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VENTURE FRAUD

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ABSTRACT

We assemble the first comprehensive sample of venture fraud cases involving 614 U.S. venture capital (VC)-backed startups founded since 2000. We find that VC-backed firms are 54% more likely to face fraud charges than comparable non-VC-backed firms within a subsample of newly public firms where detection likelihood is high and homogeneous. We then examine the role of governance in explaining venture fraud, focusing on two features that have risen in recent years—founder-friendly structures and cap table complexity. In a panel prediction model examining all venture fraud cases, we find that fraud is more likely in startups with stronger founder control rights, more convex founder cash flow rights, more investors, and greater participation of non-traditional investors. Founder-controlled boards are 88% more likely to commit fraud than VC-controlled or shared-control boards, even within the same firm. Governance variables matter much more than founder characteristics in predicting fraud. Hot funding conditions at the initial round, which weaken governance incentives, predict future fraud. Fraudulent entrepreneurs continue to found new VC-backed startups unharmed relative to non-fraudulent entrepreneurs, suggesting a lack of market discipline. Overall, our results highlight rising agency costs in VC-backed firms that could lead to misallocation and broader social costs.

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

1 Introduction

Venture capitalists (VCs) use control and cash flow rights to enhance startup performance and align incentives (Kaplan and Strömberg, 2004; Ewens et al., 2022). These contracting choices mitigate, but do not eliminate, agency costs in VC-backed firms, with founder fraud being an important indicator of the residual frictions. Different from regular risk-taking, venture fraud involves material misrepresentation and breaches of fiduciary duty, and is illegal under anti-fraud laws that govern both public and private companies. The level of venture fraud may not be socially optimal if it imposes externalities on other stakeholders, leading to misallocation.

Many papers highlight the strength of the venture capital model in addressing agency problems (Jensen, 1989; Kaplan and Stromberg, 2003; Ewens and Marx, 2018), with the implication that fraud in VC-backed firms should be low. Yet this perspective may be overly optimistic, as the unique features of VC-backed startups may accentuate the likelihood of fraud. Startups' high growth pressure generates incentives to exaggerate or fabricate performance. The use of novel and complex technologies in markets with unclear demand can raise monitoring costs as outcomes stemming from deception are often difficult to disentangle from those driven by 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 drivers 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? Is there external market discipline for fraudulent founders?

In answering the above 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, there has been a shift toward founder-friendliness since the mid-2000s, with founders increasingly retaining board control and becoming more shielded 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 who faced fraud-related lawsuits from 2000 to 2023. Examining legal actions has the advantage of focusing on cases that are economically consequential, as immaterial frauds are seldom pursued by regulators or litigants. We use data from PitchBook to identify VC-backed startups and their founders and search whether they are involved in lawsuits that allege fraud. Our data sources include the SEC and DOJ litigation releases, shareholder lawsuits in state and federal courts from Westlaw, and security 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. Holmes of Theranos), or customers (e.g. Javice of Frank).[1] Our full sample consists of 614 unique cases involving VC-backed startups founded since 2000, with a median raised capital of $47 million by the time of 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.

We take several approaches to mitigate concerns about undetected fraud—fraud that took place but were never brought to light. First, our main sample contains both enforced and non-enforced cases, including dismissed suits from Westlaw as well as investigations and allegations identified from news feeds from PitchBook and Crunchbase. This approach mirrors 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 suit, 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 opacity proxies; we also focus on predicting the start of fraud commission rather than the timing of fraud detection. Lastly, we conduct tests on a subsample of newly public firms, who face high and homogeneous detection rates. IPO represents a discrete jump in disclosure and scrutiny, raising the probability of fraud detection. Our identifying assumption is that fraud revealed shortly after the IPO reflects governance practices in place in the pre-IPO period.

We have four primary findings. First, the venture fraud rate is sizable and has been increasing over time. Among US VC-backed firms founded since 2000 that have raised at least $10 million,

[1]We exclude cases where founder's 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.

1.85% have been involved in detected fraud, and the ongoing fraud commission rate has increased from 0.11% in 2003 to 0.67% in 2021. Across all firm sizes, the venture fraud rate has been increasing over time, a trend that is unlikely to be driven by rising enforcement, as fraud rates have been declining 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 face significantly more suits than non-VC-backed firms, especially in the recent decade that coincides with the rise of founder-friendliness and increasing cap table complexity. Controlling for industry-year fixed effects, pre-IPO growth, and firm size, VC-backed firms are 4.4 percentage points more likely than non-VC-backed firms to face class-action suits within two years of IPO—a difference that is 54% of the mean. This result is robust to alternative post-IPO windows, different definitions of lawsuit materiality, and instrumenting VC-backing with local VC supply at firm founding (Hochberg, 2012). The higher fraud rates among VC-backed firms, even after controlling for fundamentals, point to potential pre-IPO governance weaknesses in these firms.

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. 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. Startups with founder-controlled boards are 88% more likely to commit fraud than those with VC-controlled or shared-control boards. A 10% lower investor ownership is associated with a 22% higher fraud likelihood relative to the mean. Cap table complexity also positively predicts fraud: One additional investor on the cap table raises the fraud likelihood by 8.4% relative to the mean, while a 10 p.p. higher fraction of non-independent VC investors (e.g., mutual funds or hedge funds) is associated with an 8.4% higher fraud likelihood. These findings are consistent with investor-side coordination frictions hindering monitoring. Finally, contracts that reduce founders' payoffs but increase their convexity, such as high liquidation preferences, are associated with higher fraud incidence, consistent with founders' reduced skin-in-the-game and stronger incentives to gamble for extreme outcomes.

To further assess the importance of governance incentives, we benchmark them against the predictive power of founder characteristics that are known to influence entrepreneurial entry and success in the literature. We find that, conditional on governance variables, education, gender, and age do not predict fraud, whereas being a solo founder and prior founder experience have weak predictive effects. Overall, governance variables are far more important than founder characteristics in explaining fraud, with the former having a predictive power 5.6 times that of the latter.

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 restricting the sample to startups that raised substantial amounts of capital, excluding dismissed cases or class actions, 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, are associated with more subsequent fraud. This speaks to a source of weakening governance, as such periods are associated with laxer screening and monitoring by investors, including through founder-friendly contracts and cap table complexity. 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 multiple at a startup's initial round is associated with a 27% higher future fraud rate relative to the mean. This effect is 139% 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 controlling for the startup's own initial round valuation, suggesting that hot markets weaken screening and/or induce contracts that are protective of founders.

Fourth, we find that the VC market in general does not discipline fraudulent founders ex post, contrasting with significant consequences for fraudulent executives in the public market (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, sector, education, gender, and age. Using this matched panel, we estimate a dynamic stacked DID (Cengiz et al., 2019) tracing these individuals' founding of new VC-backed startups. Overall, the cumulative number of startups founded by treated founders post-fraud is no different from that of the control founders.

Taken together, our findings point to sizable and increasing agency costs in VC-backed startups, shaped by founder-friendly contracts, growing cap table complexity that complicates monitoring, and hot market conditions in which capital chases deals. The lack of ex post market discipline reinforces the critical role of internal governance in policing fraud.

A key takeaway of the paper is a quantification of the downside of weak governance in venture capital. Structuring governance to allow for the level of fraud we observe may be optimal for investors who prioritize upside capturing over downside mitigation, as founder-friendly structures may incentivize value-enhancing risk-taking. But, it may also be that governance-induced fraud is excessive. When deceptive types become pervasive, they may crowd out higher-quality firms that 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 seek. 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 level of fraud is privately optimal, it will exceed the socially optimal level if negative externalities are not internalized. The Theranos case illustrates this possibility: founder Elizabeth Holmes's fabricated claims about breakthrough blood-testing technology and misled not only investors but also patients and physicians, resulting in misdiagnoses, delayed treatments, and other harms. As VC-backed firms grow in scale and economic importance, the magnitude of such externalities is likely to increase. If there is too much venture fraud, this creates a case for policy intervention, including strengthening the information or 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. This literature is interested in firm and industry characteristics that predict fraud and measures the likelihood of fraud (Beneish, 1999; Dechow et al., 1996; Beneish et al., 2013; Povel et al., 2007; Wang, 2013; Dyck et al., 2024). It also explores the economic consequences for executives of committing fraud (e.g. Karpoff et al. (2008a,b)). Our paper contributes by examining these issues for VC-backed firms, an understudied group with agency problems distinct from those in public firms. Atanasov et al. (2012) also examine lawsuits in venture capital, but they focus on suits against VCs by entrepreneurs and other investors and highlight how reputation concerns limit opportunistic behaviors. Tian et al. (2015) show that financial market disciplines VCs associated with IPO fraud by their portfolio companies. Both studies focus on earlier periods before the rise of founder-friendliness.[2] We use a comprehensive sample of venture fraud cases new to the literature to document the power of internal governance incentives in predicting fraud and a lack of external market discipline for fraudulent founders.

We also add 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) use a dynamic model of contract data to show that VCs' superior bargaining power results 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 period of 1983 to 1994, Hochberg (2012) shows that VC-backed firms adopt better corporate governance *post* IPO than non-VC-backed firms. Legal scholars highlight that startups face governance issues that are distinct and more complex than those in public firms (Bartlett III, 2006; Pollman, 2019, 2020; Winship, 2020): not only are

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

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 that is studied in Bian et al. (2023) in the context of startup fire sales by VCs. Our paper contributes by using fraud to understand the extent and drivers of agency problems in VC-backed firms, and how they have evolved over time.

Within the above 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 and social costs.

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 role of financing market conditions and founder characteristics.

2.1 Founder's Incentives and Ability to Commit Fraud

Theories on corporate control suggest that founder-friendly terms that attenuate investor control rights weaken 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) models 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 weaker 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.[3]

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.

Contracts with greater payoff convexity can intensify founders' incentives to pursue extreme outcomes, including engaging in fraud. In VC-backed firms, such convexity arises from VCs' use of convertible preferred equity rather than common equity, and it is amplified by higher liquidation preferences – the amounts preferred shareholders (investors) must be paid before common shareholders (founders) receive anything in a liquidation event. A higher liquidation multiple, similar to a higher strike price in a call option, reduces entrepreneurs' cash-flow rights and thus their "skin in the game".[4] At the same time, it also makes their payoffs more convex, increasing incentives to gamble for right-tail outcomes (Shue and Townsend, 2017). Taken together, these mechanisms imply that higher liquidation preferences heighten founders' incentives to engage in 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,

[3]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.

[4]The excess control rights literature similarly highlights that the wedge between control and cash-flow rights exacerbates agency problems (Grossman and Hart, 1988; La Porta et al., 1999; Masulis et al., 2009; Xu, 2021). When insiders hold lower cash-flow rights, they internalize a smaller share of the value loss associated with tunneling or other opportunistic actions, as declines in firm value are borne disproportionately by outside investors.

such as redemption rights and IPO rights, often at the expense of cash flow and control rights that facilitate monitoring and mitigate fraud.[5]

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 greater competition among investors for deals, shifting bargaining power toward founders. This shift amplifies both vertical and horizontal frictions, leading to more founder-friendly contracts, weaker screening, and reduced monitoring. 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.

[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.

3 Measuring Venture Fraud

3.1 Fraud Definition and Legal Framework

In this section, we lay out the legal framework for measuring venture fraud, which informs our data sources and case collection strategy.

We measure venture fraud using lawsuits initiated by a regulator, investor, or stakeholder that allege fraud by a founder or the startup. Proof of fraud focuses on four elements under both common law and securities law: 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.

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 security 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).

Third, 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.

Note that 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 security 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 generally require proof of intent and reliance, whereas civil settlements—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

[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.

of fraud. Accordingly, our robustness tests explore this tradeoff by excluding dismissed cases or security 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 fraud did not start after a firm went public. Applying these filters resulted in 614 unique firms 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 A 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. We focus on those cases 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 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 124 SEC cases involving VC-backed firms.

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

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 77 DOJ cases.

Security 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 security 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. There is the possibility that a defendant settled when there was no wrongdoing just to make the case go away. As such, in some specifications, we further restrict security class-action suits with settlement amounts higher than $3 million, a common threshold used in the law and economics literature (Dyck et al., 2024). For private firms, security class actions have a high rate of omission, since their stock prices are not efficient and do not incorporate all information. Thus, each investor bringing a suit must prove their reliance on the misinformation, which prevents them from forming and suing as a class (Winship, 2024). However, once these private firms become public, security class actions have a low omission rate with many plaintiff law firms motivated to launch such cases (Karpoff et al., 2008a). This is a feature we exploit 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. The majority of these cases involve public firms, with fewer than 3% involving private firms. We determine whether each company was ever VC-backed using PitchBook's deal history, supplemented with Jay Ritter's IPO data. Our main analysis restricts to cases with a class period starting within 2 years of IPO, yielding 276 cases by VC-backed firms. Of these, 204 cases are included in our predictive regression after imposing 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 in front of the 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 200 non-dismissed cases and 85 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 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 downside of this source is

[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.

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.

Non-IPO lawsuits. *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 (2016) for breach of contract, from the investor Partner Fund Management (2016) for fraud, and from the Arizona Attorney General for consumer fraud that were settled out of court. Theranos illustrates the challenges for security class-action suits for private companies, as such suits proved ineffective: "the Court firmly agrees with Defendants that the fraud-on-the-market presumption of reliance cannot apply here, because Theranos securities were not sold in an efficient market." (Winship, 2024). 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 founders Charlie Javice and Oliver Amar alleging violation of federal securities laws in December of 2022, after having agreed to purchase Frank for $175 million. Among JPMorgan 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 lists. There were subsequent suits by the SEC and DOJ and in September of 2025 Charlie Javice was sentenced to more than 7 years in jail 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. A lawsuit was initiated 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, where corporate funds were pledged to secure the founder's personal indebtedness without adequate disclosure to investors.

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 only available in the News sample. It was not identified through our Westlaw search, but a case was found through subsequent analysis launched by Metrolina affiliate in the Virginia court, and the case is ongoing.

IPO lawsuits. *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 for $1.75 million, with Marrone Bio's former COO Hector Absi sentenced to two years of prison. The SEC signaled that it thought that this firm's problems were common with 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. For example, just last month, the Commission brought charges against a company and a former executive for inflating financial results to meet projections that it would double revenues in its first year as a public company. Because, in part, of insufficient internal controls, the executive was able to direct his subordinates to obtain false sales and shipping documents and intentionally ship the wrong product to book sales."[9] This case is present in our SEC, DOJ, and class-action sample.

GoHealth. The VC-backed firm GoHealth was sued in a security class action in September of 2020 for its IPO earlier that year alleging that the IPO registration statement and related offering materials contained false or misleading statements and omissions. Allegations included that

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

GoHealth had entered into less favorable revenue-sharing arrangements with external sales agents, negatively affecting financial results, and that the firm failed to reveal internal projections that indicated adverse trends were likely to continue or worsen after the IPO. The case illustrates the multiple legal bases for suits. The firm was also sued in May 2021 for violation of fiduciary duty in a shareholder derivative action based on similar allegations. In 2024, the court approved a $29.25 million settlement for the security class action. The derivative action was stayed pending the resolution of the security class action, and reportedly came to an agreement in September 2025. This case is in our class action sample. There were no cases brought by the SEC or DOJ.

4 Variables and Samples

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 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, 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 firm's financing rounds, creating horizontal conflicts between investors across rounds.

We also consider a term sheet feature that affects entrepreneurs' incentives to commit fraud: investors' liquidation preference. Specifically, we compute the dollar-weighted fraction of past rounds with high liquidation multiple (>1). Figure A.3 illustrates this incentive effect by plotting founder's payoffs when investors hold convertible preferred with a 1x versus 2x liquidation preference. Relative to a 1x multiple, the 2x multiple shows both lower and more convex payoffs for founders. All the above variables that affect governance incentives are time-varying.

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] 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.

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

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 614 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 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, first round date, and fraud start date, our predictive sample includes 769,016 firm years for 78,852 unique VC-backed firms, out of which 519 were involved in fraud and 435 were involved in enforced fraud cases.

Our second sample consists of the universe of newly public firms that are either VC-backed or non-VC-backed. We identify the VC-backed status using VC deals from PitchBook as well as the VC indicator in the Jay Ritter's IPO database. We use class-action cases with the class period starting within two years of the IPO as a measure 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

[10] Top universities are defined in footnote 11. We infer founder age from BA degree year.

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. This sample contains 4,094 IPOs over the period for 2002 to 2024.

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 (124), DOJ (77), Westlaw (200), and class actions (204). After removing overlapping cases, we have 512 unique enforced cases. We additionally obtain 121 non-enforced cases, including 85 dismissed cases from Westlaw and 17 allegations or investigations from news. Combined, these data give us 614 cases, which serves as our main sample. The median fraud-involved firm raised $47 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 28 months, though with significant variation (median at 23 months and 95th percentile at 78 months). Monetary sanctions are substantial, with a mean fine amount of $9.7 million. A majority of cases result in disgorgement (65.2%), fines (61%), and bans (57%), while prison sentences are rare (3.5%), as the SEC only enforces civil cases. Most cases involve financial misrepresentation (70%), with the remaining misrepresenting products (24%) and use of funds (19%). Investors are the most frequent victims (89%), followed by the general public (6.5%), government (4.1%), and other entities (5.7%).

The DOJ cases (Panel C) have a longer average fraud duration of 38 months. Wire fraud makes up 86% of cases, followed by securities fraud (33%), 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 42% of cases, with an average duration of 87 months, and the average fine amount is $21.2 million. Additional penalties include forfeiture (16% of cases) and supervised release (33% of cases). Similar to SEC cases, investors are the most common victims, representing 69% of cases. However, the public, government, and other entities are also frequent victims, representing 22%, 16%, and 12% 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 29 months, similar to SEC cases. Securities fraud account for 41% of the cases, while corporate fraud and fiduciary duty violations account for 40% and 32% respectively. Misrepresentation typically involves firm financials (58%), products (22%), and use of funds (11%). Sanctions are not systematically reported, as many are privately settled. Investors are again the most frequent victims (66%), followed by the public (12%).

Panel E provides information on the 204 class action cases. About half are settled outside the court, with an average settlement amount of $16.6 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 the ones we are able to find case information, the average fraud duration is 17 months. The top fraud types are securities (42%), corporate (42%), and fiduciary duty violations (12%). Similar to other cases, the most common misrepresentation 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 fraud start year. There are increasing numbers of cases over time, especially after mid-2010, with a spike in 2021. The dwindling of frauds since 2023 may reflect truncation, as some cases are yet to be revealed. We observe a similar pattern in fraud committing years (i.e., from fraud start to fraud end years) in Panel B.

Among US-incorporated VC-backed firms founded since 2000 that have raised at least $1M ($10M), 1% (1.9%) 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 0.29% (0.48%) 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 dip during the 2008 financial crisis years and 2018-2020. The incidence rate increases with firm size, as proxied by the cumulative amount raised. For example, among firms raised at least $5M, the ongoing fraud engagement rate is around 0.5% in the post-2013 period. We find similar patterns in Figure A.2 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 1.2% among firms that raised $5M-$10M at first round, and rises to 2.4% among those that raised $50M-$100M. However, we caveat 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 California, Massachusetts, Pennsylvania, and Florida, and is lowest in Delaware, Colorado, and Illinois. Based on PitchBook industry sectors, we find that fraud is most prevalent in Financial Services, Healthcare, and Energy, and is least prevalent in B2B and Materials and Resources.

5.3 Governance Incentive Variables and Other Variables in the Prediction Panel

Table 2 provides summary statistics 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 0.067%. 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 6 years old, with a valuation of $23 million and total raised capital of $3.6 million. The average firm-year has a board of 3.4 members.

In terms of the governance variables explored in our predictive analysis, 49.5% of the firm-years are founder-controlled, and the average investors' converted ownership is 45.8%, suggesting that founders retain control almost half of the time. The average number of unique investors on cap table is 3.8, and non-independent VCs represent on average 51.5% of all investors on cap table, suggesting great investor heterogeneity. As a measure of founder payoff convexity we use the percentage of rounds with a liquidation multiple greater than one, weighted by round size: 2.7% of the rounds have a high liquidation multiple above one.

In terms of founding teams, 47% were founded by solo founders. The average team has 14% serial founders, 27% top-school graduates, 12% female founders, and an average age at founding of 37.[11] 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.

We gain further insight into these governance features by examining how they vary over time in our sample. Panels B to E of Figure 4 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. Notably, 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 2018, consistent with a partial reversal of rising founder-friendliness in the last few years.

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

A natural way to benchmark fraud rates among VC-backed firms is to compare them with similar non-VC-backed firms. However, 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. VC-backed firms are inherently more visible than non-VC-backed private firms as they raise funds through exempt transactions that require

[11]We define top schools as Harvard, Yale, MIT, UPenn, Stanford, Columbia, NYU, Chicago, Cornell, Berkeley, Oxford, and Cambridge.

regulatory filings, appear in commercial databases, and draw more attention from regulators and investors.

To address this comparability issue, we focus on newly public firms and compare VC-backed and non-VC-backed firms in a setting where 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, 50% of securities class actions against newly public firms have a class period starting within two years of IPO (also see Wang (2013)). This supports the idea that fraud revealed in this window likely originates from governance 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 data. Amongst all IPO firms, 8.4% have a non-dismissed class action suit at any point in time, and 5.4% have a suit that starts within 2 years of their IPO. Summary statistics, without controls, suggest that class actions are more likely in VC-backed IPO firms, as such firms account for 39% of all IPO firms (Panel A) but 59% among those facing class actions within 2 years of IPO (Panel B).

Figure 5 provides further descriptive evidence regarding class actions against newly public firms, highlighting differences between VC- and non-VC-backed firms. Panel A shows all class actions filed after IPO by filing year as a reference. Panel B shows the timing of the cases relative to IPO, based on class period start date. Cases tend to cluster around IPO year. Nearly half of the cases against VC-backed firms begin in the IPO year, and a substantial fraction start within 1-2 years after. In contrast, class actions against non-VC-backed firms are less skewed toward the IPO year. This validates the idea that class action against VC-backed firms immediately after IPO likely reflects governance problems before the IPO.

Panel C restricts to class actions starting within 2 years of IPO, the main focus of our subsequent analysis. We see an increasing number of near-IPO class actions over time, particularly against VC-backed firms in recent years, such as 2020-2021. Over 2002-2023, the class action rate is 8.6% among VC-backed firms, 2.5 times the rate of 3.4% among non-VC-backed firms. We investigate this difference further next.

6.2 Regression Results

Table 4 uses a regression framework to formally test the difference between VC-backed and non-VC-backed firms in facing class actions right after IPO. The specification controls for industry-year fixed effects. 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), and two years (Columns 3 and 6) of 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 — two characteristics that may correlate with fraud commission and detection.[12]

We consistently find a positive and significant coefficient on the VC-backed dummy and the implied economic magnitudes are substantial. VC-backed newly public firms are 3.5 p.p. more likely to face fraud-related litigation than non-VC-backed firms within a month of IPO (Column 1), a large difference that is 122% of the mean. This difference is 71% when we use a 2 year window (Column 3). We find similar results in Columns 4-6 with additional controls. In Column 6 for example, VC-backed firms are 4.4 p.p. more likely to face class action lawsuits within 2 years of IPO, a difference that is 54% of the mean. Both pre-IPO assets and revenue growth are positively associated with having a class action immediately after the IPO.

To explore patterns in venture fraud over time, we split the sample period in the middle. This split is informative, as it shows that the significant difference in fraud likelihood between VC-backed and non-VC-backed IPO firms is driven primarily by the recent decade (2013-2023). The VC-backed dummy is significant in this period (Panel C) but not in the decade post the Dot-com bubble, 2002-2012 (Panel B).

To address the possibility that the higher fraud rate among VC-backed IPO firms arises from differences in unobservables, Panel D implements an instrumental variables (IV) strategy following Hochberg (2012). The instrument for VC-backing is the logarithm of the average volume of venture capital invested in the firm's headquarters state over the three years around the firm's founding year. The identifying assumption is that greater local availability of venture capital at firms' founding increases the likelihood of VC backing but is otherwise orthogonal to subsequent firm-specific fraud risk. The instrument is strong in the first stage, with a KP F-stat of 68.8. The 2SLS estimates are consistent with the baseline OLS results. We again find positive and statistically significant coefficients for VC-backed firms.[13]

[12] The reduction in sample size after controlling for pre-IPO financials reflects limited observability of pre-IPO asset and revenue in Compustat.

[13] If the positive association between VC backing and fraud were driven solely by omitted variables (i.e., unobserved founder or firm characteristics that correlate positively with both fraud and VC-backing), we would expect the IV estimates to be attenuated relative to OLS. Instead, the 2SLS coefficients are larger than the OLS estimates and remain statistically significant. This suggests that there is likely a negative selection bias: ex-ante fraudulent entrepreneurs are less likely to receive VC backing.

Appendix Table A.3 reports several robustness tests. Panel A shows that results are similar when defining the post-IPO window using filing date rather than 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.

Overall, Table 4 provides strong evidence that VC-backed firms are more likely to be sued for fraud immediately after IPO than non-VC-backed firms, a difference that is particularly pronounced in the most recent decade that coincides with a rise in founder friendliness and cap table complexity. 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 and pre-IPO growth and size as controls, and our results are robust to an instrument variable approach. 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 for fraud.[14]

7 What Predicts Fraud in VC-Backed Startups?

7.1 Empirical Strategy

In this section, we seek to understand the determinants of fraud among VC-backed startups. One approach to studying this question is to use a predictive analysis of fraud in a startup panel. The empirical challenge is that we can only predict detected fraud, which is committed fraud multiplied by 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.

Addressing underdetection of fraud 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 from Westlaw as well as allegations and investigations identified from news. Using this broadened sample of fraud allegations mitigates concerns about selective enforcement when focusing only on enforced cases. Second, rather than predicting fraud detection, we predict the timing of

[14] Our IV results do not preclude a governance channel, as a greater supply of capital (relative to demand) could weaken investors' screening and monitoring. We explore this further at the macro level in Section 8.

fraud start, which predates detection.[15] 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 largely unobservable to regulators, and founder demographics do not typically enter into regulators' objective functions when allocating enforcement resources. Finally, in some tests we adopt a similar approach to that in Section 6 by focusing on newly public firms that just went public—a subsample in which the likelihood of detection is both high and relatively homogeneous across firms. Specifically, we focus on VC-backed IPO firms and measure fraud using class-action suits within two years of IPO. The key assumption is that fraud revealed shortly after IPO can be attributed to governance failure prior to the IPO that was baked into the firm while 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:

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

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 will 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. *High liquidation multiple* provides a measure of the impact of founder payoff convexity on fraud. *Ln(unique investors)* and *Investor_%non-IVC* capture the degree to which horizontal 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

[15] We do not know the exact fraud start time for class action cases, as class action suits focus on issues during the class period (typically at or right after IPO). For simplicity, we set the fraud start year to the IPO year for class action cases.

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 ($\alpha_{j,t}$).[16] 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.[17] These variables also account for potential life-cycle patterns in fraud as well as the growth expectations implied from valuation multiple (i.e., valuation/total raised). Finally, missing values in various firm characteristics contain important information about firm's visibility, which could drive detection. We therefore control for missing-value indicators for various variables in $\mathbb{1}(\boldsymbol{MissingVal})_{\boldsymbol{i,t}}$. This approach also helps preserve sample size when we include many predictors. For each predictor, we fill missing values with zero while controlling for its missing-value indicator. The key identifying assumption is that, conditioning on variables that drive enforcement likelihood, the remaining predictors—internal governance and founder traits—should mainly explain variations in true fraud, because these variables do not directly impact enforcement likelihood.

We note that our predictive analysis is correlational and not causal, as many of the predictor variables are endogenous, such as governance contracts. In some specifications, we additionally include industry-by-initial-round-year fixed effects to absorb funding conditions at initial VC round, which may affect founder's bargaining power vis-a-vis VCs. We also include a specification with firm fixed effects. 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. This addresses endogeneity from time-invariant unobservables—for example, unobserved founder or business characteristics. Importantly, it also addresses selective enforcement based on fixed firm characteristics, since this specification only exploits timing of fraud start (not fraud detection) within enforced firms. In Section 8, we further explore an exogenous source of contract variation that comes from variation 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

[16]For our predictive analysis, we define industry by PitchBook's primary industry code, which contains 214 industries.

[17]Following Ewens et al. (2024), we interpolate valuation between rounds.

size, *Board founder controlled* positively predicts fraud. In particular, founder-controlled boards are 88% more likely to commit fraud than non-founder-controlled boards (i.e., VC-controlled or shared-control). In Table A.5, we further distinguish between VC-controlled and shared control boards and find that VC-controlled board has a much more negative predictive effect than shared control board. 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 22% lower fraud likelihood relative to the mean. These results suggest that strong founder control rights are conducive to fraud.

We find that founders' payoff structure matters too. In particular, lower and more convex founder payoffs are associated with a higher fraud likelihood. Specifically, a 10 p.p. increase in the fraction of capital raised with liquidation multiple above one, captured by *High liquidation multiples*, is associated with a 15% increase in fraud likelihood relative to the mean.[18]

Finally, we find that cap table complexity positively predicts fraud. There is a positive and significant coefficient on both the number of unique investors and the fraction of non-IVC investors. The economic magnitudes are also large. One additional investor in the cap table increases the fraud likelihood by 8.4% relative to the mean, while a 10 p.p. higher fraction of non-IVC investors raises it by 8.4%.[19] These results suggest that horizontal frictions among investors can exacerbate the vertical frictions between investors and founders by creating coordination frictions that weaken monitoring.

We also note the importance of control variables. Firm age does not significantly predict fraud; if anything, there is a slightly negative relationship but with negligible magnitude. Both valuation and total raised amount strongly predict venture fraud. The much larger coefficient on valuation than on raised amount suggests a strong effect of valuation multiple, which reflects expected future growth and profitability.[20] This finding suggests that high expectations from investors incentivize fraud, likely through the pressure on founders to meet such expectations. Of course, these two variables could also affect enforcement likelihood, as larger firms are more likely to be targeted 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.4, largely have positive

[18]Payoff convexity should not be considered as a firm-level founder-friendliness measure, since founders typically value control rights more than cash flow rights when trading off the two (Berger et al., 2025).

[19]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))*0.024/0.067=8.4%.

[20]Based on Column 1, a 10% increase in post-money valuation increases fraud likelihood by 28% relative to the mean, while a 10% increase in total raised amount only increases fraud likelihood by 2.4% relative to the mean. This suggests that the majority of the valuation effect reflects valuation multiple rather than book value. The implied coefficient on Ln(multiple)—the difference between the coefficients on ln(valuation) and ln(raised)—is 0.186-0.016=0.170 based on Column 1.

and oftentimes 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). In other words, missing data is not random, but reflects a choice by startups that we find is predictive of fraud. As such, it is important to model this missing information in our analysis. This finding is consistent with the idea that opacity breeds fraud while transparency facilitates public monitoring. The result also suggests that variation in fraud initiation does not primarily pick up variation in detection likelihood, as the latter would imply negative coefficients on these opacity measures (i.e., opacity lowers detection).

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. We find little changes in either the magnitude or statistical significance of the governance variables; R^2 also barely changes. This suggests that founders do not sort differentially into governance contracts by their backgrounds.

Column 3 additionally includes industry-by-initial-round-year fixed effects to absorb funding conditions at initial VC round. This specification ensures that our firm-level governance variables do not reflect general market conditions at the time of firm's initial VC round, which could affect the bargaining power between VC and founders and hence initial screening or contracting. We find that, although the R^2 increased by 20% relative to that in Column 2, the coefficients remain similar. This suggests that much of the variation in firm-level contracts are 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 onset, 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, unobserved firm or founder characteristics or their sorting into certain contracts. Second, the specification addresses selective detection based on firm unobservables, since this specification only exploits the timing of fraud start (not the timing of fraud detection) within detected firms. All of the governance characteristics retain their signs and significance, with substantially larger coefficients on cap table complexity and high liquidation multiple.

7.3 Predictive Regression Results: Founder Characteristics

Table 6 explores the role of founder characteristics in predicting fraud and compares their importance with governance variables. Column 1 of Panel A drops governance variables and focuses only on team characteristics. We find that solo founders are 18% more likely to commit fraud than teams, evaluated relative to the mean. This is consistent with solo founders being free from the checks and balances from co-founders. Past founder experience also has a weakly positive relationship with fraud: a 10 p.p. higher fraction of serial founders on the team is associated with a 3.1% higher fraud rate relative to the mean. Top-school education and gender do not robustly predict fraud and have small magnitudes (10 p.p. higher fraction associated with 2.2% and 1.2% effects respectively), while age has a precisely estimated near-zero correlation with fraud. These coefficients are similar in Column 2 after controlling for governance incentive variables. Overall, team characteristics seem to matter much less 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. The partial R^2 is estimated from Column 1 of Tables 5 and 6 relative to a benchmark model without either governance or team variables (but with other controls and fixed effects). We find that, across various fixed effect specifications, the five governance variables together has about 5.6 times larger explanatory power than the five team variables in predicting fraud. This confirms the idea that internal governance incentives matter much more than founder characteristics in explaining venture fraud.

Taken together, the results from this section suggest that venture fraud is strongly shaped by governance incentives. Founder backgrounds, in contrast, play a much smaller role. These findings suggest that "nurture" is more important than "nature" in explaining venture fraud. This has implications for investors and regulators looking to detect or mitigate venture fraud—they should target firms' governance structures rather than entrepreneur types.

7.4 Robustness

Our predictive results are robust to alternative samples or definitions of fraud. Table 7 shows robustness to two alternative samples: 1) enforced cases only—i.e., dropping dismissed cases or news-based allegations (Columns 1 and 2); and 2) dropping class action cases (Columns 3 and 4). The first alternative sample prioritizes case merit at the expense of underdetection, while the second alternative sample focuses exclusively on startups that never went public. We find similar results as reported in Table 5.

Table A.6 considers subsamples of startup-years with at least \$1M, \$5M, or \$10M of cumulative funding. This accounts for the possibility that startups without meaningful traction will attract little public or regulatory attention and hence face low detection likelihoods. Fraud by these firms, detected or not, are also unlikely to be economically consequential. We find similar results for these larger firms. Table A.7 further shows that our predictive results are robust to dropping firms in the cryptocurrency or blockchain-related verticals, as defined by PitchBook. This test shows our findings do not arise from a large enforcement wave by the SEC against cryptocurrency companies in recent years.

Finally, to further address underdetection and unobserved variation in detection likelihood, we conduct a cross-sectional prediction analysis on a subsample of newly public VC-backed firms, some of which face class action lawsuits soon after IPO. All these firms face the same heightened and homogeneous enforcement environment. We estimate the following specification on a cross-section of VC-backed IPOs:

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

The sample is at the IPO-firm level, indicated 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 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 IPOs in $\boldsymbol{Governance_i}$, since dual-class shares structure entrenches founders and generates agency issues (Masulis et al., 2009; Smart and Zutter, 2003; Xu, 2021), which may drive fraud.

Table 8 shows results that echo our earlier findings, underscoring the importance of governance for fraud. As in our previous results, founder control of the board in the year before the IPO predicts lawsuits for fraud. New to this specification, we find that going public with dual class shares that entrench founder control also positively predicts fraud suits. The economic magnitudes are substantial, with founder control of the board associated with a 42% higher suit likelihood and dual class shares associated with an 80% higher suit likelihood. We also continue to find that cap table complexity, captured by the number of investors and fraction of non-IVCs, positively predicts fraud. Investor ownership continues to have a large negative coefficient, while founder payoff convexity pre-IPO has a positive coefficient, though these effects are insignificant likely due to lower power. Founder characteristics remain insignificant or weak, with only solo founder being weakly significant in Column 2.

8 VC Market Conditions and Venture Fraud

A central theme emerging from the analyses above is that governance—in particular weakened governance arising from founder-friendly structures and cap table complexity—strongly predicts fraud. To shed some light on where these governance incentives come from, we consider the impact of aggregate market conditions on fraud. In hot markets, money chases deals, and this shifts the negotiation power to founders, likely increasing founder-friendly contracting and, through participation of more investors, cap table complexity. These market conditions can also directly impact fraud by weakening screening and monitoring, and by distorting founder incentives through high growth expectations implied in high valuations. Whereas in the previous analysis we include industry $\times$ initial round year fixed effects to absorb these effects, here we explore them directly.

Descriptive evidence hints at a relationship between market conditions and fraud, and a correlation between hot market conditions and weakening governance incentives. Figure 4 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 the former lagging the latter by 2-3 years. Suggestive of a link between hot markets and weakened governance incentives, we see a similar pattern in Panels B and C where we proxy for founder-friendliness with the fraction of founder-controlled boards and average founder/employee ownership (i.e., 100 minus investor ownership). Similar to fraud rates, both time series exhibited a sharp rise after the financial crisis before flattening out after 2017. Similar strong correlations are observed for cap table complexity, as measured by the average number of investors (Panel C) and the fraction of non-IVC investors (Panel D) 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), as this is the round when a startup is screened into the VC market. We hypothesize that a hot and founder-friendly VC market would lead to lax screening, increasing the chance that VCs bet on fraudulent startups.[21] Further, regardless of screening intensity, founder-friendly contracting in the initial round may also lay the foundation for weaker future monitoring, thereby increasing the risk of fraud.

We proxy for market hotness 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 make sure 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 identify a causal effect of market conditions on future fraud by startups initially financed in those conditions, we follow Nanda and Rhodes-Kropf (2013) and Gompers and Lerner (2000) to implement an instrumental variable approach. We instrument VC valuation multiple with lagged capital inflow into buyout funds. Specifically, we construct industry-year level fundraising volume

[21] Here, we use hot market and founder-friendly market interchangeably, as we want to isolate founder-friendliness driven by the oversupply of VC capital relative to demand (i.e., money chasing deals), which pushes up average valuation.

by buyout funds in the three years before the focal year. We allocate fundraising volume into industries based on the distribution of funds' portfolio companies across industries. The identifying assumption is that the LPs' lagged decisions to invest in buyout funds is uncorrelated with the tendency to commit fraud by VC-backed startups. 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 distancing 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 (=0.66) is associated with 27% 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 1.26 percentage points, or 139% 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 69.3, 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 lax screening, or through founder-friendly contracts and ownership complexity that weaken governance incentives. Confirming the latter, Table A.8 shows that hot initial market conditions indeed predict more founder-friendly contracts and more investors over the 5 years after initial round. Regardless of whether the channel is through formal contracts or unobserved screening or monitoring, all of these result from the pressure to win access to founders' deal flow.

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' future founding of VC-backed startups. If external discipline is absent, this increases the importance of internal governance in mitigating fraud.

External discipline is more likely when being fraudulent is persistent and cannot be effectively mitigated, and when investors incur reputational costs from association with a fraudulent founder.

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. A number of studies show that VCs act to protect their reputation: high-reputation VCs are less likely to face founder lawsuits (Atanasov et al., 2012) and suffer larger penalties when associated with post-IPO fraud (Tian et al., 2015). But our setting is different—we examine whether the VC market as a whole (not necessarily the prior investor) is willing to fund new startups by a founder who has committed fraud. The reputation spillover from the founder to the next investor could be less damaging than the reputation costs on the original investor themselves.

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 fraud charge, on the number of past startups, tenure at 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). 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 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:

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

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. $Treat_{ist}$ 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 6, with Panels (a) and (b) using stacked DID and Panels (c) and (d) using imputation-based DID.

The dependent variable is the number of new VC-backed startups founded by a person in a year in Panels (a) and (c), and is the cumulative number of startups a person has founded as of a year in Panels (b) and (d). 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. Next, we confirm these results with baseline DID estimates in Table 10. Over the six years post fraud charge, treated founders started 0.017 more new startups than control founders (Columns 2 and 4), a near zero effect relative to an outcome mean of 1.335.

The above results are striking, since one would expect that the legal and reputational damage from the fraud charges would negatively affect the founders.[22] 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 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. The lack of ex post market discipline reinforces the importance of internal governance in limiting fraud.

10 Conclusion

There is a trade-off in VC investing: generating (and capturing) 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. We collect data on enforcement actions by the SEC and DOJ and through private suits, including class actions and other lawsuits filed at federal and local courts. We test whether fraud is predicted by trends in recent years that appear to have eroded governance incentives, including increased founder-friendly contracts and greater cap table complexity that hampers monitoring. We also examine whether there are market penalties for fraud by founders.

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

[22]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.

investigate the firm-level implication of a shift to weakened governance on fraud incidence. Firms with founder-controlled boards have 88% higher fraud likelihood than other boards. Fraud rate is also substantially higher 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, we find that 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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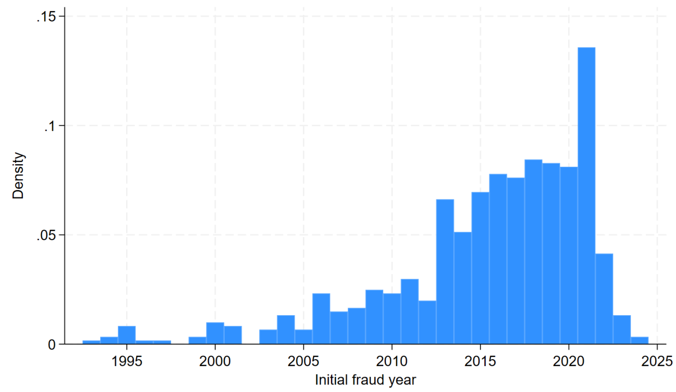
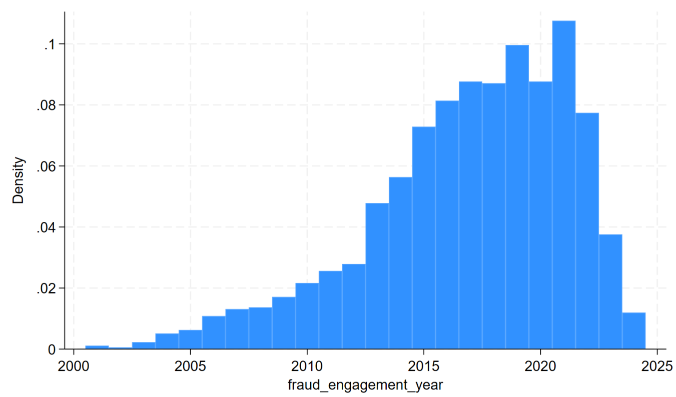
Figure 1: Timing of Fraud Among US VC-Backed Startups



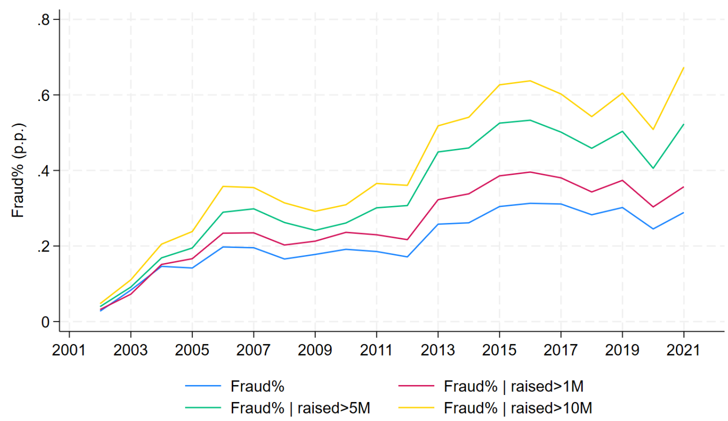
(a) Fraud Start Year



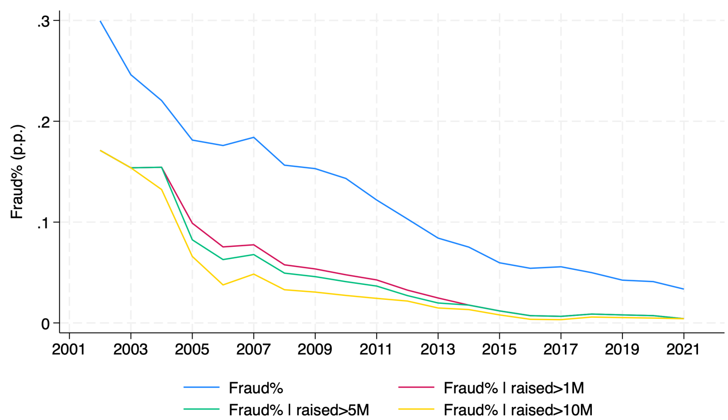
(b) Fraud Committing Years

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

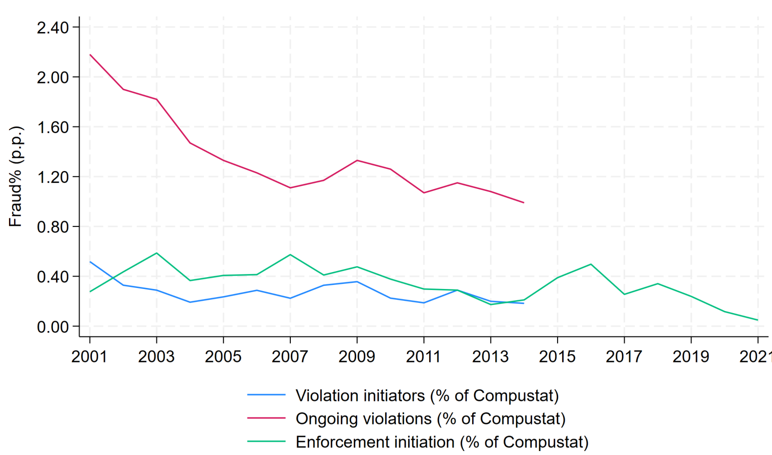
Figure 2: Fraud Likelihood Among US VC-Backed Startups vs PE-Backed or Public Firms



(a) US VC-backed firms



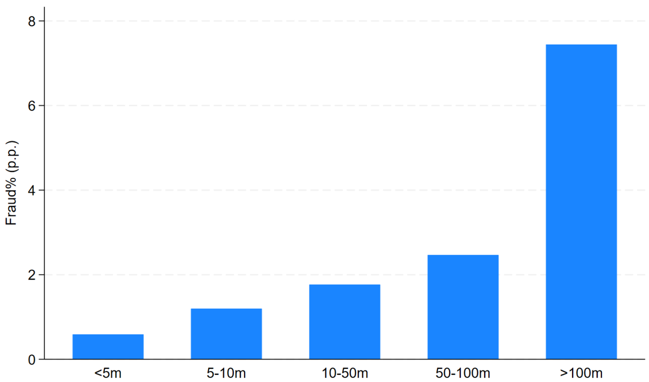
(b) US PE-backed firms



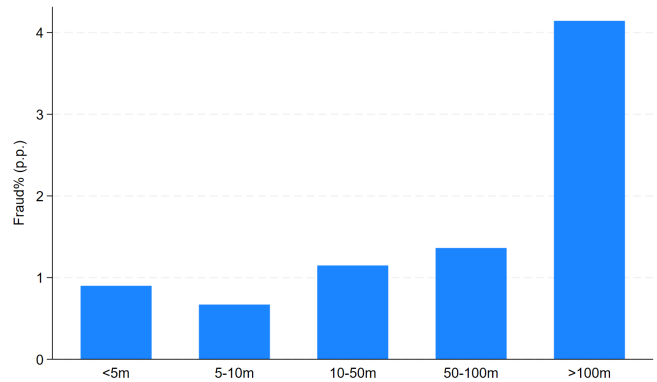
(c) US public firms (Source: Alawadhi et al. (2023))

Panel A shows annual fraud commission likelihood among US VC-backed startups by minimum raised amount. Panel B shows yearly 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).

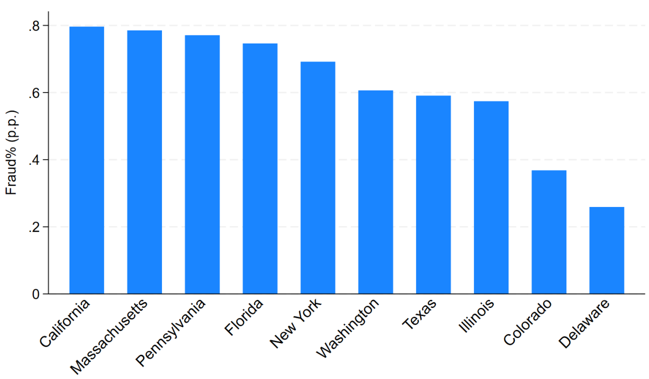
Figure 3: Variation in Fraud Likelihood Among US VC-Backed Startups



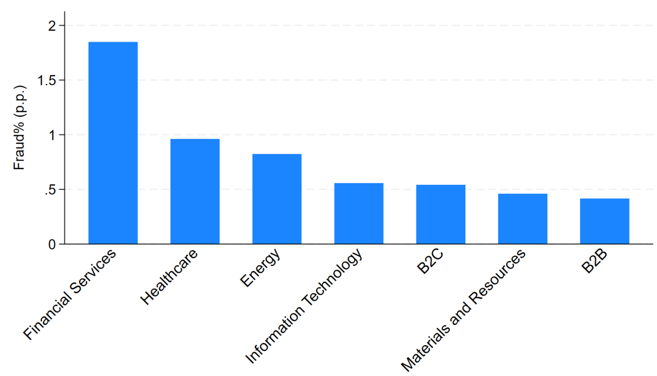
(a) By total raised amount at first round



(b) By post-money valuation at first round



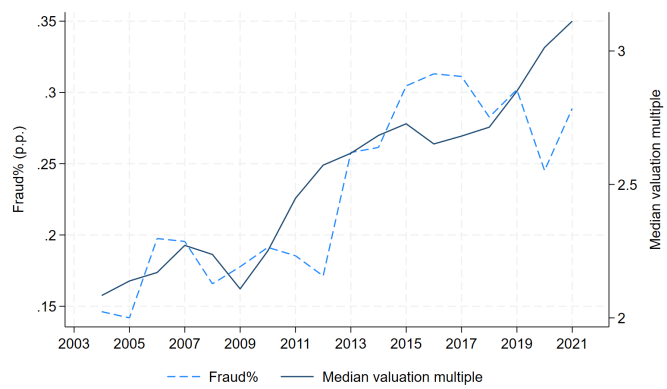
(c) By headquarter state



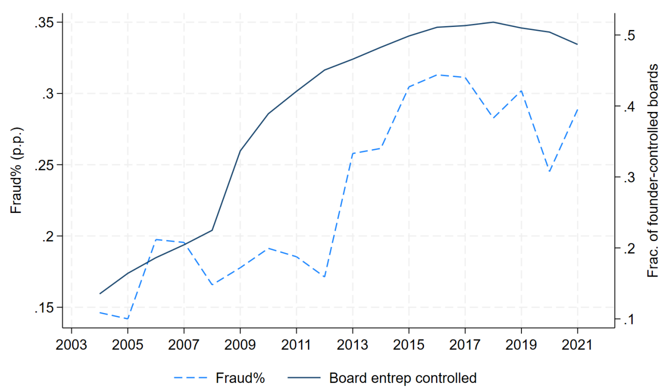
(d) By sector

This figure shows 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), headquarter state (Panel C), and sector (Panel D).

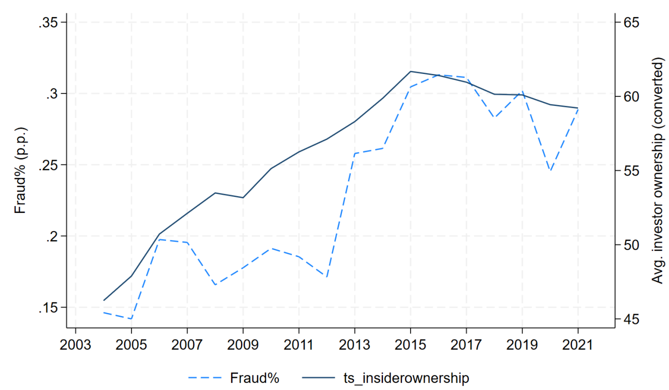
Figure 4: Trends in Fraud Likelihood and Founder Friendliness



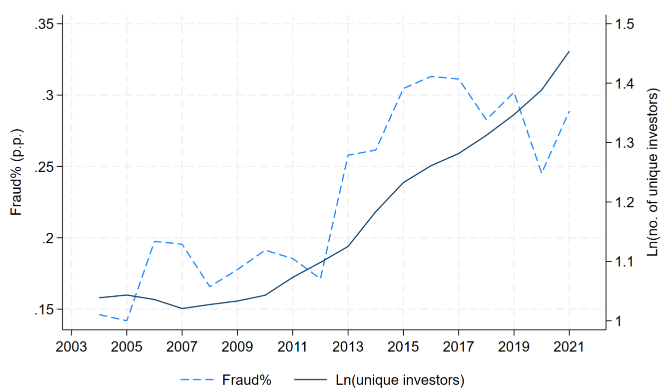
(a) Valuation multiple



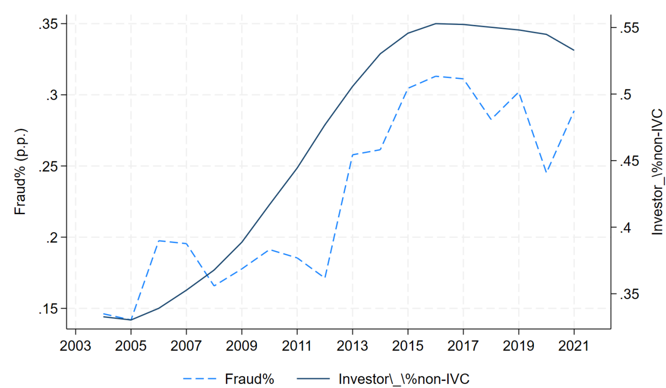
(b) Founder-controlled boards



(c) Founder/employee Ownership



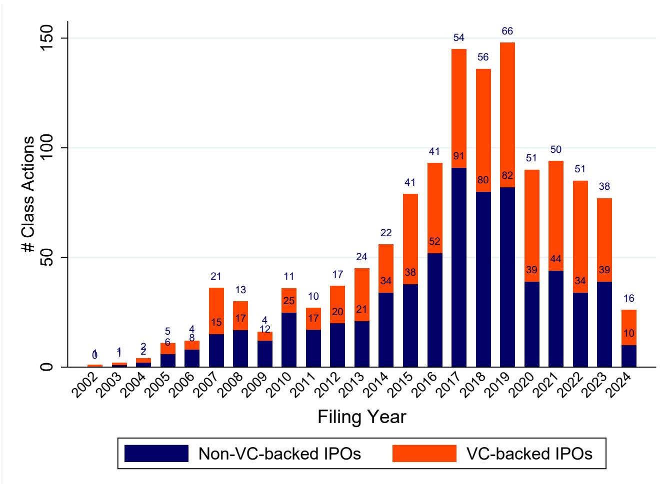
(d) Ln(no. of unique investors)



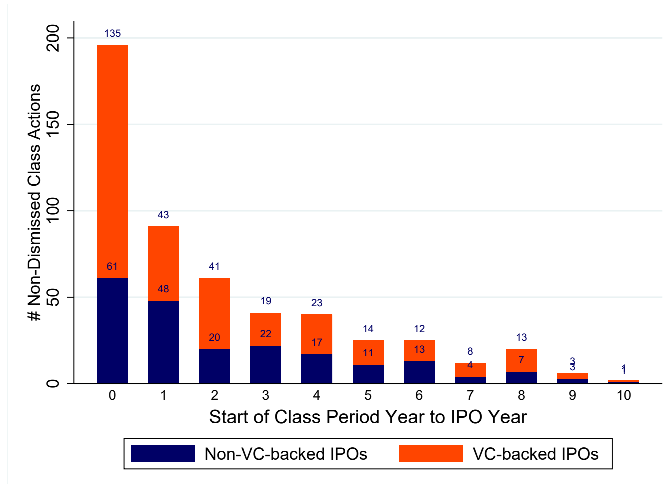
(e) Fraction of non-IVC investors

This figure plots the annual probability of fraud (in percentage points) by US VC-backed startups founded since 2000 against measures of founder-friendliness in the VC market. In all plots, the blue dashed line is ongoing fraud commission likelihood among all VC-backed startups. The solid line is the median valuation multiple (post-valuation divided/total raised amount) in Panel A, the average founder/employee ownership percentage (i.e., 100 minus investor ownership) in Panel B, the average fraction of founder-controlled boards in Panel C, the average log number of unique investors on cap table in Panel D, and the average fraction of non-independent-VC investors on cap table in Panel E.

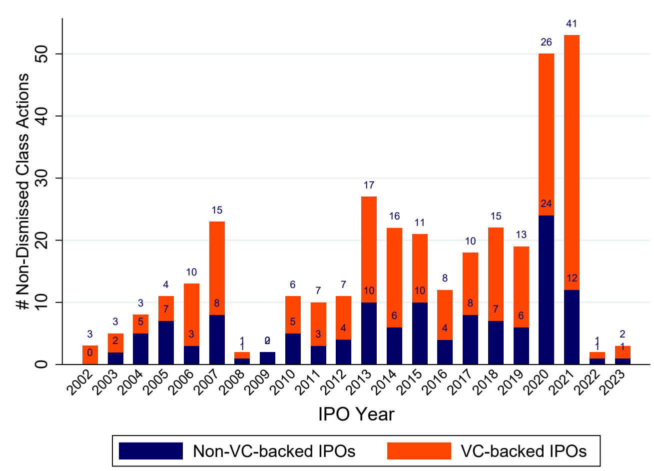
Figure 5: Class Actions by VC-Backed Status



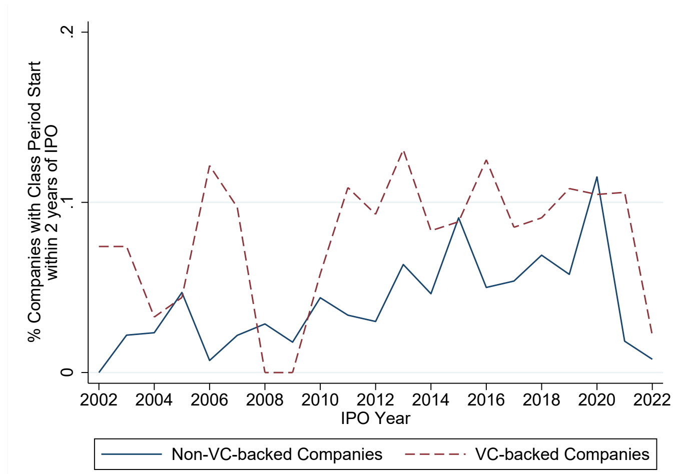
(a) Class Actions by VC Status



(b) Start of Class Period to IPO



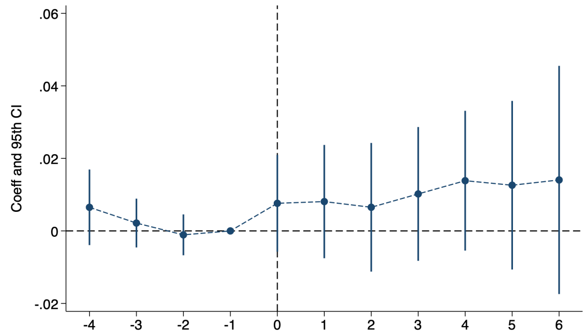
(c) Class Actions Started within 2 Years of IPO



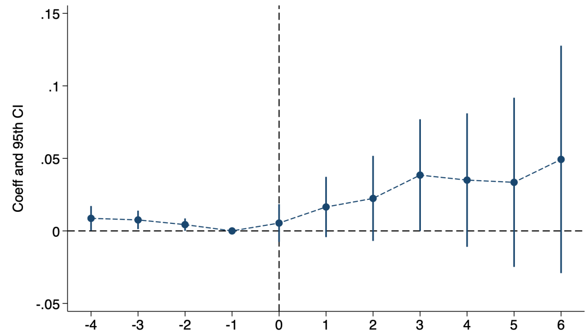
(d) Likelihood of Class Action within 2 Years of IPO: VC-Backed vs Non-VC-Backed

These figures show the distribution of securities class action lawsuits over time and by VC-backed status. The sample is restricted to non-dismissed class action suits against companies that went public after 2001. Figures (a) and (b) examine all class action suits regardless of timing relative to IPO. Figure (a) shows the distribution by filing year, while Figure (b) shows the distribution by the number of years between IPO and the start of the class period. Figures (c) and (d) focus on cases with class period starting within two years of IPO. Figure (c) shows the distribution by IPO year. Figure (d) compares the likelihood of class action suits across VC-backed and non-VC-backed IPO firms.

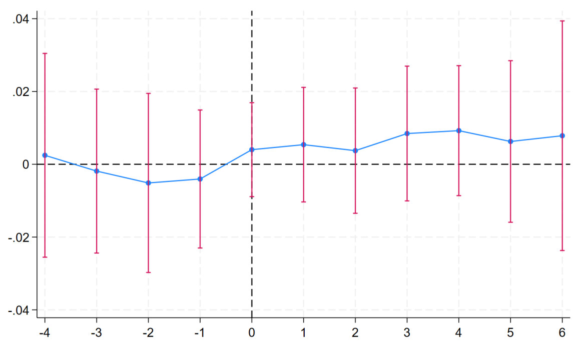
Figure 6: Effect of Fraud on Entrepreneurs' Future Founding: Dynamics Around Charge Year



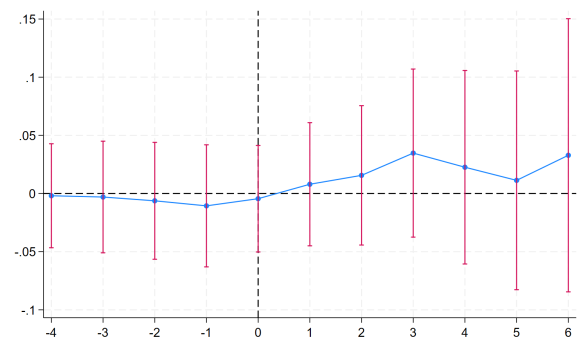
(a) Annual number of startups founded: stacked DID



(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 charge on entrepreneurs' subsequent founding activities, estimated through a matched DID on a person-year level panel. 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 $\times$ person fixed effects and stack $\times$ 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 charge, we match him/her to one control founder, based on the same number of founded startups in the past, tenure at current startup, gender, top school indicator, sector of current startup, 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	124
DOJ	77
Westlaw	200
SCAC	204
Total unique enforced cases	512
Dismissed cases from Westlaw	85
Allegations & investigations from news	17
Total unique non-enforced cases	102
Total cases	614

Panel B. SEC Cases

	Obs	Mean	Std Dev	P5	P50	P95
Fraud duration (months)	117	28.188	24.834	1.000	23.000	78.000
Fined amount ($k)	70	9,683	34,485	35	994	65,000
Judgement: fine	115	0.609	0.490	0.000	1.000	1.000
Judgement: disgorgement	115	0.652	0.478	0.000	1.000	1.000
Judgement: ban	115	0.574	0.497	0.000	1.000	1.000
Misrepresented: financials	123	0.699	0.460	0.000	1.000	1.000
Misrepresented: product	123	0.244	0.431	0.000	0.000	1.000
Misrepresented: use of funds	123	0.187	0.391	0.000	0.000	1.000
Victim: investor	123	0.894	0.309	0.000	1.000	1.000
Victim: government	123	0.041	0.198	0.000	0.000	0.000
Victim: public	123	0.065	0.248	0.000	0.000	1.000
Victim: other	123	0.057	0.233	0.000	0.000	1.000

Panel C. DOJ Cases

	Obs	Mean	Std Dev	P5	P50	P95
Fraud duration (months)	66	37.697	29.016	3.000	33.000	90.000
Fraud type: bank	77	0.078	0.270	0.000	0.000	1.000
Fraud type: wire	77	0.857	0.352	0.000	1.000	1.000
Fraud type: mail	77	0.052	0.223	0.000	0.000	1.000
Fraud type: corporate	77	0.104	0.307	0.000	0.000	1.000
Fraud type: federal	77	0.143	0.352	0.000	0.000	1.000
Fraud type: securities	77	0.325	0.471	0.000	0.000	1.000
Fraud type: tax	77	0.026	0.160	0.000	0.000	0.000
Fraud type: other	77	0.026	0.160	0.000	0.000	0.000
Sentence: months of prison	32	87.344	97.189	12.000	51.000	340.000
Sentence: fined amount ($k)	40	21,173	87,699	50	1,875	77,185
Sentence: fine	77	0.519	0.503	0.000	1.000	1.000
Sentence: prison	77	0.416	0.496	0.000	0.000	1.000
Sentence: forfeiture	51	0.157	0.367	0.000	0.000	1.000
Sentence: supervised release	51	0.333	0.476	0.000	0.000	1.000
Sentence: community service	51	0.020	0.140	0.000	0.000	0.000
Victim: investor	77	0.688	0.466	0.000	1.000	1.000
Victim: government	77	0.156	0.365	0.000	0.000	1.000
Victim: public	77	0.221	0.417	0.000	0.000	1.000
Victim: other	77	0.117	0.323	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)	200	28.714	32.967	0.783	14.233	94.900
Fraud type: fiduciary duty	200	0.325	0.470	0.000	0.000	1.000
Fraud type: wire	200	0.050	0.218	0.000	0.000	0.500
Fraud type: corporate	200	0.400	0.491	0.000	0.000	1.000
Fraud type: securities	200	0.405	0.492	0.000	0.000	1.000
Fraud type: other	200	0.130	0.337	0.000	0.000	1.000
Misrepresented: financials	200	0.580	0.495	0.000	1.000	1.000
Misrepresented: product	200	0.215	0.412	0.000	0.000	1.000
Misrepresented: use of funds	200	0.110	0.314	0.000	0.000	1.000
Misrepresented: other	200	0.095	0.294	0.000	0.000	1.000
Victim: investor	200	0.655	0.477	0.000	1.000	1.000
Victim: public	200	0.115	0.320	0.000	0.000	1.000
Victim: corporate	200	0.005	0.071	0.000	0.000	0.000
Victim: other	200	0.155	0.363	0.000	0.000	1.000

Panel E. Class Action Cases

	Obs	Mean	Std Dev	P5	P50	P95
Case settled	204	0.534	0.500	0.000	1.000	1.000
Settled amount ($k)	204	16,593	70,414	0.000	693	71,000
1934 Act Sec9: securities trading	204	0.005	0.070	0.000	0.000	0.000
1934 Act Sec10b: antifraud	204	0.809	0.394	0.000	1.000	1.000
1934 Act Sec14a: proxy solitication fraud	204	0.083	0.277	0.000	0.000	1.000
1934 Act Sec14e: tender offer antifraud	204	0.010	0.099	0.000	0.000	0.000
1934 Act Sec20a: control person liability	204	0.838	0.369	0.000	1.000	1.000
1933 Act Sec11: registration statement	204	0.422	0.495	0.000	0.000	1.000
1933 Act Sec12a2: prospectus & oral commu.	204	0.196	0.398	0.000	0.000	1.000
1933 Act Sec15: control person liability	204	0.412	0.493	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)	98	17.078	20.336	0.733	9.100	51.933
Allegations from news	102	0.167	0.375	0.000	0.000	1.000
Fraud type: fiduciary duty	102	0.118	0.324	0.000	0.000	1.000
Fraud type: wire	102	0.039	0.195	0.000	0.000	0.000
Fraud type: corporate	102	0.422	0.496	0.000	0.000	1.000
Fraud type: securities	102	0.422	0.496	0.000	0.000	1.000
Fraud type: other	102	0.118	0.324	0.000	0.000	1.000
Misrepresented: financials	102	0.539	0.501	0.000	1.000	1.000
Misrepresented: product	102	0.314	0.466	0.000	0.000	1.000
Misrepresented: use of funds	102	0.020	0.139	0.000	0.000	0.000
Misrepresented: other	102	0.127	0.335	0.000	0.000	1.000
Victim: investors	102	0.520	0.502	0.000	1.000	1.000
Victim: public	102	0.167	0.375	0.000	0.000	1.000
Victim: corporate	102	0.039	0.195	0.000	0.000	0.000
Victim: other	102	0.265	0.443	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 614 unique cases, 512 of which are enforced and 102 non-enforced (dismissed or allegations). We restrict to US VC-backed startups founded since 2000. For startups that achieved IPO, we restrict to SEC/DOJ/Westlaw cases where fraud start date is before IPO, as well as class action cases with class period start date within 2 years of IPO. Panels B to F present case attributes for SEC, DOJ, Westlaw, class actions, and dismissed cases/allegations, respectively.

Table 2: Summary Statistics on Prediction Panel

	Obs	Mean	Std Dev	P5	P50	P95
$\mathbb{1}$(Fraud start) × 100	769,016	0.067	2.597	0.000	0.000	0.000
Firm age	769,016	5.984	5.095	0.000	5.000	16.000
Ln(valuation)	154,770	3.132	1.760	0.336	3.063	6.146
Ln(raised)	392,452	1.280	2.141	-2.303	1.379	4.651
Board size	219,836	3.378	2.173	1.000	3.000	7.000
Board founder controlled	211,668	0.495	0.500	0.000	0.000	1.000
Investors' converted ownership	268,062	0.458	0.207	0.135	0.451	0.805
High liquidation multiple	272,063	0.027	0.149	0.000	0.000	0.000
Ln(unique investors)	478,793	1.337	0.995	0.000	1.386	2.996
Investor_%non-IVC	533,754	0.515	0.357	0.000	0.500	1.000
Team_solo	505,361	0.467	0.499	0.000	0.000	1.000
Team_serial%	505,361	0.143	0.304	0.000	0.000	1.000
Team_topschool%	412,543	0.274	0.407	0.000	0.000	1.000
Team_female%	505,361	0.121	0.279	0.000	0.000	1.000
Team_age at founding	312,826	36.622	9.846	23.000	35.000	55.000
Missing valuation	769,016	0.799	0.401	0.000	1.000	1.000
Missing board info	769,016	0.725	0.447	0.000	1.000	1.000
Missing investor type	769,016	0.306	0.461	0.000	0.000	1.000
Missing term sheet info	769,016	0.651	0.477	0.000	1.000	1.000
Missing team info	769,016	0.343	0.475	0.000	0.000	1.000
Missing team edu info	769,016	0.517	0.500	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 founding year to the fraud start year. Missing values are unfilled in this table to show proper summary statistics but are filled in Table 5.

Table 3: Summary Statistics on IPOs and Class Action Sample

Panel A: Sample of IPOs (2002–2024)

	Obs	Mean	Std Dev	P5	P50	P95
VC-Backed	4094	0.390	0.488	0.000	0.000	1.000
Class action	4094	0.084	0.278	0.000	0.000	1.000
Class action start within 30 days of IPO	4094	0.029	0.167	0.000	0.000	0.000
Class action start within 1y IPO	4094	0.042	0.200	0.000	0.000	0.000
Class action start within 2y IPO	4094	0.054	0.227	0.000	0.000	1.000
Log(Total Assets)_preIPO	1884	4.731	2.229	0.876	4.712	8.333
Revenue Growth_preIPO	1815	103.791	493.913	-6.216	18.000	405.602

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

	Obs	Mean	Std Dev	P5	P50	P95
VC-Backed	276	0.594	0.492	0.000	1.000	1.000
Securities fraud	276	0.736	0.442	0.000	1.000	1.000
Class period start - end year	276	2.376	1.911	0.319	1.847	6.069
Class action start within 30 days of IPO	276	0.529	0.500	0.000	1.000	1.000
Class action start within 1y IPO	276	0.775	0.418	0.000	1.000	1.000
IPO year	276	2015.283	5.607	2005.000	2017.000	2021.000
Filing year	276	2016.935	5.737	2006.000	2019.000	2024.000
Start class period year	276	2015.822	5.626	2005.000	2018.000	2022.000
Settlement amount (M$)	166	23.364	72.415	1.000	7.500	71.000
Log(Total Assets)_preIPO	181	5.411	2.087	1.762	5.559	8.446
Revenue Growth_preIPO	177	287.265	1015.046	-3.321	55.000	1112.565

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 (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	0.0350***	0.0352***	0.0385***	0.0296***	0.0347***	0.0442***
	(0.005)	(0.007)	(0.007)	(0.011)	(0.011)	(0.012)
Log(Total Assets)_preIPO				0.0079***	0.0092***	0.0100***
				(0.001)	(0.002)	(0.003)
Revenue Growth_preIPO				0.0000	0.0000*	0.0001**
				(0.000)	(0.000)	(0.000)
SIC-3 Sector FE × IPO Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.111	0.111	0.125	0.116	0.127	0.151
N	4,094	4,094	4,094	1,707	1,707	1,707
Mean dep. var.	0.0288	0.0418	0.0545	0.0492	0.0668	0.0814

Panel B: Post Dot-com Bubble Period (IPOs between 2002–2012)

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	0.0229	0.0266	0.0341	0.0350*	0.0407	0.0452
	(0.018)	(0.019)	(0.024)	(0.019)	(0.025)	(0.031)
Log(Total Assets)_preIPO				0.0015	0.0038	-0.0035
				(0.005)	(0.004)	(0.006)
Revenue Growth_preIPO				0.0000	0.0000	0.0000
				(0.000)	(0.000)	(0.000)
SIC-3 Sector FE × IPO Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.166	0.168	0.175	0.175	0.191	0.224
N	1,463	1,463	1,463	501	501	501
Mean dep. var.	0.0246	0.0273	0.0369	0.0399	0.0459	0.0639

Panel C: Recent Period (IPOs between 2013–2023)

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	0.0384***	0.0377***	0.0398***	0.0275**	0.0321**	0.0430**
	(0.007)	(0.010)	(0.011)	(0.012)	(0.015)	(0.018)
Log(Total Assets)_preIPO				0.0092***	0.0102***	0.0126***
				(0.002)	(0.003)	(0.003)
Revenue Growth_preIPO				0.0000	0.0001**	0.0001***
				(0.000)	(0.000)	(0.000)
SIC-3 Sector FE × IPO Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.087	0.089	0.104	0.097	0.109	0.130
N	2,631	2,631	2,631	1,206	1,206	1,206
Mean dep. var.	0.0312	0.0498	0.0642	0.0531	0.0755	0.0887

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

Panel D: 2SLS Estimation (IPOs between 2002-2023)

Start Class Period within:	30 days of IPO	1 year of IPO	2 years of IPO
	(1)	(2)	(3)
VC-backed	0.1879**	0.2043***	0.1928***
	(0.071)	(0.067)	(0.063)
Log(Total Assets)	0.0159***	0.0177***	0.0190***
	(0.003)	(0.005)	(0.004)
Growth Revenues$_{t;t-1}$	0.0001*	0.0001*	0.0001**
	(0.000)	(0.000)	(0.000)
1st stage Coefficient	0.043***	0.043***	0.043***
	(0.006)	(0.006)	(0.006)
1st stage KP F-stat	68.8	68.8	68.8
SIC-3 Sector FE × IPO Year FE	Yes	Yes	Yes
N	1348	1348	1348
R^2	-0.034	-0.021	0.001
Outcome Mean	0.066	0.087	0.108

Panels A, B and C of this table report 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 D reports 2SLS estimates using the average amount invested by the VC industry in the firm's headquarter state 3 years around the founding year as an instrument, following Hochberg (2012). 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. 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 and revenue growth from the year before IPO to IPO year. All regressions include SIC-3digit interacted with IPO year fixed effects. Standard errors are clustered at the SIC-3digit 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) × 100			
	(1)	(2)	(3)	(4)
Board founder controlled	0.059***	0.061***	0.062***	0.078***
	(0.018)	(0.018)	(0.019)	(0.019)
Investors' converted ownership	-0.145***	-0.144***	-0.146***	-0.205***
	(0.033)	(0.033)	(0.033)	(0.052)
High liquidation multiple	0.100**	0.100**	0.095*	0.303**
	(0.045)	(0.045)	(0.049)	(0.119)
Ln(unique investors)	0.024***	0.024***	0.024***	0.078***
	(0.005)	(0.005)	(0.005)	(0.011)
Investor_%non-IVC	0.056***	0.057***	0.054***	0.107***
	(0.011)	(0.011)	(0.011)	(0.017)
Firm age	-0.001	-0.001	-0.001	
	(0.001)	(0.001)	(0.001)	
Ln(valuation)	0.186***	0.186***	0.186***	0.179***
	(0.023)	(0.023)	(0.023)	(0.026)
Ln(raised)	0.016***	0.016***	0.016***	0.019***
	(0.004)	(0.004)	(0.004)	(0.003)
Board size	0.020***	0.020***	0.021***	0.037***
	(0.006)	(0.006)	(0.006)	(0.008)
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	769016	769016	769016	769016
R^2	0.005	0.005	0.006	0.095
Outcome Mean	0.067	0.067	0.067	0.067

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 founding year to the fraud start year. The dependent variable is 100 in the fraud start year for fraud firms and is 0 otherwise. We restrict to SEC/DOJ/Westlaw cases where fraud start year is no later than IPO year, and class action cases with class period start date within 2 years of IPO. The regression sample contains 519 fraud cases. Missing value indicators are included and their coefficients are in Table A.4 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 Governance vs Team Predictors

Dependent Variable:	$\mathbb{1}$(Fraud start) $\times$ 100	
	(1)	(2)
Team_solo	0.012*	0.017**
	(0.007)	(0.007)
Team_serial%	0.021*	0.022*
	(0.011)	(0.011)
Team_topschool%	0.015*	0.013
	(0.009)	(0.009)
Team_female%	0.008	0.006
	(0.020)	(0.020)
Team_age at founding	-0.001***	-0.001***
	(0.000)	(0.000)
Board founder controlled		0.061***
		(0.018)
Investors' converted ownership		-0.144***
		(0.033)
High liquidation multiple		0.100**
		(0.045)
Ln(unique investors)		0.024***
		(0.005)
Investor_%non-IVC		0.057***
		(0.011)
FE: ind-year	Yes	Yes
Other controls in Table 5	Yes	Yes
Missing value indicators	Yes	Yes
N	769016	769016
R^2	0.005	0.005
Outcome Mean	0.067	0.067

Panel B: Compare Explanatory Power

FE	Governance Var Partial R^2	Team Var Partial R^2	Ratio
Ind-yr	0.00016	0.00003	5.60
Ind, yr	0.00016	0.00003	5.61
None	0.00016	0.00003	5.70

Panel A reports the coefficients on team characteristics that were included in Table 5 but unreported for brevity. Column 1 includes team characteristics but drops governance variables, while column 2 corresponds to column 2 of Table 5. Other controls (firm age, valuation, raised amount, board size, missing value indicators) are included but are unreported for brevity. Panel B compares the partial R^2 of governance versus team variables. Governance variables include board control, investor ownership, high liquidation multiple, 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 Samples

Dependent Variable:	$\mathbb{1}$(Fraud start) × 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)	(0.014)	(0.014)	(0.014)
Investors' converted ownership	-0.129***	-0.130***	-0.075***	-0.076***
	(0.032)	(0.033)	(0.020)	(0.020)
High liquidation multiple	0.096*	0.094*	0.078*	0.078
	(0.051)	(0.053)	(0.046)	(0.049)
Ln(unique investors)	0.021***	0.021***	0.018***	0.017***
	(0.004)	(0.004)	(0.003)	(0.004)
Investor_%non-IVC	0.048***	0.046***	0.042***	0.040***
	(0.010)	(0.010)	(0.009)	(0.010)
Firm age	-0.001	-0.002*	-0.001**	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)
Ln(valuation)	0.171***	0.171***	0.097***	0.097***
	(0.023)	(0.023)	(0.014)	(0.014)
Ln(raised)	0.011***	0.011***	0.011***	0.011***
	(0.003)	(0.003)	(0.003)	(0.003)
Board size	0.015***	0.016***	0.005	0.005*
	(0.004)	(0.005)	(0.003)	(0.003)
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	768178	768178	767541	767541
R^2	0.005	0.006	0.002	0.003
Outcome Mean	0.057	0.057	0.051	0.051

This table demonstrates the robustness of Table 5 to using two alternative samples of fraud. Columns 1 and 2 drops non-enforced cases—i.e., dismissed cases identified from Westlaw and allegations or investigations identified from news; the sample contains 435 cases. Columns 3 and 4 drop class action cases; the sample contains 390 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 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) × 100		
	(1)	(2)	(3)
Board founder controlled	3.468**		3.617*
	(1.694)		(1.794)
DualClassIPO	6.635**		6.558**
	(2.831)		(2.785)
Investors' converted ownership	-5.902		-5.955
	(4.417)		(4.543)
High liquidation multiple	2.655		2.583
	(4.110)		(3.973)
Ln(unique investors)	1.345*		1.474*
	(0.724)		(0.765)
Investor_%non-IVC	3.495*		3.368
	(1.924)		(2.066)
Team_solo		1.724	1.999*
		(1.155)	(1.165)
Team_serial%		1.223	1.160
		(1.252)	(1.333)
Team_topschool%		-0.061	-0.127
		(1.771)	(1.687)
Team_female%		5.552	5.678
		(4.599)	(4.943)
Team_age at founding		-0.075*	-0.069
		(0.041)	(0.043)
Firm age	-0.241	-0.219	-0.242
	(0.146)	(0.182)	(0.168)
Ln(valuation)	4.344***	4.880***	4.387***
	(0.524)	(0.667)	(0.534)
Ln(raised)	1.181**	1.306***	1.198**
	(0.443)	(0.364)	(0.440)
Board size	0.096	-0.243	0.074
	(0.319)	(0.226)	(0.324)
FE: ind, yr	Yes	Yes	Yes
Missing value indicators	Yes	Yes	Yes
N	2475	2475	2475
R^2	0.089	0.085	0.090
Outcome Mean	8.252	8.252	8.252

The sample is a cross-section of US VC-backed firms founded since 2000 that went 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 204 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 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	0.369*	0.402**	1.915**	1.954**
	(0.188)	(0.190)	(0.849)	(0.882)
Firm age		(0.006)		(0.004)
		(0.016)		(0.016)
Ln(valuation)		0.360***		0.358***
		(0.085)		(0.084)
Ln(raised)		0.233***		0.239***
		(0.048)		(0.050)
Missing valuation		0.558***		0.557***
		(0.149)		(0.149)
First-stage coff on IV:				
Buyout funds raised in t-3 to t-1 ($b)			0.011***	0.011***
			(0.001)	(0.001)
First stage KP F-stat			69.3	69.3
FE: ind	Yes	Yes	Yes	Yes
FE: state-yr	Yes	Yes	Yes	Yes
N	63581	63581	63581	63581
R^2	0.007	0.010	-0.017	-0.014
Outcome mean	0.910	0.910	0.910	0.910

This table shows that hot VC market conditions at firms' initial financing round predict future fraud. The sample is at the firm level, covering 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 early round valuation multiple. Specifically, we take the average of the ratio between post-money valuation and total raised amount 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 where we instrument industry-year valuation multiple with total dollar (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 are firm age, post-money valuation, total raised amount, and missing valuation indicator, all measured at a firm's initial VC round. All columns include sector 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.

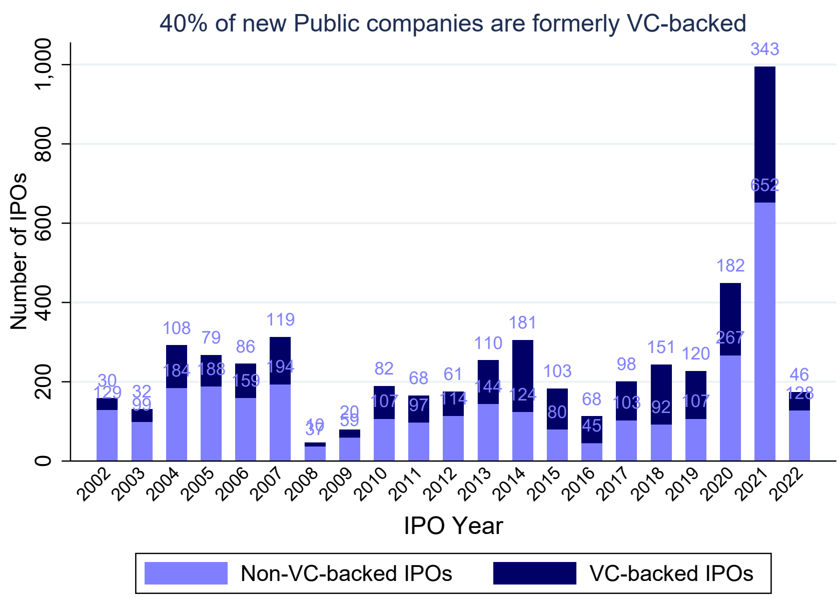
Table 10: Future Founding of VC-Backed Startups by Fraudulent Entrepreneurs

Dependent variable:	Startups founded (1)	Cumulative startups founded (2)	Startups founded (3)	Cumulative startups founded (4)
Treat × Post	0.007*	0.017	0.007	0.016
	(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	15346	15346		
R^2	0.609	0.925	-	-
Outcome mean	0.043	1.335	0.043	1.335

This table reports the baseline DID estimates for the effect of fraud revelation (i.e., initial charge) on founders' future founding of VC-backed startups. The estimates correspond to the event study plots in Figure 6. 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) and (2) estimate a stacked DID following Cengiz et al. (2019), with stack × person and stack × event year fixed effects. Columns (3) and (4) use the imputation-based DID from Borusyak et al. (2024). The dependent variable in Columns and (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 charge, we match him/her to a control founder, based on the same number of past founded startups, tenure at current startup, gender, top school indicator, sector of current startup, and closest in age. Standard errors are clustered at the person level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

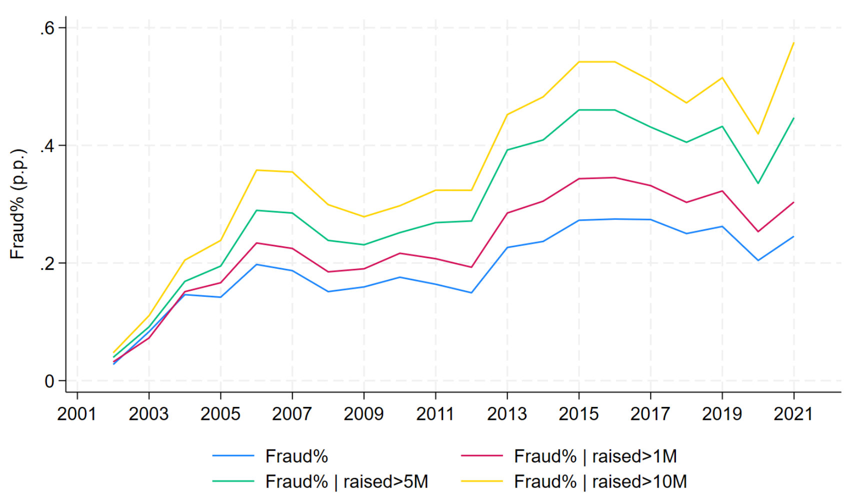
Appendix

Figure A.1: Number of VC-Backed and Non-VC-Backed IPOs



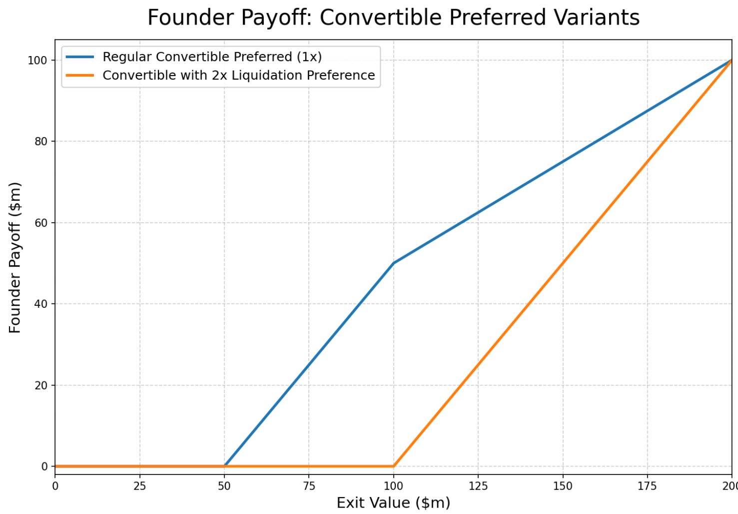
This figure plots the number of VC-backed and non-VC-backed firms by IPO year.

Figure A.2: Fraud Likelihood Among US VC-Backed: Excluding Non-Enforced Cases



This figure shows annual fraud commission likelihood among US VC-backed startups by minimum raised amount, restricting to enforced fraud cases only.

Figure A.3: Founder's Payoff When Investor Has High Liquidation Preference or Participating Rights



This figure plots founder's payoff from common equity when VCs, who hold convertible preferred equity, have high (2x) liquidation preference vs regular (1x) liquidation preference.

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 sample against the set of cases reported by the Startup Litigation Digest Blog. We use theStartup Litigation Digest Blog from the UC Center for Business Law as a benchmark for identifying high-profile cases that fit the scope and interest of this paper. This benchmark helps us evaluate the completeness of our data collection of venture fraud cases. 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 (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	0.0201***	0.0290***	0.0337***	0.0165***	0.0344***	0.0412***
	(0.004)	(0.006)	(0.006)	(0.006)	(0.008)	(0.011)
Log(Total Assets)_$IPOt-1$				0.0054***	0.0076***	0.0102***
				(0.001)	(0.001)	(0.002)
Revenue Growth_$IPOt-1;t$				0.0000**	0.0001**	0.0001**
				(0.000)	(0.000)	(0.000)
SIC-3 Sector FE × IPO Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.112	0.104	0.116	0.126	0.127	0.137
N	4,094	4,094	4,094	1,707	1,707	1,707
Mean dep. var.	0.0181	0.0366	0.0515	0.0293	0.0539	0.0762

Panel B: Including Dismissed Class Actions

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	0.0364***	0.0437***	0.0608***	0.0333***	0.0461***	0.0706***
	(0.005)	(0.006)	(0.007)	(0.011)	(0.011)	(0.015)
Log(Total Assets)_preIPO				0.0107***	0.0110***	0.0110***
				(0.002)	(0.002)	(0.002)
Revenue Growth_preIPO				0.0000	0.0000*	0.0000*
				(0.000)	(0.000)	(0.000)
SIC-3 Sector FE × IPO Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.108	0.124	0.142	0.121	0.145	0.152
N	4,535	4,535	4,535	2,001	2,001	2,001
Mean dep. var.	0.0366	0.0571	0.0818	0.0590	0.0860	0.1204

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	0.0350***	0.0352***	0.0385***	0.0296***	0.0347***	0.0442***
	(0.005)	(0.007)	(0.007)	(0.011)	(0.011)	(0.012)
Log(Total Assets)_preIPO				0.0079***	0.0092***	0.0100***
				(0.001)	(0.002)	(0.003)
Revenue Growth_preIPO				0.0000	0.0000*	0.0001**
				(0.000)	(0.000)	(0.000)
SIC-3 Sector FE × IPO Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.111	0.111	0.125	0.116	0.127	0.151
N	4,094	4,094	4,094	1,707	1,707	1,707
Mean dep. var.	0.0288	0.0418	0.0545	0.0492	0.0668	0.0814

Panel D: Non-dismissed class actions filed before 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	0.0134**	0.0138**	0.0191***	0.0165*	0.0170*	0.0244***
	(0.006)	(0.006)	(0.006)	(0.009)	(0.009)	(0.008)
Log(Total Assets)_preIPO				0.0038**	0.0046***	0.0047***
				(0.002)	(0.002)	(0.001)
Revenue Growth_preIPO				0.0000	0.0000	0.0000
				(0.000)	(0.000)	(0.000)
SIC-3 Sector FE × IPO Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.142	0.143	0.160	0.145	0.148	0.179
N	3,919	3,919	3,919	1,600	1,600	1,600
Mean dep. var.	0.0179	0.0222	0.0288	0.0288	0.0362	0.0469

This table shows the robustness of the reuslts inTable 4. Panel A uses the filing date rather than class period start date to define the timing of class actions relative to IPO. Panel B reintroduces cases that were dismissed. Panel C restricts to class actions with settlement amounts exceeding \$3 million. Panel D restricts to class actions filed between 2002 and 2019 to address potential truncation. All regressions include SIC-3digit industry interacted with IPO year fixed effects. All regressions include SIC-3 industry fixed effects based on CRSP classifications, interacted with IPO year fixed effects. Standard errors are clustered at the SIC-3 level and are reported in parentheses. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table A.4: Panel Prediction of Fraud Among VC-Backed Firms: Coefficients on Missing Value Indicators

Dependent Variable:	$\mathbb{1}$(Fraud start) $\times$ 100			
	(1)	(2)	(3)	(4)
Missing valuation	0.418***	0.418***	0.415***	0.442***
	(0.061)	(0.061)	(0.060)	(0.069)
Missing board info	0.105***	0.106***	0.112***	0.163***
	(0.026)	(0.026)	(0.027)	(0.039)
Missing investor type	0.035***	0.036***	0.038***	0.066***
	(0.008)	(0.008)	(0.009)	(0.013)
Missing term sheet info	0.003	0.003	0.003	0.009
	(0.011)	(0.011)	(0.010)	(0.021)
Missing team info	0.025***	0.035***	0.029**	
	(0.008)	(0.012)	(0.013)	
Missing team edu info	0.006	-0.013	-0.009	
	(0.007)	(0.008)	(0.008)	
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
Other controls	Yes	Yes	Yes	Yes
N	769016	769016	769016	769016
R^2	0.005	0.005	0.006	0.095
Outcome Mean	0.067	0.067	0.067	0.067

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.5: 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	-0.115***	-0.116***	-0.116***	-0.192***
	(0.037)	(0.037)	(0.036)	(0.045)
Board shared control	-0.033**	-0.035**	-0.037**	-0.031*
	(0.014)	(0.014)	(0.015)	(0.018)
Investors' converted ownership	-0.135***	-0.134***	-0.135***	-0.187***
	(0.032)	(0.032)	(0.032)	(0.050)
High liquidation multiple	0.099**	0.099**	0.094*	0.303**
	(0.045)	(0.046)	(0.049)	(0.119)
Ln(unique investors)	0.026***	0.025***	0.025***	0.080***
	(0.005)	(0.005)	(0.005)	(0.011)
Investor_%non-IVC	0.054***	0.055***	0.053***	0.104***
	(0.011)	(0.011)	(0.011)	(0.017)
Firm age	-0.001	-0.001	-0.001	
	(0.001)	(0.001)	(0.001)	
Ln(valuation)	0.187***	0.187***	0.187***	0.180***
	(0.024)	(0.024)	(0.023)	(0.026)
Ln(raised)	0.016***	0.016***	0.016***	0.020***
	(0.004)	(0.004)	(0.004)	(0.003)
Board size	0.022***	0.022***	0.023***	0.040***
	(0.006)	(0.006)	(0.006)	(0.008)
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	769016	769016	769016	769016
R^2	0.005	0.005	0.006	0.095
Outcome Mean	0.067	0.067	0.067	0.062

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 519 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.6: 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	0.061***	0.070***	0.071***
	(0.019)	(0.024)	(0.025)
Investors' converted ownership	-0.163***	-0.176***	-0.208***
	(0.035)	(0.039)	(0.044)
Ln(unique investors)	0.024***	0.033***	0.037***
	(0.005)	(0.007)	(0.009)
Investor_%non-IVC	0.075***	0.104***	0.140***
	(0.014)	(0.022)	(0.029)
High liquidation multiple	0.099**	0.102*	0.126*
	(0.047)	(0.055)	(0.066)
Firm age	-0.001*	-0.002*	-0.003*
	(0.001)	(0.001)	(0.001)
Ln(valuation)	0.220***	0.276***	0.313***
	(0.026)	(0.030)	(0.034)
Ln(raised)	0.018***	0.017***	0.016***
	(0.004)	(0.005)	(0.005)
Board size	0.019***	0.019**	0.023***
	(0.006)	(0.007)	(0.008)
FE: ind-year	Yes	Yes	Yes
FE: ind-1st round year	Yes	Yes	Yes
Team characteristic controls	Yes	Yes	Yes
Missing value indicators	Yes	Yes	Yes
N	598431	396856	301564
R^2	0.006	0.007	0.008
Outcome Mean	0.082	0.111	0.137

The table is similar to Table 5 Column 2 but restricts to firm-years with at least \$1M, \$5M, or \$10M cumulated financing. 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: Dropping Crypto/Blockchain Companies

Dependent Variable:	$\mathbb{1}$(Fraud start) $\times$ 100			
	(1)	(2)	(3)	(4)
Board founder controlled	0.040***	0.041***	0.043***	0.055***
	(0.013)	(0.013)	(0.013)	(0.017)
Investors' converted ownership	-0.114***	-0.113***	-0.115***	-0.154***
	(0.031)	(0.031)	(0.032)	(0.046)
High liquidation multiple	0.096*	0.096*	0.094*	0.264**
	(0.051)	(0.051)	(0.053)	(0.129)
Ln(unique investors)	0.019***	0.018***	0.018***	0.065***
	(0.004)	(0.004)	(0.004)	(0.008)
Investor_%non-IVC	0.043***	0.044***	0.042***	0.078***
	(0.011)	(0.011)	(0.010)	(0.019)
Firm age	-0.000	-0.000	-0.001	
	(0.001)	(0.001)	(0.001)	
Ln(valuation)	0.163***	0.163***	0.163***	0.156***
	(0.021)	(0.021)	(0.021)	(0.024)
Ln(raised)	0.013***	0.013***	0.012***	0.014***
	(0.003)	(0.003)	(0.003)	(0.002)
Board size	0.014***	0.014***	0.015***	0.028***
	(0.005)	(0.005)	(0.005)	(0.007)
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 characteristics controls	No	Yes	Yes	Yes
Missing value indicators	Yes	Yes	Yes	Yes
N	753583	753583	753583	753583
R^2	0.005	0.005	0.006	0.093
Outcome Mean	0.053	0.053	0.053	0.053

The table shows robustness of Table 5 to dropping firms in cryptocurrency or blockchain verticals. 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 399 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.8: VC Market Condition 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	0.031**	0.042	-0.016***	-0.015	0.077**	0.274**
	(0.014)	(0.043)	(0.005)	(0.018)	(0.029)	(0.118)
1st stage KP F-stat		66.8		134.1		77.6
FE: ind	Yes	Yes	Yes	Yes	Yes	Yes
FE: state-yr	Yes	Yes	Yes	Yes	Yes	Yes
N	25572	25572	27608	27608	51078	51078
R^2	0.044	-0.031	0.090	-0.028	0.084	-0.021
Outcome Mean	0.869	0.869	1.012	1.012	0.734	0.734

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 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.

A Appendix: Identifying Fraud Cases from Various Data Sources

A.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. This procedure yields 206 unique releases involving 108 companies and 214 executives in PitchBook.

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. This step identifies an additional 37 SEC releases linked to 31 companies and 88 individuals.

Combining both approaches, we identify 226 unique SEC enforcement actions from 1996 to 2023, associated with 214 companies and 261 founders/CEOs. Most cases occur after 2010, with 42 cases filed in 2023 alone. To refine the final dataset, we 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), 124 SEC cases enter our final sample used in predictive analysis.

A.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".

To identify cases involving startups, we proceed in several steps. First, we filter for cases tagged as financial or securities fraud and containing startup-related keywords (e.g., "founder", "venture capital", "Silicon Valley"). This yields 543 possible cases, of which 243 are manually matched to

162 unique companies in PitchBook.

Second, we apply a machine learning classifier (BERT) to predict the crime type where tags are missing. Among the predicted fraud cases, we manually verify 477 cases as involving startup fraud, identifying 90 additional matches to PitchBook. Third, we expand the training set to flag additional cases. 952 additional cases are predicted as startup-related fraud. We manually review these cases and confirm 477 of these, with 219 linked to PitchBook. Fourth, we review 3,250 additional cases predicted as fraud but not initially linked to VC-backed firms. Manual review yields 599 more matches with PitchBook.

Finally, we restrict the combined sample to U.S.-based firms in the PitchBook VC universe. We manually validate each case to make sure they are linked to the right PitchBook company. This results in 183 unique VC-backed companies involved in 227 DOJ cases. After imposing various filters, the final sample that enters our predictive analysis includes 77 DOJ cases.

A.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 276 cases by VC-backed firms, out of which 204 entered our predictive analysis after imposing various filters. We also obtain industry classifications from CRSP, pre-IPO financial information from Compustat (which cuts down the sample size by half) and IPO-related information from Jay Ritter's IPO database.

A.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 overlapped 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 invovled 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 include: 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 for 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 cases 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 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.

A.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.

Table A.9: Sample Construction and Case Counts

Sample Source	**Raw Cases**	**Unique Firms**	**Founding Year & Fraud Timing Filters**	**Final Sample**
DOJ	230	186	91	77
SEC	462	350	133	124
Westlaw	362	327	244	200
SCAC	1,687	1,434	224	204
Dismissed & News-based allegations	274	248	150	130
Total Cases	3,015			
Unique Firms		2,305	706	614

The table reports the construction of the analysis sample across multiple legal data sources. Column (3) retains U.S.-based, VC-backed firms founded in or after 2000, with alleged fraud starting no later than the IPO year or class period end. Column (4) further drops cases with missing information on the first VC financing round date. Unique case counts account for overlap across data sources.