit portfolio companies nearly 20 times per year (Gorman and Sahlman, 1989) and, by the last round of financing, have on average more board members than insiders (Lerner, 1995). While comparable statistics on loan officer visits do not exist, loan officer turnover of nearly two times every three years suggest significantly less intense monitoring (Scott, 2006; Uchida et al., 2012). Kroszner and Strahan (2001) find that only about 30% of large firms have any bankers on their board and the probability of banker on the board is lower for smaller and more opaque (i.e., fewer collateralizable assets) firms.

attractive laboratory because of its high monitoring intensity—particularly relative to the intensity in the commercial banking market.

Our paper also relates to the empirical literature on financial misreporting and corporate fraud. (See Yu (2013) for a survey.) In particular, researchers document that VCs discourage IPO firms' earnings management and restatements (Morsfield and Tan, 2006; Agrawal and Cooper, 2010; Lee and Masulis, 2011). Unlike these studies, which use corporate fraud as a mechanism to study IPOs per se, we focus on how monitoring failure affects a VC's subsequent interactions with the capital markets and other financial intermediaries.

A large literature on VC investments and entrepreneurial value creation also relates to our work. (See Da Rin et al. (2013) for a survey.) Instead of examining how VCs' investments affect the performance of their portfolio companies, we concentrate on the economic consequences of the VC's failure to monitor. In this respect, our paper relates to the findings of Atanasov et al. (2012), who also examine reputational effects in the VC industry. However, they explore the role of reputation in mitigating the confl na524on intini 91.la-3486.52VCl.e monitoompa524o1 mis-345.522ry922(t)-3525t,con-lio83951.9545 of future difficulty in fundraising, syndicating with other VCs, or working closely with highly reputable underwriters causes its failure to prevent the IPO fraud.[3]

2.1.2. Proxy for monitoring failure

The discovery of a securities fraud generally leads to a securities lawsuit. There are two types of securities lawsuits: SEC lawsuits and private securities class-action lawsuits. Many papers have used lawsuits to proxy for the presence of corporate financial fraud. (See Karpoff et al. (2013) for a survey.) We therefore follow the literature and measure detected fraud with securities lawsuits alleging accounting-related fraud during the IPO process and up to two years after the IPO. Our main proxy for a VC's monitoring failure is "IPO Fraud," a dummy variable equal to one in the years after the discovery of alleged IPO fraud committed by a VC's portfolio companies and zero in the years before the discovery.

2.2. Evaluating the economic consequences of a VC's monitoring failure

2.2.1. The impact from other financial intermediaries

We analyze the economic consequences after a fraud has surfaced (i.e., after t_3) faced by VCs that were involved with a portfolio company when the alleged fraud occurred. Our first set of measures explores how other financial intermediaries and institutional investors interact in subsequent deals with a reputation-damaged VC after the discovery of fraud. We first examine the interaction between a VC and its LPs in future fundraising. We postulate that, after the revelation of a fraud and the associated VC's failure to monitor before and during the IPO stage, VC firms face greater pressure from their LPs, which demand more vigilance.

We adopt both direct and indirect measures to capture this pressure. Our direct measure is the amount of capital raised from LPs ("VC Fundraising"). For each VC firm in a given year t, we calculate the natural logarithm of the total amount of funds it raised in each of the years $t+1$ through $t+3$. We posit, that after the discovery of an IPO fraud committed by one of its previously financed portfolio companies, the VC experiences a significant decline in subsequent fundraising from LPs.

Our indirect measures capture the conservativeness of a VC's investments proxied by industry concentration and geographic concentration. We measure a VC firm's industry concentration with its investment Herfindahl index. Specifically, for each VC firm in year t, its Herfindahl index equals the sum of the squares of the percentage of all investments (in terms of the number of its portfolio firms) in each of the 18 industries classified in the Venture Economics database in each of the years $t+1$ through $t+3$.[4] We hypothesize that LPs force reputation-damaged VCs to undertake a more conservative investment strategy by concentrating their investments in fewer industries to promote better monitoring.

We also capture the conservativeness of a VC's investment strategy using the locality of its subsequent portfolio companies. For each VC in each year t, "Ln(Distance)" is the natural logarithm of the weighted-average physical distance between the VC firm and its portfolio companies (weighted by the total amount of its investment in these companies) in each of the years $t+1$ through $t+3$, calculated using the great circle distance formula as in the existing literature (e.g., Tian, 2011). Alternatively, we compute the "% of Local Investment" as the dollar percentage of local investment within a VC's portfolio in each of the years $t+1$ through $t+3$. We define local investment as the investment in portfolio companies that are located within a 50-mile radius of the VC firm. Since the previous literature has documented that VC firms that invest locally have lower monitoring costs and better investment performance (e.g., Lerner, 1995; Tian, 2011), we conjecture that, in response to the LPs' demand for more vigilant monitoring, VC firms whose reputation is damaged focus more of their subsequent investments on local ventures.[5]

Next, we explore how a VC interacts with other VCs in forming syndicates after the discovery of the fraud. VCs tend to syndicate their investments rather than investing alone (Lerner, 1994; Tian, 2012). We use syndicate size to capture the scale of the investment. For each VC-portfolio company pair, the syndicate size of each portfolio company that received VC financing during the years $t+1$through $t+3$ (following the VC's funding in year t) is the number of VCs in the syndicate across all financing rounds. "Ln(# of VCs)" is then calculated as the natural logarithm of the syndicate size. We postulate that, for subsequent investment opportunities, VCs that are perceived as ineffective monitors by other VCs are likely to join smaller syndicates and syndicates with VCs that are less reputable than they are.

We measure the quality of other VCs within a syndicate by the weighted average of their reputation scores. To compute each VC firm's reputation score in a given year t based on an extended window, we follow Nahata (2008) and Bhattacharya et al. (2015) and divide the total proceeds of IPOs financed by the VC since 1980 by the total proceeds of firms that went public between 1980 and year t. Using 1980 instead of the beginning year of our sample alleviates a potential forward-looking bias. Alternatively, following Krishnan et al. (2011), we compute a VC's reputation score for a given year t based on a three-year rolling window as the VC's fraction of total IPO proceeds during years $t-3$ through $t-1$. For each portfolio company that a VC invests in during years $t+1$ through $t+3$, we then construct "High VC Reputation (Extended)" ("High VC

[3] We will use the term "prevent" to capture both detection and prevention. These two are, of course, closely related in that a VC's observable effort to detect potential fraud will deter the entrepreneur from committing it.

[4] Results are similar (not reported) if we calculate Herfindahl index based on the funding amount in an industry.

[5] Different from Tian (2011), who shows that VC firms do invest in distant ventures, we compare the locality of the portfolio companies backed by the same VC before and after the discovery of the fraud, instead of comparing the locality across different VCs.

Reputation (Rolling)") as a dummy variable equal to one if the weighted average of reputation scores (based on the extended window (rolling window)) of other VCs within the same syndicate is higher than the reputation score of the VC itself.

To illustrate, suppose that VC i has invested in company X in 1999 and has participated in syndicates funding three companies from 2000 through 2002 (i.e., during the three years after investing in company X). For each of these three companies, its "High VC Reputation (Rolling)" is set to one if the weighted average of the reputation scores of other VCs in its VC syndicate is higher than the VC i's own reputation score and zero otherwise.

Finally, VCs also interact with underwriters (investment banks) that take their portfolio companies public and that have an incentive to screen out fraud to prevent legal liability and reputational loss (Sherman, 1999). This implies that an underwriter's concern about its reputation not only discourages IPO fraud through its intensive screening and monitoring but also leads to a reluctance to work with VCs who are ineffective monitors. For each VC-IPO firm pair, we compute a VC's "IB Reputation," which is the average reputation score of the underwriting syndicate of an IPO firm backed by the same VC that goes public within three years (i.e., during years $t+1$ through $t+3$) following the current IPO. To directly capture whether VC reputational damage affects the pool of underwriters available to the VC for subsequent deals, we also compute "High IB Reputation," a dummy variable equal to one if the "IB Reputation" is higher than the average reputation score of the underwriting syndicate for the previous IPO backed by the same VC and zero otherwise.[6]

To illustrate, suppose that a VC has taken firm Y public in 1999 and has taken four firms public within three years after firm Y went public (i.e., 2000 through 2002). For each of the four IPOs, its "IB Reputation" is the average reputation score of the underwriting syndicate. Its corresponding "High IB Reputation" dummy is set to one if its "IB Reputation" is higher than the average reputation score of the underwriting syndicate of firm Y's IPO and zero otherwise.[7]

We postulate that VCs perceived as inefficient monitors are less likely to hire more reputable banks to underwrite their subsequent IPOs and instead are forced to work with less reputable underwriters.

2.2.2. Probability of future successful exit

Our second set of measures, the probability of a successful exit by the VC, captures how equity market investors perceive the quality of subsequent portfolio companies brought to market by a VC that suffers reputation damage. There are generally three ways a VC can exit its investment: an IPO, the sale of a portfolio company to a third party (M&A), and a write-off. The most profitable exit is the IPO. It generates most of a VC's returns (Sahlman, 1990); it generates a 22% "valuation premium" over M&As (Brau et al., 2003) and reflects the best quality firms in VC portfolios (Bayar and Chemmanur, 2011). Although M&As tend to generate smaller returns for VCs than IPOs, they are still profitable and widely considered to be successful exits (Hochberg et al., 2007). Obviously, the write-off is the worst. In a write-off, a VC liquidates its portfolio company and absorbs a loss.

We define "IPO Exit" as a dummy that equals one if in a given year a VC-backed portfolio company goes public and zero if it is acquired or written off. Since VCs can exit their investments via more than one venue, a VC that suffers reputational damage could compensate for its diminished exit opportunity through the IPO venue by pursuing the second best (but still generally quite profitable) alternative of an M&A and thus still achieve a high overall gain on its investments. To account for this possibility, we define "Successful Exit" as a dummy variable equal to one if a VC exits its investment via either an IPO or an M&A and zero if the investment is written off.

Our third proxy explicitly explores the potential substitution effect between an IPO exit and an M&A exit. In an analysis that is limited to just these two types of successful exits, we define "IPO vs. M&A" as a dummy variable equal to one if the exit is via an IPO and zero if via an M&A.

3. Data and sample construction

3.1. Sample construction

3.1.1. The IPO fraud sample

We extract IPO issues from the Thomson Securities Data Company (SDC) database. After excluding unit offers, rights offers, closed-end mutual funds, real estate investment trusts (REITs), American Depositary Receipts (ADRs), and partnerships, our search of the SDC database yielded 1,391 VC-backed IPO issues between January 1995 and December 2005.

We focus on accounting-related frauds by IPO firms. Following the literature (Karpoff et al., 2013), our proxy for detected IPO fraud is the filing of a securities lawsuit based on financial misreporting against an IPO firm. Since it takes roughly two years on average to discover fraud during this period (Wang, 2011), we extract a sample of fraudulent firms from the SEC's Accounting and Auditing Enforcement Releases (AAERs) and the Stanford Law School's Securities Class Action Clearinghouse (SCAC) filed between 1996 and 2007, with the fraud being committed between 1995 and 2005.

[6] We obtain underwriter reputation scores from Jay Ritter's website: http://bear.cba.ufl.edu/ritter/ipodata.htm.

[7] In our sample, 21% of VCs with fraudulent IPOs have at least one IPO within the next three years, and 79% of VCs without fraudulent IPOs have at least one IPO within the next three years.

In using lawsuits as the proxy for detected frauds, it is important to control for false detection due to frivolous lawsuits. This problem may be more severe for private class-action lawsuits in the SCAC data than for the AAERs because private securities lawsuits are more profit oriented. To control for frivolous lawsuits, we follow the literature (e.g., Wang et al., 2010) and apply the following filtering criteria: we first restrict our sample to the period after the passage of the Private Securities Litigation Reform Act of 1995, which was designed to reduce frivolous lawsuits (Johnson et al., 2000; Choi, 2007). We then follow Dyck et al. (2010) and exclude all cases where the judicial review leads to dismissal. Third, for class actions that have settled, we exclude firms where the settlement is less than $2 million, a threshold level used in previous studies to distinguish frivolous from meritorious lawsuits (Grundfest, 1995; Choi et al., 2009).

Finally, to match the litigation nature of the SEC's AAERs, we identify the nature of the class-action allegations based on the documents associated with each lawsuit (i.e., case complaints, press releases, defendants' motions to dismiss, and court decisions) to single out cases involving allegations of accounting irregularities. Our litigation sample thus contains 423 SEC AAERs and 1,085 private class-action lawsuits, among which 212 suits were subject to both SEC enforcement and private class-action litigation.

We then merge the litigation sample with our VC-backed IPO sample. We check the timing of the alleged frauds based on the information in the litigation documents and identify 205 frauds that occurred before the IPO or within two years after the IPO. We label these cases as IPO frauds. Among the 205 fraudulent VC-backed IPOs, 34 are from AAERs, 197 are from the SCAC database, and 26 suits are subject to both SEC enforcement and private class-action litigation (and hence were in both the AAER and SCAC databases).

3.1.2. The VC sample

For each IPO fraud that occurs between 1995 and 2005 and is detected between 1996 and 2007, we examine the economic consequences for the associated VCs over the three-year window after the discovery of fraud. We extract data on portfolio companies and VC investments in these firms from the Thomson Venture Economics database during 1995–2008. We exclude financial firms and those with missing or inconsistent data.

The Venture Economics database provides detailed information on the portfolio company's development stage at the first VC financing round (i.e., startup/seed, early, expansion, late, or buyout/acquisition stage), its industry classification, its date of establishment, the identity of its VC firms, the date of each financing round, and the date and type of the eventual outcome for each portfolio company (IPO, M&A, or write-off). However, the database does not identify all companies that are eventually written off. Therefore, based on the fact that industry practices require that VCs liquidate their investments within 10 years from fund inception in most cases, we also classify a portfolio company as a write-off if it did not receive any financing within the 10 years after its last financing round.[8] Companies that are not classified into one of the three exit categories are classified as still under active investment (i.e., still in their incubation stage) and therefore are excluded from our sample. Venture Economics also provides detailed information about the characteristics of VC firms. Our final sample consists of 11,500 unique portfolio companies backed by VCs and 1,770 unique VC firms from 1995 to 2008.

3.2. Descriptive statistics

Table 1 Panel A reports the calendar-year distribution of the sample of VC-backed fraudulent and non-fraudulent IPO firms. Panel A shows that IPO timing follows the booms and busts of the stock market and that this pattern is likewise reflected in both fraudulent and nonfraudulent IPOs. During the peak of the tech boom in 1999, there are 47 fraudulent and 221 nonfraudulent firms that went public. By contrast, only four fraudulent and 17 nonfraudulent firms went public in 2002 after the bust of the internet bubble.

Table 1 Panel B reports summary statistics for our sample VC firms and their portfolio companies. Among the 1,770 unique VCs that have taken at least one of their portfolio companies public during our sample period, 196 (11.1%) of them have funded a fraudulent IPO firm. A typical VC firm has an industry Herfindahl index of 0.55 and invests in portfolio companies that are on average 214 miles away. An average VC firm is 13.6 years old with $2.9 billion capital under management.

Among the 11,500 unique sample portfolio companies that have received VC financing, 9.6% of them do an IPO during our sample period; 69.1% of them (7,944 portfolio companies) engage in either an IPO or M&A. Among the 7,944 portfolio companies that exit successfully, 13.9% exit through an IPO instead of M&A. 53.6% of our sample firms receive VC financing during the early stages of their life cycle (seed stage, early stage, and expansion stage). The average size of a VC syndicate for a sample portfolio company is three VCs.

Panel C of Table 1 reports the descriptive statistics for the 205 VC-backed fraudulent IPO firms and the 1,186 VC-backed nonfraudulent IPO firms. An average fraudulent firm receives funding from seven VCs, and VC funding begins four years after the firm's founding. More interestingly, 66% of these fraudulent firms receive VC financing at their seed or early stage, in contrast to portfolio companies in general (Panel B) of which only 25.6% receive VC funding at their seed or early stage.

[8] We update and fill in the missing observations for the date that the portfolio firm was established. We use Jay Ritter's database (http://bear.cba.ufl.edu/ritter/ipodata.htm) for the subset of firms that went public and Factiva, LexisNexis, D&B, and CorpTech databases for firms remaining private. An alternative cut-off to classify write-off firms is whether the portfolio firm did not receive any follow-on financing within five years after its very last financing rounds. Results (not reported) are robust to this alternative write-off classification.

Table 1

This table reports the summary statistics of our sample. Panel A reports the calendar-year distribution of the sample of fraudulent and nonfraudulent IPO firms. Panel B reports the summary statistics for VC firms and portfolio companies that they backed from 1995 to 2008. Data about portfolio companies and VC investors are obtained from the Venture Economics database. Variables are defined in the text. Statistics for industry concentration, physical distance between VCs and portfolio companies, VC age, capital under management, and VC reputation scores are based on VC-year observations. Panel C reports the summary statistics for VC-backed IPO firms that are alleged to commit fraud between 1995 and 2005 and VC-backed nonfraudulent IPO firms. Panel D reports the univariate comparisons between VCs that have funded fraudulent IPO firms during the sample period and those that have not. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Calendar-year distribution of fraudulent and nonfraudulent IPOs

IPO year	Fraudulent VC-backed IPOs	Nonfraudulent VC-backed IPOs
1995	20	166
1996	37	217
1997	17	119
1998	15	67
1999	47	221
2000	33	208
2001	4	33
2002	4	17
2003	9	21
2004	12	75
2005	7	42
Total	205	1,186

Panel B: Characteristics of VCs and their portfolio companies

	Mean	Std. dev.	# of obs.
Characteristics of VC firms			
% of VCs that have funded fraudulent IPO firms	11.07		1,770
% of VCs that have funded post-exit fraudulent firms	8.70		1,770
Industry concentration	0.55	0.30	9,971
Distance between VCs and entrepreneurial firms	214.30	198.32	8,392
VC age	13.56	13.38	9,323
Capital under management ($ billion)	2.92	19.80	9,973
VC reputation score (%)			
Based on three-year rolling window	0.10	0.27	5,756
Based on extended window	0.10	0.23	5,756
Characteristics of VC's portfolio companies			
% of companies going public	9.61		11,500
% of companies going public or being acquired	69.08		11,500
% of companies going public instead of being acquired	13.91		7,944
% of companies receives 1st VC investment at its			
Seed stage	9.02		11,500
Early stage	16.61		11,500
Expansion stage	27.98		11,500
Late stage	2.89		11,500
Buyout stage	6.94		11,500
Other stage	37TJT60o940(f)-338.89ag-335(o)0(f)-338.6(companies)-3308mmary stat-13176ent		

Table 1 (*continued*)

Panel C: Characteristics of VC-backed IPOs

	Fraudulent IPOs			Nonfraudulent IPOs		
	Mean	Std. dev.	# of obs.	Mean	Std. dev.	# of obs.
Buyout stage	6.00		205	6.58		1,186
Other stage	1.33		205	3.88		1,186

Panel D: Univariate comparisons

	VCs that have backed fraudulent IPOs	VCs that have not backed fraudulent IPOs	Difference
VC age	7.21	6.05	1.16
Capital under management ($ billion)	1.625	0.953	0.672
VC's # of portfolio companies	84.08	31.74	52.34***
VC's # of rounds	167.35	58.30	109.05***
Amount VC has invested ($ million)	884.30	337.77	546.54***
VC reputation score (%)			
Based on three-year rolling window	0.12	0.02	0.10***
Based on extended window	0.13	0.02	0.10***
% of California VCs	32.65	20.08	12.58***
% of Massachusetts VCs	17.29	7.24	7.04***
% of New York VCs	15.06	15.88	−0.06
# of obs.	196	1,574	

These findings suggest that a VC's failure to prevent IPO fraud is more likely driven by its ineffective monitoring, as VCs are involved with these firms at the very beginning stage of their life cycles. Lastly, the characteristics of VC-backed nonfraudulent IPOs generally resemble those of VC-backed fraudulent IPOs. This finding suggests that our main results, documented below, are unlikely driven by differences in characteristics between fraudulent and nonfraudulent IPOs other than different VC monitoring efforts.

Panel D compares the characteristics of VCs that have backed a fraudulent IPO firm to those that have not. VCs that have funded fraudulent IPOs tend to have invested in a significantly larger number of entrepreneurial companies, participated in a significantly large number of financing rounds, and garnered a considerably higher reputation (score). These VCs are also older and manage larger amounts of capital, though the difference is not statistically significant. The failure to prevent IPO fraud by a VC's portfolio companies thus is unlikely driven by the lack of the VC's experience.

3.3. Determinants of IPO fraud in VC-backed firms

What kinds of VCs are more likely to fail to prevent fraud in their portfolio companies before or during the IPO stage? To answer this question we need to control for, in our main regressions, the effect of nonfraudulent reputational concerns from those related to financial misreporting. In this subsection, we investigate the determinants of fraud committed during the IPO stage by VC-backed firms to identify these nonfraudulent drivers.

First and most importantly, we include proxies for VC reputation in this set of analyses. The literature documents that more reputable VCs are linked to less earnings management, better corporate governance, and better post-IPO long-term portfolio firm performance (e.g., Morsfield and Tan, 2006; Lee and Masulis, 2011; Krishnan et al., 2011). In addition, more reputable VCs are less likely to sell overpriced shares in an IPO (Lin and Smith, 1998) and to behave opportunistically (Atanasov et al., 2012).

In particular, we note that the incidences of financial misreporting by a VC-backed company before or during its IPO stage clearly indicate its VC's monitoring failure. Different from other types of fraud, a firm's incentive for financial misreporting during the IPO stage is driven by the desire to raise capital through the IPO (Wang et al., 2010). Since the Krishnan et al. (2011) proxy for VC reputation is based on a three-year rolling window, it better captures (dynamically) the timeframe during which monitoring failure happens in our setting. We thus adopt their measure as the key proxy for VC reputation.

Because fraud associated with a VC-backed IPO can stem from VC opportunism, we further include in our analyses a variety of proxies for VC quality and experience, following Atanasov et al. (2012): the age of a VC firm, computed as the number of years since its founding year; the cumulative amount of funds under management by a VC since 1980; the VC network-degree measure, defined as by Hochberg et al. (2007); the fraction of VC portfolio companies that go public; and the number of financing deals that a VC invests in.

Next, IPO firms may differ in their quality and certain firms may be inherently more prone to fraud. We therefore control for IPO offering characteristics and IPO firm quality. For example, it has been shown that the probability of restatement by an

Table 2
This table reports the probit regression results for the determinants of IPO fraud. The unit of observation is at the IPO firm level. The dependent variable is a dummy variable equal to one if an IPO firm turns out to be fraudulent and zero otherwise. The independent variables are described in the text and tabulated in the appendix. Data about portfolio companies and VC investors are obtained from the Venture Economics database. A portfolio company's industry classification is based on the Venture Economics 18-industry classifications. Standard errors clustered at the IPO firm level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VC Reputation	−0.390**						−0.694***
	(0.201)						(0.173)
VC age		−0.001***					−0.001***
		(0.000)					(0.000)
Ln(Funds under management)			−0.001				−0.001
			(0.003)				(0.003)
Pct deals going public				−0.123**			−0.122**
				(0.062)			(0.049)
Network degree					−0.000		−0.002
					(0.003)		(0.003)
Number of deals						−0.002	0.009
						(0.008)	(0.010)
Ln(Offer size)	0.024***	0.024***	0.025***	0.026***	0.025***	0.025***	0.022***
	(0.009)	(0.008)	(0.009)	(0.008)	(0.009)	(0.009)	(0.008)
IPO underpricing	0.004	0.004	0.004	0.003	0.004	0.004	0.003
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)
Underwriter rank	0.004	0.004	0.004	0.004	0.004	0.004	0.003
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)
Age at IPO year	−0.000	0.000	0.000	0.000	0.000	0.000	−0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Price revision	−0.007	−0.004	−0.008	−0.006	−0.008	−0.008	−0.001
	(0.026)	(0.026)	(0.026)	(0.025)	(0.027)	(0.027)	(0.023)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# of obs.	857	857	857	857	857	857	857
Pseudo R^2	0.065	0.066	0.061	0.066	0.061	0.061	0.081

IPO firm relates positively to underwriter reputation and negatively to VC backing, VC reputation, and VC maturity (Agrawal and Cooper, 2010). The literature also finds a stronger reduction in earnings management by an IPO issuer when more reputable VCs are matched with more reputable underwriters (Lee and Masulis, 2011). To avoid falsely attributing the underwriter effect to VC reputation, we control for underwriter reputation (measured as the weighted-average reputation score for the underwriting syndicate of the VC-backed portfolio company that goes public) in the regressions.

Older age at the time of the IPO is among the common proxies in the literature to capture more established and less risky IPO issuers (Ritter, 1984; Barry et al., 1990). The extent of an issuing firm's offer price revision and initial day return (underpricing) reflects the degree of information asymmetry when the market assesses its intrinsic quality (Hanley, 1993; Benveniste et al., 2003). Some researchers argue that a larger offer size reflects greater uncertainty about selling shares to the public and is thus associated with greater underpricing (Barry et al., 1990; Lin and Smith, 1998). Therefore we include in our analyses the issuing firm's age at the time of the IPO, underpricing (computed as the difference between the first trading day's closing price and the final offer price, scaled by the final offer price), offer size (measured as the natural logarithm of total IPO proceeds), and price revision (calculated as the final offer price divided by the midpoint of the initial filing range, minus one).

Lastly, fraud incentive tends to vary over time and depends on industry characteristics (Wang et al., 2010). Therefore we control for year and industry fixed effects to account for time-specific and industry-specific unobserved characteristics affecting a firm's incentive to commit fraud before or during the time of going public.

Table 2 reports the results from our probit regressions.[9] The unit of observation is the IPO firm. The dependent variable is an indicator variable equal to one if an IPO firm turns out to be fraudulent and zero otherwise. In columns (1) through (6), we include six VC reputation variables individually. In column (7), we include all of them together in the regression. The Krishnan et al. (2011) VC reputation proxy always relates negatively and significantly to the incidence of IPO fraud. In addition, the signs of the coefficient estimates for all remaining five VC quality proxies are consistent with prior studies, and two of them are statistically significant. Our evidence thus indicates that more reputable VCs are associated with a lower propensity of IPO fraud by their portfolio companies. This is consistent with the findings of previous studies that more reputable VCs are linked to less earnings management by IPO firms, better corporate governance, and less opportunistic behavior.

[9] Greene (2002), among others, cautions against including fixed-effects in nonlinear models such as probit regressions due to the incidental-parameters problem and biases in the fixed effects estimator. Greene (2002) further points out that ignoring heterogeneity in a probit model is not necessarily worse than using the fixed effects estimator to account for it. In untabulated regressions, we re-estimate all our probit regressions excluding the fixed effects. Our findings are robust.

Regarding control variables, the offer size relates positively and significantly to the incidence of IPO fraud. This finding is intuitive: when a firm attempts to raise more capital, the incentive to manipulate financial information is stronger in order to make the firm more attractive to a larger investor base. It is also broadly consistent with existing evidence that offer size is negatively related to an IPO firm's long-term performance and market-to-book ratio (Krishnan et al., 2011). Most of the remaining control variables show signs consistent with the literature, though they are not statistically significant.

Table 2 suggests that the reputation of a VC is important in discouraging financial fraud by its portfolio companies. To clearly tease out the effect of nonfraudulent reputational concerns of VCs from those pertaining to financial misreporting in portfolio companies, we use five proxies to control for VC reputation in all subsequent regression analyses. Specifically, following Krishnan et al. (2011) and Atanasov et al. (2012), we include VC age, the cumulative amount of funds under management, the VC network, the fraction of VC portfolio companies that go public, and the number of deals that a VC

Table 3
This table reports the OLS regression results for the fundraising, industry concentration, and locality of VC firms. The unit of observation is the VC firm-year. The dependent variable in columns (1)–(4) is the VC firm's fundraising, industry concentration, "Ln(Distance)" and "% of Local Investment," respectively. The key independent variable is the IPO fraud dummy. Control variables are defined in the text and tabulated in the appendix. Data about portfolio companies and VC investors are obtained from the Venture Economics database. Standard errors clustered at the VC firm-year level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	Ln(Fundraising) (1)	Industry concentration (2)	Ln(Distance) (3)	% of Local investment (4)
IPO fraud dummy	−0.404***	0.016*	−0.061*	0.046**
	(0.082)	(0.010)	(0.028)	(0.017)
VC age	−0.020	0.001	−0.037	0.001
	(0.025)	(0.003)	(0.035)	(0.010)
Ln(Funds under management)	−0.332	−0.227***	0.640***	−0.065
	(0.197)	(0.058)	(0.206)	(0.042)
Pct deals going public	−0.143*	0.032***	−0.196	0.030
	(0.073)	(0.009)	(0.144)	(0.035)
Network degree	0.025***	−0.001	0.008	−0.001
	(0.007)	(0.001)	(0.008)	(0.001)
Number of deals	0.010	−0.006	0.025	0.005
	(0.016)	(0.008)	(0.031)	(0.006)
VC's prior performance	−0.144*	−0.138***	0.219**	−0.012
	(0.077)	(0.018)	(0.095)	(0.025)
VC's industry expertise	−0.034	0.650***	−0.493***	−0.006
	(0.064)	(0.027)	(0.075)	(0.018)
Ln(Past fundraising)	0.786***	0.002	0.024**	−0.003*
	(0.037)	(0.004)	(0.011)	(0.001)
Constant	1.190***	0.497***	4.850***	0.382***
	(0.185)	(0.034)	(0.171)	(0.056)
Year FE	Yes	Yes	Yes	Yes
VC FE	Yes	Yes	Yes	Yes
# of obs.	9,974	9,971	8,956	8,956
R^2	0.880	0.791	0.422	0.476

4.2. Pressure from VC peers

VCs often form syndicates when investing. If they are concerned about their reputation, they will be reluctant to team up with a VC who is known as a poor monitor. This might force a reputation-damaged VC to syndicate with lower quality VCs. To evaluate the subsequent syndicate quality of reputation-damaged VCs, we first examine the reputation of other VCs in the syndicate that the reputation-damaged VC joins.

Table 4 reports the regression results in which VC reputation is measured based on a three-year rolling window (column (1)) and an extended window (column (2)), as described in Section 2. Specifically, the dependent variable is a dummy variable that equals one if the weighted-average reputation score of other VCs in the syndicate is higher than the VC's own reputation score and zero otherwise.

The unit of observation is the VC firm-portfolio company pair. Thus we re-define the IPO fraud as a dummy variable equal to one if at least one of the portfolio companies previously backed by the VC is discovered to have committed IPO fraud and thus the VC is revealed as a poor monitor and zero otherwise. To illustrate, consider a VC that invested in four firms in 1999, two in 2001, and one in 2006. Assume that in 2004 one of the VC's previously backed IPO firms is found to be fraudulent. In this case, the IPO dummy equals one for the portfolio company that the VC invested in 2006 and zero for companies that the same VC invested in in 1999 and 2001. This variable is zero for VCs that have never funded fraudulent IPO firms. Standard errors are clustered at both the VC firm and portfolio company levels.

Besides the control variables in Table 3, following previous studies (e.g. Lerner, 1994; Tian, 2012), we include dummies that indicate the development stage of a portfolio company when it receives its first round VC financing, as well as the natural logarithm of total VC investment across all financing rounds in a portfolio company. Since including VC-firm fixed effects does not always converge in the probit model, we include industry fixed effects to control for any variations that only vary across industries but cannot explain our main results.[10] We report the marginal effects of the independent variables because they are easier to interpret than the raw coefficients of a probit model.

In columns (1) and (2), the coefficient estimates for the IPO fraud dummy are negative and significant. To illustrate, a VC who has previously backed fraudulent IPOs is 3.2% less likely to team up with VCs whose reputation score is higher than its

[10] Alternatively, we re-estimate Table 4 using a linear probability model and control for VC fixed effects. We obtain similar findings (not tabulated). However, we would like to caution that when interpreting of the results in Table 4 column (5) and also later, in Table 5 Panel B, the usual caveats common to using the linear probability model with binary dependent variables apply. While the linear probability model generally tends to give a qualitatively correct effect of independent variables on dependent variables, the coefficient estimates and standard errors are likely to be biased. Therefore the results obtained from the linear probability model should be treated only as suggestive.

Table 4
This table reports the regression results for the syndication of VC firms and for the underwriter reputation for VC-backed IPO firms. The unit of observation is the VC-portfolio company pair for columns (1)–(3) and is the VC-IPO firm pair for columns (4) and (5). The dependent variable is the high VC Reputation (Rolling) dummy, the high VC Reputation (Extended) dummy, the natural logarithm of the number of VC firms in a syndicate, "IB Reputation," and the high IB reputation dummy, respectively. The key independent variable is the IPO fraud dummy. Control variables are defined in the text and tabulated in the appendix. Data about portfolio companies and VC investors are obtained from the Venture Economics database. A portfolio company's industry classification is based on the Venture Economics 18-industry classifications. Standard errors are clustered at both the VC firm and portfolio company levels (columns (1)–(3)) and at the VC firm and IPO firm levels (columns (4)–(5)), respectively, and are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable	High VC reputation (Rolling)	High VC reputation (Extended)	Ln(# of VCs)	IB reputation	High IB reputation
	Probit	Probit	OLS	OLS	Probit
	(1)	(2)	(3)	(4)	(5)
IPO fraud dummy	−0.032**	−0.037*	−0.095***	−0.154***	−0.030
	(0.016)	(0.022)	(0.030)	(0.056)	(0.026)
VC age	−0.000	−0.001**	−0.005	0.045***	−0.001
	(0.000)	(0.001)	(0.013)	(0.009)	(0.001)
Ln(Funds under management)	0.003	0.007**	−0.048	0.001	−0.005
	(0.002)	(0.003)	(0.074)	(0.036)	(0.003)
Pct deals going public	0.074	0.067	0.165***	0.248	−0.167**
	(0.055)	(0.074)	(0.055)	(0.368)	(0.067)
Network degree	−0.002**	−0.001	0.008***	0.008**	−0.003
	(0.001)	(0.001)	(0.002)	(0.004)	(0.002)
Number of deals	−0.041***	−0.062***	−0.096***	0.102	0.032**
	(0.014)	(0.011)	(0.018)	(0.067)	(0.015)
VC's prior performance	−0.277***	−0.338***	−0.036	−0.092	0.105***
	(0.031)	(0.040)	(0.045)	(0.198)	(0.039)
VC's industry expertise	0.004	−0.016	−0.006	0.145	0.153***
	(0.029)	(0.035)	(0.031)	(0.166)	(0.045)
Ln(Past fundraising)	0.003	0.003	0.007**	−0.003	−0.010*
	(0.003)	(0.003)	(0.003)	(0.010)	(0.005)
Seed stage	−0.020	−0.005	0.033	−0.050	−0.080***
	(0.015)	(0.017)	(0.034)	(0.058)	(0.012)
Early stage	0.008	−0.010	−0.023	−0.103	0.018
	(0.012)	(0.017)	(0.028)	(0.197)	(0.046)
Expansion stage	0.013	0.005	0.041*	0.128**	−0.070***
	(0.012)	(0.017)	(0.024)	(0.065)	(0.026)
Late stage	−0.012	−0.013	0.112***	0.147**	−0.062**
	(0.017)	(0.022)	(0.025)	(0.065)	(0.025)
Ln(Total VC investment amount)	−0.037***	−0.023*	0.086***	0.039	−0.094***
	(0.013)	(0.012)	(0.015)	(0.066)	(0.028)
Underwriter fees				0.125	0.035
				(0.094)	(0.052)
Ln(Offer size)				0.541***	0.030**
				(0.046)	(0.014)
Ln(Age at IPO year)				−0.020	−0.011
				(0.021)	(0.009)
Constant			1.326***	5.202***	
			(0.156)	(0.688)	
VC FE	No	No	Yes	Yes	No
Year FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	No	No	Yes
# of obs.	16,055	16,055	9,926	5,882	5,859
(Pseudo) R^2	0.282	0.308	0.452	0.376	0.062

own (column (1)). The evidence suggests that reputational damage hurts a VC's ability to join a syndicate with more reputable members. Instead, it has to team up with poorer quality VCs in its subsequent deals.

Next, we examine the size of VC syndicates that the reputation-damaged VCs join after the revelation of fraud. Specifically, we regress the size of VC syndicates on the IPO fraud dummy and controls, as well as VC firm fixed effects and year fixed effects. Table 4 column (3) reports the results. The coefficient estimate of the IPO fraud dummy is negative and significant at the 1% level. This finding suggests that a VC joins syndicates in subsequent investments that are 9.5% smaller after the discovery of IPO fraud. Regarding control variables, better quality VCs, measured by capital under management and the number of past IPOs, team up with more reputable VCs and join larger syndicates. A VC's past fundraising also relates positively to the quality and size of the syndicate that the VC joins.

In summary, the results in columns (1)–(3) of Table 4 suggest that reputation-damaged VCs tend to join smaller syndicates and syndicates with less reputable VCs. Thus peer sanctions from other VCs represent another economic consequence of monitoring failure.

4.3. Pressure from IPO underwriters

To examine the tendency of a reputable underwriter to work with reputation-damaged VCs, we first regress the underwriter reputation score for subsequent IPO deals on the IPO fraud dummy. The unit of observation here is the VC-IPO firm pair. Therefore, the IPO fraud dummy is again re-defined as an indicator variable that equals one if at least one of the portfolio companies previously backed by the VC is discovered to have committed IPO fraud and zero otherwise. Standard errors are clustered at both the VC and IPO firm levels. To avoid falsely attributing VC reputation effects to underwriter reputation, we again control for all VC reputation variables, following Atanasov et al. (2012), as well as a VC's prior performance, industry expertise, and past fundraising.

Following the literature (Gompers, 1996; Tian, 2012), we also control for the IPO-deal and portfolio-company characteristics such as IPO size, firm age at the time of the IPO, management fees, the portfolio company's development stage when it receives its first round of VC financing, and the amount of financing that the firm has received from the current VC syndicate. In addition, we include VC firm fixed effects to absorb time-invariant VC characteristics that can affect the reputation of the underwriter that takes the VC's portfolio company public. Lastly, we include year fixed effects to absorb omitted time-varying factors (such as hot and cold IPO markets) that can affect the reputation of an underwriter.

The results in column (4) of Table 4 show that the IPO fraud dummy relates negatively and significantly to the average reputation score of the underwriters involved in subsequent IPOs by the same VCs. The evidence suggests that underwriters with better reputations are less willing to underwrite IPOs of firms backed by the VCs who are perceived as poor monitors.

In column (5), we replace the dependent variable with an indicator variable that equals one if the average reputation score of the current underwriters is higher than the average reputation score of underwriters involved in the same VC's earlier IPOs and zero otherwise. We estimate the regression using a probit model and report the marginal effects of the independent variables. Industry fixed effects along with year fixed effects are included. The effect of the IPO fraud dummy is negative, though it is not statistically significant.

Regarding control variables, it appears that more reputable VCs measured by VCs with a higher network degree and with a larger number of past deals are associated with working with more reputable underwriters. In addition, IPO offer size is positively associated with underwriter reputation, which is consistent with previous studies (Benveniste et al., 2003; Fernando et al., 2005).

5. Failure to monitor and future exits

In this section, we examine the impact of IPO fraud committed by VC-backed companies on future VC exit venues in a probit regression framework. The sample consists of VC-backed companies that have exited during the sample period; the unit of observation is thus the VC-portfolio company pair. Hence the IPO fraud dummy is an indicator variable that equals one if at least one of the portfolio companies previously backed by the VC is discovered to have committed IPO fraud and zero otherwise. To illustrate, consider a VC that invested in four firms in 1999. Two of the four exited in 2003, one exited in 2005, and the last exited in 2006. In 2004, one of the VC's previously backed IPOs is found to be fraudulent. For the exit analysis, the dummy equals one for the portfolio companies that exited in 2005 and 2006 and zero for companies exited in 2003. This variable is zero for VCs that have never funded fraudulent IPO firms. Here we use this variable to evaluate the effect of a VC being viewed as a failed monitor on its ability to harvest its future portfolio companies using a more desirable exit. To avoid correlations across observations from multiple VC firms financing one portfolio company, standard errors are clustered at both the VC-firm level and at the portfolio-company level and are reported in parentheses.[11]

Table 5 Panel A reports the results. Again, we report the marginal effects of independent variables to facilitate the interpretation of the economic significance. Column (1) investigates the relation between the revelation of a VC being a poor monitor and the probability of the best exit by its portfolio companies—the IPO. Specifically, the dependent variable, "IPO Exit," equals one if a portfolio company exits through an IPO and zero if it exits through an M&A or is written off. The variable of interest is the IPO fraud dummy. We control for IPO waves, defined as the number of IPOs at the time of exit (Benveniste et al., 2003), and VC characteristics, such as all five VC reputation measures. We also control for portfolio company characteristics (Nahata, 2008; Tian, 2012), such as the portfolio company's development stage when it received its first round of VC financing, its age at the time of its first financing round, the number of VCs investing in the first round, the total amount of VC investment received during its incubation period, and industry fixed effects where industries are based on Venture Economics 18-industry classifications.[12]

We find that the coefficient estimate for the IPO fraud dummy relates negatively and significantly to the probability of subsequent IPO exits. Compared to portfolio companies backed by VCs that are not perceived as inefficient monitors, those that are backed by VCs who suffer reputational damage are 16.7% less likely to exit via an IPO after fraud is detected. This

[11] Of portfolio firms financed by VCs who suffer reputational damage 25.5% have an IPO exit, and 43.8% have a successful exit, respectively.

[12] We include four development-stage dummies in the regression (with the buyout stage as the omitted stage) to reflect the portfolio company's stage at the time when it received its first round of VC financing.

Table 5

This table reports the regression results for the exit outcomes of VC firms. The unit of observation is the VC-portfolio company pair. The dependent variable in columns (1)–(3) is the IPO Exit dummy, the Successful Exit dummy, and the IPO vs. M&A dummy, respectively. The key independent variable is the IPO fraud dummy. Control variables are defined in the text and tabulated in the appendix. In Panel A, the probit regression also includes portfolio-company industry fixed effects. In Panel B, the linear probability model includes VC-firm fixed effects in addition to portfolio-company industry fixed effects. Data about portfolio companies and VC investors are obtained from the Venture Economics database. A portfolio company's industry classification is based on the Venture Economics 18-industry classifications. Standard errors clustered at both the VC firm level and at the portfolio company level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Panel A: Probit model			**Panel B: Linear probability model**		
Dependent variable	IPO exit (1)	Successful exit (2)	IPO vs. M&A (3)	IPO exit (1)	Successful exit (2)	IPO vs. M&A (3)
IPO fraud dummy	−0.167***	−0.117***	−0.061**	−0.056**	−0.094***	−0.053***
	(0.042)	(0.029)	(0.031)	(0.026)	(0.027)	(0.019)
IPO wave	0.002***	0.001***	0.002***	0.001***	0.000**	0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Age at 1st round	−0.122***	−0.096***	−0.025	−0.065**	−0.066**	−0.022
	(0.036)	(0.027)	(0.019)	(0.025)	(0.027)	(0.015)
Ln(No. of VCs)	−0.396***	−0.068**	−0.303***	−0.172***	0.026	−0.394***
	(0.046)	(0.029)	(0.028)	(0.028)	(0.025)	(0.019)
Ln(Total VC investment amount)	−0.023*	−0.012	−0.014	−0.016	−0.053***	−0.021***
	(0.012)	(0.008)	(0.015)	(0.012)	(0.017)	(0.008)
Seed stage	−0.092***	−0.120***	0.017	0.024	−0.002	0.057***
	(0.032)	(0.031)	(0.023)	(0.024)	(0.019)	(0.021)
Early stage	−0.105***	−0.067***	−0.053*	0.026	0.055**	−0.025
	(0.036)	(0.023)	(0.027)	(0.025)	(0.020)	(0.016)
Expansion stage	−0.121***	−0.105***	−0.025	0.008	0.022	0.007
	(0.038)	(0.026)	(0.027)	(0.019)	(0.020)	(0.015)
Late.37457J/F161Tf18.60460TD()Tj/F11215Tm.0001Tc7.9258J/F161Tf16.58690TD()Tj/F11Tf34.50Tm4					0.055****	

finding suggests that public equity-market investors are more reluctant to purchase shares of companies backed by the VCs revealed as ineffective monitors, and that these VCs face greater difficulty in exiting their subsequent investments via IPOs.

In our next test, we replace the "IPO Exit" with "Successful Exit" as our dependent variable. Column (2) of Table 5 Panel A shows that the IPO fraud dummy relates negatively and significantly to the probability of a future successful exit. For example, the marginal effect of the IPO fraud dummy in column (2) suggests that VC investors are 11.7% less likely to take their portfolio companies public or sell them in the three years after the revelation that they are ineffective monitors. Together with column (1), these findings suggest that damage to a VC's reputation is not limited to IPO exits but extends to both of the successful exit pathways.

These results also imply that VCs who fail to monitor tend to substitute IPO exits with the less attractive exit, as the marginal effect of the IPO fraud dummy tends to be smaller for future successful exit than for just the first best exit strategy via an IPO. For example, VCs who have previously backed fraudulent IPO firms are 11.7% less likely to achieve a successful exit within three years after the fraud surfaces (column (2)), which is smaller than the 16.7% reduction in the probability of just the first best exit via an IPO (column (1)). The substitution of an M&A exit for an IPO also reflects the damage to a VC's reputation.

In column (3), we explicitly investigate whether VCs substitute the more profitable IPO exit with the less profitable M&A exit by focusing on the subsample of portfolio companies that have successfully exited. The dependent variable in this probit regression is a dummy equal to one if the VC exits its subsequent investment via an IPO and zero if via an M&A. We observe that the coefficient estimate of the IPO fraud dummy is negative and significant at the 5% level, suggesting that VCs who have backed fraudulent IPO firms are 6.1% less likely to pursue an IPO exit in favor of an M&A exit.

Regarding control variables, portfolio companies are more likely to have an IPO exit during an IPO wave, consistent with findings in the literature (Benvensite et al., 2003). Portfolio companies financed by more VCs are more likely to have an IPO or a successful exit, also consistent with earlier findings (Tian, 2012). In addition, more reputable VCs with more capital under management and older VCs are more likely to have an IPO or a successful exit.

When estimating the exit probability using the probit regressions (Table 5 Panel A), we follow the literature (Atanasov et al., 2012) and control for the characteristics of VC firms and of their portfolio companies as well as industry fixed effects. However, portfolio companies backed by VCs that suffer reputation damage may differ systematically from those backed by VCs that do not. To address this concern, we estimate a linear probability model with VC firm fixed effects included as well. Table 5 Panel B reports these results. Similar to Panel A, the coefficient estimate of the IPO fraud dummy relates negatively to future IPO exits and successful exits and to the IPO exit over the M&A exit.

To summarize, the results in Table 5 indicate that VC firms revealed to have failed at monitoring their portfolio companies effectively will face greater difficulty in achieving successful exits. In addition, there is a shift in the choice of exits once a VC's reputation is damaged: instead of pursuing the more profitable IPO, these VCs opt for the less profitable pathway of M&A.

6. Cross-sectional comparisons

When disciplining VCs for monitoring failure, the VCs who have the most intensive involvement with their fraudulent IPO firms should suffer the most. To examine whether the market assigns a different magnitude of discipline depending on the extent of the interaction between VCs and their fraudulent portfolio companies, we first split the sample based on the stage of investment (i.e., earlier-stage and later-stage) in which the VC investor specializes. A VC firm is defined as an “early-stage VC” if more than half of its past investment since 1980 is in seed, early, or expansion stages and as a “late-stage VC” if more than half of its past investment since 1980 is invested in late or buyout stages ventures.[13] Intuitively, VCs that typically invest in earlier-stage ventures have a lengthier interaction with their portfolio companies and hence engage in more protracted monitoring. On the other hand, VC investors who typically invest in later-stage ventures may be excused for a lack of effective monitoring by market participants because of their limited involvement with companies subsequently revealed to be fraudulent. Therefore we expect the economic consequences of reputational damage to be more pronounced for VCs that are more responsible for intensive monitoring of their portfolio companies, that is, earlier-stage VCs.

Alternatively, we divide our sample based on the role played by the VC (lead versus nonlead) when funding fraudulent portfolio companies. For each VC-backed IPO firm, we identify its “lead VC” as the one that invests the largest amount in the firm across all financing rounds and others as “nonlead VCs,” following the VC literature (Tian and Wang, 2014).[14] VCs that act as a lead VC are expected to have greater involvement and do more monitoring than those that participate in the syndicate as nonlead VCs. Consequently, in the event that the portfolio company turns out to be fraudulent, the economic consequences of reputational damage should be more pronounced for lead VCs.

VC firms with better reputations tend to have greater expertise and ample resources to monitor their portfolio companies, deterring them from committing fraud. By contrast, VC firms with lesser reputations may not have sufficient expertise, experience, or resources, and the market may have a lower expectation regarding the quality of their portfolio companies. As a result, we expect that the economic consequences of reputational damage would be greater for more reputable VCs.

We classify a VC as a “high-reputation VC” (“low-reputation VC”) if its VC reputation score based on an extended window is above (below) the sample median before the fraud committed by its portfolio company surfaces.[15] Results are similar using a rolling-window-based VC reputation score (untabulated). To examine whether more reputable VCs suffer more from monitoring failure, we divide our sample based on the VC reputation *before* the discovery of IPO fraud in their portfolio companies.

Lastly, in the light of the wave of corporate financial fraud that surfaced in the early 2000s, researchers have linked the incidence of fraud with investor beliefs about business conditions (Wang et al., 2010). The market may be more tolerant of ineffective VC monitoring when general business conditions appear to promote greater incentive for fraud. To examine whether VCs who fail to provide effective monitoring are blamed less if the fraud occurred during a wave of fraud, we split our sample based on whether the number of fraud cases in the year when the IPO fraud occurred is above or below the sample median (i.e., during a wave or not during a wave).

[13] Six-five percent (35%) of VCs that have backed fraudulent IPOs are classified as “early-stage VC” (“late-stage VC”), while these corresponding numbers are 72.4% and 27.6% for VCs that have not financed fraudulent IPOs, respectively.

[14] Forty-three percent (57%) of VCs that have backed fraudulent IPOs are classified as “lead VC” (“nonlead VC”), while these corresponding numbers are 16.6% and 83.4% for VCs that have not financed fraudulent IPOs, respectively.

[15] In our sample, 74.4% (25.6%) of VCs that have financed fraudulent IPOs are classified as “high-reputation VC” (“low-reputation VC”) and 45.1% (54.9%) of VCs that have not financed fraudulent IPOs are “high-reputation VC” (“low-reputation VC”).

Table 6
This table reports the regression results to examine the effect of IPO fraud for various subsample comparisons. Panel A compares between VCs that invest in the early-stage versus late-stage ventures. A VC firm is an early-stage investor if more than half of its past investment since 1980 is in seed, early, or expansion stages. A VC is late-stage if more than half of its past investment since 1980 is invested in ventures in late or buyout stages. Panel B compares between lead and nonlead VCs. A VC firm is classified as a lead VC if it invests the largest amount of fund in the fraudulent IPO firm across all financing rounds. All other VCs are defined as nonlead VCs. Panel C compares between high-reputation and low-reputation VCs. A VC firm is classified as high-reputation (low-reputation) if its VC reputation score based on an extended window is above (below) the sample median before the discovery of the fraud by its portfolio company. Panel D compares between fraud wave period and nonfraud wave period. The unit of observation is the VC-year pair for columns (1)–(3), is the VC-IPO firm pair for column (5), and is the VC-portfolio company pair for columns (4) and (6)–(8). The dependent variables are VC fundraising, the VC firm's industry concentration, the VC firm's investment locality measured by the natural logarithm of the physical distance between the VC firm and its portfolio companies, the high VC Reputation dummy (rolling), the high IB Reputation dummy, the IPO Exit dummy, the Successful Exit dummy, and the IPO vs. M&A dummy, respectively. The key independent variable is the IPO fraud dummy. Control variables (untabulated) are identical to the corresponding tests included in Tables 3–5. The differences in the coefficients on IPO fraud dummy across subsamples are reported at the bottom of the table. To test the statistical significance of this difference across subsamples, we run a simultaneous equation analysis. For the related probit analysis, the simultaneous equation analysis is based on a linear probability model. Variables are defined in the text and tabulated in the appendix. Data about portfolio companies and VC investors are obtained from the Venture Economics database. A portfolio company's industry classification is based on the Venture Economics 18-industry classifications. Standard errors are reported in parentheses and are clustered at the VC firm-year level for columns (1)–(3), at VC-IPO firm level for column (5), and at VC-portfolio company level for columns (4) and (6)–(8). ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable		Ln(Fundraising)	Industry concentration	Ln(Distance)	High VC reputation	High IB reputation	IPO exit	Successful exit	IPO vs. M&A
		OLS	OLS	OLS	Probit	Probit	Probit	Probit	Probit
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: VCs investing in the early stage versus late stage									
Early Stage	IPO fraud	−0.421***	0.018*	−0.167*	−0.040***	−0.057***	−0.237***	−0.178***	−0.025**
		(0.102)	(0.010)	(0.086)	(0.015)	(0.020)	(0.054)	(0.032)	(0.011)
Late Stage	IPO fraud	−0.022	0.025	−0.076	−0.016	−0.005	−0.032	−0.01	−0.02
		(0.279)	(0.032)	(0.174)	(0.014)	(0.040)	(0.022)	(0.026)	(0.045)
Difference		−0.399***	−0.007**	−0.091***	−0.024***	−0.052***	−0.205***	−0.168***	−0.005*
Panel B: Lead VCs versus Non-lead VCs									
Lead	IPO fraud	−0.310***	0.017*	−0.191**	−0.086***	−0.078*	−0.147***	−0.094**	−0.112*
		(0.101)	(0.010)	(0.074)	(0.026)	(0.043)	(0.053)	(0.043)	(0.058)
Non-lead	IPO fraud	−0.201	0.020	−0.016	−0.005	−0.047	−0.055	−0.017	−0.025
		(0.149)	(0.019)	(0.220)	(0.029)	(0.038)	(0.101)	(0.059)	(0.019)
Difference		−0.109***	−0.003	−0.175***	−0.081***	−0.031***	−0.092***	−0.077***	−0.087***
Panel C: High-reputation versus low-reputation VCs									
High-reputation	IPO fraud	−0.283***	0.025**	−0.122***	−0.045*	−0.084**	−0.093**	−0.052*	−0.120*
		(0.074)	(0.011)	(0.032)	(0.025)	(0.034)	(0.044)	(0.031)	(0.074)
Low-reputation	IPO fraud	0.003	−0.011	−0.053	0.025	−0.031	−0.052	0.027	−0.063
		(0.150)	(0.072)	(0.407)	(0.055)	(0.045)	(0.038)	(0.041)	(0.064)
Difference		−0.286***	0.036***	−0.069***	−0.070***	−0.053***	−0.041***	−0.079***	−0.057***
Panel D: In or out of a fraud wave									
Out of Fraud Wave	IPO fraud	−0.311***	0.029*	−0.082*	−0.060**	−0.133	−0.172***	−0.103**	−0.092*
		(0.106)	(0.016)	(0.049)	(0.023)	(0.107)	(0.054)	(0.041)	(0.062)
In Fraud Wave	IPO fraud	−0.208**	0.010	−0.039	−0.020	−0.107	−0.108***	−0.064*	−0.029
		(0.096)	(0.015)	(0.022)	(0.061)	(0.089)	(0.037)	(0.036)	(0.027)
Difference		−0.103***	0.019***	−0.053***	−0.040***	−0.026***	−0.064***	−0.039***	−0.063***
	Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	Industry FE	No	No	No	Yes	Yes	Yes	Yes	Yes
	Year FE	Yes	Yes	Yes	Yes	Yes	No	No	No
	VC FE	Yes	Yes	Yes	No	No	No	No	No

We re-estimate our regressions in Tables 3–5 for these four sets of subsamples and report the results in Panels A through D of Table 6, respectively. For brevity, we only tabulate the coefficient estimates for the variable of interest and suppress all other control variables. The coefficients are significant for early-stage, lead, highly reputable VCs, and nonfraud wave subsamples but are insignificant or less significant for late-stage, nonlead, low-reputation VCs and fraud wave subsamples. Furthermore, the OLS coefficient estimates and the marginal effects for the probit regressions are mostly economically larger for the early-stage VC, lead VC, high-reputation VC, and nonfraud wave subsamples. When we explicitly test the difference in coefficients for the IPO dummy across each pair of subsamples, the differences are all statistically significant except for one specification in Panel B.[16]

The evidence suggests that markets do distinguish among VCs with differing degrees of involvement with fraudulent IPO firms and differing expertise and experience. The economic consequences of monitoring failures are more pronounced for VC firms that have better reputations and higher monitoring responsibility for their portfolio companies. In addition, there is some evidence that the market is more tolerant and that a VC is punished less for its ineffective monitoring if general business conditions are more conducive to fraud.

7. Robustness

7.1. Propensity-score matching

To alleviate concern about endogeneity, our main regressions control for VC firm fixed effects whenever possible. We further address this issue in a propensity-score matching framework following Atanasov et al. (2012). Specifically, we match VCs that have not backed fraudulent IPO firms to VCs that have along all five VC reputational proxies: VC age, number of deals, funds under management, network degree, and percentage of deals going public, as calculated in the year when fraud is discovered. In the propensity-score matching framework (i.e., Rosenbaum and Rubin, 1983; Deheja and Wahba, 1999, 2002), a matched peer VC is identified as the one with the smallest distance measure in propensity scores (the nearest neighbor) to the VC that has backed a fraudulent IPO firm. The match is based on the year of the IPO and is constructed with replacement.

We then rerun our analyses in Tables 3–5 based on this matched sample and report the results in Table IA-1 of the internet appendix. The coefficient estimates of the IPO fraud dummy are statistically significant except for the industry concentration test and successful exist test. Overall, our findings are generally robust to comparing VCs that have backed fraudulent IPOs with those that have not based on a propensity-score matching algorithm.

7.2. Post-exit fraud

To check the robustness of our finding, we examine whether a VC is still held accountable even when the fraud occurs *after* it harvests its investment in the firm. We postulate that a VC suffers much less reputational damage if the fraud occurs after it exits. Unfortunately, the exact date of the VC exit is not observable. Given that approximately 30% of VCs exit more than two years after the IPO (Gompers and Lerner, 1998), we define "Post-exit fraud" as a dummy equal to one if the IPO firm backed by a VC committed fraud four years or more after its IPO date. Doing so allows us to more precisely capture the *absence* of a VC during the fraudulent period of its portfolio company. We re-estimate the regressions in Tables 3–5, replacing the dummy for IPO fraud with the "Post-exit fraud" dummy. As Table IA-2 in the internet appendix shows, the coefficient estimates of the post-exit fraud dummy are insignificant except for "Successful Exit." While a VC pays for its monitoring failure around the IPO stage by facing negative consequences when pursuing subsequent investment opportunities and exits, the effect is mostly negligible if the VC leaves the fraudulent firm long before the fraud occurs. These findings suggest that the market holds VCs less accountable if a VC is less involved with its portfolio companies after the IPO.

7.3. Other robustness

Since the exact timing of a VC leaving its portfolio company after taking it public (i.e., when it completes its exit by selling all of its remaining shares or distributing them to LPs) is unknown, we use the cutoff years (i.e., 2 years) suggested by Gompers and Lerner (1998) to capture the presence of a VC firm during the fraudulent period of its portfolio company. It is possible that the IPO fraud is discovered prior to the VC's exit from the portfolio firm. While the noise in measuring the timing of VC exit works against us in finding our results, as a robustness check, we re-define IPO fraud as a fraud committed before or during a portfolio company's IPO stage. As Tables IA-3 through IA-6 of the internet appendix reveal, our findings are robust.

Our main analyses do not distinguish between VCs that have funded only one and those who have funded multiple fraudulent firms. There are 20 VC firms that have served as lead VCs and financed more than one fraudulent firm during the sample period. The results are similar if we exclude these VCs and their portfolio companies from our sample.

Ideally, we would like to include VC fixed effects in all probit regressions to better absorb VC-specific time-invariant unobservables and mitigate endogeneity concerns. Unfortunately, not all probit regressions converge once the VC firm fixed effects are included. For example, in Table 4, the estimation using "High VC Reputation (Extended)" does not converge. As a robustness check, we estimate the probit regressions with VC firm fixed effects for those that indeed converge. As Tables IA-7 and IA-8 reveal, our findings are generally robust.

[16] To compare the OLS coefficient estimates between two subsamples, we undertake the Chow-test. However, doing so in nonlinear probit regressions may not be appropriate. So instead, for the related probit analyses, we run the Chow-test based on a linear probability model to test the statistical significance of the difference in the coefficient estimates of IPO fraud across subsamples.

8. Conclusion

Much of the information-based theories of financial intermediation focus on delegated monitoring. However, there is little evidence on how markets discipline financial intermediaries that fail to perform this function. We use the VC market to address this gap in the empirical literature by examining the economic consequences for VC firms that fail to prevent fraud by their portfolio companies. We find that reputation-damaged VCs are punished by peers, as they interact differently in subsequent deals with their LPs, other VCs, and underwriters because they are perceived by these groups as ineffective monitors. This sanction forces them to raise less funding from LPs, invest more conservatively, team up with less reputable VC firms in subsequent deals, and work with less reputable underwriters in future IPOs. Furthermore, reputation-damaged VCs face greater difficulty in taking future portfolio companies public, and they are more likely to substitute for the most profitable exit—an IPO—with a much less profitable exit—M&A.

Appendix A. Supporting information

Supplementary data associated with this article can be found in the online version at http://dx.doi.org/10.1016/j.jacceco.2015.09.004.

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