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VENTURE FRAUD

Alexander Dyck
Freda Fang
Camille Hebert
Ting Xu

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ABSTRACT

We assemble the first dataset of venture fraud cases involving 654 U.S. VC-backed startups. Venture fraud has increased over the past two decades. Among newly public firms, VC-backed companies are more likely to face fraud charges than comparable non-VC-backed firms. Governance characteristics, rather than founder traits, are the strongest predictors of fraud: fraud is more prevalent in startups with founder-friendly contracts, complex cap tables, and initial rounds raised in hot market conditions. Fraudulent entrepreneurs continue to found new VC-backed startups unharmed, suggesting weak market discipline. These findings highlight growing agency costs in private markets.

Alexander Dyck
University of Toronto
adyck@rotman.utoronto.ca

Freda Fang
University of Toronto
freda.fang@rotman.utoronto.ca

Camille Hebert
University of Toronto
cam.hebert@utoronto.ca

Ting Xu
University of Toronto
Rotman School of Management
and NBER
tingxu.xu@rotman.utoronto.ca

"*All securities transactions, even exempt transactions, are subject to the anti-fraud provisions of the federal securities laws.*" – SEC Chair Mary Jo White

1 Introduction

Venture capitalists (VCs) use control and cash-flow rights to improve startup performance and align incentives between founders and investors (Kaplan and Strömberg, 2004; Ewens et al., 2022). While these contractual choices mitigate agency conflicts, they do not eliminate them entirely. Venture fraud represents an important manifestation of such residual frictions. Unlike regular risk-taking, venture fraud involves material misrepresentation and is illegal under anti-fraud laws that govern both public and private companies. The level of venture fraud is not socially optimal if it imposes externalities on other stakeholders, leading to capital misallocation.

Many papers highlight the effectiveness of the VC model in addressing agency problems (Jensen, 1989; Kaplan and Stromberg, 2003; Ewens and Marx, 2018), implying that fraud in VC-backed firms should be relatively rare. Yet this view may be overly optimistic, as the unique features of VC-backed startups can increase the likelihood of fraud. Startups' high growth pressures create incentives to exaggerate or fabricate performance. The use of novel and complex technologies in markets with uncertain demand can raise monitoring costs, as outcomes driven by deception are often difficult to disentangle from those resulting from technological or market risk. Moreover, the strength of VC governance may have been overestimated. With fat-tailed return distributions, VCs have incentives to prioritize upside capturing over downside mitigation (Kerr et al., 2014; Denes et al., 2023).

This paper measures venture fraud and examines its determinants and consequences. We ask three questions: How prevalent is fraud among VC-backed companies, and how has it evolved over time? What are the determinants of venture fraud? And does the market discipline founders implicated in fraud?

In addressing these questions, we are particularly interested in the role of governance, a traditional strength of the venture capital model that appears to have eroded in recent years (Lerner and Nanda, 2020). First, venture financing has become increasingly founder-friendly since the mid-2000s, with founders more likely to retain board control and be insulated from investor pressure (Janeway et al., 2021; Ewens and Malenko, 2024). Second, startups' cap tables have become more complex. Startups are staying private longer and raising larger rounds from a broader set of investors, including many non-traditional investors (Ewens and Farre-Mensa, 2020; Chernenko et al., 2021). This leads to more investors, reduced ownership stakes by traditional VCs, and new horizontal conflicts among investors with different incentives and objectives—all of which could undermine the monitoring of founders (Pollman, 2019). These internal governance incentives are particularly important if external market discipline is lacking. If "fake it till you make it" is the norm, and being caught for fraud is seen as just bad luck, there will be little punishment when founders seek capital for their next startup.

We compile a comprehensive database of VC-backed firms and founders involved in fraud-related lawsuits from 2000 to 2023. Focusing on legal actions allows us to capture economically meaningful cases, as immaterial frauds are rarely pursued by regulators or litigants. We use PitchBook to identify VC-backed startups and their founders and then search for fraud allegations involving these individuals or firms. Our sources include SEC and DOJ litigation releases, lawsuits in state and federal courts from Westlaw, and securities class action suits from the Stanford Securities Class Action Clearinghouse (SCAC). In all these lawsuits, founders are alleged to have made material misrepresentations or omissions regarding financials, operations, technology (e.g., Elizabeth Holmes of Theranos), or customers (e.g., Charlie Javice of Frank).[1] Our final sample consists of 654 unique cases involving VC-backed startups founded since 2000, with a median of $45 million raised capital prior to fraud detection. We manually code the attributes of each case, including fraud period, fraud type, charges, case outcome, and victims. We complement this data with information on founders, investors, and term sheets from PitchBook, as well as board characteristics from Ewens and Malenko (2024) and Form D filings.

A key challenge is that some fraud may never be detected. We take several steps to mitigate this concern. First, our main sample includes not only enforced cases but also non-enforced cases, including dismissed lawsuits as well as investigations and allegations identified from PitchBook and Crunchbase newsfeeds. This approach follows Atanasov et al. (2012), who argue that many dismissed suits are not meritless given the legal expenses, emotional stress, and reputational harm from launching a lawsuit, and the fact that dismissals often happen for procedural reasons. Second, in our predictive analysis of venture fraud, we control for determinants of detection likelihood, such as size, valuation, and proxies for opacity. We also focus on predicting the start of fraud commission rather than the timing of fraud detection. Finally, we conduct tests on a subsample of newly public firms, where detection rates are likely to be both high and relatively homogeneous. An IPO represents a sharp increase in disclosure and external scrutiny, raising the likelihood that existing misconduct is uncovered. Our identifying assumption is that fraud revealed shortly after the IPO reflects governance practices in place while the firm was still private.

We document four main findings. First, the venture fraud rate is sizable and has been increasing over time, with significant social externalities. Among U.S. VC-backed firms founded since 2000

[1] We exclude cases where founders' misconduct does not involve fraud allegations, such as anti-trust, bribery, insider trading, labor violations, and personal misconduct. We also exclude lawsuits where founders accuse investors of abuse or those focusing on horizontal conflicts among investors.

that raised at least $10 million, 1.75% have been involved in detected fraud, and the ongoing fraud commission rate has increased from 0.10% in 2003 to 0.63% in 2021. This increase is unlikely to be driven by stronger enforcement, as fraud rates declined over the same period among both public firms and PE-backed private firms. In a subsample analysis of class action suits against newly public firms, we find that VC-backed firms are 9.3 percentage points more likely than non-VC-backed firms to face class-action suits within two years of the IPO—a difference that is 90% of the mean. This estimate controls for industry-year fixed effects, pre-IPO growth, and firm size, and is robust to alternative post-IPO windows, lawsuit-materiality definitions, and using PE-backed IPO firms as the control group. The higher fraud rates among VC-backed firms, even after controlling for fundamentals, suggest potential pre-IPO governance weaknesses.

Second, we find that governance incentives strongly predict venture fraud. We use a hazard-style firm-level panel to predict the start of venture fraud, focusing on variables that capture governance incentives and controlling for startup life cycle, size, valuation, innovation, opacity, and industry shocks. The sample tracks all US VC-backed firms founded since 2000 from founding, with fraud firms remaining in the sample until the fraud start year and non-fraud startups until closure or exit. We find that governance strongly predicts fraud. Startups with founder-controlled boards are twice as likely to commit fraud as those with VC-controlled or shared-control boards. A 10% lower investor ownership is associated with a 19% increase in fraud likelihood relative to the mean. Cap table complexity also predicts fraud: one additional investor on the cap table increases the fraud likelihood by 10% relative to the mean, while a 10 p.p. higher share of non-independent VC investors (e.g., mutual funds or hedge funds) is associated with a 9% higher fraud likelihood. These findings are consistent with founder-friendly contracts and investor-side coordination frictions weakening the monitoring of founders.

To further assess the importance of governance incentives, we compare their predictive power with that of founder characteristics that are known to influence entrepreneurial entry and success in the literature. We find that, conditional on governance variables, gender and age do not predict fraud, whereas being solo, past founder experience, and top-school education have weak predictive effects. Overall, governance variables are far more important than founder characteristics in predicting fraud, exhibiting 5.1 times greater predictive power.

The above results are robust to a range of alternative specifications and sample restrictions. The findings persist when we include industry-by-initial-round-year fixed effects to absorb initial funding conditions, as well as when we add firm fixed effects to exploit within-firm variation in the timing of fraud onset while absorbing firm-specific detection rates. The results are also robust to excluding dismissed cases or class action cases, restricting the sample to startups that raised substantial amounts of capital, dropping crypto- or blockchain-related startups, and to a subsample test on a cross-section of VC-backed IPO firms that face high and homogeneous detection rates.

Third, we find that hot market conditions, in which investors chase founders, lead to more subsequent fraud. This speaks to a source of weakened governance: when investors compete for access to the hottest deals, they cede governance rights to win them, generating endogenous entrenchment of founders (Lerner and Nanda, 2020; Janeway et al., 2021). Such periods are accordingly associated with laxer screening and monitoring, including through founder-friendly contracts and cap table complexity, a finding we confirm in the data. At the market level, time series of aggregate VC valuation tracks aggregate venture fraud rates with a 2-3-year lag. In a cross-sectional firm-level regression, we find that hot market conditions at a startup's initial VC round—proxied by a higher average valuation multiple of early-stage deals in the same industry-year—strongly predict future fraud. A one-standard-deviation increase in market-wide valuation multiple at a startup's initial round is associated with a 19% higher future fraud rate relative to the mean. This effect is 137% when we instrument for the VC market condition using lagged capital inflows into buyout funds, following Nanda and Rhodes-Kropf (2013). These results hold even when controlling for the startup's own initial round valuation, suggesting that hot markets weaken screening and/or lead to contracts that are more protective of founders.

Fourth, we find no evidence that the VC market disciplines fraudulent founders ex post, in contrast to substantial penalties faced by fraudulent executives in public firms (Karpoff et al., 2008a,b). We conduct a matched event study at the person level around fraud detection. For each treated founder in the year before initial fraud detection, we match them with a similar control founder who was never involved in fraud but had similar past founding experience, tenure at the current startup, eventual exit status, sector of the current startup, education, gender, and age. Using this matched panel, we estimate a stacked difference-in-differences (Cengiz et al., 2019) that traces the creation of new VC-backed startups following fraud detection. We find no significant decline in entrepreneurial activity among fraudulent founders relative to matched controls, even in high-profile or publicly enforced cases. If anything, cases with greater media coverage are followed by more subsequent founding, consistent with an "any publicity is good publicity" dynamic in the VC market. Fraudulent founders are, however, more likely than control founders to turn to new investors to finance their next startup, suggesting that prior backers withdraw while new investors disregard past misconduct.

A key takeaway of the paper is a quantification of the costs of weak governance in venture capital. The observed level of fraud may be privately optimal for investors if they prioritize upside capture over downside mitigation, as founder-friendly contracts may incentivize value-enhancing risk-taking. However, governance-induced fraud may also be excessive. When deceptive founders become pervasive, they may crowd out higher-quality founders who are more likely to deliver the very right-tail outcomes VCs seek, leading to capital misallocation. If the reduction in monitoring incentives from cap table complexity is unanticipated or costly to re-contract, then fraud may exceed the level investors would prefer.[2] Such a distortion is likely to intensify as retail and other non-institutional investors, who are less equipped to screen or bear losses, play an increasing role in private markets (Phalippou and Magnuson, 2025).

Regardless of whether the observed level of fraud is privately optimal, it exceeds the socially optimal level when negative externalities are not internalized. We find that almost sixty percent of venture fraud cases involve non-investor victims—including customers, the public, business partners, and employees. The Theranos case provides a vivid example: founder Elizabeth Holmes's false claims about breakthrough blood-testing technology misled not only investors but also patients and physicians, resulting in misdiagnoses, delayed treatments, and other harms. We also find that fraud firms have grown larger and employed more workers over time, amplifying the magnitude of potential externalities. If venture fraud is excessive from a social perspective, this creates a rationale for policy intervention, including strengthening the information and resources available to regulators, who may be better positioned to offset investors' limited incentives to deter fraud.

This paper contributes to several strands of literature. First, we extend the literature on corporate fraud, which largely focuses on public firms (Ritter, 2008; Griffin and Kruger, 2024). This literature examines firm and industry characteristics that predict fraud and estimates the likelihood of fraud (Beneish, 1999; Dechow et al., 1996; Povel et al., 2007; Wang, 2013; Liu, 2016; Dyck et al., 2024). It also explores the economic consequences for executives of committing fraud (Karpoff et al., 2008a,b). A related literature studies fraud by financial institutions or advisors (Dimmock et al., 2018; Egan et al., 2019; Griffin, 2021; Griffin et al., 2023; Bian et al., 2023; Tookes and Yimfor, 2025). Our paper differs by examining these issues for VC-backed firms, an understudied group with agency problems distinct from those in public firms. Atanasov et al. (2012) examine lawsuits against VCs by entrepreneurs and other investors and highlight how reputation concerns limit VCs' opportunistic behaviors. Tian et al. (2015) show that the financial market disciplines VCs associated with IPO fraud by their portfolio companies. Both studies focus on investors and earlier periods before the rise of founder-friendliness.[3] We use a comprehensive sample of venture fraud cases new to the literature to document the power of governance incentives in predicting fraud and a lack of external market discipline for fraudulent founders.

We also contribute to the literature on governance and contracting in VC-backed firms. Analyzing VC contracts, Kaplan and Stromberg (2003) examine how VCs allocate cash flow, control, and liquidation rights to incentivize entrepreneurs while protecting themselves. Ewens et al. (2022) develop a dynamic model of venture contracts to show that VCs' superior bargaining power results

[2]Illustrating this point, Matt Levine from Bloomberg, in commenting on our paper, writes "*I suppose another model is 'if you are an early investor in a company, you should want the founder to get better at lying to later investors.' At some level your success case is 'later investors pay more than I do,' whatever happens with the business.*" (Levine, 2026).

[3]Atanasov et al. (2012)'s sample ends in 2007 while Tian et al. (2015)'s sample ends in 2005.

in more investor-friendly contracts, thereby affecting the equity split and value creation for startups. Lerner (1995) and Ewens and Marx (2018) examine VC governance through CEO turnover. Ewens and Malenko (2024) study the evolution of startup boards over the firm lifecycle and highlight the mediating role of independent directors. Examining the 1983-1994 period, Hochberg (2012) shows that VC-backed firms adopt better corporate governance after going public than non-VC-backed firms. Legal scholars argue that startups face governance challenges that are distinct and often more complex than those of public firms (Bartlett III, 2006; Pollman, 2019, 2020; Winship, 2020): not only are there vertical conflicts between shareholders, boards, and founders, there are also horizontal conflicts between shareholders with different payoff structures (e.g., preferred vs common), an issue Bian et al. (2023) study in the context of VCs' fire sales of startups. Our paper contributes by using fraud to understand the extent and determinants of agency problems in VC-backed firms, and how they have evolved over time.

Within this literature, several papers document the rise of founder-friendliness, potentially driven by private market deregulation, increasing competition for deals, and the participation of non-traditional venture investors (Nanda and Rhodes-Kropf, 2013; Ewens and Farre-Mensa, 2020; Lerner and Nanda, 2020; Chernenko et al., 2021; Janeway et al., 2021). Several theory papers examine the optimality of founder-friendliness (Banerjee and Szydlowski, 2024; Broughman and Wansley, 2023), arriving at different views. Our paper is the first to link the rise of founder-friendliness to the rise of venture fraud, highlighting its potential misallocation costs and social externalities.

2 Hypothesis Development

In this section, we lay out the theoretical motivations for our focus on several governance dimensions in VC-backed firms in explaining venture fraud. We also consider the roles of financing market conditions and founder characteristics.

2.1 Founder's Incentives and Ability to Commit Fraud

Theories of corporate control suggest that founder-friendly terms that weaken investor control rights reduce monitoring and increase founders' scope for fraud (Shleifer and Vishny, 1986; Aghion and Bolton, 1992; Hellmann, 1998). A central founder-friendly mechanism is board control. The board plays a key role in startups, deciding on the hiring and firing of the CEO, determining compensation, and approving all key decisions. Importantly, board control confers initiative rights that investors cannot replicate through protective provisions or voting rights, which primarily grant veto power over certain contingencies. Board control allocates residual control rights over unforeseen decisions under contractual incompleteness (Hart and Moore, 1990).

Hermalin and Weisbach (1998) model board independence as an endogenous outcome of bargaining between the CEO and the board, with more independent boards engaging in greater monitoring and lower agency costs, implying lower fraud risk in our setting. Consistent with this view, VC-backed firms have historically had more independent boards. Using IPOs through 1987, Baker and Gompers (2003) show that VC-backed firms have more independent boards than non-VC-backed firms, reflecting VCs' stronger bargaining power and access to replacement CEOs.

We examine the impact of board control on fraud over 2002–2023, a period characterized by declining VC control of boards, which we hypothesize is associated with higher fraud. Ewens and Malenko (2024) document substantial cross-sectional and time variation in board control, with a secular decline in VC control and a rise in founder control. For example, the likelihood of VC control of the board after the second financing round fell from 60% in 2003 to just 25% in 2013—a pattern we replicate and find persists. They further document a broader shift toward founder-friendly contracting and attribute these trends to increases in entrepreneurs' bargaining power, driven by abundant private capital supply and declining capital intensity of startups.[4]

In founder-friendly financings investors also often have lower ownership stakes (on a converted-to-common basis) due to higher valuation. When all shareholders vote, investors will have less influence, since most startup shares (common or preferred) have one vote per share and dual-class shares are typically created only in late-stage financing or at IPO. Thus, we hypothesize that, all else equal, founder-friendly terms, as captured by founders' control of the board and lower investor stakes, will raise the likelihood of venture fraud.

2.2 Cap Table Complexity and Monitoring Incentives

Horizontal frictions among investors can also weaken investors' incentives or ability to monitor founders. One source of horizontal friction is investor heterogeneity, arising from staged financing and different contractual terms across rounds. Chernenko et al. (2021) document the growing presence of non-traditional VC investors in late-stage rounds who care more about liquidity than traditional VC investors. They find that mutual fund investors prioritize liquidity-related provisions, such as redemption rights and IPO rights, often at the expense of cash flow and control rights that facilitate monitoring and mitigate fraud.[5]

[4]The move toward founder-friendly governance coincides with the rise of the "PayPal mafia" (e.g., Musk, Thiel, Hoffman) and the emergence of founder-centric investors, including Founders Fund and Y Combinator. These investors strengthened founders' negotiating power and promoted founder-friendly instruments (e.g., SAFEs) and a "founder-as-king" philosophy.

[5]Legal scholars, such as Bartlett III (2006), Pollman (2019), and Winship (2020), identify additional sources of investor heterogeneity that arise from differences in interests of preferred and common shareholders, or among common shareholders, which can similarly result in less attention to traditional governance.

A second source of investor friction stems from the increasing number of investors on startups' cap table. As firms remain private longer and raise more rounds, and as VCs increasingly adopt a "spray and pray" strategy, cap tables become more crowded, exacerbating coordination problems among investors. Such coordination problems, coupled with free-riding incentives when investors hold smaller stakes, hamper investors' collective monitoring of founders. We thus hypothesize that investor-side frictions, proxied by a "messy and crowded" cap table, will increase the likelihood of venture fraud.

2.3 Hot Market Conditions and Fraud

Macroeconomic conditions also shape incentives for fraud. In hot markets, founders and investors hold more optimistic beliefs, increasing the expected payoff from fraud while weakening monitoring incentives, thereby raising founders' propensity to engage in misconduct (Povel et al., 2007; Wang et al., 2010; Jensen, 2005). Hot markets are also characterized by intense competition among investors for deals, shifting bargaining power toward founders. This competition for access generates endogenous entrenchment (Lerner and Nanda, 2020; Janeway et al., 2021), with fraud serving as an observable marker.

The vertical consequence is that investors compete not only by bidding up valuations but by ceding governance rights, offering "founder-friendly" terms to gain access to the hottest rounds. Founders thereby accumulate control rights that insulate them from monitoring, and entrenched founders are, in turn, prone to overreach and to fraud. The horizontal friction takes the form of a prisoner's dilemma in which no investor unilaterally imposes discipline for fear of being screened out of the round. Relaxing governance to win a deal is therefore individually rational whenever rival investors are prepared to do the same, even though the collective equilibrium is socially inefficient. Hot markets thus breed fraud not merely by raising its expected payoff, but by structurally eroding the governance that would otherwise constrain it. Consistent with these mechanisms, we hypothesize that hot market conditions increase the likelihood of venture fraud.

2.4 Founder Characteristics and Fraud

Another potential determinant of fraud is founder characteristics, either innate or developed over time. A large literature documents the importance of personal traits—such as gender and age—in shaping entrepreneurial entry and success (Åstebro et al., 2014; Levine and Rubinstein, 2017; Azoulay et al., 2020; Hebert, 2025). Related work highlights the role of "nature", including genetics, in explaining entrepreneurial entry (Nicolaou et al., 2008; Nicolaou and Shane, 2009; Zhang et al., 2009) and tolerance for dishonesty (Loewen et al., 2013). Past founder experience and education are also well-established predictors of entrepreneurial success (Hsu, 2007; Lafontaine and Shaw, 2016; Van der Sluis et al., 2008; Gupta et al., 2024). As such, a priori, these founder characteristics could influence founders' propensity to engage in fraud beyond the effects of formal contracts and incentive structures.

3 Measuring Venture Fraud

3.1 Fraud Definition and Legal Framework

In this section, we outline the legal framework underlying our measure of venture fraud, which guides both our data sources and case collection strategy.

We define venture fraud as lawsuits initiated by regulators, investors, or stakeholders that allege fraud by a founder or a startup. In the U.S., lawsuits alleging fraud can be brought under three legal bases: i) violations of state or federal securities or criminal laws; ii) violations of fiduciary duty obligations in state corporate laws; and iii) violations of contract under state contract laws. Detailed descriptions of these legal bases are in Appendix 10. Under both common law and securities law, proof of fraud generally requires four elements: misrepresentation, materiality, reliance, and intent. Misrepresentations, which include omissions, are broadly defined and may relate to a firm's financial performance, business operations, risk exposure, legal compliance, partnerships, forward-looking guidance, or other disclosures or communications. Intentional misrepresentation and its illegality separate fraud from regular risk-taking.

We use the term *fraud* in a deliberately broad sense, with the more precise characterization being *alleged fraud*. All cases in our sample involve allegations of plausibly material misrepresentation. For cases pursued by the SEC or DOJ, limited enforcement resources imply that the decision to bring an action reflects the perceived importance of the misconduct. For securities class actions, we focus on non-dismissed cases and, in some tests, further require substantial financial settlements. In our main sample, we follow Atanasov et al. (2012) and consider all cases brought by private investors as plausibly material. Atanasov et al. (2012) describe a cost-benefit calculus that discourages frivolous lawsuits against VCs or VC-backed companies: such lawsuits are costly for plaintiffs, entail significant legal expenses, emotional strain, and reputational consequences. Moreover, many lawsuits are dismissed for technical or procedural reasons rather than on the merits.[6]

Legal standards for establishing liability also vary across case types. Criminal enforcement actions (e.g., by DOJ) generally require proof of intent and reliance, whereas civil settlements—

[6] For example, derivative suits alleging breaches of fiduciary duty are often dismissed when shareholders fail to show that presenting the complaint to directors would be futile, even if the underlying conduct is questionable. Some suits are also dismissed for failure to exhaust mandatory arbitration.

particularly in securities class actions—rarely involve admissions of wrongdoing, and dismissed cases do not involve adjudication on the merits. Crucially, neither dismissal nor settlement implies that the alleged misconduct was meritless or that the defendant was innocent. Settlements represent negotiated resolutions that allow defendants to avoid the uncertainty, cost, and reputational risk associated with a judicial finding of liability, even when the claims are potentially credible. As a result, imposing stricter legal thresholds, such as excluding dismissed cases or focusing solely on enforced actions, reduces the risk of false positives but comes at the cost of under-capturing true instances of fraud. Accordingly, our robustness tests explore this tradeoff by excluding dismissed cases or securities class actions, recognizing that doing so likely yields a more conservative but incomplete measure of underlying fraud.

3.2 Data Sources for Venture Fraud

To identify lawsuits involving fraud allegations against VC-backed firms, we rely on five data sources. The same fraud allegation may appear in multiple sources or just one, so drawing on multiple sources helps maximize detection. Our initial screening identifies 2,305 fraud-involved firms that have ever received VC backing. We further require that the firm is headquartered in the US, is founded in or after 2000, has a non-missing initial VC round date, and that the alleged fraud did not start after a firm went public. Applying these filters resulted in 654 unique cases entering our final sample. Table 1, Panel A shows the case count by source and a breakdown of cases into those that are enforced and non-enforced. Appendix 10 provides more details on our data collection process.

SEC. Our first data source consists of civil cases brought by the SEC, alleging federal securities law violations that were adjudicated or settled, with most cases involving penalties for founders and/or firms. This source is a compelling one, given that the SEC chose to pursue the case and the defendants settled.

To identify SEC enforcement actions involving VC-backed startups and their founders, we begin by scraping three types of SEC releases from 1995 to 2023: litigation releases, administrative proceedings, and Accounting and Auditing Enforcement Releases (AAERs). From each of these releases, we extract the respondent (i.e., violator) name, which may be either an individual name or a company name.

We employ two approaches to identify SEC cases by VC-backed firms that we can link to PitchBook. Our first approach links SEC respondents to PitchBook companies and founders using a combination of exact and fuzzy name matching on the respondent name.[7] Our second approach

[7]We start by retaining matches in which the release date falls between a company's founding and five years after

applies keyword-based filters to release texts to flag additional potential cases involving VC-backed startups or founders (e.g., "venture capital", "startup", "founder", "Silicon Valley"). We then manually verify and match flagged cases to PitchBook. After these two steps, we further exclude cases involving non-relevant violations such as delinquent filings, unregistered broker activity, insider trading, or market manipulation. After imposing filters relevant to our analysis, we obtain 126 SEC cases involving VC-backed firms.

DOJ. Our second data source consists of criminal cases that involve fraud allegations initiated by the U.S. Department of Justice (DOJ), which are resolved in its favor, resulting in defendants being required to serve time and/or pay financial penalties. This is a compelling source for fraud cases, as there is judicial certification of fraud and criminal cases are typically more severe than civil cases.

We collect information on criminal enforcement actions from the DOJ by scraping all publicly available releases from 2013 to 2024. DOJ enforcement of white-collar crime is more sparse before 2013. We use Westlaw to extend the DOJ sample back to 2000. 65% of these cases include subject-matter tags, out of which "Financial Fraud" and "Securities, Commodities, & Investment Fraud" are our focus. Using the tagged sample as a training sample, we apply a machine learning classifier (BERT) to tag the remaining untagged releases.

We take several steps to identify DOJ cases involving startups. First, we filter for cases containing startup-related keywords (e.g., "founder", "venture capital", "startup", "Silicon Valley"). We then manually verify the matching to VC-backed startups and founders in PitchBook. Using the manually verified sample as the training sample, we then train a BERT classifier to identify potential cases that may involve startups, and repeat the manual verification. We iterate several times between BERT classification and manual check to arrive at our final sample. After imposing our sample filters, we obtain 89 DOJ cases.

Securities Class Actions. Our third data source is security class action suits under the anti-fraud provisions of the federal securities laws. As a minimum test of materiality, we only include non-dismissed cases, since securities class action suits are more likely to be frivolous than private investor suits (Cox et al., 2008). This is another compelling source for fraud cases, albeit somewhat weaker than SEC or DOJ cases. It is possible that a defendant settled without wrongdoing just to make the case go away. As such, in some specifications, we further restrict securities class action suits to those with settlement amounts higher than $3 million, a common threshold used in the law and economics literature (Dyck et al., 2024). We note that securities class actions are very rare

its last financing round or IPO. For individuals, we ensure the release date falls within five years of their departure from the VC-backed company.

in private firms, as each investor bringing a suit must prove their reliance on the misinformation and thus cannot sue as a class (Winship, 2024). However, once these private firms become public, securities class actions have a low omission rate with many plaintiff law firms motivated to launch such cases (Karpoff et al., 2008a). We exploit this feature in our analysis of VC-backed IPO firms (see Section 4).

We obtain a comprehensive dataset of securities class action lawsuits from the Stanford Securities Class Action Clearinghouse (SCAC). We restrict the sample to non-dismissed cases filed in the U.S. between 2002 and 2023 by firms that went public in 2002 and onwards. We determine whether each company was ever VC-backed using PitchBook's deal history, supplemented with Jay Ritter's IPO data. Our main analysis considers cases with a class period starting within 2 years of the IPO, yielding 265 cases by VC-backed firms. Of these, 183 cases are included in our predictive regression after imposing additional filters.

Westlaw. Our fourth data source is Westlaw, a comprehensive legal database that covers both adjudicated and non-adjudicated lawsuits filed in federal and state courts (see Atanasov et al., 2012). Westlaw expands our sample beyond public-market litigation by capturing common-law fraud claims, contract and commercial disputes, and shareholder derivative suits, including cases involving alleged breaches of fiduciary duty. We focus on cases with published judicial opinions and those resolved through summary judgment or private settlement, which ensures meaningful judicial scrutiny. In the main analysis, we include cases that are voluntarily withdrawn by plaintiffs or dismissed by the court, typically for insufficient evidence or procedural deficiencies. In robustness tests, we exclude these dismissed/withdrawn cases.

We use three modules of Westlaw: court cases, dockets, and administrative rulings. The court cases module includes adjudicated suits that went before a judge with issued legal opinions. Dockets cover cases that never yield a published opinion, such as lawsuits that were settled out of court, withdrawn, or dismissed.[8] The administrative ruling module provides comprehensive coverage of SEC and DOJ news releases, which allows us to extend our DOJ sample back to 2000. To make the search manageable, we pre-screen adjudicated cases and dockets using legal classifications and keyword searches for fraud- and fiduciary-duty-related terms. We use a combination of machine learning and manual reading to verify the cases and their attributes. After imposing relevant filters, we obtain 234 non-dismissed cases and 99 dismissed cases involving VC-backed companies.

PitchBook and Crunchbase News Feeds. Lastly, to further mitigate concerns of omitted fraud, we use news feeds on VC-backed companies from PitchBook and Crunchbase to identify

[8]The dockets also provide the entire history of each case, allowing us to trace the timing of the initial suit or charge regardless of case outcome.

allegations of fraud that are not captured by the other sources. The advantage of this database is that each news article is already linked to firms by PitchBook and CrunchBase, minimizing matching errors. News also contains credible allegations and ongoing investigations, which provide a broader set of likely fraud or concerns for fraud, allowing us to mitigate concerns about omitted frauds. This is a credible source of fraud allegations because reputable media outlets are subject to fact-checking standards, legal liability, and reputational concerns that discourage the dissemination of rumors or misleading information. This sample has lower omission rates than enforced cases, with the tradeoff being that they may be less compelling. An additional limitation of this source is that the news feed data begins primarily in 2017. We aggregate all news articles linked to startups or their founders. We then use a combination of keyword filters, manual checks, and a machine learning classifier trained on over 6,000 hand-labeled articles to identify articles about startup fraud. After initial screening, we identify 242 news-based cases. Of these, 83% are also found in Westlaw (therefore entering into our Westlaw sample), leaving 42 news-only cases. Applying our sample filters yields 17 cases of news-based allegations or investigations.

3.3 Illustrative Examples of Venture Fraud

Below, we describe a few venture fraud cases in our sample to illustrate different types of misrepresentations (technology, customers, financials, regulatory compliance), different legal bases (securities laws, fiduciary duty, contract law), and the data sources that capture these cases. Tables A.1 and A.2 provide more examples of prominent cases.

Theranos. In a March 2018 suit the SEC charged Theranos, Elizabeth Holmes, and Ramesh Balwani for violations of federal securities laws. In a June 2018 suit the DOJ charged Holmes and Balwani with wire fraud and conspiracy to commit wire fraud. Allegations included misrepresenting the blood testing technology and making false and misleading statements about the business and financial prospects. Theranos was also subject to suits from partner Walgreens for breach of contract, from the investor Partner Fund Management for fraud, and from the Arizona Attorney General for consumer fraud. Elizabeth Holmes was sentenced to more than 11 years in prison and $452 million in restitution. This case appears in our SEC, DOJ and Westlaw samples.

Frank. Investor JPMorgan launched a civil lawsuit against Frank's founders Charlie Javice and Oliver Amar alleging violations of federal securities laws in December of 2022, after having agreed to purchase Frank for $175 million. Among the claims are that Javice repeatedly told the bank Frank had about 4.25 million student users, when in reality it had closer to 300,000 actual users, and that Frank fabricated data. There were subsequent suits by the SEC and DOJ. In September of 2025 Charlie Javice was sentenced to more than 7 years in prison and ordered to pay $300 million. This case is in our SEC and DOJ sample.

Bolt. Bolt is a one-click payments platform once valued at $11 billion. Its investor Activant Capital filed a lawsuit in July 2023 in the Delaware Court of Chancery against the firm and its founder Ryan Breslow, alleging breaches of fiduciary duty. The allegations include Breslow borrowing $30 million secured by company assets, failing to repay the debt, and removing directors who sought to protect the company's interests. The suit was settled in 2024 and included repayment of the debt and other penalties. This case appears in our Westlaw sample and illustrates a misrepresentation of the use of funds.

Spartan Micro. In an investor suit, investors alleged that Spartan Micro's CEO, Eric Stoppenhagen, fraudulently induced them to invest by misrepresenting the company's financial condition and regulatory compliance, while concealing severe financial distress and mismanaging funds. Claims include securities fraud, breach of fiduciary duty, and contractual violations. This is a dismissed case in our sample. Notably it was not dismissed based on the merits of the claim, which were not assessed by the judge, but was dismissed to arbitration given the presence of mandatory arbitration clauses for such disputes. This case is in our Westlaw sample.

Aceyus. The Charlotte Business Journal reports in 2022 that Metrolina Capital sued Aceyus, alleging breach of fiduciary duty, negligent misrepresentation and possible fraud that led Metrolina to accept a "low ball buyout" of its investment. This case is ongoing and is only available in our news sample.

Marrone Bio Innovations. This VC-backed firm went public in 2013. The firm was accused of violating securities laws around its IPO relating to improper revenue recognition, misleading financial statements, expense misconduct, and failure of disclosure obligations. The security class action settled for $12 million and the SEC case settled for $1.75 million, with Marrone Bio's former COO Hector Absi sentenced to two years in prison. The SEC commented that this firm's problems were common among VC-backed high-growth firms: "Rapidly growing enterprises present significant risks if the appropriate control structure is not in place. Time and again, we have seen companies go public and grow at a pace that exceeds their control structure."[9] This case is in our SEC, DOJ, and class-action sample.

4 Variables and Samples

4.1 Key variables

We use PitchBook and Form D to measure key governance features of VC-backed firms at the firm-year level. PitchBook also provides information on founder characteristics, as well as startup

[9]See SEC release: `www.sec.gov/newsroom/press-releases/2016-32`.

financing amounts and valuations that are used as controls in our regressions.

To measure founder friendliness, we use two metrics: board control and converted investor ownership. We use PitchBook and SEC Form D filings to extend the time-varying startup board composition data from Ewens and Malenko (2024). Following Ewens and Malenko (2024), we define a board as "founder-controlled" if founders/executives hold strictly more than 50% of seats, or if they hold exactly 50% of seats and investors hold strictly less. The remaining cases are either VC-controlled or shared control (i.e., VC and executive directors holding the same number of seats). We also measure investors' control power using investors' total ownership on a fully converted basis. Because we do not observe contingent payoffs, converted investor ownership is calculated by PitchBook based on all preferred shares converted to common and hence does not capture investors' actual cash flow rights. However, it is a close proxy for investors' voting rights vis-à-vis founders, as most shares (common or preferred) have one vote per share, and dual-class shares are typically not created until late stages before or at IPO. Together, these two variables capture the extent to which founders can unilaterally make decisions, versus whether investors or independent directors have sufficient control power to discipline founders.

We use two variables to capture cap table complexity that can hamper investors' collective monitoring of founders. First, we use the number of unique investors to measure coordination frictions among investors and potential free-riding incentives in monitoring. Second, we use the fraction of investors who are non-independent VCs on cap table. These investors, including angels, corporate venture capital funds (CVCs), mutual funds, hedge funds, and sovereign wealth funds (SWFs), are known to be weaker monitors than independent VCs due to their liquidity pressures and/or a lack of expertise (Chernenko et al., 2021; Shane, 2008). They also tend to come in either very early (e.g., angels) or late (e.g., mutual funds) in a firm's financing rounds, creating horizontal conflicts between investors across rounds.

Finally, we consider several time-invariant founder characteristics and their predictive power for fraud. We include an indicator for solo founder, the fractions of the founding team members who are serial founders, graduates of top universities, or female, as well as the average founder age at founding.[10] A solo founder may find it easier to commit fraud due to the absence of checks and balances from co-founders. Founders' experience, education, gender, and age may also shape their propensity to commit fraud through their reputation, career concerns, or risk tolerance.

4.2 Estimation samples

We rely on two main samples to study the likelihood and determinants of venture fraud.

[10] Top universities are Harvard, Yale, MIT, UPenn, Stanford, Columbia, NYU, Chicago, Cornell, Berkeley, Oxford, and Cambridge. We infer founder age from BA degree year.

Our predictive analysis uses a panel of US-incorporated VC-backed startups that were founded since 2000 and have received at least one round of VC financing with non-missing round date. The panel goes from 2000 to 2023. For this analysis, our main case sample includes all 654 enforced and non-enforced venture fraud cases from the SEC, DOJ, class actions, Westlaw, and news, and thus includes frauds in VC-backed firms while they are private or detected within 2 years of the IPO for those that went public. We construct a hazard-style panel of private firm-years to predict the start of fraud commission. We trace each fraud firm from its founding to the year the fraud starts, and each non-fraud firm from its founding to its closure or exit. After imposing non-missing founding year and fraud start date, our predictive sample includes 992,613 firm years for 105,692 unique VC-backed firms, out of which 595 were involved in fraud and 519 were involved in enforced fraud cases.

Our second sample consists of the universe of newly public firms headquartered in the U.S. and listed on major U.S. exchanges (NYSE or NASDAQ) retrieved from SDC New Issues module and matched to Jay Ritter's IPO database. We exclude firms listed on the OTC or Pink Sheets markets. In addition, we remove penny stocks (offer price below $5).[11] This sample contains 2,777 IPOs that went public between 2002 and 2023. We consider both formerly VC-backed and non-VC-backed firms. We identify the (former) VC-backed status using VC deals from PitchBook as well as the VC indicator in Jay Ritter's IPO database. We use class-action cases with the class period starting within thirty days, one year and two years of the IPO as measures of fraud. This sample aligns the enforcement environment faced by VC-backed and non-VC-backed firms, as both types of firms face the same public market enforcement, disclosure requirements, and intense scrutiny by regulators, underwriters, and other market participants upon IPO. As a result, this sample helps mitigate the issue of low and differential detection rates in venture fraud. The key assumption is that fraud detected soon after IPO is likely to be related to pre-IPO governance choices and the incentives in place while the firm was private.

5 Descriptive Statistics

5.1 Fraud Cases by Source

Table 1 reports summary statistics for the fraud cases in our sample. Panel A reports case count by sources. We obtain enforced cases from the SEC (126), DOJ (89), Westlaw (234), and class actions (183). After removing overlapping cases, we have 538 unique enforced cases. We additionally obtain 116 non-enforced cases, including 99 dismissed cases from Westlaw and 17 allegations or investigations from news. Combined, these data give us 654 cases, which serves as our main sample.

[11] We also flag SPAC IPOs using the SDC New Issues Blank check dummy variable.

The median fraud-involved firm raised \$45 million of capital by the time of fraud detection.

Panels B to F report specific case attributes for SEC, DOJ, Westlaw, class action suits, and dismissed/alleged cases separately. Among SEC cases (Panel B), frauds last on average 29.7 months. Monetary sanctions are substantial, with a mean fine amount of \$9.6 million. A majority of cases result in disgorgement (65%), fines (59%), and bans (59%), while prison sentences are rare (3.4%), as the SEC only enforces civil cases. Most cases involve financial misrepresentation (70%), with the remaining misrepresenting products (21%) and use of funds (21%). Investors are the most frequent victims (91%), followed by the general public (5.5%), other entities (5.5%), and government (3.9%).

The DOJ cases (Panel C) have a longer average fraud duration of 40 months. Wire fraud makes up 85% of cases, followed by securities fraud (33%), federal fraud (13%), corporate fraud (10%), and bank fraud (8%). Sanctions are also more severe than SEC cases, consistent with DOJ enforcing criminal cases: prison sentences occur in 43% of cases, with an average duration of 86 months, and the average fine amount is \$19 million. Additional penalties include forfeiture (15% of cases) and supervised release (35% of cases). Similar to SEC cases, investors are the most common victims, representing 66% of cases. However, the public, government, and other entities are also frequent victims, representing 23%, 18%, and 10% of the cases respectively. This is consistent with the DOJ's broader enforcement scope than the SEC and the social externalities these criminal cases generate.

For Westlaw cases (Panel D), which include criminal and civil cases in both federal and state courts, the average fraud duration is 28 months, similar to SEC cases. Securities fraud accounts for 38% of the cases, while corporate fraud and fiduciary duty violations account for 40% and 32% respectively. Misrepresentation typically involves firm financials (56%), products (23%), and use of funds (12%). Sanctions are not systematically reported, as many are privately settled. Investors are again the most frequent victims (63%), followed by the public (12%) and other entities (16%). Investor is the plaintiff in 39% of these cases, while government and business partners each represent 15%.

Panel E provides information on the 183 class action cases. Half are settled outside the court, with an average settlement amount of \$13.8 million, which is substantial. The most common grounds for suit are the anti-fraud provision (1934 Act Section 10(b)), control person liability (1934 Act Section 20(a), 1933 Act Section 15), and misleading IPO disclosure and oral communications (1934 Act Sections 11 and 12(a)2).

Panel F provides information on the dismissed cases from Westlaw and allegations/investigations from news. For cases for which we are able to find case information, the average fraud duration is 20 months. The top fraud types are securities (40%), corporate (41%), and fiduciary duty violations (10%). Similar to other cases, the most common misrepresentations are about financials and products, and investors are the most common victims.

Table A.1 lists the top cases in our sample with detailed case information. Table A.2 further compares our sample with the list of prominent cases featured in the Startup Litigation Digest, which is compiled by the UC Center for Business Law San Francisco. Eighty percent of the featured cases are captured by our fraud sample, with the remaining mainly filtered out due to our sample selection criteria (e.g., US firms founded post 2000).

5.2 Timing and Incidence of Venture Fraud

Figure 1 describes the timing of fraud based on our main sample of fraud cases. Panel A shows the distribution of cases by the year fraud starts. The number of cases has increased over time, especially after 2010, with a spike in 2021. The dwindling of frauds afterward may reflect truncation, as some cases have yet to be revealed. We observe a similar pattern in fraud-committing years (i.e., from the fraud start to the fraud end years) in Panel B.

Among US-incorporated VC-backed firms founded since 2000 that have raised at least $1M ($10M), 0.94% (1.75%) of the firms have ever been involved in fraud, and the ongoing fraud commission rate—fraction of startups engaging in fraud in a given year—is 0.28% (0.46%) on average. Based on SEC and DOJ data, Alawadhi et al. (2023) report an ongoing fraud commission rate of 0.96% among US publicly listed firms. Dyck et al. (2010) report a detected fraud rate of 1% to 4% among large US public firms based on securities class actions and SEC Accounting and Auditing Enforcement Releases (AAER) data. As such, although the enforcement environment can be quite different across the public and private markets, the detected fraud rate is comparable across the two markets. Given that the private market arguably has a lower detection rate, the true venture fraud rate is likely to be at least of similar magnitude to that in the public market.

Panel A of Figure 2 shows the trend in ongoing fraud commission rate among US VC-backed firms founded post 2000. To minimize truncation, we end the period in 2021. We find an overall increasing trend, with a small reversal in 2017-2020 before increasing again. Among firms raised at least $10M, the ongoing fraud engagement rate increased from 0.1% in 2003 to 0.63% in 2021. The incidence rate increases with firm size, as proxied by the cumulative amount raised. We find similar patterns in Figure A.3 using enforced cases only. Panels A and B of Figure 3 confirm the increasing relationship with size when binning fraud rate by total raised amount or post-money valuation at the first VC round. For example, fraud rate is 1.1% among firms that raised $5M-$10M at first round, and rises to 3.5% among those that raised more than $50M. However, we caveat that this positive relationship could reflect either a higher underlying true fraud rate or a higher detection rate among larger firms (both more visible and with more attractive cost-benefit analysis in pursuing lawsuits).

One concern with interpreting time trends in fraud likelihood is that the trends may arise from time variation in detection likelihood, i.e., enforcement intensity. To examine this concern, we first compare venture fraud trends with trends in fraud rate among PE-backed firms. PE-backed firms are a useful benchmark because, similar to VC-backed firms, they are private, have large sophisticated investors, and are more visible and economically meaningful than the average private firm; they also have the same ultimate investors—i.e., LPs—as VC-backed firms. As such, regulators such as the SEC likely allocate similar attention to these firms as to VC-backed firms, even though both may receive less attention than public firms. This data, presented in Panel B of Figure 2, shows no such upward trend. Rather, the panel shows a clear declining trend in fraud rates among PE-backed firms over the same period of 2001-2021. As a further comparison, we also plot the trend in fraud rates among US publicly listed firms in Panel C, based on Table 1 from Alawadhi et al. (2023). We observe a declining or flat trend in public firm fraud rate over time. The contrasting trends in fraud rates between VC-backed and other private or public firms suggest that the rise in venture fraud rate is unlikely to be driven by rising enforcement intensity over time by regulators.

Panels C and D of Figure 3 examine variation in venture fraud rate by geography and industry. Among states with at least 2000 startups, the venture fraud rate is highest in New Jersey, California, Pennsylvania, and New York, and is lowest in Delaware, Maryland, and Ohio. Based on PitchBook industry sectors, we find that fraud is most prevalent in Financial Services, Energy, and Healthcare, and is least prevalent in B2B and Materials and Resources.

5.3 Potential Social Externalities of Venture Fraud

Fraud is a greater social concern if venture firms are larger, have more stakeholders, and when the harm to those stakeholders is not internalized by investors. As a first step toward gauging these potential social externalities, we examine the size of venture fraud firms. Figure A.1 plots the median size of venture fraud firms (measured by cumulative capital raised or valuation) in the year before detection, by initial detection year. We see a clear upward trend. Figure A.2 tells a consistent story: the median employment of venture fraud firms prior to detection has likewise grown over time, and fraudulent startups employ substantially more workers than their non-fraudulent counterparts. Together, these patterns suggest that venture fraud firms have become economically more significant over time—potentially exposing a broader swath of society to harm.

To assess the harm to stakeholders, we identify all alleged victims named in the suits for each case. We present this data by source in Table 1. For example, the DOJ data show that in 23% of cases the public is identified as a victim. Figure 4 provides a comprehensive view of victim types, pooling all fraud cases regardless of source. Panel A shows that 41% of cases involve *only* investors as victims, while in the majority of cases—59%—the alleged harm extends to non-investors. Panel B breaks down non-investor victim types: customers and the public are most common at 44%, followed by business partners (32%), employees (26%), and governments (21%). Taken together, these statistics suggest that the social harms of fraud by VC-backed firms are not confined to a handful of high-profile cases such as Theranos or FTX—rather, they are a pervasive feature.

5.4 Governance Incentive Variables and Other Variables in the Prediction Panel

The variables of most interest to us in the predictive regressions are those that capture governance incentives. Table 2 provides summary statistics for these variables along with controls for the prediction panel. All statistics in this table are based on observed values, whereas in the regression we fill missing values with zeros to preserve sample size and include missing-value indicators. The mean of the dependent variable fraud start indicator in our hazard-style panel is 0.06%. This number is low because the panel stops at the fraud start year, rather than including the years when fraud was committed. The average firm-year is 5.8 years old, with a valuation of $21 million and total raised capital of $2.2 million. The average firm-year has a board of 3.3 members.

In terms of the governance variables explored in our predictive analysis, 53% of the firm-years are founder-controlled, and the average investors' converted ownership is 45.7%, suggesting that founders retain control almost half of the time. The average number of unique investors in the cap table is 3.8. Non-independent VCs represent, on average 57% of all unique investors on the cap table, suggesting great investor heterogeneity.

We gain further insight into these governance features by examining how they vary over time. Panels B to E of Figure 5 show, respectively, the trend in the fraction of founder-controlled boards, average founder/insider ownership (i.e., 100 minus investor ownership), average number of investors, and average fraction of non-traditional VC investors. These measures in general show an increase over time, consistent with the hypothesis of growing governance challenges in VC-backed firms. We also observe a flattening of some of these variables since 2017, consistent with a partial reversal of rising founder-friendliness in the last few years. In each figure, we also plot the fraud commission likelihood, showing indications of a correlation between heightened governance challenges and fraud likelihood.

In terms of founding teams, 49% were founded by solo founders. The average team has 13% serial founders, 26% top-school graduates, 13% female founders, and an average age at founding of 36.5. We also measure the extent of missing information in PitchBook, which is a common pattern in private firm databases but captures important information about a firm's opacity. Missing values (at the firm-year level) are most frequent for valuation, board composition, and term sheets, and less so for investors and founder information.

6 Fraud Incidence in VC-Backed Firms: The IPO Sample

We start by examining whether VC-backed firms have a fraud problem, relative to similar non-VC-backed firms. A direct comparison with private, non-VC-backed firms is problematic because data on their size and financing are scarce and often not publicly available. Moreover, fraud detection rates increase with firm visibility and VC-backed firms are inherently more visible than non-VC-backed private firms: they raise funds through exempt transactions that require regulatory filings, appear in commercial databases, and thus may attract greater attention from regulators and investors. Rather, we take the approach of focusing on newly public firms and comparing VC-backed and non-VC-backed firms. In this setting, size, disclosure requirements, and public visibility are more closely aligned. The IPO event serves as a natural benchmark: it imposes standardized disclosure requirements and similar market and regulatory oversight; it also represents a discrete jump in fraud detection rate.

In our sample of U.S. firms listed on major U.S. stock exchanges between 2002 and 2023, 10.7% of them ever faced a non-dismissed securities class action. The majority of these cases—66% of them—occurred within two years of the IPO (based on the start of the class period), and 36% of them happened within a month of the IPO (Table 3). Panel A of Figure 6 shows that the timing of class action suits is heavily skewed toward the IPO year, with 51% happening in the same year as the IPO. This supports the idea that fraud revealed immediately after IPO likely originates from governance problems in the pre-IPO period. Overall, this approach ensures that we study fraud events that are both economically significant and comparable across VC- and non-VC-backed firms.

6.1 Descriptive Evidence

Table 3 describes this sample. Summary statistics, without controls, suggest that class actions are more likely in VC-backed IPO firms. For example, such firms account for a minority (41%) of all IPO firms (Panel A) but a majority (61%) of firms facing class actions within 2 years of the IPO (Panel B). Panels B and C of Figure 6 further highlight differences between VC- and non-VC-backed firms, focusing on cases filed within 2 years of the IPO. In most years, VC-backed firms face more class action suits than non-VC-backed firms (Panel B), and a similar pattern holds for the suit rate (Panel C).

6.2 Regression Results

Table 4 tests for a difference between VC-backed and non-VC-backed firms in the likelihood of facing class actions for fraud right after IPO. All specifications include industry-year fixed effects.[12] Panel A examines the full sample of non-dismissed class actions over 2002-2023. The dependent variable equals one if a firm is sued within one month (Columns 1 and 4), one year (Columns 2 and 5), or two years (Columns 3 and 6) of the IPO, based on the start of the class period. In Columns 4-6, we additionally include controls for pre-IPO firm size and revenue growth to capture other potential differences between the two ownership types that may be associated with fraud commission and/or detection. These specifications also include an indicator for NASDAQ listing as a proxy for a firm's innovativeness. We prefer these specifications with additional controls, although we note their inclusion does reduce sample size due to the limited observability of pre-IPO financials in Compustat.

We consistently find a positive and significant coefficient on the VC-backed dummy, and the implied economic magnitudes are substantial. Without controls, we find that VC-backed newly public firms are approximately 4 p.p. more likely to face fraud-related litigation than non-VC-backed firms (Columns 1-3). In our preferred specification that adds controls and focuses on suits within 2 years of the IPO (Column 6), VC-backed firms are 9.32 p.p. more likely to face class action. This effect is economically large, amounting to 90% of the outcome mean. Both pre-IPO assets and revenue growth are positively associated with facing a class action shortly after the IPO.

To explore patterns in venture fraud over time, Panel B splits the sample period at its midpoint. This split is informative: it shows that the significant difference in fraud likelihood between VC- and non-VC-backed IPO firms is driven primarily by the recent decade (2013-2023). The VC-backed dummy is significant in this period (Columns 4 to 6) but not in the decade post the Dot-com bubble, 2002-2012 (Columns 1 to 3), and the coefficients are generally larger in the recent decade.

To address concerns that the higher likelihood of fraud among VC-backed IPO firms may be driven by the presence of large sophisticated investors, Panel C restricts the control group to IPO firms that were previously PE-backed. PE-backed firms provide a useful comparison because, like VC-backed firms, they have experienced ownership by sophisticated private investors and may have faced similar growth and exit pressures before going public. About 17% of the IPOs in our sample were PE-backed. We continue to find a higher suit rate for VC-backed IPO firms relative to PE-backed IPO firms. Based on Column 6, VC-backed IPO firms are 15.7 p.p. more likely to face a class action lawsuit within two years of the IPO than comparable PE-backed IPO firms, a large difference that is 135% of the outcome mean. These results suggest that the effect is not solely driven by concentrated ownership by sophisticated financial investors or the growth orientation of

[12] We define industry as SIC 2-digit code at the time of the IPO.

these firms; instead it likely reflects governance features specific to VC-backed companies.

Table A.3 reports several robustness tests. Panel A shows that results are similar when defining the post-IPO window using the filing date rather than the class period start date. Panel B includes dismissed suits, which may still signal credible fraud concerns. Panel C restricts to more serious cases with settlement amounts above $3 million. Panel D addresses sample truncation issues by ending the sample in 2019. Panel E excludes SPAC IPOs. Panel F adds more control variables, such as age at IPO, log amount of IPO proceeds, and a rollup dummy. Panel G re-estimates our main model using industry-year fixed effects based on SIC 3-digit. Finally, Panel H follows Hochberg (2012) and instruments VC-backing using the logarithm of the volume of venture capital invested in a firm's headquarters state in the year of its founding. The identifying assumption is that greater local availability of venture capital at firms' founding increases the likelihood of VC backing in that region but is otherwise orthogonal to subsequent firm-specific fraud risk. The 2SLS coefficients are again positive and statistically significant.

Overall, we find robust evidence that VC-backed firms are more likely to be sued for fraud immediately after the IPO than non-VC-backed firms, a difference that is particularly pronounced in the most recent decade. These findings are not driven by public versus private ownership, disclosure requirements, or firm visibility. They are also unlikely to be driven by differences in fundamentals between VC- and non-VC-backed firms, as we include industry-year fixed effects, pre-IPO growth, size and Nasdaq listing as controls, and the results survive a comparison with PE-backed IPO firms. Governance weaknesses in VC-backed firms, which appear to have increased, may help explain these findings. We test for this in the next section by examining the predictive power of governance features for fraud.

7 What Predicts Fraud in VC-Backed Startups?

7.1 Empirical Strategy

In this section, we aim to understand the determinants of fraud among VC-backed startups and use a predictive analysis of fraud in a startup panel. The empirical challenge is that we can only predict detected fraud, which is the product of committed fraud and detection likelihood. Conditioning on enforced fraud has the advantage of focusing on material cases with significant economic impact, as this is also the criterion regulators use to allocate enforcement resources. The downside, however, is that we must also worry about predictors of fraud correlating with drivers of enforcement or detection likelihood—an omitted-variable problem.

We take a number of steps to address this challenge. First, our main sample includes both enforced and non-enforced fraud cases to mitigate concern about under-detection or selective enforcement. The non-enforced cases include dismissed cases identified in Westlaw, as well as allegations and investigations identified in the news. Using this broader sample of fraud mitigates concerns about selective enforcement when focusing only on cases that were enforced. Second, rather than predicting fraud detection (date at which the fraud is revealed), we predict the timing of fraud start, which predates detection.[13] This helps distance our outcome variable from detection, especially in a specification with firm-fixed effect where we exploit only within-firm variation in the timing of fraud start while removing firm-specific detection rates. Third, we remove potential confounders that affect fraud detection by controlling for a set of drivers of enforcement likelihood. The remaining governance variables and founder traits should be largely orthogonal to enforcement, since internal governance contracts are essentially unobservable to regulators, and founder demographics do not typically enter into regulators' objective functions when they allocate enforcement resources. Finally, in some tests we adopt a similar approach to that in Section 6 by focusing on VC-backed IPO firms—a subsample in which the likelihood of detection is both high and homogeneous—and measuring fraud using class-action suits within two years of the IPO. The key assumption is that fraud revealed shortly after the IPO can be attributed to governance failures that were baked in while the firm was private.

Panel analysis of VC-Backed startups We use a panel regression to predict the start of fraud among VC-backed startups, leveraging the fraud commitment period we hand-collected. The sample is a hazard-style panel of US VC-backed firms founded since 2000. For fraud firms, we track them from their founding year to the year the fraud starts. For non-fraud firms, we trace all years from a firm's founding to its closure/exit. Hence, we exploit not only the cross-sectional variation in fraud incidence across firms but also variation in the timing of fraud. Specifically, we estimate the following specification:

$$\begin{aligned}\mathbb{1}(FraudStart)_{i,t} \times 100 = \alpha_{j,t} &+ \boldsymbol{\beta_1 Governance_{i,t}} + \boldsymbol{\beta_2 Team_i} + \boldsymbol{\beta_3 Controls_{i,t}} \\ &+ \boldsymbol{\beta_4 \mathbb{1}(MissingVal)_{i,t}} + \epsilon_{i,t}\end{aligned} \tag{1}$$

The dependent variable is an indicator for the fraud start year, multiplied by 100 to interpret changes in percentage points. Our key variables of interest are the time-varying governance incentives variables ($\boldsymbol{Governance_{i,t}}$) that we hypothesize impact fraud. *Board founder controlled* and *Investors' converted ownership* capture the extent to which founders can unilaterally make decisions, versus whether investors or independent directors have sufficient control power to discipline founders. *Ln(unique investors)* and *Investor_%non-IVC* capture the degree to which horizontal

[13] We manually collected the fraud start date from legal documents. For class action cases, we use the class period start year as the fraud start date. Our predictive results are similar using the IPO year or the year before the IPO as the fraud start year for class action cases (see Table A.4).

frictions amongst investors may reduce investor monitoring. $\boldsymbol{Team_i}$ is a vector of time-invariant team characteristics that includes an indicator for solo founder, the fractions of the founding team members who are serial founders, graduates of top universities, or female, as well as the average age of founders at founding.

We control for several potential determinants of fraud detection (which could also affect fraud commission likelihood), so that our remaining predictors pick up only variations in true fraud likelihood. First, enforcement against fraud may be higher in certain industries or periods, such as the cryptocurrency industry from 2020 to 2024. We therefore control for industry-year fixed effects ($\alpha_{j,t}$).[14] These fixed effects also absorb industry business conditions that can affect the incentives to commit fraud (Wang et al., 2010). Older, larger, and more visible firms are more likely to attract attention from the public or regulators, increasing detection likelihood. We therefore control for firm age, board size, post-money valuation, and cumulative raised amount (all time-varying).[15] These variables also help to account for potential life-cycle patterns in fraud and the growth expectations implied from valuation multiple (i.e., valuation/total raised). We additionally include the number of patents granted to a startup in a year to capture variation in business novelty that may affect risk-taking or monitoring difficulty.

Finally, missing values in firm characteristics may themselves contain important information about the firm's visibility and transparency, both of which can affect the likelihood of fraud detection. We therefore include missing-value indicators for the predictors in $\mathbb{1}\boldsymbol{(MissingVal)_{i,t}}$. This approach also preserves sample size when we include a large number of predictors. For each variable, missing observations are set to zero and accompanied by a corresponding missing-value indicator. The key identifying assumption is that, conditional on factors that influence detection and enforcement, the remaining predictors—particularly governance characteristics and founder traits—primarily capture variation in true fraud rather than differences in the likelihood of detection.

Identification and caveat on causality Like other predictive studies (e.g., Kaplan et al. (2012); Deming (2017); Levine and Rubinstein (2017); Guenzel et al. (2026)), our analysis is not designed to establish causality. In particular, governance contracts are likely to be endogenous. Nevertheless, we try our best to mitigate major omitted variable concerns. In tighter specifications, we additionally include industry-by-initial-round-year fixed effects to absorb unobserved funding conditions at the initial financing round, which may affect founders' bargaining power vis-à-vis VCs and the type of matches that are formed. We also include a specification with firm fixed effects.

[14] For our predictive analysis, we define industry by PitchBook's primary industry group, which contains 42 industries.

[15] Following Ewens et al. (2024), we interpolate valuation between rounds. We obtain startup patenting data from Ewens and Marx (2024).

This specification exploits only within-firm variation in the timing of fraud onset, rather than cross-sectional differences in whether a firm ever engages in fraud or not. This addresses endogeneity from time-invariant unobservables such as unobserved founder characteristics (e.g., charisma, integrity, or risk preferences) and business characteristics (e.g., technological complexity). It also addresses concerns about selective enforcement based on fixed firm characteristics, since this specification only exploits the timing of fraud start (not fraud detection) within the set of enforced firms. Our time-varying controls account for remaining variations in firm life cycle, size, valuation, visibility, and innovativeness over time. Finally, in Section 8, we examine an exogenous source of governance variation that comes from fluctuations in capital supply.

7.2 Predictive Regression Results: Governance

Table 5 presents the panel prediction results, focusing on the role of governance incentives in fraud. We find strong predictive power of founder-friendly governance features. Holding constant board size, *Board founder-controlled* positively predicts fraud. Founder-controlled boards are twice as likely to commit fraud as non-founder-controlled boards (i.e., VC-controlled or shared-control). In Table A.6, we further distinguish between VC-controlled and shared control boards and find that VC-controlled boards have a 3.3 times more negative predictive effect than shared control boards. Confirming the importance of control rights, we find a negative and significant coefficient on *Investors' converted ownership*: A 10% higher ownership by investors (on a converted basis) is associated with a 19.3% lower fraud likelihood relative to the mean. These results suggest that strong founder control rights are conducive to fraud.

Next, we find that cap table complexity is a strong predictor of fraud. Both the number of unique investors and the fraction of non-IVC investors are positively and significantly associated with fraud. The economic magnitudes are large. One additional investor on the cap table increases the likelihood of fraud by 10% relative to the mean. A 10 p.p. higher fraction of non-IVC investors raises it by 9%.[16] These results suggest that horizontal frictions among investors can amplify vertical agency conflicts between investors and founders by creating coordination frictions that weaken monitoring.

Several control variables are also informative. Firm age does not significantly predict fraud; if anything, there is a slightly negative relationship, though negligible in magnitude. Both valuation and total raised amount strongly predict venture fraud. The much larger coefficient on valuation than on raised amount suggests that valuation multiple, reflecting expectations about future growth and profitability, plays an important role.[17] This finding suggests that high investor expectations

[16]Based on Table 2, the mean investor count is 3.8. Hence, the percentage effect of one additional investor is (ln(4.8)-ln(3.8))*0.026/0.060=10.1%.

[17]Based on Column 2, a 10% increase in post-money valuation increases fraud likelihood by 13% relative to

increase the pressure on founders to meet ambitious growth targets, thereby raising the incentives to engage in fraud. At the same time, valuation and raised amount could also affect detection and enforcement, as larger firms are more likely to be scrutinized by regulators, particularly those with more assets in place to pay a penalty. Finally, we find that a larger board is associated with a higher likelihood of fraud.

The missing-value indicators, whose coefficients are reported in Table A.5, have positive and often significant coefficients. Note that much of this information is sourced from startups' Form D filings, yet many startups choose not to file them to stay under the radar (Ewens and Malenko, 2024; Hanley and Yu, 2023). Missing data therefore is not random but reflects startup choices that are themselves predictive of fraud. Thus, it is important to model this missing information in our analysis. The positive coefficients on missing value indicators are consistent with the idea that opacity facilitates fraud while transparency enables monitoring. They also suggest that our main results are unlikely to be driven primarily by variation in detection likelihood. If these indicators mainly capture opacity-driven differences in detection, we should expect negative coefficients, since greater opacity should reduce the probability that fraud is uncovered.

Columns 2-4 of Table 5 assess the robustness of the governance variables in predicting venture fraud. In Column 2, we add team characteristics specified in Equation 1 (i.e., solo, serial, education, gender, and age), with their coefficients reported in Column 2 of Table 6, Panel A. The magnitude and statistical significance of the governance coefficients change little, and the R^2 remains nearly unchanged. This suggests that founders do not sort differentially into governance characteristics based on their backgrounds.

Column 3 additionally includes industry-by-initial-round-year fixed effects to absorb funding conditions at the initial VC round. This specification ensures that our firm-level governance variables do not reflect general market conditions at the time of the firm's initial VC round, which could affect the bargaining power between VC and founders and, in turn, initial screening or contracting. We find that the coefficients remain similar with R^2 increased by 16% relative to that in Column 2. This suggests that much of the variation in firm-level contracts is firm-specific, even though initial financing conditions can explain part of the variation in fraud. We explore this more in Section 8.

In Column 4, we further tighten the specification by including firm fixed effects, so the estimated impact exploits only within-firm variation in the timing of fraud start, rather than cross-sectional differences in whether a firm ever engages in fraud. This brings two identification benefits. First, it addresses endogeneity from time-invariant firm-level unobservables—for example,

the mean, whereas a 10% increase in total raised amount only increases fraud likelihood by 1.9% relative to the mean. This suggests that the majority of the valuation effect reflects the valuation multiple rather than scale. The implied coefficient on Ln(multiple)—the difference between the coefficients on ln(valuation) and ln(raised)—is 0.136-0.020=0.116 based on Column 1.

unobserved firm or founder characteristics or their sorting into certain contracts. Second, the specification addresses selective detection based on firm unobservables, since it exploits only the timing of fraud start (not the timing of fraud detection) within detected firms. All of the governance characteristics retain their signs, while the coefficients on cap table complexity become larger.

7.3 Predictive Regression Results: Founder Characteristics

Table 6 explores the role of founder characteristics in predicting fraud and compares their explanatory power with that of governance variables. Column 1 of Panel A excludes governance variables and focuses only on team characteristics, while Column 2 adds governance characteristics. We find that solo founders are 12–20% more likely to commit fraud than teams relative to the mean, though with weak statistical significance. This finding is consistent with solo founders facing fewer internal checks and balances than co-founders in teams. Past founding experience and top school education have significantly positive coefficients but relatively small magnitudes: a 10 p.p. higher fraction of serial (top-school educated) founders on the team is associated with a 4% (2.7%) higher fraud rate relative to the mean. Gender has a statistically and economically insignificant effect, while age has a precisely estimated near-zero effect. Overall, founder characteristics seem to play a much smaller role than governance incentives in explaining venture fraud.

To quantify the relative importance of governance incentives versus founder characteristics, Panel B of Table 6 compares the partial R^2 of these two sets of variables.[18] We find that, across various fixed effect specifications, the four governance variables together have 5.1 times greater explanatory power than the five founder characteristics in predicting fraud. This confirms the idea that internal governance incentives are substantially more important than founder characteristics in explaining venture fraud.

Taken together, the results from this section suggest that venture fraud is shaped primarily by governance incentives rather than founder backgrounds. In other words, "nurture" matters more than "nature" in explaining venture fraud. This finding has implications for investors and regulators seeking to detect or mitigate venture fraud: governance structures appear to be more informative than founder types.

7.4 Robustness

Types of cases. Table 7 shows robustness to predicting two alternative sets of fraud cases: 1) enforced cases only—i.e., dropping dismissed cases or news-based allegations (Columns 1 and 2);

[18] Partial R^2 is computed based on Column 1 of Tables 5 and 6, relative to a benchmark model that excludes both governance and team characteristics but retains all other controls and fixed effects.

and 2) dropping class action cases (Columns 3 and 4). The enforced cases subsample prioritizes case merit at the expense of underdetection, while the private only subsample focuses exclusively on startups that never went public. We find similar results as Table 5.

Alternative samples. Table A.7 examines subsamples of startup-years with at least \$1M, \$5M, or \$10M in cumulative funding. This addresses the concern that startups with limited funding and traction attract little public or regulatory attention and hence face lower detection likelihoods. Fraud by these firms, whether detected or not, is also less likely to be economically consequential. We find similar results for these larger firms. Table A.8 further shows that our findings are robust to excluding firms operating in the cryptocurrency or blockchain-related verticals, as defined by PitchBook. This test shows that our findings are not driven by the recent SEC enforcement wave targeting cryptocurrency companies.

Homogeneous detection environment. Finally, to further mitigate concerns about underdetection and unobserved variation in detection likelihood, we conduct a cross-sectional analysis on a subsample of newly public VC-backed firms, some of which face class action lawsuits soon after IPO. These firms face a relatively homogeneous disclosure and enforcement environment following the transition to public markets. We estimate the following specification on a cross-section of VC-backed IPOs:

$$\begin{aligned}\mathbb{1}(ClassAction)_i \times 100 = \alpha_j + \theta_y + \boldsymbol{\beta_1 Governance_i} + \boldsymbol{\beta_2 Team_i} + \boldsymbol{\beta_3 Controls_i} \\ + \boldsymbol{\beta_4 \mathbb{1}(MissingVal)_i} + \epsilon_i \qquad (2)\end{aligned}$$

The sample is at the IPO-firm level, indexed by i. The dependent variable is an indicator (multiplied by 100) for whether the firm was involved in a class action suit within 2 years of the IPO (defined based on class period start date). We include fixed effects for the firm's industry (j) and IPO year (y). All independent variables are as described in Equation 1, but measured in the year before IPO. Additionally, for this test, we also include an indicator for dual-class shares IPOs in $\boldsymbol{Governance_i}$, since dual-class shares structure entrenches founders and generates agency issues (Masulis et al., 2009; Xu, 2021), which may drive fraud.

Table 8 shows results that echo our earlier findings. As in our previous results, founder control of the board in the year before the IPO strongly predicts class action lawsuits, with an effect equal to 48% of the mean. Lower investor ownership also positively predicts fraud, although with weaker statistical significance. New to this specification, we find that going public with dual class shares that entrench founder control positively predicts class action lawsuits, with an 80% higher likelihood relative to the mean. We also continue to find that cap table complexity, captured by the number of investors and the fraction of non-IVCs, positively predicts fraud, though the coefficient on investor count is not statistically significant, likely due to lower power. Founder characteristics are mostly insignificant, with only gender being weakly significant in Column 2.

8 VC Market Conditions and Venture Fraud

A central theme emerging from the analyses above is that governance incentives —in particular weakened governance associated with founder-friendly structures and cap table complexity— strongly predict venture fraud. To better understand the origins of these governance incentives, we consider the role of aggregate market conditions. In hot financing markets, money chases deals, shifting the negotiation power toward founders. This shift can lead to more founder-friendly contracts and greater cap table complexity through the participation of a larger set of investors. Hot markets can also affect fraud more directly by weakening investor screening and monitoring and by increasing pressure on founders to meet the high growth expectations implied in high valuations. Whereas in the previous analysis we absorb these effects with industry × initial round year fixed effects, in this section we examine them directly.

Descriptive evidence points to a relationship between hot market conditions, governance incentives, and venture fraud. Figure 5 Panel A shows a strong time-series correlation between annual VC valuation multiple and venture fraud rate, as both rose sharply after the financial crisis, with increases in valuation multiples preceding increases in fraud by two to three years. Consistent with a link between hot markets and weakened governance, Panels B and C show similar patterns for two measures of founder-friendliness: the fraction of founder-controlled boards and the average founder/employee ownership (i.e., 100 minus investor ownership). Both measures increase sharply after the financial crisis before flattening out after 2017. Similar correlations are observed for cap table complexity, as measured by the average number of investors (Panel D) and the fraction of non-IVC investors (Panel E) on startups' cap table.

To formally test for the effect of VC market conditions on startups' future probability of committing fraud, we measure funding conditions at the time of a firm's first VC round (Nanda and Rhodes-Kropf, 2013). This round is particularly important because it determines which startups are screened into the market and establishes the initial governance structure. We hypothesize that hotter VC markets lead to laxer screening, increasing the chance that VCs finance fraudulent startups. In addition, founder-friendly contracts negotiated during the initial financing round may create persistent governance weaknesses that increase future fraud risk.

We proxy for hot market conditions using the average valuation multiple at the industry-initial-funding-year level, where industry here is one of the seven broad sectors in PitchBook. To ensure that our average valuation multiple does not pick up general industry growth potential or local macro shocks, we control for industry fixed effects and state-year fixed effects. As such, we exploit variation in market conditions within an industry across years and within a year across industries. In some specifications, we also control for firm's age, post-money valuation, and total amount raised measured at the firm's initial VC round. Since we focus on the initial VC round year, we stop our sample period in 2021 to allow for a few years in the end to observe subsequent fraud.

To assess whether the relationship is causal, we follow Gompers and Lerner (2000) and Nanda and Rhodes-Kropf (2013) and implement an instrumental-variables strategy. We instrument industry-level VC valuation multiple at a startup's initial round with lagged capital inflow into buyout funds over the preceding three years.[19] We allocate fundraising volume into industries based on the distribution of funds' portfolio companies across industries. The identifying assumption is that LPs' lagged decisions to commit capital to buyout funds are unrelated to the future propensity of VC-backed startups to commit fraud. However, the fact that LPs allocate capital to the private equity asset class as a whole leads to a correlation between the supply of venture capital and buyout capital. We use the fundraising amount in the three years before the focal firm's initial VC round year because raised funds typically take one to three years to be fully deployed, and this helps further distance the instrument from current VC opportunities.

Table 9 reports the results. We find that firms initially financed in hotter markets—those with higher valuation multiples—are more likely to commit fraud at some point in the future. A one standard deviation higher market multiple (=0.65) is associated with 19% higher likelihood of future fraud relative to the mean (Column 1). This effect is slightly larger when controlling for the firm's age and its own valuation and raised amount at initial round (Column 2). We observe a much larger effect in Columns 3 and 4 when we instrument industry-level valuation multiple with lagged capital inflow into buyout funds. Based on Column 3, a one standard deviation increase in market valuation multiple increases fraud rate among startups initially funded in those markets by 1.46 percentage points, or 137% relative to the mean. This larger 2SLS effect is not due to our instrument being weak, as the Kleibergen-Paap F-stat in the first stage is 64.2, way above the conventional threshold of 10 or the Stock-Yogo threshold of 16.4 for a 10% size distortion.

Overall, these results suggest that in hot VC markets—where capital aggressively chases deals—investors are more likely to finance fraud-prone startups. This could happen either through unobserved weaker screening, or through founder-friendly contracts and ownership complexity that weaken governance incentives. Consistent with the governance channel, Table A.9 shows that startups financed in hotter markets subsequently adopt more founder-friendly governance structures

[19] Because LPs allocate capital with attention to industry coverage and not just vintage-year timing (Coller Capital, 2014; Gompers et al., 2009), buyout fundraising and deployment vary meaningfully across industries within the same year.

and attract a larger number of investors over the five years following their initial financing round. Regardless of whether the channel operates through formal contracts or unobserved screening or monitoring, all of these results stem from the pressure to win access to founders' deal flow, which generates endogenous entrenchment of founders (Lerner and Nanda, 2020; Janeway et al., 2021).

9 Does the VC Market Discipline Fraudulent Founders?

Another potentially powerful factor that could influence venture fraud is external discipline from the VC market. In this section, we investigate whether such a market mechanism exists by studying how fraud detection affects entrepreneurs' ability to found subsequent VC-backed startups. If founders face reputational consequences after being caught committing fraud, external market discipline may help deter misconduct. Conversely, if fraudulent founders can continue to raise VC and launch new startups with little penalty, the role of internal governance becomes even more important in mitigating fraud.

External discipline is more likely when fraudulent behavior is persistent and difficult to mitigate, and when investors incur reputational costs from associating with fraudulent founders. Both mechanisms are possible but not guaranteed. While a fraudulent founder is more likely to be fraudulent in future transactions, greater ex ante attention to governance can reduce the extent of the problem. Prior research shows that VCs value and protect their reputations: high-reputation VCs are less likely to face founder lawsuits (Atanasov et al., 2012), and investors incur reputational penalties when their portfolio companies are implicated in post-IPO fraud (Tian et al., 2015). However, our setting is different. Rather than studying the consequences for existing investors, we examine whether the VC market as a whole is willing to finance subsequent startups by a founder who has previously committed fraud. The reputation spillovers from a fraudulent founder to her future investors may be smaller than the reputation costs borne by her original investors.

To isolate the treatment effect of fraud detection on entrepreneurs, we conduct a matched event study at the individual level. For each treated founder in the year before the initial fraud charge, we match them to a similar control founder who was never involved in any fraud event. Specifically, we match exactly, in the year before the fraud charge, on the number of past startups, tenure at the current startup, exit status of the current startup, the current startup's sector, gender, and top school indicator. Within this set, we then select a control founder closest in age to the treated founder. Matching on past founding experience, age, and tenure at current startup is important, as it ensures that the treatment and control founders are at similar stages of their careers, since founding activities typically follow a life-cycle pattern (Azoulay et al., 2020). Matching on eventual exit status (closed, acquired, or other) of the current startup ensures that we do not capture any mechanical restart or replacement effect, since fraud-involved startups are more likely to close or be sold.

We then construct a person-year level panel by tracing these individuals' founding of VC-backed startups over time, as observed in PitchBook. Founding a VC-backed startup represents both launching a new firm and successfully raising VC financing. We do not track the founding of non-VC-backed firms, which could indicate exile from the VC market.

Using this matched panel, we estimate a dynamic DID. To account for potential bias from staggered treatment events, we implement a stacked DID following Cengiz et al. (2019). This method explicitly pairs each treated founder with a never-treated control founder to form a "stack". In particular, we estimate the following specification:

$$Annual(cumulated)founding_{ist} = \sum_{y \neq -1} \beta_y \cdot Treated_{ist} \cdot \mathbf{1}\{\text{EventYear}_{ist} = y\} + \gamma_{si} + \delta_{sy} + \varepsilon_{ist} \quad (3)$$

In this equation, s indicates stack, i indicates person, t indicates calendar year, and y indicates event year relative to the treated founder's initial fraud charge year. $Treated_{ist}$ is an indicator equal to one for fraudulent founders after initial fraud charge, and is zero otherwise. The specification includes stack $\times$ founder fixed effects (γ_{si}) and stack $\times$ event year fixed effects (δ_{sy}) (which absorb stack $\times$ calendar year fixed effects). As an alternative method, we also implement the imputation-based DID from Borusyak et al. (2024).

Overall, we do not find any significantly negative effect of fraud detection on fraudulent entrepreneurs' future founding activities. We first provide evidence in event study plots in Figure 7, with Panels (a) and (b) using stacked DID and Panels (c) and (d) using imputation-based DID. The dependent variable in Panels (a) and (c) is the number of new VC-backed startups founded by a person in a year. In Panels (b) and (d) the dependent variable is the cumulative number of startups a person has founded as of that year. The plots show that fraud-involved founders, if anything, found slightly more new startups after fraud revelation (about 0.01 per year on average). The results are highly similar across the two DID methods. We confirm these results with baseline DID estimates in Table 10 Panel A. Over the six years post fraud charge, treated founders started 0.006 more new startups than control founders (Column 2), a near zero effect relative to an outcome mean of 1.268.

The results are striking, as one would expect that the legal and reputational consequences of fraud charges would impair founders' future entrepreneurial prospects.[20] Instead, we find that these fraudulent founders were able to continue unharmed, launching their next startup and successfully raising VC financing. This result suggests that the VC market does not discipline fraudulent

[20]Note that only a small number of fraudulent founders faced incarceration, which would physically prevent them from launching a new startup in the near term.

founders ex post. This finding is consistent with rising founder-friendliness, in which VCs compete to fund a limited supply of startups and are thus willing to give up bargaining power and relax screening. It is also consistent with the Silicon Valley culture that embraces failure regardless of the cause.

A possible reason for finding no penalty for past fraud is that founders turn to new investors, who may not be aware of or care about past misconduct. To explore this possibility, we measure the overlap between investors in a founder's subsequent startup and the original investors in their fraud-involved startup. Panel B of Table 10 shows that, relative to control founders, fraudulent founders are less likely to raise capital from their original investors when launching a subsequent startup. 28% of fraudulent founders raise from at least one of the original investors in their prior venture after fraud detection, compared with 41% of control founders. We find a similar pattern when examining investor overlap share, defined as the fraction of all possible investor pairs across a founder's prior and subsequent startups that involve the same investor. The average overlap is just 0.5% among treated founders, compared with 5.1% among control founders. These findings suggest that fraud revelation weakens the relationship between founders and their existing investors. At the same time, the fact that fraudulent founders continue to raise from new investors indicates that information about past misconduct either does not travel in the VC market or is not weighted heavily by prospective investors when making funding decisions.

To examine whether the above results are driven by information transmission frictions in the VC market, we construct a measure of the media coverage of the fraud through Google searches, a plausible mechanism for investor due diligence.[21] Column 1 of Panel C Table 10 shows that cases covered by major media outlets surprisingly have a large and significant positive coefficient—an effect equal to 43% of the outcome mean. In contrast, cases without such coverage show almost no effect (Column 2). This pattern suggests that, when it comes to raising VC for one's next startup, "any publicity is good publicity." We similarly do not find a significantly negative coefficient for cases enforced by the DOJ and SEC (Column 3), which are much more high-profile and visible than other cases.[22] These results speak against the interpretation that news fails to travel in segmented VC markets; instead, they point toward the conclusion that new investors (and the broader VC market) do not penalize past misconduct. The lack of ex post market discipline reinforces the importance of internal governance in limiting fraud.

[21]Media coverage is measured using the top 50 Google News search results about the fraud event, with news data around and after the initial charge date. We then filter articles for relevance and accuracy, retaining only coverage from major newswires, general-interest news outlets, specialized business news, and large regional outlets. We exclude coverage from legal websites as well as trading or financial platforms.

[22]Table A.10 shows similar subsample results using imputation-based DID.

10 Conclusion

There is a trade-off in VC investing: generating upside versus mitigating downside. A shift towards founder-friendly governance in VC investment, including ceding control to founders and a "spray-and-pray" approach, may encourage risk taking and the potential for upside. This paper contributes by providing quantitative evidence that fraud constitutes an important cost of this shift.

We assemble the first comprehensive database of fraud in US VC-backed companies from 2000 to 2023 to study the incidence, determinants and consequence of venture fraud. We have four primary findings. Among firms that recently had an IPO, VC-backed firms are significantly more likely to be sued for fraud than non-VC-backed firms, consistent with weaker governance in the pre-IPO period. The difference is particularly strong in the past decade, which saw a prevalence of founder-friendly structures. Second, we use panel predictive regressions to investigate the firm-level implication of a shift to weakened governance on fraud incidence. Startups with founder-controlled boards are twice as likely to commit fraud as other startups. Fraud rate also rises with lower VC ownership or a more complex cap table. These governance incentives have a far greater impact than founder characteristics. Third, at the aggregate level, hot VC market conditions positively predict future venture fraud. Fourth, the VC market does not discipline fraudulent founders ex post, underscoring the importance of internal governance structures in mitigating fraud.

A founder-friendly approach that prioritizes upside capture over downside mitigation may be privately optimal for investors if the left-tail losses are outweighed by the right-tail gains. However, an equilibrium with weak screening and monitoring raises concerns. If deceptive types are pervasive, they could crowd out non-deceptive firms that could deliver the very right-tail outcomes VCs seek, leading to capital misallocation. Our finding that cap table complexity predicts fraud, in particular, suggests fraud might be excessive, as growing investor complexity may not have been fully anticipated and re-contracting is costly. Moreover, venture fraud can be socially costly through its negative externalities on stakeholders, such as customers, suppliers, employees, and future (including retail) investors. Our findings raise the question of whether the pendulum shift towards laxer governance in VC-backed firms may have swung too far. Because ex-post discipline is weak, and cap table complexity is likely to increase with rising retail participation in VC, credible contract design and targeted public enforcement are necessary to mitigate fraud and its negative externalities.

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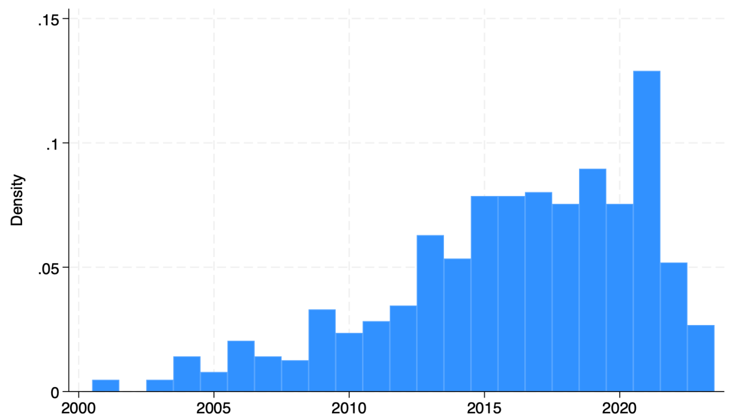
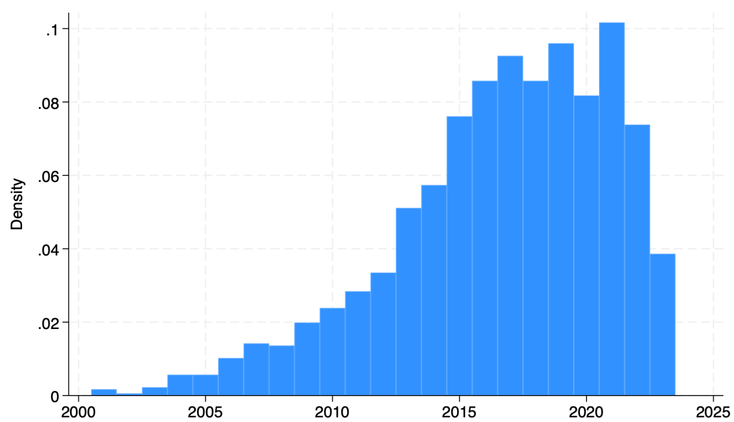
Figure 1: Timing of Fraud Among US VC-Backed Startups



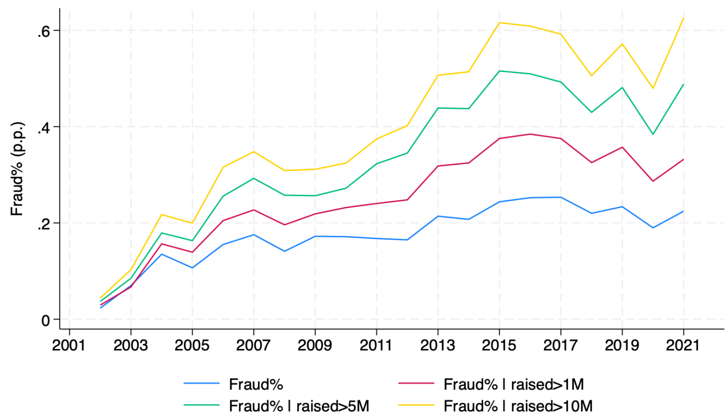
(a) Fraud Start Year



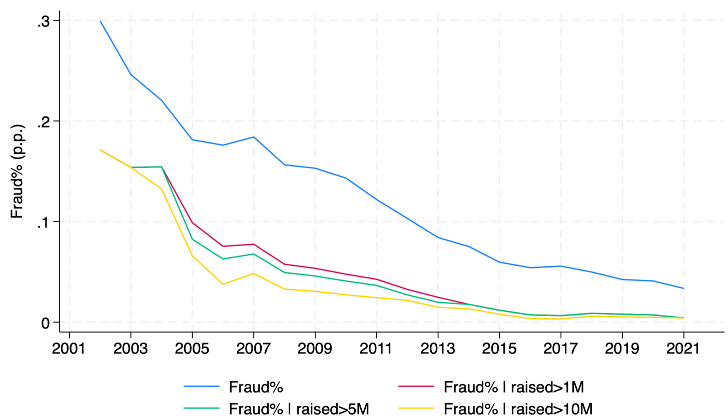
(b) Fraud Committing Years

This figure shows the timing of fraud among US VC-backed startups founded since 2000. Panel A shows the distribution of the fraud start years. Panel B shows the distribution for fraud commission years (i.e., years from fraud start to fraud end).

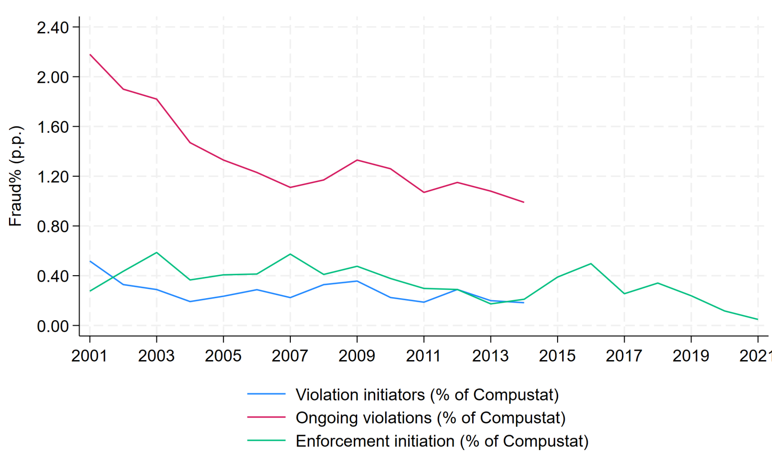
Figure 2: Fraud Likelihood Among US VC-Backed Startups vs PE-Backed or Public Firms



(a) US VC-backed firms



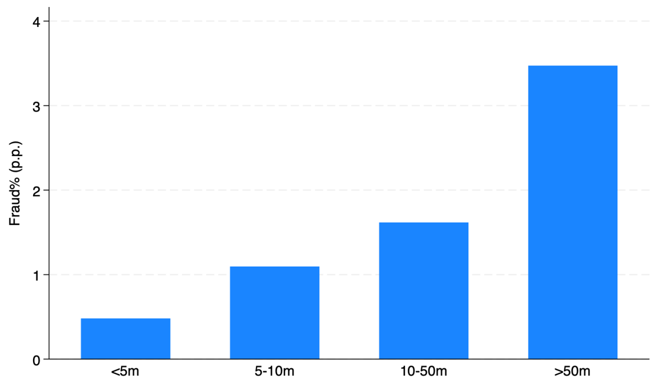
(b) US PE-backed firms



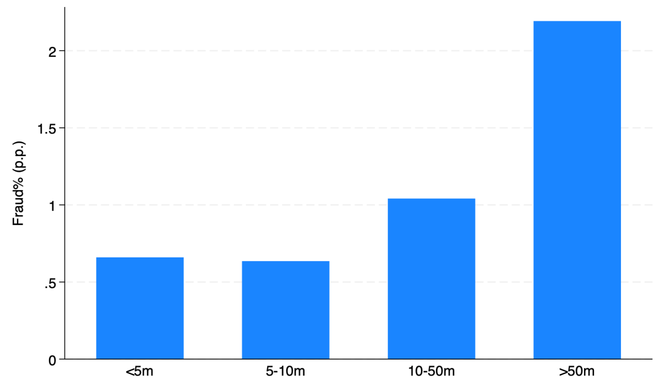
(c) US public firms (Source: Alawadhi et al. (2023))

Panel A presents the annual fraud commission rate among US VC-backed startups, measured as the number of firms actively committing fraud divided by the total number of active startups, by minimum raised amount. Panel B shows the annual fraud commission rate among US PE-backed private firms. Panel C shows the fraud rates among US publicly listed firms from Alawadhi et al. (2023).

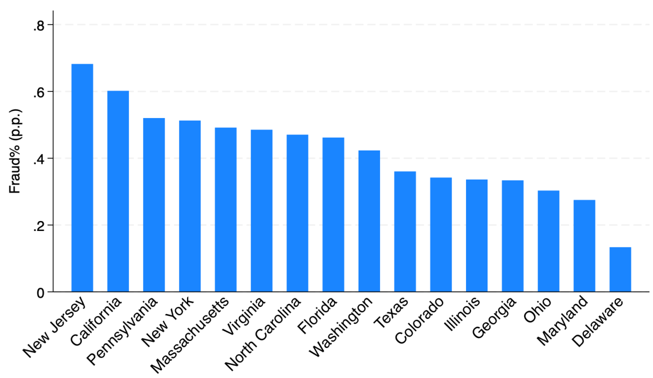
Figure 3: Variation in Fraud Likelihood Among US VC-Backed Startups



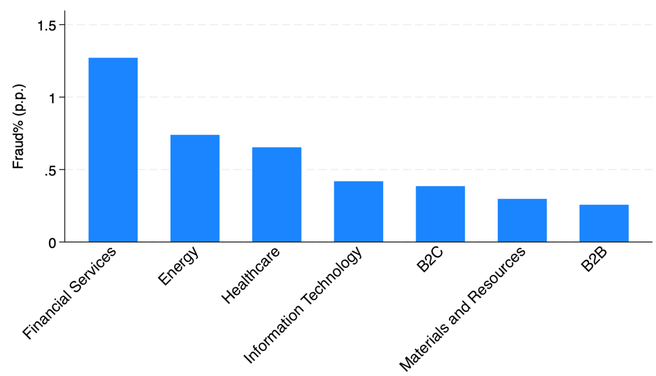
(a) By total raised amount at first round



(b) By post-money valuation at first round



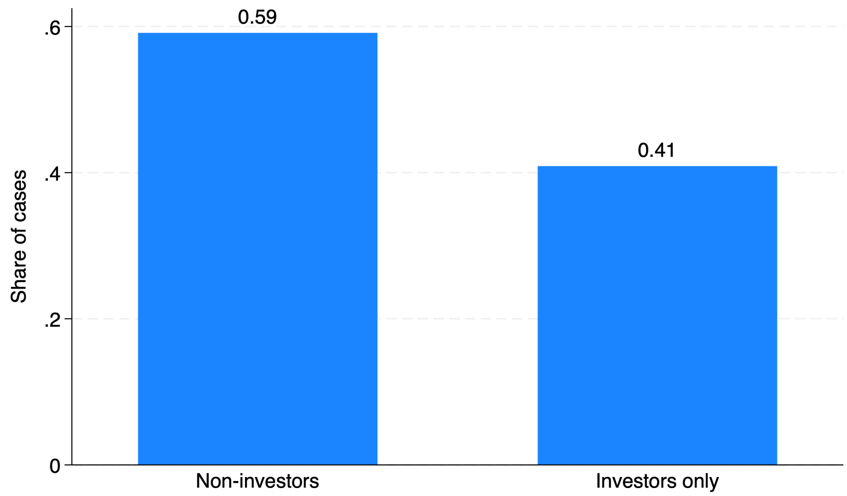
(c) By headquarter state



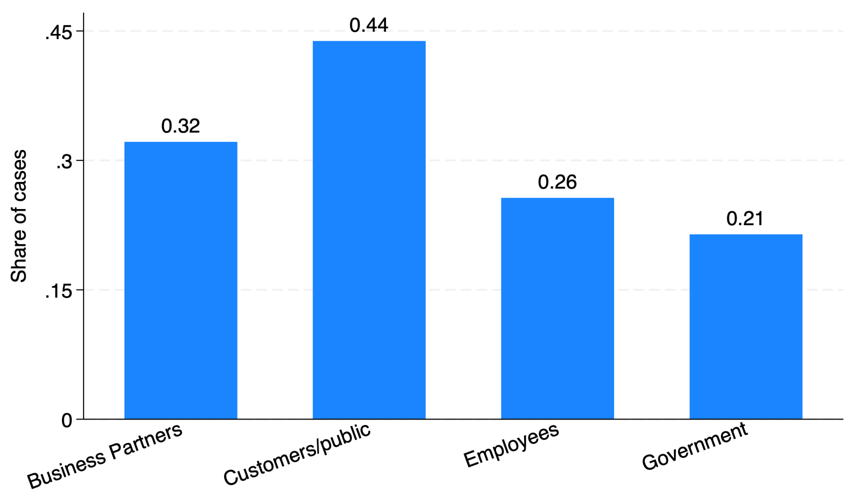
(d) By sector

This figure shows the fraud likelihood among US VC-backed startups by total raised amount at first VC round (Panel A), post-money valuation at first VC round (Panel B), headquarters state (Panel C), and sector (Panel D).

Figure 4: Social Externalities of Venture Fraud: Non-Investor Victims



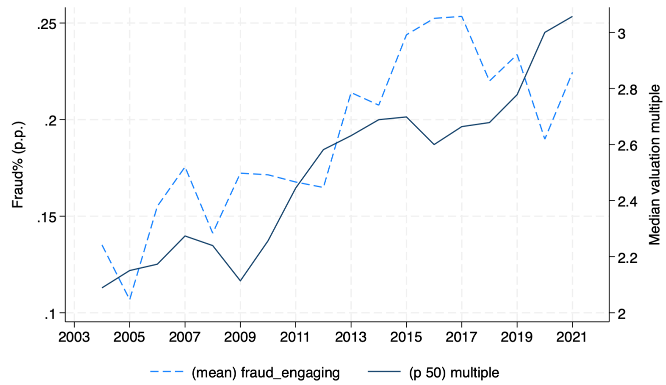
(a) Victim Categories



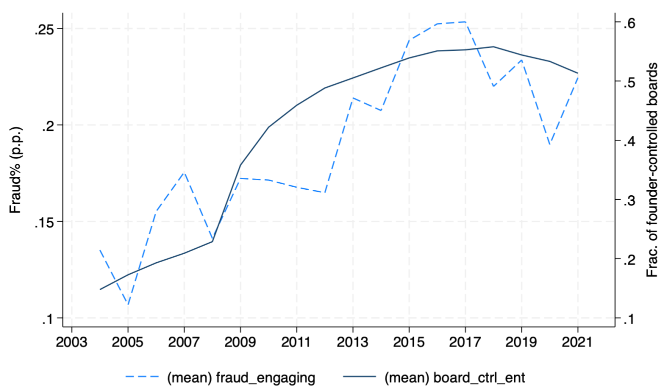
(b) Decomposition of Non-Investor Victims

These figures provide evidence on the social externalities generated by venture fraud beyond investor loss, using combined and deduplicated case descriptions from all non-class-action sources. Panel A reports the share of cases that harm only investors versus those that also harm non-investors. Panel B decomposes non-investors by victim type. Categories can overlap as many fraud cases involve multiple victim groups.

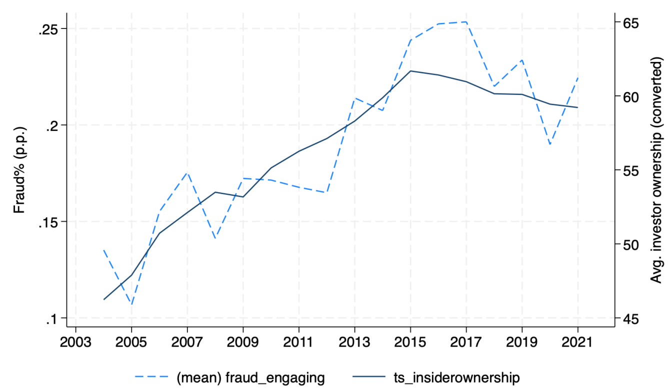
Figure 5: Trends in Venture Fraud and Founder-Friendliness



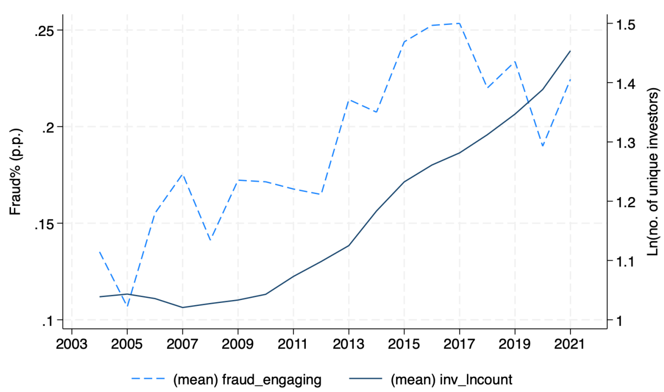
(a) Valuation multiple



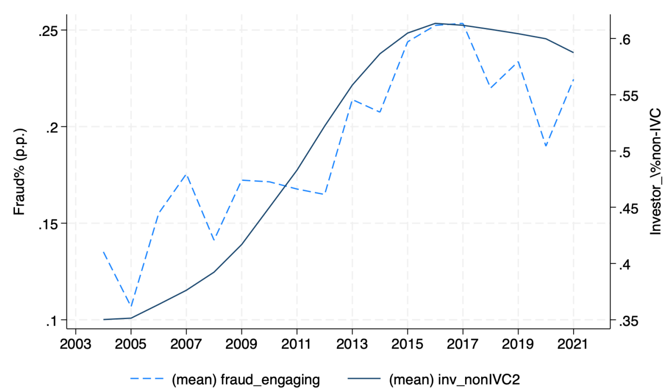
(b) Founder-controlled boards



(c) Founder/employee Ownership



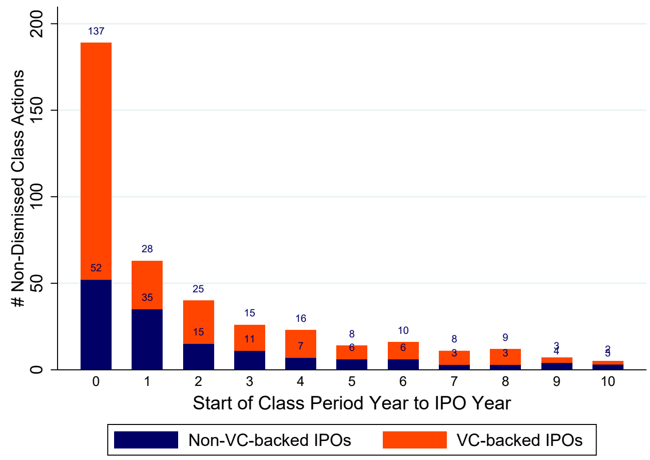
(d) Ln(no. of unique investors)



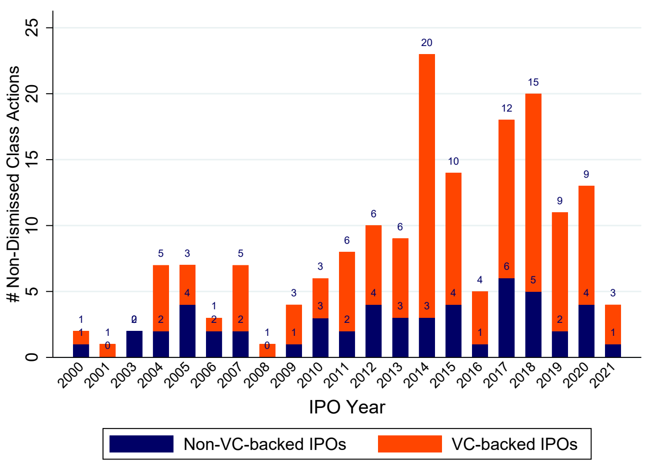
(e) Fraction of non-IVC investors

This figure plots the annual fraud commission rate among US VC-backed startups founded since 2000 against several measures of founder-friendliness in the VC market. In each panel, the blue dashed line represents the likelihood that a startup is actively committing fraud in a given year. The solid navy line represents the median valuation multiple (post-money valuation divided by total capital raised) in Panel A, the average fraction of founder-controlled boards in Panel B, the average founder and employee ownership share (i.e., 100 minus investor ownership) in Panel C, the average log number of unique investors on the cap table in Panel D, and the average fraction of non-independent VC investors on the cap table in Panel E.

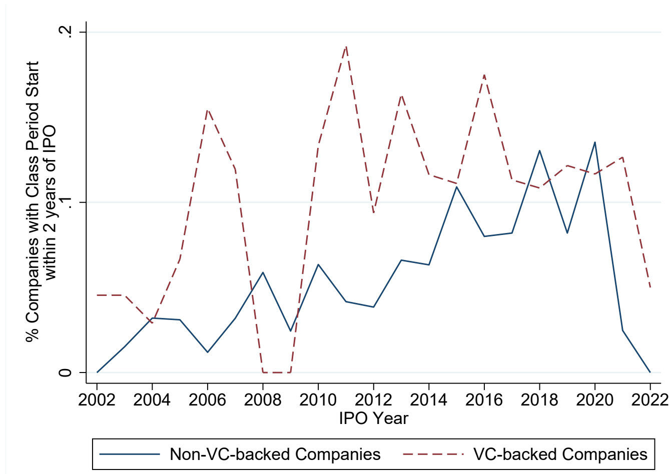
Figure 6: Class Actions by VC-Backed Status



(a) Start of Class Period to IPO



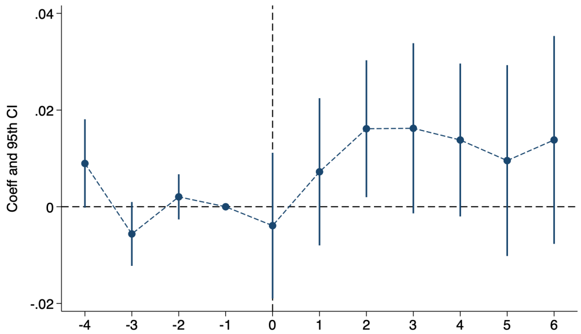
(b) Class Actions Started within 2 Years of IPO



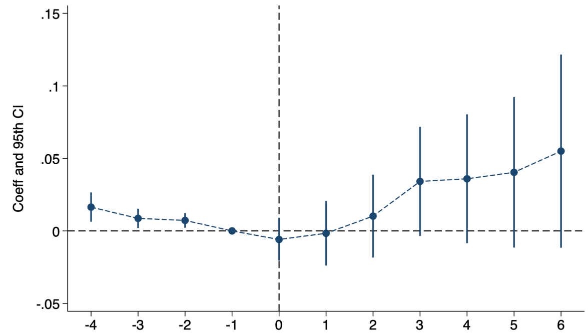
(c) Likelihood of Class Action within 2 Years of IPO: VC-Backed vs Non-VC-Backed

These figures show the distribution of securities class action lawsuits by VC-backed status. The sample is restricted to non-dismissed class action suits with a class period starting within two years of IPO. Panel A shows the distribution of cases by the number of years between IPO and the start of the class period. Panel B shows the distribution of cases by IPO year. Panel C compares the likelihood of class action suits across VC-backed and non-VC-backed IPO firms.

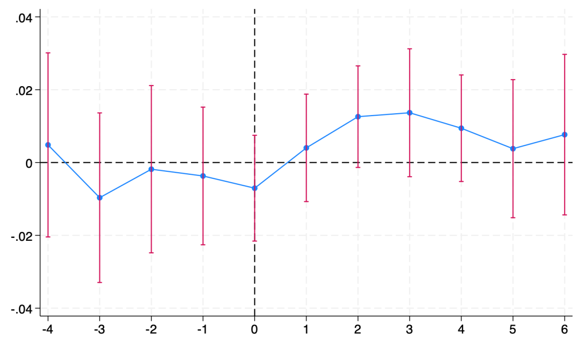
Figure 7: Effect of Fraud on Entrepreneurs' Future Founding: Dynamics Around Charge Year



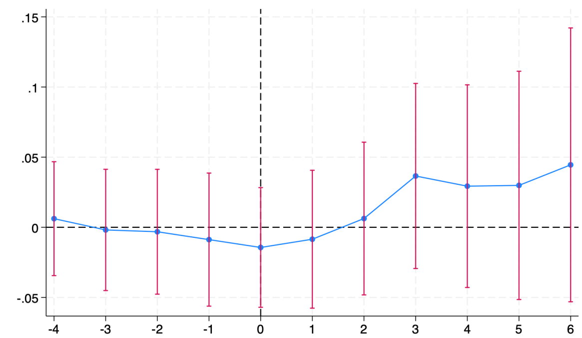
(a) Annual number of startups founded: stacked DID



(b) Cumulative number of startups founded: stacked DID



(c) Annual number of startups founded: imputation DID



(d) Cumulative number of startups founded: imputation DID

This figure shows the event study plots of the effect of fraud charges on entrepreneurs' subsequent founding activities, estimated using a matched DID. The sample is at the person-year level. Panels A and B estimate a stacked DID following Equation 3 (Cengiz et al., 2019), with stack × person fixed effects and stack × event-year fixed effects. Panels C and D estimate the imputation-based DID from Borusyak et al. (2024) with person, year, and event-year fixed effects. The dependent variable in Panels A and C is the number of new startups founded by a person in a year. The dependent variable in Panels B and D is the cumulative number of startups a person has founded as of a year. For each treated (i.e., fraudulent) founder in the year before the charge, we match them to one control founder, based on the same number of founded startups in the past, tenure at current startup, exit status of current startup, sector of current startup, gender, top-school indicator, and closest in age. Standard errors are clustered at the person level. Bars represent 95th confidence intervals.

Table 1: Summary Statistics on Fraud Cases

Panel A. Case Count by Source

SEC	126
DOJ	89
Westlaw	234
SCAC	183
Total unique enforced cases	538
Dismissed cases from Westlaw	99
Allegations & investigations from news	17
Total unique non-enforced cases	116
Total cases	654

Panel B. SEC Cases

	Obs	Mean	Std Dev	P5	P50	P95
Fraud duration (months)	119	29.731	24.311	1.000	24.000	78.000
Fined amount ($k)	69	9556.7	34706.1	35.0	1000.0	65000.0
Judgement: fine	117	0.590	0.494	0.000	1.000	1.000
Judgement: prison	117	0.034	0.182	0.000	0.000	0.000
Judgement: disgorgement	117	0.650	0.479	0.000	1.000	1.000
Judgement: ban	117	0.581	0.495	0.000	1.000	1.000
Misrepresented: financials	126	0.698	0.461	0.000	1.000	1.000
Misrepresented: product	126	0.214	0.412	0.000	0.000	1.000
Misrepresented: use of funds	126	0.214	0.412	0.000	0.000	1.000
Victim: investor	126	0.905	0.295	0.000	1.000	1.000
Victim: government	126	0.040	0.196	0.000	0.000	0.000
Victim: public	126	0.056	0.230	0.000	0.000	1.000
Victim: other	126	0.056	0.230	0.000	0.000	1.000

Panel C. DOJ Cases

	Obs	Mean	Std Dev	P5	P50	P95
Fraud duration (months)	76	40.132	39.670	3.000	33.000	90.000
Fraud type: bank	89	0.079	0.271	0.000	0.000	1.000
Fraud type: wire	89	0.854	0.355	0.000	1.000	1.000
Fraud type: mail	89	0.056	0.232	0.000	0.000	1.000
Fraud type: corporate	89	0.101	0.303	0.000	0.000	1.000
Fraud type: federal	89	0.135	0.343	0.000	0.000	1.000
Fraud type: securities	89	0.326	0.471	0.000	0.000	1.000
Fraud type: tax	89	0.022	0.149	0.000	0.000	0.000
Fraud type: other	89	0.022	0.149	0.000	0.000	0.000
Sentence: months of prison	38	85.763	91.344	12.000	54.000	340.000
Sentence: fined amount ($k)	46	19082.4	81865.3	50.0	1875.0	34370.0
Sentence: fine	89	0.517	0.503	0.000	1.000	1.000
Sentence: prison	89	0.427	0.497	0.000	0.000	1.000
Sentence: forfeiture	60	0.150	0.360	0.000	0.000	1.000
Sentence: supervised release	60	0.350	0.481	0.000	0.000	1.000
Sentence: community service	60	0.033	0.181	0.000	0.000	0.000
Victim: investor	89	0.663	0.475	0.000	1.000	1.000
Victim: government	89	0.180	0.386	0.000	0.000	1.000
Victim: public	89	0.225	0.420	0.000	0.000	1.000
Victim: other	89	0.101	0.303	0.000	0.000	1.000

Table 1: Summary Statistics on Fraud Cases (Continued)

Panel D. Westlaw Cases

	Obs	Mean	Std Dev	P5	P50	P95
Fraud duration (months)	234	28.134	32.544	0.833	14.233	94.933
Fraud type: fiduciary duty	234	0.316	0.466	0.000	0.000	1.000
Fraud type: wire	234	0.051	0.221	0.000	0.000	1.000
Fraud type: corporate	234	0.402	0.491	0.000	0.000	1.000
Fraud type: securities	234	0.380	0.487	0.000	0.000	1.000
Fraud type: other	234	0.150	0.357	0.000	0.000	1.000
Misrepresented: financials	234	0.564	0.497	0.000	1.000	1.000
Misrepresented: product	234	0.231	0.422	0.000	0.000	1.000
Misrepresented: use of funds	234	0.115	0.320	0.000	0.000	1.000
Misrepresented: other	234	0.090	0.286	0.000	0.000	1.000
Victim: investor	234	0.628	0.484	0.000	1.000	1.000
Victim: public	234	0.124	0.330	0.000	0.000	1.000
Victim: corporate	234	0.004	0.065	0.000	0.000	0.000
Victim: other	234	0.162	0.370	0.000	0.000	1.000
Plaintiff: investor	234	0.385	0.488	0.000	0.000	1.000
Plaintiff: government	234	0.137	0.344	0.000	0.000	1.000
Plaintiff: business partner/customer	234	0.265	0.442	0.000	0.000	1.000
Plaintiff: other	234	0.214	0.411	0.000	0.000	1.000

Panel E. Class Action Cases

	Obs	Mean	Std Dev	P5	P50	P95
Fraud duration (months)	183	12.182	10.927	0.567	9.267	29.633
Case settled	183	0.497	0.501	0.000	0.000	1.000
Settled amount ($k)	183	13854.1	63571.8	0.000	0.000	54500.0
1934 Act Sec10b: antifraud	183	0.776	0.418	0.000	1.000	1.000
1934 Act Sec14a: proxy solicitation fraud	183	0.082	0.275	0.000	0.000	1.000
1934 Act Sec14e: tender offer antifraud	183	0.011	0.104	0.000	0.000	0.000
1934 Act Sec20a: control person liability	183	0.809	0.394	0.000	1.000	1.000
1933 Act Sec11: registration statement	183	0.475	0.501	0.000	0.000	1.000
1933 Act Sec12a2: prospectus & oral commu.	183	0.230	0.422	0.000	0.000	1.000
1933 Act Sec15: control person liability	183	0.464	0.500	0.000	0.000	1.000

Panel F. Dismissed Cases From Westlaw and Allegations/Investigations from News

	Obs	Mean	Std Dev	P5	P50	P95
Fraud duration (months)	110	19.655	24.322	1.000	9.300	73.067
Allegations from news	116	0.147	0.355	0.000	0.000	1.000
Fraud type: fiduciary duty	116	0.095	0.294	0.000	0.000	1.000
Fraud type: wire	116	0.043	0.204	0.000	0.000	0.000
Fraud type: corporate	116	0.414	0.495	0.000	0.000	1.000
Fraud type: securities	116	0.397	0.491	0.000	0.000	1.000
Fraud type: other	116	0.138	0.346	0.000	0.000	1.000
Misrepresented: financials	116	0.534	0.501	0.000	1.000	1.000
Misrepresented: product	116	0.345	0.477	0.000	0.000	1.000
Misrepresented: use of funds	116	0.017	0.131	0.000	0.000	0.000
Misrepresented: other	116	0.103	0.306	0.000	0.000	1.000
Victim: investors	116	0.483	0.502	0.000	0.000	1.000
Victim: public	116	0.190	0.394	0.000	0.000	1.000
Victim: corporate	116	0.043	0.204	0.000	0.000	0.000
Victim: other	116	0.267	0.444	0.000	0.000	1.000
Plaintiff: investor	99	0.293	0.457	0.000	0.000	1.000
Plaintiff: government	99	0.040	0.198	0.000	0.000	0.000
Plaintiff: business partner/customer	99	0.424	0.497	0.000	0.000	1.000
Plaintiff: other	99	0.242	0.431	0.000	0.000	1.000

This table presents the summary statistics for our sample of venture fraud cases. Panel A reports the number of cases by source. The sample contains 654 unique cases, 538 of which are enforced and 116 non-enforced (i.e., dismissed, ongoing allegations and investigations). We restrict to US VC-backed startups founded since 2000. For startups that completed an IPO, we restrict the SEC, DOJ, and Westlaw samples to cases in which the fraud began before the IPO, and the class action sample to cases with a class period beginning within two years of the IPO. Panels B to F present case characteristics for SEC, DOJ, Westlaw, securities class action, and dismissed/allegation cases, respectively.

Table 2: Summary Statistics on the Prediction Panel

	Obs	Mean	Std Dev	P5	P50	P95
$\mathbb{1}$(Fraud start) × 100	992,613	0.060	2.448	0.000	0.000	0.000
Firm age	992,613	5.796	5.010	0.000	5.000	16.000
Ln(valuation)	163,985	3.039	1.869	-0.082	3.003	6.188
Ln(raised)	470,579	0.793	2.443	-3.689	0.956	4.578
Board size	244,476	3.261	2.154	1.000	3.000	7.000
Ln(1+patents)	992,613	0.036	0.223	0.000	0.000	0.000
Board founder controlled	234,758	0.527	0.499	0.000	1.000	1.000
Investors' converted ownership	269,765	0.457	0.208	0.132	0.449	0.804
Ln(unique investors)	478,740	1.337	0.994	0.000	1.386	2.996
Investor_%non-IVC	635,294	0.570	0.372	0.000	0.583	1.000
Team_solo	606,218	0.487	0.500	0.000	0.000	1.000
Team_serial%	606,218	0.132	0.296	0.000	0.000	1.000
Team_topschool%	479,851	0.262	0.403	0.000	0.000	1.000
Team_female%	606,218	0.132	0.293	0.000	0.000	1.000
Team_age at founding	356,861	36.544	9.925	23.000	35.000	55.000
Missing valuation	992,613	0.835	0.371	0.000	1.000	1.000
Missing board info	992,613	0.763	0.425	0.000	1.000	1.000
Missing investor type	992,613	0.360	0.480	0.000	0.000	1.000
Missing term sheet info	992,613	0.728	0.445	0.000	1.000	1.000
Missing team info	992,613	0.389	0.488	0.000	0.000	1.000
Missing team edu info	992,613	0.567	0.495	0.000	1.000	1.000

This table reports the summary statistics for the hazard-style firm panel used in our predictive analysis in Table 5. The sample is at the firm-year level. For non-fraud firms, the panel includes all years from firm founding to the earlier of 2024 or firm exit. For fraud firms the panel goes from the founding year to the fraud start year. Missing values are unfilled in this table to show proper summary statistics but are filled with zero in Table 5.

Table 3: Summary Statistics on IPO Firms and Class Action Sample

Panel A: Sample of All U.S. IPOs (2002–2023)

	Obs	Mean	Std Dev	P5	P50	P95
VC-backed	2777	0.411	0.492	0.000	0.000	1.000
PE-backed	2777	0.172	0.378	0.000	0.000	1.000
Class action	2777	0.107	0.310	0.000	0.000	1.000
Class action start within 30 days of IPO	2777	0.038	0.192	0.000	0.000	0.000
Class action start within 1y IPO	2777	0.055	0.229	0.000	0.000	1.000
Class action start within 2y IPO	2777	0.071	0.257	0.000	0.000	1.000
NASDAQ Stock	2777	0.662	0.473	0.000	1.000	1.000
Log(Total Assets)_$preIPO$	1286	4.804	2.136	1.177	4.760	8.199
Revenue Growth_$preIPO$	1251	89.969	438.990	-6.088	17.629	337.849

Panel B: Sample of Class Actions that Start within 2 years of IPO

	Obs	Mean	Std Dev	P5	P50	P95
VC-backed	226	0.611	0.489	0.000	1.000	1.000
PE-backed	226	0.301	0.460	0.000	0.000	1.000
Securities fraud	226	0.730	0.445	0.000	1.000	1.000
Class action start within 30 days of IPO	226	0.544	0.499	0.000	1.000	1.000
Class action start within 1y IPO	226	0.783	0.413	0.000	1.000	1.000
Class action start within 2y IPO	226	1.000	0.000	1.000	1.000	1.000
IPO year	226	2015	5.431	2005	2016	2021
Start class period year	226	2015	5.442	2005	2017	2022
Filing year	226	2016	5.568	2007	2018	2024
SPAC IPO	226	0.093	0.291	0.000	0.000	1.000
NASDAQ Stock	226	0.628	0.484	0.000	1.000	1.000
NYSE Stock	226	0.372	0.484	0.000	0.000	1.000
Log(Total Assets)_$preIPO$	152	5.399	2.031	1.762	5.491	8.906
Revenue Growth_$preIPO$	150	241.676	962.101	-4.010	50.856	615.226
Settlement amount (M$)	143	22.148	72.325	0.650	7.500	71.000

This table reports the summary statistics for the IPO and class action sample used in Column 1 of Table 4, Panel A.

Table 4. Class Actions Against VC- vs Non-VC-Backed Firms

Panel A: Full period (All IPOs between 2002–2023)

Start Class Period within:	30 days of IPO	1 year of IPO	2 years of IPO	30 days of IPO	1 year of IPO	2 years of IPO
	(1)	(2)	(3)	(4)	(5)	(6)
VC-backed	0.0402***	0.0413***	0.0426***	0.0578**	0.0730***	0.0932***
	(0.010)	(0.010)	(0.014)	(0.022)	(0.022)	(0.023)
Log(Total Assets)_*preIPO*				0.0070*	0.0091*	0.0104*
				(0.004)	(0.005)	(0.006)
Revenue Growth_*preIPO*				0.0000	0.0000	0.0000**
				(0.000)	(0.000)	(0.000)
NASDAQ Stock				-0.0119	-0.0305	-0.0417*
				(0.022)	(0.023)	(0.024)
SIC-2 Sector FE × IPO Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.232	0.219	0.225	0.262	0.253	0.259
N	2,777	2,777	2,777	1,192	1,192	1,192
Outcome Mean	0.0382	0.0555	0.0709	0.0638	0.0856	0.1032

Panel B: Sample Split between the Post Dot-com bubble period (2002–2012) and the recent period (2013–2023)

	IPOs between 2002–2012			IPOs between 2013–2023		
Start Class Period within:	30 days of IPO	1 year of IPO	2 years of IPO	30 days of IPO	1 year of IPO	2 years of IPO
	(1)	(2)	(3)	(4)	(5)	(6)
VC-backed	0.0652	0.0531	0.0584	0.0519**	0.0786***	0.1029***
	(0.038)	(0.039)	(0.039)	(0.023)	(0.024)	(0.026)
Log(Total Assets)_*preIPO*	-0.0013	0.0026	-0.0056	0.0096*	0.0112*	0.0154*
	(0.006)	(0.009)	(0.008)	(0.006)	(0.006)	(0.008)
Revenue Growth_*preIPO*	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
NASDAQ Stock	-0.0096	-0.0105	-0.0319	-0.0184	-0.0436	-0.0534*
	(0.047)	(0.048)	(0.044)	(0.020)	(0.029)	(0.028)
SIC-2 Sector FE × IPO Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.311	0.306	0.298	0.243	0.234	0.246
N	391	391	391	801	801	801
Outcome Mean	0.0563	0.0614	0.0818	0.0674	0.0974	0.1136

Table 4. Class Actions Against VC- vs Non-VC-Backed Firms (Continued)

Panel C: VC-backed IPOs versus PE-backed IPOs (IPOs between 2002–2023)

Start Class Period within:	30 days of IPO (1)	1 year of IPO (2)	2 years of IPO (3)	30 days of IPO (4)	1 year of IPO (5)	2 years of IPO (6)
VC-backed	0.0480*	0.0692***	0.0687**	0.0987**	0.1355***	0.1572***
	(0.025)	(0.024)	(0.026)	(0.045)	(0.042)	(0.038)
Log(Total Assets)_*preIPO*				0.0067	0.0118	0.0138
				(0.005)	(0.008)	(0.009)
Revenue Growth_*preIPO*				0.0001	0.0000	0.0000
				(0.000)	(0.000)	(0.000)
NASDAQ Stock				-0.0164	-0.0408	-0.0570**
				(0.028)	(0.025)	(0.024)
SIC-2 Sector FE × IPO Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.246	0.236	0.244	0.253	0.242	0.249
N	1,204	1,204	1,204	841	841	841
Outcome Mean	0.0623	0.0839	0.1005	0.0725	0.0999	0.1165

This table reports OLS estimates of the relationship between (formerly) VC-backed status and the likelihood of a securities class action lawsuit within 30 days to two years of the firm's IPO. Panel B splits the sample into two subperiods. Panel C restricts the control group to PE-backed IPOs. Timing of litigation relative to IPO is defined based on the start date of the class period. The sample includes all U.S. IPOs between 2002 and 2023 from SDC New Issues. Class actions are from the SCAC database and exclude dismissed cases. Columns 4-6 include controls for firm size (log total assets) the year before IPO, revenue growth from the year before IPO to IPO year, and an indicator for Nasdaq listing. All regressions include SIC-2 digit interacted with IPO year fixed effects. SIC codes are defined at IPO. Standard errors are clustered at the SIC-2digit level and are reported in parentheses. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 5: Panel Prediction of Fraud Among VC-Backed Firms

Dependent Variable:	$\mathbb{1}$(Fraud start) $\times$ 100			
	(1)	(2)	(3)	(4)
Board founder controlled	0.059***	0.060***	0.061***	0.075***
	(0.017)	(0.018)	(0.018)	(0.017)
Investors' converted ownership	-0.117***	-0.116***	-0.118***	-0.149***
	(0.032)	(0.032)	(0.031)	(0.055)
Ln(unique investors)	0.026***	0.026***	0.025***	0.077***
	(0.005)	(0.005)	(0.005)	(0.010)
Investor_%non-IVC	0.053***	0.054***	0.050***	0.097***
	(0.009)	(0.009)	(0.009)	(0.015)
Firm age	-0.001	-0.001	-0.002	
	(0.001)	(0.001)	(0.001)	
Ln(valuation)	0.136***	0.136***	0.135***	0.116***
	(0.018)	(0.018)	(0.018)	(0.019)
Ln(raised)	0.020***	0.020***	0.020***	0.024***
	(0.003)	(0.003)	(0.003)	(0.003)
Board size	0.026***	0.027***	0.027***	0.045***
	(0.007)	(0.007)	(0.007)	(0.009)
Ln(1+patents)	0.090	0.089	0.090	0.144**
	(0.059)	(0.059)	(0.060)	(0.071)
FE: ind-year	Yes	Yes	Yes	Yes
FE: ind-1st round year	No	No	Yes	No
FE: ind-year, firm	No	No	No	Yes
Team characteristic controls	No	Yes	Yes	Yes
Missing value indicators	Yes	Yes	Yes	Yes
N	992613	992613	992613	992613
R^2	0.0038	0.0038	0.0044	0.0881
Outcome Mean	0.060	0.060	0.060	0.060

The sample is a hazard-style panel of firm-years for US VC-backed firms founded since 2000. For non-fraud firms, the panel includes all years from firm founding to the earlier of 2024 or exit. For fraud firms the panel goes from the founding year to the fraud start year. The dependent variable is 100 in the fraud start year of fraud firms and is 0 otherwise. We restrict to SEC/DOJ/Westlaw cases where the fraud start year is no later than the IPO year, and class action cases with a class period start date within 2 years of the IPO. The regression sample contains 595 fraud cases. Missing value indicators are included, and their coefficients are reported in Table A.5 for brevity. Standard errors are clustered at the industry level and are reported in parentheses. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 6: Panel Prediction of Fraud Among VC-Backed Firms: Governance vs Founder Traits

Panel A: Coefficients on Team Characteristics

Dependent Variable:	$\mathbb{1}$(Fraud start) $\times$ 100	
	(1)	(2)
Team_solo	0.007	0.012*
	(0.006)	(0.007)
Team_serial%	0.024***	0.024***
	(0.008)	(0.008)
Team_topschool%	0.018**	0.016*
	(0.008)	(0.008)
Team_female%	0.006	0.004
	(0.016)	(0.016)
Team_age at founding	-0.001***	-0.001***
	(0.000)	(0.000)
Board founder controlled		0.060***
		(0.018)
Investors' converted ownership		-0.116***
		(0.032)
Ln(unique investors)		0.026***
		(0.005)
Investor_%non-IVC		0.054***
		(0.009)
FE: ind-year	Yes	Yes
Other controls in Table 5	Yes	Yes
Missing value indicators	Yes	Yes
N	992613	992613
R^2	0.004	0.004
Outcome Mean	0.060	0.060

Panel B: Compare Explanatory Power

FE	Governance Var Partial R^2	Team Var Partial R^2	Ratio
Ind-yr	0.00013	0.00002	5.15
Ind, yr	0.00013	0.00002	5.12
None	0.00012	0.00002	5.10

Panel A reports the coefficients on team characteristics that were included in Table 5 but unreported for brevity. Column 1 includes team characteristics but excludes governance variables, while column 2 corresponds to column 2 of Table 5 with governance variables added back. Other controls (firm age, valuation, raised amount, board size, patenting, and missing-value indicators) are included but are unreported for brevity. Panel B compares the partial R^2 of the four governance versus the five team variables. Governance variables include board founder control, investor ownership, investor count, and non-IVC%. Team variables include solo, serial, top school, female, and age at founding. The partial R^2 for governance (team) variables is calculated based on column 1 of Table 5 (column 1 of this table), relative to a benchmark model without these variables. The last column reports the ratio between the partial R^2 of governance variables and the partial R^2 of team variables. Standard errors are clustered at the industry level and are reported in parentheses. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 7: Panel Prediction of Fraud Among VC-Backed Firms: Alternative Fraud Samples

Dependent Variable:	$\mathbb{1}$(Fraud start) $\times$ 100			
Sample:	Dropping Non-Enforced Cases		Dropping Class Actions	
	(1)	(2)	(3)	(4)
Board founder controlled	0.040***	0.042***	0.024*	0.026**
	(0.013)	(0.013)	(0.012)	(0.013)
Investors' converted ownership	-0.101***	-0.102***	-0.066***	-0.067***
	(0.033)	(0.033)	(0.020)	(0.020)
Ln(unique investors)	0.023***	0.022***	0.018***	0.019***
	(0.004)	(0.004)	(0.003)	(0.003)
Investor_%non-IVC	0.045***	0.042***	0.038***	0.036***
	(0.009)	(0.008)	(0.007)	(0.008)
Firm age	-0.001**	-0.002**	-0.001*	-0.002*
	(0.000)	(0.001)	(0.001)	(0.001)
Ln(valuation)	0.121***	0.121***	0.082***	0.082***
	(0.017)	(0.017)	(0.012)	(0.012)
Ln(raised)	0.016***	0.016***	0.010***	0.011***
	(0.003)	(0.004)	(0.002)	(0.002)
Board size	0.020***	0.021***	0.007**	0.008**
	(0.005)	(0.005)	(0.003)	(0.003)
Ln(1+patents)	0.071	0.072	0.032	0.032
	(0.058)	(0.059)	(0.028)	(0.028)
FE: ind-year	Yes	Yes	Yes	Yes
FE: ind-1st round year	No	Yes	No	Yes
Team characteristic controls	Yes	Yes	Yes	Yes
Missing value indicators	Yes	Yes	Yes	Yes
N	991731	991731	991256	991256
R^2	0.004	0.004	0.002	0.003
Outcome Mean	0.050	0.050	0.046	0.046

This table demonstrates the robustness of Table 5 to using two alternative samples of fraud. Columns 1 and 2 drop non-enforced cases—i.e., dismissed cases identified from Westlaw and allegations or investigations identified from news; the sample contains 491 cases. Columns 3 and 4 drop class action cases; the sample contains 456 cases. In all columns, the sample is a hazard-style panel of firm-years for US VC-backed firms founded since 2000. For non-fraud firms, the panel includes all years from firm founding to the earlier of 2024 or exit. For fraud firms, the panel goes from the founding year to the fraud start year. The dependent variable is 100 in the fraud start year for fraud firms and is 0 otherwise. Missing value indicators are included but are not reported for brevity. Standard errors are clustered at the industry level and are reported in parentheses. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 8: Cross-Sectional Prediction of Class Action Suit Among VC-Backed IPOs

Dependent Variable:	$\mathbb{1}$(Fraud start) $\times$ 100		
	(1)	(2)	(3)
Board founder controlled	3.414**		3.538**
	(1.603)		(1.691)
DualClassIPO	5.960**		5.858**
	(2.731)		(2.166)
Investors' converted ownership	-6.710		-6.713*
	(4.133)		(3.882)
Ln(unique investors)	1.076		1.184
	(0.719)		(0.694)
Investor_%non-IVC	4.450**		4.323**
	(2.114)		(2.030)
Firm age	-0.190	-0.165	-0.187
	(0.134)	(0.164)	(0.114)
Ln(valuation)	4.187***	4.600***	4.220***
	(0.477)	(0.589)	(0.628)
Ln(raised)	1.006**	1.075***	1.022***
	(0.469)	(0.372)	(0.348)
Board size	0.069	-0.256	0.048
	(0.287)	(0.174)	(0.241)
Ln(1+patents)	0.817	1.083	0.837
	(0.980)	(1.177)	(1.029)
Team_solo		1.356	1.638
		(1.033)	(1.260)
Team_serial%		1.621	1.552
		(1.033)	(1.532)
Team_topschool%		-0.224	-0.268
		(2.336)	(1.537)
Team_female%		5.826	5.872*
		(4.111)	(3.385)
Team_age at founding		-0.089*	-0.082**
		(0.045)	(0.039)
FE: ind, yr	Yes	Yes	Yes
Missing value indicators	Yes	Yes	Yes
N	2475	2475	2475
R^2	0.084	0.080	0.086
Outcome Mean	7.322	7.322	7.322

The sample is a cross-section of US VC-backed firms founded since 2000 that have gone public. The dependent variable is 100 for fraud firms involved in class actions within 2 years of IPO and is 0 otherwise. The sample contains 181 fraud cases from class action suits. Missing value indicators are included but are not reported for brevity. Standard errors are clustered at the industry level and are reported in parentheses. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 9: VC Market Condition at Initial VC Round and Future Fraud

Dependent variable:	$\mathbb{1}$(Future Fraud) × 100			
Model:	OLS	OLS	2SLS	2SLS
	(1)	(2)	(3)	(4)
Industry-year avg. valuation multiple	0.203*	0.222*	1.464**	1.481**
	(0.116)	(0.118)	(0.584)	(0.652)
Firm age		0.004		0.005
		(0.013)		(0.013)
Ln(valuation)		0.320***		0.318***
		(0.069)		(0.068)
Ln(raised)		0.171***		0.175***
		(0.027)		(0.029)
Ln(1+patents)		(0.339)		(0.334)
		(0.320)		(0.322)
Missing valuation		0.350***		0.349***
		(0.104)		(0.103)
First-stage coff on IV:				
Buyout funds raised in t-3 to t-1 ($b)			0.010***	0.010***
			(0.001)	(0.001)
First stage KP F-stat			64.2	64.1
FE: ind	Yes	Yes	Yes	Yes
FE: state-yr	Yes	Yes	Yes	Yes
N	86428	86428	86428	86428
R^2	0.006	0.009	-0.014	-0.011
Outcome mean	0.698	0.698	0.698	0.698

This table shows that hot VC market conditions at firms' initial financing rounds predict future fraud. The sample is at the firm level and covers initial VC rounds between 2000 and 2021. The dependent variable is a dummy indicating whether the firm is ever involved in fraud, multiplied by 100. We proxy for VC market hotness at the industry × initial-round-year level using average first-round valuation multiple. Specifically, we take the average of the ratio of post-money valuation to cumulative amount raised across all deals labeled seed or early stage in that industry × initial round year. Industry is defined as a firm's primary sector in PitchBook (7 sectors). Columns 1 and 2 are OLS. Columns 3 and 4 are 2SLS estimates where we instrument the industry-year valuation multiple with the total capital (in $ billions) raised by buyout funds in that industry over the prior three years, following the approach in Nanda and Rhodes-Kropf (2013). Firm-level controls include firm age, post-money valuation, total amount raised, patent count, and a missing valuation indicator, all measured at a firm's initial VC round. All columns include industry fixed effects and state-year fixed effects. Standard errors are clustered at the industry level and are reported in parentheses. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 10: Future Founding of VC-Backed Startups by Fraudulent Entrepreneurs

Panel A: Baseline DID Estimates

Dependent variable:	No. of startups founded (1)	Cumulative no. of startups founded (2)	No. of startups founded (3)	Cumulative no. of startups founded (4)
Treated × Post	0.007*	0.006	0.007	0.009
	(0.004)	(0.013)	(0.007)	(0.019)
Model	Stacked DID		Imputation-based DID	
FEs	stack×person, stack×event-year		person, year, event-year	
N	17468	17468	17468	17468
R^2	0.608	0.908	-	-
Outcome mean	0.042	1.268	0.042	1.268

Panel B: Likelihood of Returning to Original Investors for the Next Startup

	Control (1)	Treated (2)	P-val of t-test of control > treated (3)
Avg. $\mathbb{1}$(any investor overlap)	40.8%	27.6%	0.037
Avg. investor overlap share	5.08%	0.47%	0.006

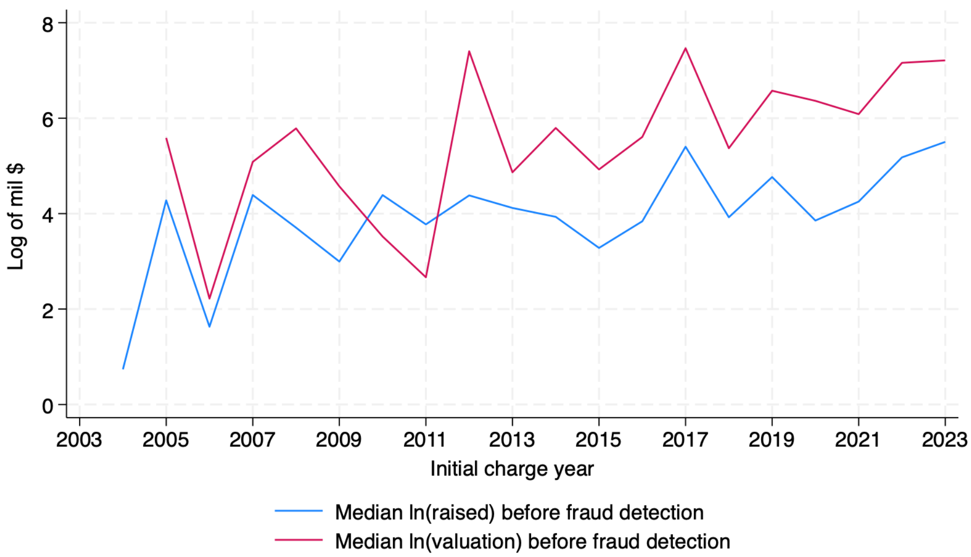
Panel C. Subsamples by Visibility and Severity

Dependent variable:	No. of startups founded			
Case type:	Major media coverage (1)	No major media coverage (2)	DOJ/SEC (3)	Non-DOJ/SEC (4)
Treated × Post	0.018***	-0.001	0.005	0.007*
	(0.006)	(0.004)	(0.007)	(0.004)
Model	Stacked DID			
FEs	stack×person, stack×event-year			
N	7376	10092	4074	10950
R^2	0.607	0.609	0.629	0.547
Outcome mean	0.042	0.042	0.049	0.044

Panel A reports the difference-in-differences estimates for the effect of fraud revelation (i.e., initial charge) on founders' subsequent founding of VC-backed startups. The sample is at the person-year level, focusing on a fixed event window from 4 years before to 6 years after initial charge. Columns 1-2 estimate a stacked DID following Cengiz et al. (2019). Columns 3-4 use the imputation-based DID from Borusyak et al. (2024). The dependent variable in Columns 1 and 3 is the number of new startups founded by a person in a year. The dependent variable in Columns 2 and 4 is the cumulative number of startups a person has founded as of a year. For each treated (i.e., fraudulent) founder in the year before the charge, we match them to a control founder, based on the same number of past founded startups, tenure at current startup, exit status of current startup, sector of current startup, gender, top school indicator, and closest in age. Panel B compares the degree of investor overlap between a founder's prior startup and her next startup for treated and control founders examined in Table 10. For treated founders, the prior startup is the fraud-involved startup. The sample restricts to founders that have launched a subsequent VC-backed startup after the event year (i.e., initial fraud detection year). The first row reports the average likelihood of the prior and the subsequent startups sharing at least one common investor. The second row reports the average investor overlap share, defined as the fraction of all possible investor pairs between the prior and the subsequent startups that involve the same investor. Column 3 shows the p-value of one-sided t-test of the control mean being larger than the treated mean. Panel C reproduces Panel A Column 1 on subsamples by case visibility or severity. Standard errors are clustered by person. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Internet Appendix A: Additional Figures and Tables

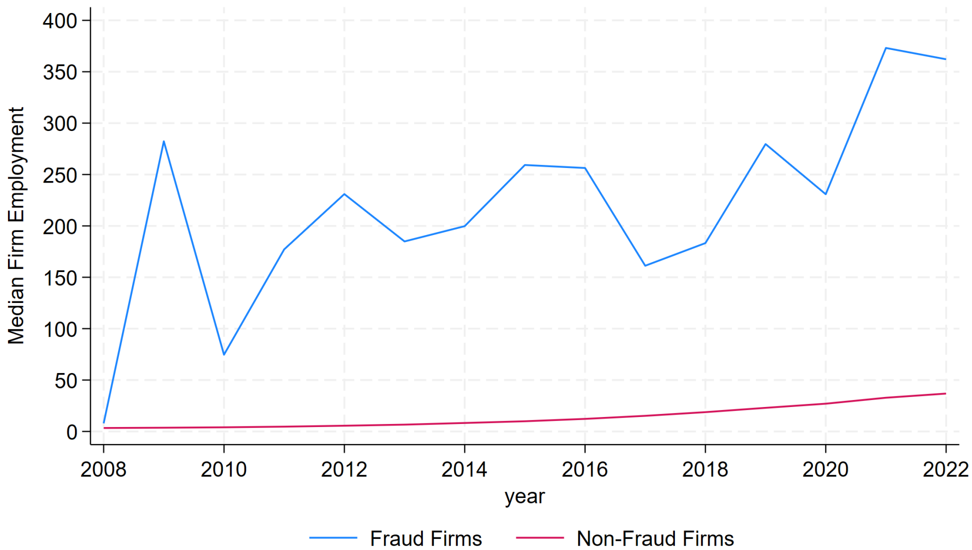
Figure A.1: Median Size of Fraudulent VC-Backed Startup Before Fraud Detection



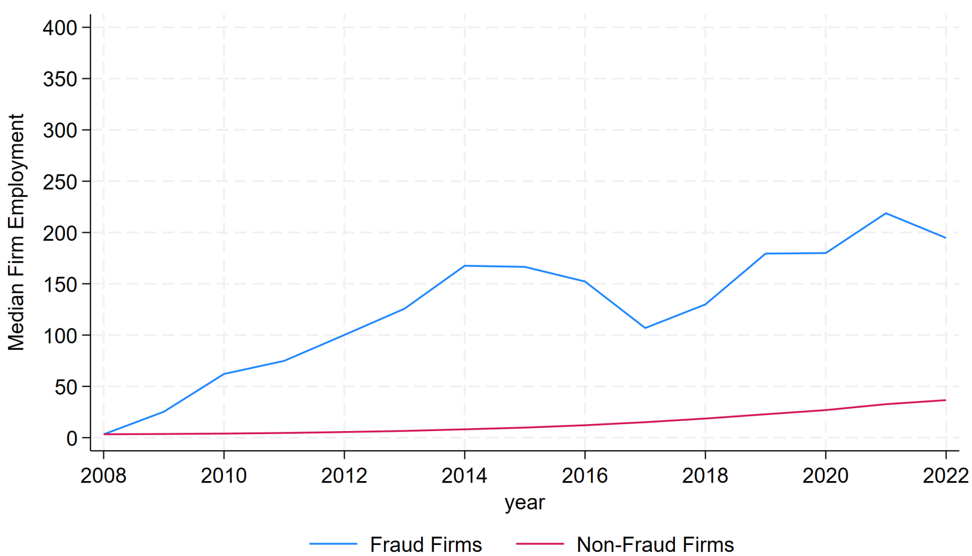
This figure plots, by fraud detection year, the median log raised amount and median log valuation of fraudulent VC-backed firms in the year before fraud detection.

Figure A.2: Employment Size of Fraud Firms

(a) All

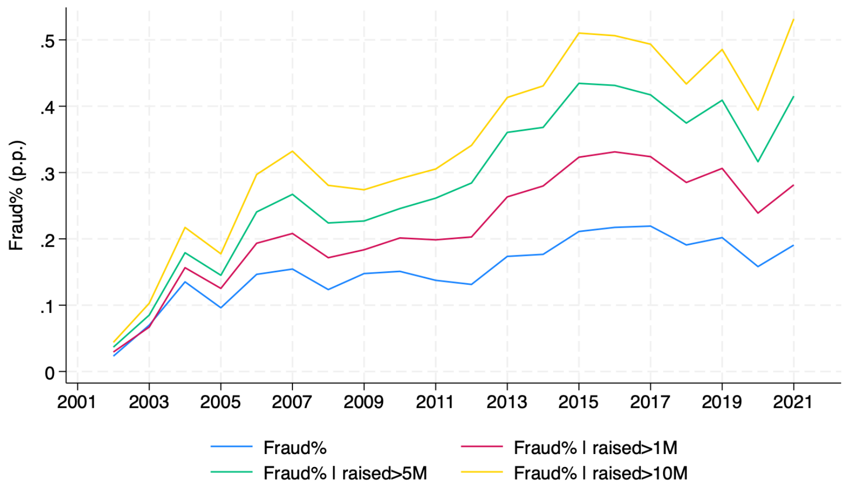


(b) Excluding class-action cases



These figures compare median employment at VC-backed fraud firms between fraud start year and initial charge year with the median employment at VC-backed non-fraud firms, using Revelio Labs employment panel data. We exclude firms whose fraud began before 2008, the first year for which Revelio employment panel data are available. Panel (a) reports results for all fraud firms, while Panel (b) reports results for fraud firms excluding class-action cases.

Figure A.3: Fraud Likelihood Among US VC-Backed: Excluding Non-Enforced Cases



This figure shows annual fraud commission rate among US VC-backed startups by minimum raised amount, restricting to enforced fraud cases only (i.e., excluding dismissed cases, allegations, and investigations).

Table A.1: Top Cases in Our Sample

Panel A. Top Class Action Cases Against VC-Backed Firms within Two Years of IPO (Ranked by Settlement Amount)

Company	**IPO Date**	**Key Allegations**	**Class Period / Case Filing**	**Settlement Outcome**
LendingClub Corp.	Dec 11, 2014	Misrepresenting loan quality and internal controls	Class period: Dec 11, 2014 – May 2016; Case filed 2016 (*In re LendingClub Sec. Litig.*, N.D. Cal.)	$125 million (2019)
Zuora, Inc.	Apr 12, 2018	Misrepresenting performance of the software; Overstated consumer demand	Class period: Apr 12, 2018 – May 30, 2019; Case filed 2019 (*Roberts v. Zuora, Inc.*, N.D. Cal.)	$75.5 million (2023)
Groupon, Inc.	Nov 4, 2011	Overstated revenue by using improper accounting methods (e.g., gross rather than net revenue)	Class period: Nov 4, 2011 – Mar 30, 2012; Case filed 2012 (*In re Groupon, Inc. Sec. Litig.*, N.D. Ill.)	$45 million (2016)
GoHealth, Inc.	Jul 15, 2020	IPO registration statement omitted elevated churn, customer retention problems, unfavorable revenue-sharing, and deteriorating product mix	Class period: Jul 14, 2020 – Jan 10, 2021; Case filed Sep 2020 (*In re GoHealth, Inc. Sec. Litig.*, N.D. Ill.)	$29.25 million (2024)
Lyft, Inc.	Mar 29, 2019	Misrepresenting the frequency of sexual assaults involving drivers and passengers. Misrepresenting the risks of treating drivers as independent contractors.	Class period: Mar 29, 2019 – post-IPO disclosures; Case filed Apr 2019 (*In re Lyft, Inc. Sec. Litig.*, N.D. Cal.)	$25 million (2022)
Zynga, Inc.	Dec 16, 2011	Withholding key metrics: declining bookings and user engagement in its core games; Overstated growth, especially on Facebook's platform.	Class period: Dec 16, 2011 – Jul 25, 2012; Case filed 2012 (*In re Zynga, Inc. Sec. Litig.*, N.D. Cal.)	$23 million (2015)
Bumble, Inc.	Sep 10, 2021 (SPO)	SPO registration statement failed to disclose declining paying users, negative impact of price increases, and Badoo payment transition issues	Class period: Sep 10, 2021 – Jan 24, 2022; Case filed Jan 2022 (*In re Bumble, Inc. Sec. Litig.*, S.D.N.Y.)	$18 million (2023)
Fitbit, Inc.	Jun 18, 2015	Misrepresenting the accuracy of their heart-rate technology. Fitbit knew the problem based on internal testing and consumer complaints prior IPO; Inflated demand and revenues	Class period: Jun 18, 2015 – Jan 6, 2016; Case filed Jan 2016 (*In re Fitbit, Inc. Sec. Litig.*, N.D. Cal.)	$15 million (2020)
Marrone Bio Innovations, Inc.	Aug 2, 2013	Misstated revenues and product viability; accounting fraud inflated growth	Class period: Aug 2, 2013 – Sep 2, 2014; Case filed 2014 (*In re Marrone Bio Innovations, Inc. Sec. Litig.*, E.D. Cal.)	$12 million (2016)

Table A.1: Top Cases in Our Sample (Continued)

Panel B. Top DOJ Cases (Ranked by Prison Length)

Company	**Year**	**Key allegations**	**Prison (months)**	**Other Sentence**	**Major VC backers**
FTX	2024	Misappropriation of customer funds; securities/wire fraud	300	$11.02 Billion in forfeiture + $12.7 Billion to compensate victims	Sequoia; Paradigm; Temasek; SoftBank; Tiger Global; OTPP
Theranos	2022	Misled investors and patients; blood-testing technology did not work as claimed	290	$54 Million in restitution	High-net-worth/family offices (e.g., Murdoch, Walton, DeVos)
Slync.io	2024	Defrauded investors; misappropriated $25M+ from company; wire fraud and money laundering	240	$65 Million in restitution	Goldman Sachs Growth; ACME Ventures; Blumberg Capital; 235 Capital Partners; Correlation Ventures; Gaingels
Sanovas, Inc.	2020	Siphoned $2.6M+ from company; wire fraud & money laundering; used funds to buy $2.5M home; false statements/obstruction	135	3 years supervised release	Undisclosed Investor
Bitwise Industries	2024	Defrauded investors and lenders using fabricated financials; wire-fraud conspiracy; losses >$100M	132	$114 Million in restitution	529 Ventures; Cap Table Coalition; Gaingels; Ingeborg Investments

Panel C. Top SEC Cases (Ranked by Fine Amount)

Company	**Year**	**Key allegations**	**Fine Amount ($k)**	**Major VC backers**
Terraform Labs	2024	Securities fraud tied to UST/LUNA collapse (civil penalties)	420000	Galaxy Digital; Coinbase Ventures; Pantera; Hashed
Luckin Coffee	2020	Fabricated revenue/expenses; accounting fraud	180000	Centurium Capital; Joy Capital; GIC; CICC
Nikola	2021	Exaggerated technology/products and business prospects (post-SPAC)	125000	(Institutional/PIPE heavy) ValueAct; Fidelity
Robinhood	2020	Misleading customers (payment for order flow disclosures / best execution)	65000	Sequoia; NEA; Ribbit; DST; Thrive
Telegram Group Inc.	2020	Unregistered offering of “Gram” digital tokens (Section 5 violations); court-ordered return of investor funds	18500	Manta Ray Ventures, Golden Falcon Capital, 3e Capital Group

Table A.2: Startup Litigation Digest Case Coverage in Our Sample

Case Title	Edition	Our Case Source	Why Not In Our Sample
HeadSpin & Manish Lachwani	1	Westlaw, SEC, DOJ	
Ozy Media & Carlos Watson	1	SEC, DOJ	
Bolt & Ryan Breslow	1	Westlaw	
Naveen Gupta v. Fungible	1	Not in sample	Missed because filings for this charge type are typically not accessible in Westlaw
Bitwise, Jake Soberal and Irma Olguin Jr.	1	SEC, DOJ	
Katerra & Michael Marks et al.	1	Not in sample	Missed because of no fiduciary/fraud keywords in dockets record.
Frank & Charlie Javice	1	SEC, DOJ	
FTX & Samuel Bankman-Fried	1	SEC, DOJ	
SoftBank v. IRL	2	SEC	
Leder v. Eaze	2	Westlaw	
Stimwave Technologies & Laura Perryman	3	SEC, DOJ	
Slync & Christopher Kirchner	3	SEC, DOJ	
OneTrust Governance Dispute	3	Not in sample	Initially captured the counterclaim case, but was later screened out by our data filters.
New Enterprise Associates v. Rich	3	Westlaw	
Binance & Changpeng Zhao	4	Westlaw	
Terraform & Do Kwon	4	Westlaw, SEC, DOJ	
Joonko & Ilit Raz	4	SEC, DOJ	
Toptal & Taso du Val Litigation	4	Not in sample	Initially captured, but was later screened out by our data filters.
Outcome Health & Rishi Shah et al.	4	SEC, DOJ	
SKAEL & Baba Nadimpalli	5	SEC, DOJ	
Medley Health	5	SEC	
U.S. v. Ruthia He and David Brody	5	DOJ	
U.S. v. Joanna Smith-Griffin	6	Not in sample	Outside sample period
U.S. v. Alexander Beckman and Valerie Lau Beckman	6	SEC, DOJ	

This table benchmarks our fraud sample against the set of cases reported by the Startup Litigation Digest Blog. We use the Startup Litigation Digest Blog from the UC Center for Business Law as a benchmark for identifying high-profile cases. This benchmark helps us evaluate the completeness of our data collection. We exclude cases from the blog that (1) sue investors (3 cases), (2) involve non-VC-backed companies (3 cases), or (3) were filed after 2025 (2 cases), leaving 24 eligible cases out of the original 32. The table summarizes which of the cases in the blog appear in our sample, the data source, and the reasons for any exclusion. Overall, our full sample covers about 80% of the cases from the blog.

Table A.3. Robustness Checks: Class Actions of VC- vs Non-VC-Backed Firms

Panel A: Class Action Filings (IPOs between 2002–2023)

Filing within:	1 year of IPO (1)	2 years of IPO (2)	3 years of IPO (3)	1 year of IPO (4)	2 years of IPO (5)	3 years of IPO (6)
VC-backed	0.0256***	0.0332**	0.0415***	0.0360*	0.0698***	0.0850***
	(0.009)	(0.013)	(0.014)	(0.018)	(0.025)	(0.024)
Log(Total Assets)_*preIPO*				0.0076	0.0092	0.0099
				(0.005)	(0.006)	(0.007)
Revenue Growth_*preIPO*				0.0001**	0.0000**	0.0000**
				(0.000)	(0.000)	(0.000)
NASDAQ Stock				0.0281	-0.0034	-0.0349
				(0.027)	(0.019)	(0.021)
SIC-2 Sector FE × IPO Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.229	0.210	0.212	0.301	0.272	0.263
N	2,777	2,777	2,777	1,192	1,192	1,192
Outcome Mean	0.0234	0.0465	0.0659	0.0378	0.0696	0.0965

Panel B: Including Dismissed Class Actions (IPOs between 2002–2023)

Start Class Period within:	30 days of IPO (1)	1 year of IPO (2)	2 years of IPO (3)	30 days of IPO (4)	1 year of IPO (5)	2 years of IPO (6)
VC-backed	0.0362**	0.0450***	0.0580***	0.0458*	0.0590*	0.0923**
	(0.014)	(0.016)	(0.017)	(0.026)	(0.029)	(0.039)
Log(Total Assets)_*preIPO*				0.0125***	0.0139***	0.0109*
				(0.004)	(0.004)	(0.006)
Revenue Growth_*preIPO*				-0.0000	0.0000	0.0000*
				(0.000)	(0.000)	(0.000)
NASDAQ Stock				-0.0158	-0.0282	-0.0616
				(0.024)	(0.032)	(0.048)
SIC-2 Sector FE × IPO Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.197	0.202	0.215	0.234	0.228	0.234
N	3,212	3,212	3,212	1,483	1,483	1,483
Outcome Mean	0.0492	0.0747	0.1090	0.0755	0.1072	0.1531

Panel C: Non-dismissed class actions with settlement amount ≥$3M

Start Class Period within:	30 days of IPO (1)	1 year of IPO (2)	2 years of IPO (3)	30 days of IPO (4)	1 year of IPO (5)	2 years of IPO (6)
VC-backed	0.0402***	0.0413***	0.0426***	0.0578**	0.0730***	0.0932***
	(0.010)	(0.010)	(0.014)	(0.022)	(0.022)	(0.023)
Log(Total Assets)_*preIPO*				0.0070*	0.0091*	0.0104*
				(0.004)	(0.005)	(0.006)
Revenue Growth_*preIPO*				0.0000	0.0000	0.0000**
				(0.000)	(0.000)	(0.000)
NASDAQ Stock				-0.0119	-0.0305	-0.0417*
				(0.022)	(0.023)	(0.024)
SIC-2 Sector FE × IPO Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.232	0.219	0.225	0.262	0.253	0.259
N	2,777	2,777	2,777	1,192	1,192	1,192
Outcome Mean	0.0382	0.0555	0.0709	0.0638	0.0856	0.1032

Panel D: Non-dismissed class actions filed before 2019 (IPOs between 2002–2019)

Start Class Period within:	30 days of IPO (1)	1 year of IPO (2)	2 years of IPO (3)	30 days of IPO (4)	1 year of IPO (5)	2 years of IPO (6)
VC-backed	0.0506***	0.0533***	0.0557***	0.0684***	0.0738***	0.0965***
	(0.012)	(0.014)	(0.019)	(0.022)	(0.024)	(0.026)
Log(Total Assets)_*preIPO*				0.0055	0.0117***	0.0143**
				(0.004)	(0.004)	(0.006)
Revenue Growth_*preIPO*				0.0000	0.0000	0.0001
				(0.000)	(0.000)	(0.000)
NASDAQ Stock				-0.0108	-0.0021	-0.0024
				(0.037)	(0.034)	(0.030)
SIC-2 Sector FE × IPO Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.251	0.244	0.251	0.312	0.279	0.289
N	1,868	1,868	1,868	847	847	847
Outcome Mean	0.0391	0.0503	0.0696	0.0579	0.0708	0.0909

Table A.3. Robustness Checks: Class Actions of VC- vs Non-VC-Backed Firms (Continued)

Panel E: Excluding SPAC IPOs (IPOs between 2002–2023)

Start Class Period within:	30 days of IPO (1)	1 year of IPO (2)	2 years of IPO (3)	30 days of IPO (4)	1 year of IPO (5)	2 years of IPO (6)
VC-backed	0.0379**	0.0451***	0.0521***	0.0580**	0.0730***	0.0931***
	(0.014)	(0.013)	(0.016)	(0.022)	(0.022)	(0.023)
Log(Total Assets)_*preIPO*				0.0071*	0.0091*	0.0103*
				(0.004)	(0.005)	(0.006)
Revenue Growth_*preIPO*				0.0000	0.0000	0.0000**
				(0.000)	(0.000)	(0.000)
NASDAQ Stock				-0.0119	-0.0305	-0.0416*
				(0.022)	(0.024)	(0.024)
SIC-2 Sector FE × IPO Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.230	0.237	0.247	0.262	0.253	0.259
N	2,155	2,155	2,155	1,190	1,190	1,190
Outcome Mean	0.0487	0.0650	0.0817	0.0639	0.0857	0.1034

Panel F: Full Period with Additional Controls (IPO between 2002–2023)

Start Class Period within:	30 days of IPO (1)	1 year of IPO (2)	2 years of IPO (3)	30 days of IPO (4)	1 year of IPO (5)	2 years of IPO (6)
VC-backed	0.0466***	0.0517***	0.0553***	0.0428*	0.0532**	0.0697***
	(0.012)	(0.014)	(0.017)	(0.022)	(0.022)	(0.021)
Ln(1+ Age at IPO)	0.0052	0.0053	0.0032	-0.0053	-0.0139	-0.0157
	(0.007)	(0.008)	(0.010)	(0.012)	(0.019)	(0.019)
Roll-up Shares	0.0623	0.0541	0.1043	0.0197	-0.0017	0.0372
	(0.053)	(0.055)	(0.075)	(0.074)	(0.077)	(0.099)
NASDAQ Stock	-0.0111	-0.0238	-0.0267	0.0017	-0.0156	-0.0235
	(0.017)	(0.019)	(0.019)	(0.024)	(0.026)	(0.024)
Ln(IPO proceeds)	0.0220***	0.0282***	0.0317***	0.0489***	0.0588***	0.0713***
	(0.004)	(0.005)	(0.006)	(0.009)	(0.015)	(0.022)
Log(Total Assets)_*preIPO*				-0.0120***	-0.0130**	-0.0164**
				(0.004)	(0.005)	(0.006)
Revenue Growth_*preIPO*				-0.0000	0.0000	0.0000
				(0.000)	(0.000)	(0.000)
SIC-2 Sector FE × IPO Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.243	0.230	0.239	0.274	0.266	0.277
N	2,378	2,378	2,378	1,141	1,141	1,141
Outcome Mean	0.0429	0.0627	0.0795	0.0666	0.0894	0.1078

Panel G: Full Period with SIC-3 digit FE (IPOs between 2002–2023)

Start Class Period within:	30 days of IPO (1)	1 year of IPO (2)	2 years of IPO (3)	30 days of IPO (4)	1 year of IPO (5)	2 years of IPO (6)
VC-backed	0.0492***	0.0458***	0.0429***	0.0514*	0.0618**	0.0743***
	(0.011)	(0.013)	(0.015)	(0.027)	(0.028)	(0.026)
Log(Total Assets)_*preIPO*				0.0032	0.0059	0.0086
				(0.004)	(0.006)	(0.008)
Revenue Growth_*preIPO*				0.0000	0.0001*	0.0000
				(0.000)	(0.000)	(0.000)
NASDAQ Stock				0.0072	-0.0228	-0.0420*
				(0.013)	(0.019)	(0.022)
SIC-3 FE × IPO Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.274	0.251	0.267	0.292	0.275	0.296
N	2,507	2,507	2,507	1,029	1,029	1,029
Outcome Mean	0.0347	0.0511	0.0646	0.0603	0.0816	0.0972

Table A.3. Robustness Checks: Class Actions of VC- vs Non-VC-Backed Firms (Continued)

Panel H. 2SLS Estimation

Start Class Period within:	30 days of IPO (1)	1 year of IPO (2)	2 years of IPO (3)
VC-backed	0.4797**	0.5412**	0.5710**
	(0.221)	(0.239)	(0.223)
Log(Total Assets)$_{preIPO}$	0.0142	0.0205	0.0223
	(0.011)	(0.015)	(0.017)
Revenue Growth$_{preIPO}$	0.0001	0.0000	0.0001
	(0.000)	(0.000)	(0.000)
NASDAQ Stock	-0.0430	-0.0690	-0.0886*
	(0.054)	(0.048)	(0.047)
1st stage coefficient:			
Local VC supply at founding	0.030***	0.030***	0.030***
	(0.007)	(0.007)	(0.007)
1st stage F-stat	10.6	10.6	10.6
SIC-2 Sector FE × IPO Year FE	Yes	Yes	Yes
N	1114	1114	1114
R^2	-0.266	-0.242	-0.202
Outcome Mean	0.067	0.091	0.108

This table shows the robustness of the results in Table 4. Panel A uses the filing date rather than the class period start date to define the timing of class actions relative to the IPO. Panel B reintroduces cases that were dismissed. Panel C restricts to class actions with settlement amounts exceeding $3 million. Panel D stops the sample period at 2019 to address potential truncation. Panel E excludes SPAC IPOs. Panel F adds additional controls—log firm age at IPO, log amount of IPO proceeds, and an indicator for rollup transactions. All regressions include SIC-2 digit industry interacted with IPO year fixed effects. Panel G includes SIC-3 digit industry interacted with IPO year fixed effects. Panel H reports 2SLS estimates, instrumenting VC-backed status with the average amount of venture capital invested in the firm's headquarters state in its founding year, following Hochberg (2012). Standard errors are clustered by industry and are reported in parentheses. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table A.4: Alternative Definitions of Fraud Start Year for Class Action Cases

Dependent Variable:	$\mathbb{1}$(Fraud start) × 100			
SCAC Fraud Start Year:	IPO Year		IPO Year - 1	
	(1)	(2)	(3)	(4)
Board founder controlled	0.060***	0.062***	0.049***	0.051***
	(0.017)	(0.017)	(0.016)	(0.017)
Investors' converted ownership	-0.133***	-0.136***	-0.090***	-0.092***
	(0.032)	(0.031)	(0.030)	(0.030)
Ln(unique investors)	0.025***	0.024***	0.027***	0.027***
	(0.005)	(0.005)	(0.005)	(0.005)
Investor_%non-IVC	0.053***	0.049***	0.045***	0.041***
	(0.009)	(0.009)	(0.009)	(0.009)
Firm age	-0.001	-0.001	-0.001**	-0.002**
	(0.001)	(0.001)	(0.001)	(0.001)
Ln(valuation)	0.166***	0.166***	0.137***	0.137***
	(0.022)	(0.022)	(0.016)	(0.016)
Ln(raised)	0.015***	0.015***	0.015***	0.016***
	(0.003)	(0.003)	(0.004)	(0.004)
Board size	0.022***	0.023***	0.017***	0.018***
	(0.006)	(0.006)	(0.005)	(0.005)
Ln(1+patents)	0.070	0.071	0.074*	0.075*
	(0.053)	(0.054)	(0.040)	(0.040)
FE: ind-year	Yes	Yes	Yes	Yes
FE: ind-1st round year	No	Yes	No	Yes
Team characteristic controls	Yes	Yes	Yes	Yes
Missing value indicators	Yes	Yes	Yes	Yes
N	992531	992531	992391	992391
R^2	0.005	0.005	0.004	0.004
Outcome Mean	0.060	0.060	0.060	0.060

This table demonstrates the robustness of Table 5 to using two alternative definitions of fraud start year for securities class action cases. Columns 1 and 2 set the fraud start year to IPO year for the class action cases, while Columns 3 and 4 set the fraud start year to the year before IPO year for these cases. In all columns, the sample is a hazard-style panel of firm-years for US VC-backed firms founded since 2000. For non-fraud firms, the panel includes all years from firm founding to the earlier of 2024 or exit. For fraud firms, the panel goes from the founding year to the fraud start year. The dependent variable is 100 in the fraud start year for fraud firms and is 0 otherwise. Missing value indicators are included but are not reported for brevity. Standard errors are clustered at the industry level and are reported in parentheses. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table A.5: Panel Prediction of Fraud Among VC-Backed Firms: Coefficients on Missing Value Indicators

Dependent Variable:	$\mathbb{1}$(Fraud start) × 100			
	(1)	(2)	(3)	(4)
Missing valuation	0.284***	0.284***	0.282***	0.280***
	(0.045)	(0.045)	(0.045)	(0.048)
Missing board info	0.123***	0.123***	0.128***	0.186***
	(0.029)	(0.029)	(0.029)	(0.041)
Missing investor type	0.036***	0.036***	0.035***	0.057***
	(0.009)	(0.009)	(0.010)	(0.012)
Missing term sheet info	0.014	0.014	0.014	0.020
	(0.013)	(0.013)	(0.012)	(0.026)
Missing team info	0.010**	0.017*	0.014	
	(0.005)	(0.008)	(0.009)	
Missing team edu info	0.007	-0.008	-0.006	
	(0.006)	(0.006)	(0.006)	
FE: ind-year	Yes	Yes	Yes	Yes
FE: ind-1st round year	No	No	Yes	No
FE: ind-year, firm	No	No	No	Yes
Governance variables	Yes	Yes	Yes	Yes
Team variables	No	Yes	Yes	Yes
N	992613	992613	992613	992613
R^2	0.004	0.004	0.004	0.088
Outcome Mean	0.060	0.060	0.060	0.060

This table reports the coefficients on missing value indicators included in Table 5. Standard errors are clustered at the industry level and are reported in parentheses. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table A.6: Panel Prediction of Fraud Among VC-Backed Firms: Decomposing Board Control into VC-Controlled vs Shared Control

Dependent Variable:	$\mathbb{1}$(Fraud start) $\times$ 100			
	(1)	(2)	(3)	(4)
Board VC controlled	-0.116***	-0.117***	-0.117***	-0.188***
	(0.041)	(0.041)	(0.040)	(0.048)
Board shared control	-0.034***	-0.035***	-0.037***	-0.029*
	(0.012)	(0.012)	(0.012)	(0.016)
Investors' converted ownership	-0.106***	-0.105***	-0.107***	-0.130**
	(0.032)	(0.032)	(0.031)	(0.054)
Ln(unique investors)	0.027***	0.027***	0.026***	0.078***
	(0.005)	(0.005)	(0.005)	(0.011)
Investor_%non-IVC	0.052***	0.053***	0.049***	0.095***
	(0.009)	(0.009)	(0.009)	(0.014)
Firm age	-0.001	-0.001	-0.002*	
	(0.001)	(0.001)	(0.001)	
Ln(valuation)	0.137***	0.137***	0.136***	0.117***
	(0.018)	(0.018)	(0.018)	(0.019)
Ln(raised)	0.020***	0.020***	0.020***	0.024***
	(0.003)	(0.003)	(0.003)	(0.003)
Board size	0.028***	0.028***	0.029***	0.048***
	(0.007)	(0.007)	(0.007)	(0.009)
Ln(1+patents)	0.089	0.088	0.089	0.144**
	(0.059)	(0.060)	(0.060)	(0.071)
FE: ind-year	Yes	Yes	Yes	Yes
FE: ind-1st round year	No	No	Yes	No
FE: ind-year, firm	No	No	No	Yes
Team characteristic controls	No	Yes	Yes	Yes
Missing value indicators	Yes	Yes	Yes	Yes
N	992613	992613	992613	992613
R^2	0.004	0.004	0.004	0.088
Outcome Mean	0.060	0.060	0.060	0.060

The table shows an alternative version of Table 5, decomposing board control into whether a board is VC-controlled or has control shared between VC and founders (i.e., VC and executive directors holding the same number of seats), with founder-controlled board as the omitted group. The sample is a hazard-style panel of firm-years for US VC-backed firms founded since 2000. For non-fraud firms, the panel includes all years from firm founding to the earlier of 2024 or firm closure. For fraud firms the panel goes from firm founding year to the fraud start year. The dependent variable is 100 in the fraud start year for fraud firms and is 0 otherwise. The sample contains 537 fraud cases. Standard errors are clustered at the industry level and are reported in parentheses. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table A.7: Panel Prediction of Fraud Among VC-Backed Firms: Raised More than \$1M, \$5M, or \$10M

Dependent Variable:	$\mathbb{1}$(Fraud start) × 100		
Sample:	Raised \$1M+	Raised \$5M+	Raised \$10M+
	(1)	(2)	(3)
Board founder controlled	0.063***	0.073***	0.075***
	(0.019)	(0.025)	(0.026)
Investors' converted ownership	-0.150***	-0.164***	-0.200***
	(0.035)	(0.039)	(0.043)
Ln(unique investors)	0.024***	0.033***	0.040***
	(0.005)	(0.007)	(0.008)
Investor_%non-IVC	0.081***	0.109***	0.144***
	(0.014)	(0.022)	(0.029)
Firm age	-0.002**	-0.003**	-0.003*
	(0.001)	(0.001)	(0.002)
Ln(valuation)	0.177***	0.223***	0.250***
	(0.022)	(0.026)	(0.029)
Ln(raised)	0.028***	0.028***	0.028***
	(0.005)	(0.005)	(0.006)
Board size	0.026***	0.028***	0.032***
	(0.008)	(0.009)	(0.009)
Ln(1+patents)	0.077	0.074	0.070
	(0.063)	(0.069)	(0.076)
FE: ind-year	Yes	Yes	Yes
Team characteristic controls	Yes	Yes	Yes
Missing value indicators	Yes	Yes	Yes
N	633001	414966	316102
R^2	0.004	0.005	0.006
Outcome Mean	0.086	0.116	0.142

The table is similar to Table 5 Column 2 but restricts to firm-years with at least \$1M, \$5M, or \$10M cumulative amount raised. Standard errors are clustered at the industry level and are reported in parentheses. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table A.8: Panel Prediction of Fraud Among VC-Backed Firms: Dropping Crypto/Blockchain Companies

Dependent Variable:	$\mathbb{1}$(Fraud start) × 100			
	(1)	(2)	(3)	(4)
Board founder controlled	0.039***	0.041***	0.042***	0.050***
	(0.013)	(0.013)	(0.013)	(0.016)
Investors' converted ownership	-0.085***	-0.084***	-0.086***	-0.101**
	(0.031)	(0.031)	(0.031)	(0.048)
Ln(unique investors)	0.020***	0.020***	0.019***	0.063***
	(0.004)	(0.004)	(0.004)	(0.007)
Investor_%non-IVC	0.042***	0.043***	0.040***	0.074***
	(0.009)	(0.009)	(0.008)	(0.016)
Firm age	-0.001*	-0.001	-0.001*	
	(0.000)	(0.000)	(0.001)	
Ln(valuation)	0.113***	0.113***	0.113***	0.093***
	(0.016)	(0.016)	(0.016)	(0.017)
Ln(raised)	0.017***	0.017***	0.017***	0.020***
	(0.003)	(0.003)	(0.003)	(0.003)
Board size	0.020***	0.020***	0.021***	0.035***
	(0.005)	(0.005)	(0.005)	(0.008)
Ln(1+patents)	0.071	0.071	0.073	0.118*
	(0.056)	(0.056)	(0.057)	(0.068)
FE: ind-year	Yes	Yes	Yes	Yes
FE: ind-1st round year	No	No	Yes	No
FE: ind-year, firm	No	No	No	Yes
Team characteristic controls	No	Yes	Yes	Yes
Missing value indicators	Yes	Yes	Yes	Yes
N	974702	974702	974702	974702
R^2	0.004	0.004	0.004	0.086
Outcome Mean	0.046	0.046	0.046	0.046

The table shows robustness of Table 5 to dropping firms in cryptocurrency or blockchain verticals, as labeled by PitchBook. The sample is a hazard-style panel of firm-years for US VC-backed firms founded since 2000. For non-fraud firms, the panel includes all years from firm founding to the earlier of 2024 or firm closure. For fraud firms the panel goes from firm founding year to the fraud start year. The dependent variable is 100 in the fraud start year for fraud firms and is 0 otherwise. The sample contains 453 fraud cases. Standard errors are clustered at the industry level and are reported in parentheses. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table A.9: VC Market Conditions at Initial Round and Future Governance Contracts

Dependent Variable:	Board founder controlled		Investors' converted ownership		Ln(unique investors)	
Model:	OLS	2SLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Ind-yr avg. valuation multiple	0.033***	0.126***	-0.015***	-0.016	0.082***	0.318**
	(0.012)	(0.039)	(0.005)	(0.016)	(0.025)	(0.124)
1st stage F-stat		19.3		27.5		14.0
FE: ind	Yes	Yes	Yes	Yes	Yes	Yes
FE: state-yr	Yes	Yes	Yes	Yes	Yes	Yes
N	28634	28634	30979	30979	60050	60050
R^2	0.052	-0.033	0.104	-0.027	0.083	-0.021
Outcome Mean	0.741	0.741	0.869	0.869	0.602	0.602

This table shows the effect of hot VC market conditions at firms' initial round on their subsequent governance contracts. The sample is at the firm level, covering initial VC rounds between 2000 and 2021. The dependent variables are board founder control, investors' ownership, and log number of unique investors, all averaged over the five years after the initial VC round year. The specifications otherwise follow Table 9. All columns include industry fixed effects and state-year fixed effects. Standard errors are clustered at the industry level and are reported in parentheses. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table A.10: Heterogeneity in Future Founding of VC-Backed Startups by Fraudulent Entrepreneurs: Imputation-Based DID

Dependent variable:	No. of startups founded			
Case type:	Major media coverage	No major media coverage	DOJ/SEC	Non-DOJ/SEC
	(1)	(2)	(3)	(4)
Treat × Post	0.019*	-0.002	0.003	0.008
	(0.010)	(0.009)	(0.013)	(0.008)
Model	Imputation-based DID			
FEs	person, year, event-year			
N	7376	10092	4074	10950
Outcome mean	0.042	0.042	0.049	0.044

This table shows robustness of Panel C of Table 10 to using imputation-based DID from Borusyak et al. (2024).

Internet Appendix B: Legal Framework for Fraud in the U.S.

In the U.S., lawsuits alleging fraud can be brought under three legal bases: i) violations of state or federal securities or criminal laws; ii) violations of fiduciary duty obligations in state corporate laws; and iii) violations of contract under state contract laws.

The most important regulatory laws are the federal anti-fraud provisions in the 1933 Securities Act, which relate to securities issuance, and the 1934 Securities Exchange Act, primarily in Section 10b, which addresses fraud related to already issued securities. Importantly, these provisions apply equally to publicly traded and VC-backed firms. VC-backed private firms raise funds through exempt transactions that require regulatory filings (e.g., Form D with the SEC). As former SEC Commissioner Mary Jo White stated in a 2016 speech: "All securities transactions, even exempt transactions, are subject to the anti-fraud provisions of the federal securities laws. This means that you and your company will be responsible for false or misleading statements that you or others on your behalf make regarding your company, the securities offered, or the offering."

Suits under federal securities laws can be brought by the SEC and private actors. Private suits are mostly class-action lawsuits against publicly traded companies, where the fraud-on-the-market theory helps satisfy the reliance requirement—investors who rely upon the integrity of the market price are presumed to have relied on the misstatement. States have their own securities laws, which include anti-fraud provisions, and investors can also sue in state courts, which may have a lower burden of proof than federal courts (e.g., not requiring proof of intent). Fraud in securities regulations cases also often overlaps with wire and mail fraud in criminal laws (18 U.S.C. Sections 1341, 1343), as the mail or wires are often used to further the fraud. If the DOJ pursues a criminal case, it invokes these statutes.

The second legal basis for fraud allegations is fiduciary duty suits brought by investors. These claims are filed in state court under corporate law and allege that directors and officers—including founders—have breached their fiduciary duties. Fiduciary duties encompass both the duty of care and the duty of loyalty, the latter requiring fairness in self-interested transactions or full disclosure and appropriate approval. Fiduciary duties are owed primarily to the corporation and, in some cases, to shareholders. As a result, fiduciary duty suits are often derivative suits brought on behalf of the corporation. In Delaware, where most VC-backed firms are incorporated and most suits are filed, derivative suits must satisfy a demand requirement: shareholders must first make a demand on the board or plead why the board is too conflicted to act. We focus only on fiduciary duty violations that involve allegations of misrepresentation (most suits do include such allegations).

Finally, fraud allegations can arise in suits alleging breach of contract. These cases, typically pursued in state courts, involve claims similar to those asserted under securities laws (e.g., failure to disclose material information, misrepresentation, or fraudulent financial statements), as well as other grounds (e.g., failure to provide required information, such as books and records). Contracts may include clauses requiring investor disputes to be resolved through out-of-court arbitration.

Internet Appendix C: Collecting Fraud Cases from Multiple Data Sources

C.1 SEC Enforcement Actions

To identify SEC enforcement actions involving VC-backed startups and their founders, we begin by scraping three types of SEC releases from 1995 to 2023: litigation releases (11,209), administrative proceedings (17,763), and Accounting and Auditing Enforcement Releases (AAERs) (3,230), which overlap with the first two when related to financial reporting violations. From these releases, we extract 64,830 unique respondents associated with 32,545 releases.

Our first matching approach links SEC respondents to PitchBook companies and individuals using a combination of exact and fuzzy name matching. We retain matches where the release date occurs after the company's founding and first financing, and within five years of either its last financing round or IPO. For individuals, we ensure the release date falls within five years of their departure from a VC-backed company.

In a complementary second approach, we apply keyword-based filters to litigation release texts to flag potential cases involving VC-backed startups or founders (e.g., "venture capital", "startup", "securities fraud", "Ponzi scheme"). We manually verify and match flagged cases to PitchBook.

Combining both approaches, we collect 462 SEC enforcement actions from 1995 to 2023, associated with 350 companies. We further exclude cases involving non-relevant violations such as delinquent filings, unregistered broker activity, or market manipulation. Finally, we read and manually validate each case. After imposing filters relevant for our analysis (e.g., founded post 2000, has non-missing fraud start year and non-missing first VC round date), 126 SEC cases enter our final sample used in predictive analysis.

C.2 DOJ Enforcement Actions

We collect information on criminal enforcement actions from the U.S. Department of Justice (DOJ) by scraping all publicly available releases from 2013 to April 2024, resulting in a dataset of 203,226 cases. 65% of these cases include subject-matter tags; among the tagged cases, 18,125 are labeled "financial fraud" and 1,494 are tagged Securities, Commodities, & Investment Fraud".

We identify startup-related fraud cases in three steps. First, we filter for cases tagged as financial or securities fraud and containing startup-related keywords (e.g., "founder", "venture capital", "Silicon Valley"). Second, for untagged cases, we use a BERT classifier to predict crime type and manually retain fraud-relevant cases. This yields 1,497 relevant cases, 552 of which involve firms or founders matched to PitchBook. Third, we manually review 3,250 additional cases predicted as fraud but not initially linked to VC-backed firms, identifying 599 further PitchBook matches.

Finally, we restrict the combined sample to U.S.-based firms in the PitchBook VC universe. We manually validate each case to make sure it is linked to the right PitchBook company. This results in 186 unique VC-backed companies involved in 230 DOJ cases. After imposing various filters, the final sample that enters our predictive analysis includes 89 DOJ cases.

C.3 Securities Class Action Lawsuits

Our class action data collection begins with a comprehensive dataset of 6,601 securities class action lawsuits filed between January 1996 and April 2024, drawn from the Stanford Securities Class Action Clearinghouse (SCAC). We restrict the sample to the 2,275 non-dismissed cases involving U.S. companies filed between 1999 and 2023. For consistency with our other data sources, we further restrict the sample to companies that went public in 2002 or later.

We determine whether each company was venture-backed using PitchBook's VC deal history, supplemented with Ritter's VC flag, which is particularly useful for identifying VC backing prior to 2010. The final sample includes 917 class action cases, of which 536 involve (previously) VC-backed firms. Restricting to cases within 2 years of IPO yields 265 cases by VC-backed firms, out of which 183 entered our predictive analysis after imposing various filters.

C.4 Westlaw Database

Westlaw expands fraud-related litigation coverage in two complementary ways. First, its court cases database broadens the legal scope beyond regulatory and securities class actions by capturing common-law fraud suits, contract and commercial disputes, shareholder derivative actions, and bankruptcy proceedings, thus extending the sample to private, creditor-driven, and state court cases that would otherwise be missed. Second, Westlaw dockets add a time dimension by recording cases that never yield a published opinion, often providing the earliest – and sometimes only – evidence of a firm being sued for fraud. Together, court cases and dockets allow us to capture both the broader legal settings in which fraud allegations arise and the earlier stages of litigation that might otherwise disappear through dismissal or settlement.

Westlaw Case Documents

Westlaw cases record litigation that went to court and yielded published opinions. We apply the following filter to identify cases involving fraudulent activities where the defendant is likely a startup or its founder: any words in ["fraud", "misrepresent", "deceit", "misconduct", "Ponzi scheme", "embezzl", "securities fraud", "wire fraud"] **AND** any words in ["startup", "start-up", "early-stage", "venture-backed", "tech comp", "founder", "venture capital", "silicon valley"] **AND** belong to topics in ["fraud", "false pretenses", "securities transactions", "securities regulation"]. This query returns 3274 cases.

The same process is used to identify fiduciary-duty breach cases using the following keywords: ["breach of fiduciary duty", "self-deal", "conflicts of interest", "duty of loyalty", "duty of care"] **AND** belong to topics in ["Corporations and Business Organizations"]. This query returns 1193 fiduciary-duty-only cases, plus around 550 cases overlapping with the fraud sample, suggesting that fiduciary duty breaches are quite often also associated with fraudulent activities.

From each case, we use text parsing to extract the full list of defendants. We then identify all defendants that are likely companies. For a substantial number of cases that list only individual defendants, we conduct a detailed review of the factual background for each such case to identify any companies involved in the fraud scheme but omitted from principal defendants. After completing these two rounds of firm name extraction, we match them to our universe of VC-backed companies in PitchBook. The matching process combines fuzzy name matching with manual verification to ensure accuracy, yielding 240 VC-backed firms associated with 219 cases.

Next, we extract the specific variables required for the analysis. For this step, we employ the GPT API to perform the initial extraction, followed by research assistant verification to confirm the accuracy of the information for each case. The list of variables we extract includes: 1) Defendant Type: `"Person"`, `"Company"`, `"Person and Company"`, or `"Other"`; 2) Defendant's role in the company during the fraud period; 3) Fraud Type: `"bank"`, `"fiduciary duty"`, `"wire"`, `"mail"`, `"corporate"`, `"federal"`, `"securities"`, `"tax"`, or `"other"`; 4) What was misrepresented: `"financials"`, `"product"`, `"use of funds"`, or `"other"`. 5) Victim: `"investors"`, `"government"`, `"public"`, or `"other"`; 6) Fine amount in thousands; 7) Prison length in months; 8) Judgment date; 9) Fraud start date; 10) Fraud end date; 11) Judge in favor of defendant: `"Yes"`, `"No"`, `"Partially"`.

Because all cases in our database produce publicly available judicial opinions, we verify each case's final disposition and classify outcomes as dismissed (if the appellate court affirms), settled, or decided (a ruling in favor of the plaintiff). To ensure that fiduciary-duty cases in our sample genuinely involve fraud-related conduct, we additionally require evidence of misrepresentation in the allegation. Accordingly, cases involving purely self-dealing, conflicts of interest, insider trading, aiding and abetting, or without any misrepresentation are excluded. This ensures that our analysis focuses on fiduciary-duty matters related to fraud.

The final sample comprises 13 non-dismissed fiduciary-duty cases and 47 non-dismissed fraud cases, along with 6 dismissed fiduciary-duty cases and 56 dismissed fraud cases.

Westlaw Dockets

Dockets are essentially court record identifiers: they confirm that a filing exists and track how the status of the case evolves within the court filing system over time. However, dockets do not provide substantive details about the proceedings or allegations. They allow us to track allegations that never get a chance to yield any published opinion.

We apply a series of advanced search filters on *Westlaw US Dockets* to identify three categories of fraud cases: settled, ongoing, and those resulting in final judgment. Dockets are chronological and often lack factual detail, making it difficult to identify startup defendants through keyword searches. Therefore, we apply a more generous filter, focusing on the presence of fraud-related allegations and procedural outcomes. Our filters are: dockets must contain any words in ["wire fraud", "mail fraud", "bank fraud", "securities fraud", "other fraud"] **AND** any words in ["settlement", "stipulation", "settled", "active", "judgment", "verdict", "summary judgment"] **AND** exclude cases with words ["class action"] **AND** belong to topics in ["Business Organizations", "Civil", "Contracts", "Fraud & Misrepresentation", "Other Federal Statutes", "Other Fraud"].

This process yielded 22498 dockets related to fraud and 13125 dockets related to fiduciary duty breaches. Due to download restrictions, we cannot download the full content of all these dockets. Instead, we obtained spreadsheets containing basic metadata for each docket, including the title, filing date, court, and presiding judge (if available). The case title, uniformly formatted as "plaintiff v. defendant," provides one defendant name for each case, which may refer to either a firm or an individual. We begin by screening the dockets using the defendant name listed in the title. From these records, we extract the list of defendant firms and match them to the PitchBook universe of VC-backed firms using a combination of fuzzy matching and manual review. For defendants that are individuals, we identify the associated firms by performing exact name matching between defendant individuals and board and founding team members in PitchBook, using first and last names as the matching key. Each match is then manually verified by reviewing the docket content, which provides a full list of all defendants, including firm names. A case is retained only when both the firm name and individual names are matched.

Through this procedure, we identified 478 dockets in total. We then collect detailed case variables by manually searching for associated court filings for each docket. These searches relied on a wide variety of sources, including Westlaw, Justia, official government websites, specialized legal platforms such as Law360, and relevant news or legal articles. We found filings for 400 of the 478 dockets.

For all cases with available filings, we collect two key variables: (1) the initial charge date, defined as the first date on which the plaintiff files a complaint (subsequent transfers or reopenings are not counted); and (2) the final case outcome. We determine each case's ultimate resolution using docket records and, where relevant, appellate opinions. We classify outcomes into four mutually exclusive categories: settled, ongoing, dismissed, and decided. Settled indicates cases concluded with a stipulation of dismissal, a documented settlement agreement, or a settlement record. Dismissed cases are those closed by court order dismissing the plaintiff's claims (including dismissals without published opinions) and also include voluntary dismissals or withdrawn cases by the plaintiff. Decided cases are those in which the court rules in favor of the plaintiff. We treat settled, ongoing, and decided cases as non-dismissed cases.

Once the filings were assembled, we employed the GPT API to extract structured variables of interest, similar to the approach described earlier. Automated extraction was followed by careful manual verification to ensure the reliability of our dataset. This procedure yields 239 non-dismissed dockets and 109 dismissed cases.

Dockets and News Feeds

We collect news articles from Crunchbase and PitchBook to construct a dataset that includes alleged fraud involving VC-backed startups. News contains credible allegations and ongoing investigations, which provide a broader set of "likely fraud" or "concerns for fraud", allowing us to mitigate concerns about omitted fraud cases through our previous search procedure.

We aggregate all news articles linked to startups or their founders. We then screen articles using a combination of keyword filters, manual checks by research assistants, and a machine learning classifier trained on over 6,000 hand-labeled articles to identify articles about startup fraud. We manually check all news articles and identify 5,622 fraud-related news articles covering 1,103 unique startups. These include firms linked to SEC or DOJ actions, class action lawsuits, and other cases involving credible fraud allegations. We merge these firms with our existing sample and retain only those uniquely captured through news coverage. After applying filters, this step yields 242 firms and 529 articles.

We then search each company in the Westlaw Docket System to determine whether a corresponding lawsuit exists. Over 80% of these firms (199 out of 242) are associated with at least one lawsuit, primarily SEC actions or securities class actions. For these records, we follow the same procedure as in the previous subsection, manually locating complaints and filings and using GPT to assist with variable extraction. Many securities class actions not already in our SCAC dataset are dismissed cases; following standard practice in the literature, we exclude dismissed SCAC filings.

Finally, we locate 63 non-dismissed cases and 61 dismissed Non-SCAC cases from the news source. For the remaining news articles not associated with a lawsuit in Westlaw, we go through them manually and drop post-IPO-related investigations. This procedure leads to 17 firms in our "news-only" fraud allegations.

C.5 DOJ News Release Before 2014

Finally, we extend our pre-2014 DOJ sample by searching the DOJ news release database in Westlaw Administrative Rulings using the same fraud-related keywords. After applying our standard screening process, we identify three cases involving VC-backed startups founded in or after 2000.