Common Venture Capital Investors and Startup Growth

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Ofer Eldar
Duke University and ECGI

Jillian Grennan
Santa Clara University and University of California

Katherine Waldock
Columbia Law School

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Abstract

We exploit the staggered introduction of liability waivers when investors hold stakes in conflicting business opportunities as a shock to venture capital (VC) investment and director networks. We find increases in within-industry VC investment and common directors serving on startup boards after the law changes. Despite the potential for rent-extraction, same-industry startups inside VC portfolios benefit by raising more capital, failing less, and exiting more successfully. VC directors serving on other startup boards are the primary mechanism associated with positive outcomes, consistent with common VC investment facilitating informational exchanges in VC portfolios.

Keywords: Entrepreneurship, Startups, Venture Capital, Corporate Governance, Fiduciary Duty, Duty of Loyalty, Conflict of Interest, Corporate Opportunity Waivers, Board of Directors, Initial Public Offerings (IPOs), Raising Capital

JEL Classifications: G32, G24, G28

Ofer Eldar
Professor of Law, Economics and Finance
Duke University, School of Law
210 Science Drive Durham, NC 27708
Durham, NC 27708 , USA
phone: +! 919-613-7068
e-mail: eldar@law.duke.edu

Jillian Grennan*
Associate Professor of Finance
Santa Clara University
500 El Camino Real
Santa Clara, CA 95053, USA
phone: +1(202)265-6111
e-mail: jgrennan@scu.edu

Katherine Waldock
Researcher
Columbia Law School
435 W 116th St
New York, NY 10027, USA
e-mail: kpw2114@columbia.edu

*Corresponding Author

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OFER ELDAR — Duke University & ECGI

JILLIAN GRENNAN — Santa Clara University

KATHERINE WALDOCK — Columbia University

ABSTRACT

We exploit the staggered introduction of liability waivers when investors hold stakes in conflicting business opportunities as a shock to venture capital (VC) investment and director networks. We find increases in within-industry VC investment and common directors serving on startup boards after the law changes. Despite the potential for rent-extraction, same-industry startups inside VC portfolios benefit by raising more capital, failing less, and exiting more successfully. VC directors serving on other startup boards are the primary mechanism associated with positive outcomes, consistent with common VC investment facilitating informational exchanges in VC portfolios.

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*Authors: Eldar, Duke University and European Corporate Governance Institute (e-mail: eldar@law.duke.edu); Grennan, Santa Clara University (e-mail: jgrennan@scu.edu); Waldock, Columbia University (e-mail: kpw2114@columbia.edu).

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Venture Capital ("VC") investors play an important role in advising, monitoring, and providing expertise to entrepreneurial startups (Lerner, 1995; Kaplan and Stromberg, 2001; Hellmann and Puri, 2002; Gompers and Lerner, 2004; Bernstein et al., 2016). VC investors typically have substantial control rights (Kaplan and Stromberg, 2001; Gompers et al., 2020), and actively seek to constrain managerial discretion over key decisions through the appointment of board representatives (Amornsiripanitch et al., 2019). A key – yet often overlooked – feature of VC investments is that VC portfolios tend to include many startups in the same industry. In fact, the rate of startups in the same industry with a common VC investor has risen dramatically in recent years (Eldar and Grennan, 2021). Most startups nowadays share a VC investor with at least one other startup in the same industry. Even startups that operate in the same line of business, such as Uber and Lyft, often raise capital from the same VC investors.[1]

What is the relation between common VC investment and startups' trajectory for growth and success? On one hand, VC investors could play favorites, killing off one startup so another can succeed or diverting valuable competitive information from one startup to another (Fried and Ganor, 2006; Fulghieri and Sevilir, 2009; Pollman, 2019). To use an example from a recent court case, a common investor might use confidential information acquired through a board representative to benefit another company in its portfolio.[2] Startups in the same VC portfolio may be vulnerable not only when they compete in the exact same product category, but even if they operate in complementary spaces within the same industry (such as software and media). Startups in the same industry are thus likely to seek similar business opportunities, whether it is developing a new service or pursuing an attractive contract,[3] and there is a risk that VCs will favor some startups at the expense of others.

On the other hand, VC investors can act as incubators for valuable information and expertise.

[1] VC investors, such as All Blue Capital, Atop Capital, G Squared, and Next Equity, made investments in both Uber and Lyft prior to their initial public offerings.

[2] See Alarm.Com Holdings Inc. v. ABS Capital Partners Inc., C.A. 2017-0583-JTL (June 15, 2018).

[3] For example, the following companies all received VC investments from Benchmark and Greylock: Vudu, a box that allows users to rent or buy content through the internet; ManiaTV, an internet television network; Newport Media, a content developer for TV-enabled phones; SnappyTV, an inexpensive tool to provide TV content to digital audiences; and Metacafe, a provider of short-form video entertainment in various media.

The expertise acquired through common investments at the industry level could benefit all portfolio companies as well as maximize VC investors' returns. While startups may learn from one another via observable events (Kostovetsky and Manconi, 2020; Yang, 2020), a common VC investor would be privy to hard-to-observe information.[4] Rather than divert business opportunities from one startup to favor another, VCs can allocate different business opportunities efficiently to the startups that, based on the common VC's information, are best positioned to pursue them. Consistent with this hypothesis, there is some evidence that common VC investors facilitate strategic alliances (Lindsey, 2008) and innovation spillovers among startups in the same industry that share a common VC investor (Gonzalez-Uribe, 2020).

In this study, we provide a comprehensive analysis of the relationship between common VC investment and startup performance. Our goal is to evaluate whether startups with common VC investment are better or worse off than startups without common VC investment. We build on existing studies by making three key contributions: First, we use a novel empirical strategy based on plausibly exogenous legal changes to states' corporate laws. These changes facilitated common VC investment by reducing the liability risk associated with VCs holding stakes in multiple startups in the same industry. Second, we uncover a direct mechanism through which common VC investment creates a platform for informational exchanges, namely the appointment of directors in startup boards. To the best of our knowledge, our paper is the first to highlight the presence of overlapping directors in startups that operate in the same industry. Third, we evaluate the relationship between common VC investment and a variety of new and important startup outcomes, including additional rounds of financing, IPOs, sales, and failures. These are first-order measures of financial success for new ventures, and thus they are critical for evaluating the impact of common VC investment.

We begin our analysis by evaluating the incidence of common VC investors among VC-backed startups using a quasi-natural experiment: the staggered adoption of laws across eight states from 2000 to 2016 that enable corporations incorporated in these states to adopt corporate opportunity waivers (COWs). These waivers exempt investors and directors from litigation risk if they usurp a

[4]These informational advantages are often so important that banks only serve one firm in an industry (Asker and Ljungqvist, 2010; Massa and Zaldokas, 2017).

business opportunity in a way that conflicts with the firm's best interest (Talley, 1998; Rauterberg and Talley, 2017; Licht, 2018; Velasco, 2018). VC investments are typically accompanied by a board seat for a VC investor (Kaplan and Stromberg, 2003; Fried and Ganor, 2006; Bengtsson and Sensoy, 2015). These VC board members are privy to information that may bolster their expertise and effectiveness in managing the startups in their portfolios, but could also expose them to liability if it enables them to divert business opportunities from one startup to another. These risks are particularly acute in the context of VC investment in startups in the same industry because these startups compete for similar business opportunities. COWs are necessary to relieve VC investors and their director appointees from such potential liability.

To test the importance of the legal changes for common VC investment, we use a difference-in-differences estimator with the staggered adoption of the state laws permitting COWs serving as the treatment. In order to implement this strategy, we construct a panel dataset of startups' states of incorporation across time from 1995 to 2018. The reason is that state corporate laws apply based on the state in which a firm is incorporated rather than where it is located. To develop this dataset, we source the states of incorporation from filings available in Lexis Advance for all startups that receive VC financing based on the Preqin Venture Deals dataset. The final sample includes almost 143,000 observations and 15,000 startups. To the best of our knowledge, this is the most comprehensive dataset documenting the state of incorporation for private firms, and it represents virtually the full value of VC investments in the sample period.

Using this dataset, we find that on average, startups incorporated in treated states are 11.8 percentage points more likely to have a within-industry common VC investor after the law change. This result holds across a variety of specifications including ones with startup, VC, and headquarter-state-by-industry-by-year fixed effects as well as many control variables that help account for factors such as growth potential and VC reputation. In addition, when we evaluate year-by-year coefficient estimates, we find evidence consistent with the parallel trends assumption. The results are also robust to (i) excluding startups initially incorporated in Delaware, where a majority of startups are incorporated, (ii) excluding industries where the "spray and pray" VC investment strategy is common (Ewens et al., 2018) and thus could indirectly result in more common investment, and (iii) alternative sample constructions, such as those involving different startup lifecycle phases. Taken together, the evidence indicates that COW legislation is followed by a significant and sudden shift in common VC investment in startups in the same industry.

We examine the impact of common VC investment on various startup outcomes using the passage of the laws as an instrumental variable (IV) for common VC investment. The basis of our analysis is that the variation in liability risk generated by the staggered adoption of laws that permitted COWs is plausibly exogenous and satisfies the exclusion restriction. We undertake several steps – both qualitative and quantitative – to demonstrate this.

First, we explore the history of the laws, and find that the impetus for the state law changes was to eliminate uncertainty arising from case law. There was no apparent evidence of lobbying prior to the passage of these laws.

Second, we conduct a legal analysis demonstrating that these law changes had a very narrow application to the liability that arises from commonly owning and managing multiple startups with similar businesses (see Section 2.1 and Online Appendix A). Thus, unlike other laws that may affect the scope of managerial discretion in multiple ways,[5] these laws are more likely to affect outcomes only through their effect on common investments.

Third, to address the concern that firms' choices of their states of incorporation are endogenous, we conduct a survey of lawyers that work on VC deals and advise startups on governance issues and financing terms (see Section 2.2 and Online Appendix B). The main concern here is that high-growth startups are more likely to incorporate in Delaware, which is the most popular state of incorporation, and the first adopter of the COW legislation. Our survey suggests that by far the most important reason startups choose to incorporate in Delaware is the familiarity of lawyers with Delaware law and its precedents. Based on the survey, the chief reason for firms not to incorporate in Delaware was the personal preference of the founder or lawyers. Moreover, substantive laws, especially the COW laws, do not meaningfully affect incorporation choices, and the state of incorporation is rarely negotiated in VC deals.

[5]For example, constituency statutes may both reduce the risk of managers' liability for violating their fiduciary duties to shareholders and increase managers' attentiveness to stakeholders' interests.

Fourth, we extend the difference-in-difference analysis to startup outcomes (see Online Appendix G), which is the reduced form for any regression that uses the law changes as an IV. We also evaluate the parallel trends figures for each outcome, and see no evidence of differential secular trends prior to treatment. While the reduced-form analyses show that the IV significantly relates to startup growth, it may also be indirectly related to startup growth through other channels. Accordingly, we follow the approach pioneered in the econometrics literature (Conley et al., 2012) and conduct a placebo test to demonstrate that for a subsample in which the IV does not affect common VC investment, common investment is not significantly associated with startup outcomes (see Online Appendix F).

Fifth, we address the potential concern that the legal changes as a shock may reflect an intent to treat rather than treatment on the treated. The reason is that the laws only permitted firms to adopt COWs rather than automatically exempting investors and directors from liability. We provide qualitative evidence that adoptions of COWs have been standard practice in the VC industry given the potential harmful impact of litigation on VCs' reputations (Atanasov et al., 2012). In particular, we rely on our survey of lawyers because the organizational documents of private startups are not publicly available. The survey reveals that COWs are standard provisions in the charter or bylaws of startups, and if the COW provision is not included in the charter or bylaws, it is usually included in another legal document, such as a privately negotiated contract.

Accordingly, we believe that our analysis supports the validity of the exclusion restriction, and indicates that the laws that permitted firms to adopt COWs are plausibly exogenous.

Using the COW laws as IVs, we first examine whether there is an association between common VC investment and VC director networks. While previous research has shown evidence of informational exchanges within VC portfolios (Lindsey, 2008; Gonzalez-Uribe, 2020), to the best of our knowledge, no study has identified the channel through which startups benefit from positive spillovers in the portfolios of the same VCs. We hypothesize that directors are the key mechanism through which information can be exchanged and coordination among commonly held startups can occur. We find evidence that startups with a common VC owner have 1.8 more VC directors on their boards, on average. Further, we find that the VC directors who sit on the boards of the commonly held startups have thicker networks, meaning that they sit on the boards of multiple startups, especially same-industry startups.

Having a common VC investor is not necessarily beneficial for startups because informational flows to VC investors can also provide a platform for diverting value from one startup to another. Indeed, we find that VC funds with investments in multiple startups in the same industry outperform their benchmark index. To disentangle value creation from value shifting, we evaluate the relation between common VC investment and startup outcomes.

First, we find that common VC investment is associated with greater likelihood of receiving an additional round of VC funding. Having a common VC investor is associated with raising 1.14 additional rounds of financing. This estimate represents a meaningful increase given that the median startup in our sample raises only two rounds of financing.[6] However, additional VC funding might primarily serve the interests of the VCs, and thus it is necessary to examine more direct measures of startup success.

Our findings suggest that common VC investors generate real benefits for startups. Specifically, we find that common VC investment is associated with (i) a higher probability of an exit through an IPO, (ii) higher valuations when startups undergo IPOs, (iii) a higher probability of sale, and (iv) a lower probability of failure. This fact pattern is consistent with common VC investors creating value for startups rather than advantaging one startup at the expense of another. Overall, the results suggest that common VC investors enable startups to increase profits through the sharing of valuable information, and efficient allocation of opportunities among startups due to accumulated expertise.

Importantly, we link the results on startup growth and successful exits to informational spillovers (see Section 6.2). We first find that common VC investors are associated with a higher likelihood of a sale to another firm in the common VC investor's portfolio. We then link startup outcomes to the the cross-appointment of directors. If overlapping directors is the channel through which information flows benefit startups, we would expect startup outcomes to be stronger for startups

[6]The point estimate in the IV regression is about 2.5 times that of the endogenous OLS estimate. This is plausible as common VC investment likely involves expert VCs investing in more risky startups (Nanda and Rhodes-Kropf, 2013), and therefore the endogenous OLS estimates are likely negatively biased.

with thicker director networks. Thus, we re-run the startup growth and exit tests for different subsamples of startups: those without a VC director, those with a VC director, and those with a connected VC director that sits on the board of at least one additional startup. The evidence is consistent with little to no effect on startup growth and exits for startups without a VC director but stronger effects for those with VC directors and well-connected ones. This is consistent with the idea that information flows through VC directors drive positive outcomes for startups.

We extend the analysis to consider the possibility of a variety of heterogeneous treatment effects, whereby some startups benefit and others lose as a result of common VC investment. First, we examine a subsample where the common VC investment only occurs early in the life cycle of the startups, where favoritism may be more salient. Our findings are qualitatively similar to those in the main sample, suggesting that VCs do not use common VC positions to exploit early-stage startups. Second, we find a stronger likelihood of an IPO in industries that have strong intellectual property (IP) protection (e.g., pharmaceuticals), though overall we find positive outcomes also in industries with low IP protection.[7] Third, we explore the possibility that common VC investment increases the variance in startup outcomes. We find no evidence that common VC investment is associated with greater risk; rather the evidence is consistent with the whole distribution of outcomes benefiting from common VC investment.

Finally, all the IV results are robust to alternative specifications, including evaluating subsamples in high entrepreneurial states, employing matching estimators, limiting the sample to startups initially incorporated out of Delaware or non-Delaware startups incorporated in a different state from their headquarter state, excluding "spray and pray" industries, the bursting of the dotcom bubble, and the financial crisis (see Online Appendix E). We further show that our results are robust to nuances in the legal interpretation of the state law changes (see Online Appendix A) and to alternative sample constructions, such as minimum and maximum deal sizes (see Online Appendix I).

[7]We might expect more favoritism in industries with strong IP protection because IP could be used to protect market share and discourage innovation by other startups (Abrams et al., 2020).

1 Literature Review and Hypothesis Development

1.1 Contribution to the Literature

Our research is primarily related to other research that examines the impact of common VC investors. Lindsey (2008) shows that alliances are more frequent among startups sharing a common VC. Gonzalez-Uribe (2020) finds evidence of exchanges of innovation resources inside venture capital portfolios. Startups are more likely to cite the patents of other startups in the same VC portfolio. More recently, Li et al. (2023) find that in the pharmaceutical industry, a common VC investor reduces duplication of R&D across projects of its portfolio companies and improves innovative efficiency.

We make three important contributions to the literature. First, we develop a novel identification strategy based on the plausibly exogenous legal changes that validated COWs. These waivers reduce the risk of liability associated with VCs making investments in startups that operate in the same industry. We bolster the credibility of our approach to identification by conducting a survey of lawyers that work on VC deals. This survey indicates that the adoption of COWs by startups incorporated in states that permit COWs is widespread, and that incorporation decisions are not typically based on the substance of states' legal provisions (particularly not on whether these states permit COWs). Incidentally, the survey contributes to the literature on VC deal terms (see recent survey evidence by Gompers et al. (2020)).

Second, although existing studies show a relationship between informational exchanges and common VC investment, none of them uncovers a channel through which such information exchanges occur. Our study finds that the key channel is VC directors. Specifically, common VC investment is associated with thicker VC director networks, and we relate evidence on startup growth to these networks. These findings are consistent with studies that document the role of VC directors in advising founders, negotiating deals, and mediating conflicts (Hellmann and Puri, 2002; Kaplan and Stromberg, 2004; Broughman, 2013; Amornsiripanitch et al., 2019; Ewens and Malenko, 2022).

Third, we examine a broader set of startup outcomes than those examined in prior studies, such as investment rounds, IPOs, including IPO valuations, mergers, and business failure. Examining a broad set of outcomes is essential for evaluating the impact of common VC investment on both startups and VCs.

Our study contributes more broadly to several other important kinds of literature. We contribute to entrepreneurship research by identifying institutional features that enable VCs to achieve higher returns (Moskowitz and Vissing-Jørgensen, 2002; Cochrane, 2005), startup growth and innovation (Lindsey, 2008; Hellman and Puri, 2000; Furman and Stern, 2011). Relatedly, we add to a large body of research that explores how VCs make investment decisions (Gompers, 1995; Kaplan and Stromberg, 2004; Kaplan and Schoar, 2005; Gompers et al., 2008; Puri and Zarutskie, 2012; Ewens et al., 2018; Gompers et al., 2020), especially studies emphasizing networks and economic ties (Hochberg, Ljungqvist, and Lu, 2007, 2010; Hochberg, Lindsey, and Westerfield, 2015).

We contribute to the study of the role of overlapping directors. Our findings are consistent with studies that document information flows among directors. Bouwman (2011) finds that governance practices are partly the outcome of network effects among public firms with common directors. Our study suggests that in the VC context, common directors are highly consequential and have an effect on real outcomes. Our study thus complements a rich literature that shows that directors add value to firms (Adams, 2017) through their expertise (Güner et al., 2008; Dass et al., 2014), social connections (Fracassi and Tate, 2012), and reputations (Fich and Shivdasani, 2006; Fahlenbrach et al., 2010).

Next, we contribute to the emerging study of common ownership by extending it to the VC industry. Theoretical research on common ownership predicts that it can be anticompetitive (Bresnahan and Salop, 1986; Gilo et al., 2006), and recent empirical studies support anticompetitive effects (He and Huang, 2017; Azar et al., 2018, 2020).[8] Our study does not address competition in the product market, though Eldar and Grennan (2021) suggest that common VC ownership may mitigate the anticompetitive effects of common ownership of public firms through improved prod-

[8]Subsequent research questions these findings (Backus et al., 2021; Dennis et al., 2022; Gilje et al., 2020; Lewellen and Lowry, 2021).

uct quality. One challenge with the common ownership literature is that it lacks a clear channel through which common owners could influence firm policy. Although our focus is on informational spillovers, our study is the first to link common ownership to the cross-appointment of directors, thus providing evidence of an active channel through which common owners can influence firm policy (Anton, Ederer, Giné, and Schmalz, 2018; Hemphill and Kahan, 2018).

Finally, we contribute to the law and finance literature on what constitutes sound practice in corporate governance (Shleifer and Vishny, 1997). Our results are consistent with a view that governance is not one-size-fits-all (Giroud and Mueller, 2010, 2011), and in some cases what might be considered lax governance – diluting the duty of loyalty with COWs – can be beneficial (Cremers et al., 2017; Eldar, 2018; Grennan, 2018).

1.2 Hypothesis Development

From a theoretical perspective, VC investors may not seek to maximize the profits of each startup, but rather maximize the aggregate returns on their joint holdings. This could mean that VC investors will not necessarily use the traditional approach of intensive monitoring through board representatives, provision of value added services, and exercise of their control rights to help each of their startups to maximize value (Kerr et al., 2014). Rather, VC investors may use their control rights and board appointees to acquire information about individual startups, and use that information in a way that benefits only some startups in their portfolios. To this end, VC investors could transfer knowledge or shift business opportunities from one startup to another, an approach consistent with favoritism and exploitation (Hellmann, 2002). Alternatively, VC investors could allocate resources and business opportunities among startups efficiently in a way that benefits all startups.

It is also important to consider why entrepreneurial founders might accept the risk that VC investors would appropriate information to benefit another startup when founders can control who their shareholders are. First, VC investors often back entrepreneurs who were not successful in their first endeavor, or bring them on as part of the leadership team for other business ventures

(Kupor, 2019). Thus, receiving VC financing may benefit an entrepreneur regardless of the success of his or her startup. Second, raising a round of capital earlier gives an entrepreneur a first-mover advantage, which is especially important because entrepreneurial ideas are often not unique (Bhide, 2001; Hellman and Puri, 2000). Third, VC firms have strong reputational incentives already to help startups by guiding them to a successful exit (Krishnan and Masulis, 2012). Finally, entrepreneurs may know that VCs with investments in other startups in the same industry could benefit their prospects given the informational advantages and expertise that they bring.

Thus, in our empirical analyses, we test for both good and bad outcomes for startups by examining exits. We focus on the bottom of the distribution by studying whether same-industry startups that share a VC investor fail at the same rate as other startups. We evaluate startups that survive and estimate their ability to raise additional capital. Finally, we look to the upper end of the distribution by examining startup exits via IPOs and sales. For example, we examine startups' IPO valuations relative to public market peers to evaluate whether common ownership enables VC firms to improve the performance of the stronger startups that exit through an IPO.

2 Institutional Background

2.1 Legal and Corporate Practice

In this section, we provide brief institutional background explaining the legal changes that we exploit in our analysis, and why they mattered for common VC investments. We include a more detailed discussion in Online Appendix A.

We rely on the staggered adoption of laws across nine states from 2000 to 2016 that enable corporations to adopt waivers from the corporate opportunity doctrine, or COWs (Rauterberg and Talley, 2017). Table 1 reports the states and the dates when firms were first permitted to adopt the COW in treated states. These waivers exempt directors, officers and shareholders from litigation risk if they usurp a business opportunity in a way that conflicts with the firm's best interest (Talley, 1998; Rauterberg and Talley, 2017; Licht, 2018; Velasco, 2018). The waivers are an element of the duty of loyalty, which more broadly regulates conflicts of interest and requires fiduciaries to act in the best interest of the corporation. The main motivation for the legal change was to eliminate legal uncertainty regarding firms' ability to adopt COWs. In reviewing the legislative session notes, there is virtually no evidence of lobbying prior to the adoption of the COW laws.

To better understand the importance of these legal changes, consider the following example. GoDaddy Inc., a firm that provides domain name registration services worldwide, adopted a COW in its certificate of incorporation.[9] As of 2019, GoDaddy had five VC partners sitting on its board. Those five directors sat on 31 other boards, including another web domain company. Prior to the law change, these board seats would have subjected the directors to substantial liability risk. As Little and Orien (2014) detail, general partners serving on multiple company boards had to strategically manage liability landmines, which occur because VC investors are often approached with many investment opportunities once they signal their interest in a space. Without the law change, they faced potential conflicts of interest if they invested in two different but closely related businesses. As stated by one legal expert, if investors "...will need to worry that all their subsequent private investments in other possibly related firms will be attacked as usurped opportunities of the first company they bought into, they will justifiably think twice before committing their capital; hence the need for waiver of the doctrine" (Grossman, 2009).

We do not observe whether or not the startups in our sample actually adopt COWs because private startups are not required to disclose information about the adoption of COWs. Thus, in theory, using the legal changes as a shock may reflect intent to treat rather than treatment on the treated. Although the duty of loyalty (including the corporate opportunity doctrine) may seem immutable, a majority of public corporations are electing to dilute it by adopting COWs (Rauterberg and Talley, 2017). Given the hurdles that the corporate opportunity doctrine poses for VC investment, and the negative impact of litigation on VC firms' reputations (Atanasov et al., 2012), it is likely that the rates of COW adoption in startups are significantly higher. Consistent with this view, the standard form certificate of incorporation provided as a model legal document by the National Venture Capital Association includes a COW provision. Further, COWs do not

[9]See https://www.sec.gov/Archives/edgar/data/1609711/000119312515120133/d903539dex31.htm

even require amendments to the certificate of incorporation (which entails shareholder approval); rather, they can be easily adopted ad hoc through a bylaw provision or any contractual instrument in the context of specific transactions. In the next section, we provide survey evidence from lawyers that work on VC deals that confirm that startups almost invariably adopt COWs when permitted to do so.

2.2 Survey Evidence

Our empirical analysis relies on evaluating outcomes for startups that are incorporated in states that adopted laws permitting COWs. In order to evaluate whether we can plausibly rely on these laws as quasi-exogenous shocks, we conducted a survey of lawyers to gain their insight into the factors that affect startups' incorporation decisions and propensity to adopt COWs. Before running our survey, we began by interviewing lawyers at leading U.S. law firms that either represented venture capitalists (VCs) or entrepreneurs. We incorporated their input into the design of our survey instrument. We sent the survey to a list of email addresses maintained by the Duke University Law School (74 alums) and the Fuqua School of Business (46 alums) that indicate their primary work experience is in the VC industry. From this list, we received 25 responses, representing a 21% response rate, which is similar to existing financial field studies (Graham and Harvey, 2001; Graham, Grennan, Harvey, and Rajgopal, 2022; Gompers, Gornall, Kaplan, and Strebulaev, 2020). We describe here the main results of the survey, but further details are available in Online Appendix B.

Table B.1 summarizes the first series of questions which explores the entrepreneur's choice of state of incorporation. Q1 asks "How often do VC firms and startups negotiate state of incorporation in a VC deal?" The modal response is "rarely" (40%) and the remaining responses ranged from never (12%) to always (16%) suggesting some disagreement among practitioners or idiosyncratic views. Q2 asks "Please rank the two most common reasons for startups to incorporate in Delaware." The vast majority of respondents (84%) said "familiarity with the law and the body of precedents." No other choice received even half as much support. Tied for second, the next most commonly selected responses were the "expertise of Delaware's judiciary on business law issues" and "investors will withhold investment if the startup does not incorporate in Delaware." We also sought to understand if firms may self-select into Delaware for other legal reasons, but there were few instances where Delaware was selected for a specific law and the reasons listed were the *Trados* decision, which held that directors' duty is to maximize exclusively the interests of equity holders (Cable, 2019), and business combination laws. The final question asks, "Some high-growth startups that seek VC investments choose not to incorporate in Delaware. Please rank the two most common reasons to not incorporate in Delaware." Here the most common reason, with 68% of respondents, was "personal preference of a founder and/or lawyer." The second most common reason at 36% was "loyalty toward the headquarter state."

Table B.2 summarizes the second series of questions in the survey which explore COW adoption patterns. Q4 asks, "The National Venture Capital Association's sample certificate of incorporation includes a waiver from the corporate opportunity doctrine, which is permitted under section 122(7) of the Delaware Corporate Law Code. Is this a standard provision in the charter or bylaws of startups that you observe in practice?" Three-quarters of respondents said "yes." Q5 asks "If this provision is not included in the charter or bylaws, do you typically include it in another legal document (such as the investment agreement with a VC firm or an employment contract with a director)?" Sixty-two percent of respondents say always or most of the time. This suggests that the treatment which is based on firms' states of incorporation does not merely reflect an intent to treat. Finally, Q6 explores the rationale for waiving the corporate opportunity doctrine. Here, 85% of respondents , "VC investors' frequently engage with founders and other investors, and they want to avoid the risk of unexpected litigation from holding board seats at these potentially related startups." Only one respondent said that startups seeking financing from VC investors have no choice but to let the VC investors pursue business opportunities that may belong to the startups. Similarly, only two respondents suggested that the founders agree to the waiver to get a higher valuation from the VC investor. These responses suggest the mechanism through which the COW facilitates common VC investment is by reducing the legal risk of such common investment, and not through other possible channels.

Finally, a common critique of surveys is that the respondents may bias their responses by overweighting outcomes they think the researchers want to hear and under-weighting less favorable outcomes. To ascertain whether there is an appreciable bias in the survey responses, we included a question from the survey conducted by Gompers et al. (2020) and compared our survey responses with their findings. The benchmark study evaluates how VCs structure investments in practice by asking VCs which of the many contract terms that they use are most important and how flexible they are in negotiating them. We ask about three common terms from Gompers et al. (2020),[10] COW, and state of incorporation. Our survey replicates the ranking of term negotiability in Gompers et al. (2020), and incidentally contributes to the literature on VC contracting.

In conclusion, our survey of VC lawyers suggests that the laws that permit COWs are an important shock to investors' and directors' liability risk, and that startups almost invariably adopt COWs. This is consistent with the institutional analysis in section 2.1.

3 Data and Sample

Our goal is to study the effects of common VC investors on startups by exploiting variation that occurs at the state of incorporation level over time. To achieve this goal, we build a data set in which the primary unit of analysis is a VC-funded startup j in industry n, headquartered in state h and incorporated in state i during year t. The primary source for data on VC-funded startups, their industry, and headquarter state is Preqin and the primary source for data on state of incorporation is Lexis Advance. We supplement these sources with data from VentureXpert, Crunchbase, and Compustat.

3.1 VC-funded Startups

The data on startups and VC funds is sourced from Preqin and the sample period extends from 1995 through 2018. To determine startup founding year, we use the earliest of the following three variables: year of first incorporation (from Lexis Advance), year of first deal (from Preqin),

[10]The three terms are (1) board control, (2) pro rata clause, and (3) redemption rights.

and founding year (from Preqin or VentureXpert when missing). To determine startup exit year, we supplement the Preqin data on exits with data from VentureXpert and manual searches in Crunchbase, because Preqin does not have a variable to indicate whether a portfolio company goes out of business or fails. We label a startup as having failed if it is listed as defunct, out of business, or in bankruptcy. Finally, we conservatively code as failed any startup that has not raised capital in five years.

We exclude startups not located in the United States. We also exclude nonprofit startups and those that are not incorporated (such as LLCs), as they would not be subject to the state legislation changes. For state of headquarters, we use the state of location. Given that determining state of incorporation is a complicated and time-consuming process, we limit the total number of startups in the sample. To select this sample, we include all startups that reach at least a Series A round or receive at least \$10 million in VC funding. To mitigate concerns that this cutoff produces a selected sample that is not externally valid, we added to the data: (1) all Massachusetts startups that raised \$1 to \$10 million, and (2) startups that belong to two high-fixed-cost industries (semiconductors and pharmaceuticals) and two low-fixed-cost industries (internet retail and internet business) that raised \$1 to \$10 million. These are startups that are either located in an entrepreneurial hub (i.e., Massachusetts) or belong to industries that are the focus of VC investments but have not raised substantial capital from VCs. The results of our study are not sensitive to these additions.

3.2 State of Incorporation

A major challenge in identifying the state of incorporation is that Preqin does not provide this information. We use Lexis Advance Public Records to manually identify this variable. The nationwide business locator tool on Lexis allows for searches by name and location. The source records include all corporate filings collected from secretaries of state, Uniform Commercial Code filings, and Experian business records. Of the initial set of VC-funded startups, we are able to identify about 92 percent of the startups using the nationwide business locator. We then download all corporate records from the secretary of state of the state in which the startup is headquartered.

We examine the filings and determine whether the state of incorporation is categorized as "Foreign" or "Domestic." If listed as "Foreign," we identify the "Foreign State of Incorporation," which is typically Delaware, as the state of incorporation. If listed as "Domestic," we identify the "Place Incorporated" as the state of incorporation. We further code startups for which no filings are available on Lexis using data obtained from the Delaware and California secretaries of state. Our final sample of startups with state of incorporation data comprises 14,991 unique startups. While this sample represents only approximately 58 percent of the unique startups in Preqin's database, it represents 99 percent of the deal value in Preqin. Although a few studies collect data on the incorporation of private firms (Dammann and Schundeln, 2011; Broughman, Fried, and Ibrahim, 2014), to the best of our knowledge, this is the most comprehensive dataset to date.

3.3 Industry Classification

For each VC-funded startup, Preqin provides two descriptions of industry: primary industry and subindustries. The subindustries are listed in order of relevance, so we focus on the first subindustry. As examples, Zocdoc's primary industry is Healthcare IT and its subindustry is web applications. Lending Club's primary industry is financial services and its subindustry is e-financial. The narrowest definition of industry uses both primary industry and subindustry and includes 130 unique industries. Using only the primary industry produces 78 unique industries, and the coarsest definition includes 10 unique industries. Table D.1, Table D.2, Table D.3, and Table D.4 provide additional details on these definitions.

3.4 Common VC Investment

We create two measures of common VC investment at the startup-year level. The measures are (i) an indicator for whether any VC investor is an investor in another startup in the same industry and (ii) a count of the total number of VC investors that invested in other startups in the same industry. We use the Preqin variable corresponding to a VC investor, such as Benchmark Capital or Sequoia Capital, to define the investor level. We use the Preqin variable of primary and first subindustry to define the industry.

We also create three measures of within-industry VC investments at the startup-year level, using the Preqin primary industry classification. The three measures are indicator variables for (i) whether any VC investor made a within-industry investment in that startup, (ii) whether such investment is on the extensive margin, and (iii) whether such investment is on the intensive margin. The indicator variable for extensive margin investment is equal to one when the VC investor invests in a new startup in a given year that is in the same industry as a portfolio company that it currently holds in its portfolio. The indicator variable for intensive margin investment is equal to one when the VC investor invests more money in a startup that it has previously invested in.

3.5 Within-industry Directorships

We gather data on directors and VC partners from Preqin to construct three measures related to board members. The first measure is simply the total number of VC directors per startup-year. The second is the average number of additional board appointments that the VC directors hold per startup-year. The third is the average number of other board appointments per startup-year that are within the same industry as the startup. The second and third measures capture the thickness of director networks. These networks may serve as the basis for informational exchanges within VC portfolios.

3.6 VC Investor Reputation

We use two measures of VC reputation. The first is the average ranking of VCs based on the Lee-Pollock-Jin's VC reputation index (Lee et al., 2011) over 1995-2010 (the years that overlap with our sample period). We further follow the prior literature which shows that age, size, industry expertise, and successful IPOs lead to an elite reputation (Hsu, 2004; Robinson and Sensoy, 2013; Kahle and Stulz, 2017; Guzman and Stern, 2020). Specifically, for each startup year in our sample, we construct averages across prior VC investors for the following variables: VC age (VC founding year less current year), size (assets under management), the number of funds, total rounds of startup investment, total IPOs of startups invested in, percent of startups invested in with the same headquarter state as the VC, and percent of startups invested in with the same primary industry as any of the industries listed for a specific fund in Preqin. These latter two variables help control for the fact that VCs can add value passively to startups simply by attaching their names to startups, especially in innovation hubs (Bernstien et al., 2022).

3.7 Startup Deal Outcomes and Exits

We source the VC funding raised by startups from Preqin. For deal count, we add the number of rounds of financing a startup raises each year. Late-stage rounds are defined as those that are either at the Series B-J stage of financing or are described as "Pre-IPO." We define deal amount as the log of U.S. dollar deal size reported by Preqin, adjusted for inflation. We define the time between deal rounds on an annual basis. We gather exit data from Preqin, which includes IPOs, trade sales and mergers. We define "sale" as any trade sale or a merger where the acquirer is a practicing entity.

When the startup is sold, Preqin provides the name of the acquirer. We further define common acquisition as any merger where the acquirer and the target have a common VC investor and create an indicator variable for whether a startup is acquired by a firm that is in the portfolio of a common VC investor.

3.8 VC Fund Performance

We gather VC fund performance data from Preqin's Cash Flow data set. The Cash Flow data set provides periodic snapshots of fund performance relative to a benchmark. It only covers a limited number of VC funds, so coverage is incomplete. However, to approximate complete coverage, Preqin benchmarks the performance to established indexes (e.g., early stage, general venture, etc.) and ranks each funds' performance relative to the benchmark. We use the annual benchmarked quartiles as a proxy for portfolio returns.

3.9 IPO valuations

For startups that undergo an IPO, we supplement the Preqin data with IPO valuation data and publicly available incorporation data from SEC Edgar. We follow a similar approach to that introduced by Purnanandam and Swaminathan (2004) to determine the IPO value. This procedure involves computing three multiples for each IPO firm based on Sales, EBITDA and Earnings divided by the equivalent multiple for a matched public firm. We describe each of the three measures: $\left(\frac{P}{V}\right)_{Sales}$, $\left(\frac{P}{V}\right)_{EBITDA}$, and $\left(\frac{P}{V}\right)_{Earnings}$ in Appendix C.

4 Descriptive Statistics

Due to the merging of various datasets, our main startup-year sample includes 142,174 startup-years belonging to 14,991 Preqin startups with data on their states of incorporation. Table 2 displays summary statistics for common VC investments, VC directors, VC deals, and startup exits. Separate statistics are provided for the full sample as well as the treated and control samples. Startup-years are only included in the treated sample if a startup is incorporated in a state that passed a COW law for years after the law was adopted. Control startup-years consist of startup-year observations for the never-treated group as well as startup-year observations for years prior to the treatment for the treated group. About 71 percent of all startup-years are in the treated sample. This is because about 66 percent of the startup-year observations come from startups incorporated in Delaware (see Table D.5).[11] Naturally, the large proportion of Delaware firms in the data may give rise to concerns about selection into the incorporation state. As discussed above, the survey and interview evidence indicate that familiarity with the law rather than specific legal provisions is the main motivation for incorporation in Delaware. Nevertheless, we use several robustness tests to address this concern, including specifications that exclude Delaware startups.

Panel A of Table 2 depicts the measures of common VC investment. The statistics reveal a

[11] This is consistent with Broughman et al. (2014) who rely on similar sources for identifying the state of incorporation, and find that 68 percent of startups that have received VC financing are incorporated in Delaware.

meaningful increase in common VC investors after the law changes. Among the treated startups, 60 percent have a common VC investor while only 32 percent of control startups have a common VC investor. Both the total number of common VC investments and within-industry investment follow a similar pattern. The doubling of within-industry investment holds for both the intensive and extensive margin.

Panel B of Table 2 summarizes our three measures of director cross-appointments. The average number of VC directors is 1.72 per firm-year. On average, each VC director holds 2.53 board positions at other VC firms in a given firm-year and 0.61 board positions are at same-industry startups in a given startup-year. Across all measures of director thickness, we observe higher values for the treated sample.

Panel C of Table 2 summarizes the VC reputation variables. The startups headquartered in treated states receive investments from VC investors who are more reputable and established in terms of age, fund number and assets under management. To account for this heterogeneity across VC investors, we control for VC characteristics and VC fixed effects.

Panel D of Table 2 considers deal variables. We observe 0.28 deals per startup-year. Late-stage deals occur less frequently and constitute only 0.10 deals per startup-year. The average inflation-adjusted dollar deal volume per year is $4.7 million, although this amount is averaged across years that do and do not include deals. The treated sample has a higher average deal count, including late-stage deals, and higher deal amounts than the untreated sample.

Panel E of Table 2 depicts the statistics about exits. Treated startups undergo IPOs and sales at higher rates than control startups. Treated startups are also more frequently acquired by a firm with a common VC investor. However, they fail at higher rates too, suggesting that the impact of the treatment on startups is not trivial.

We use this startup-year data to describe the sample correlation between common VC investors and startup growth. Table D.6 reports the associations based on the endogenous OLS regressions controlling for startup, VC, headquarter-state-by-year, and industry-by-year fixed effects as well as controls for startup growth potential and average VC characteristics. The table shows that having a common VC investor is associated with raising 0.45 additional rounds of financing in a given year.

Next, the table includes the partial correlations associated with the total number of common VC investors. It reveals a correlation of 0.22 for raising additional rounds of financing. Similarly, we observe positive correlations with late-stage financing and deal value. Finally, the table illustrates that the number of common VC investors is correlated with a higher probability of IPO and sale, and negative correlations with failure.

5 Empirical Design

Identifying the effects of common VC investors on startup outcomes is challenging due to inherent selection issues that arise from VC investment decisions. For example, endogeneity may arise if VC investors choose to invest in startups with higher risk or at times when the price of risk is higher. Likewise, changes in a startup's growth trajectory may cause VC investors to invest in other startups in the same industry.

To get closer to the ideal of a random shock that generates exogenous variation in VCs' within-industry investments, we use the set of quasi-natural experiments stemming from the law changes that permit COWs. Specifically, we exploit the state of incorporation level shocks as quasi-natural experiments to establish a link between a startup's ability to adopt COWs and common VC investment. This setting has two main appealing features. First, the variation in liability risk generated by the COW laws is arguably exogenous to startup-level attributes. As discussed above, there was no apparent evidence of lobbying prior to the legislation, and these law changes had a very narrow application to liability that arises from investing in startups that seek similar business opportunities. Moreover, our survey shows that incorporation decisions are not driven by the legal changes and that startups virtually always adopt COWs. Second, because variation is at the state of incorporation level, we can compare startups in the same industry that are headquartered in the same state but are subject to different legislation. This empirical design significantly mitigates the confounding effects resulting from regional economic shocks or conglomeration effects in entrepreneurial hubs.

To understand whether these laws did unlock a sudden and significant shift in the extent to which VCs hold stakes in startups in the same industry, we estimate six regressions. Specification (1) is a standard difference-in-differences specification with year fixed effects. Specification (2) also includes startup fixed effects. Specification (3) includes startup, year, and VC fixed effects as well as a control for late treatment. Specification (4) includes startup, VC, and headquarter-state-by-year fixed effects, and specification (5) augments that specification with industry-by-year fixed effects. Finally, specification (6), which is the most demanding one, includes startup, VC, and HQ-state-by-industry-by-year fixed effects. As an example, the equation for specification (5) is:

$$Y_{jnhit} = \alpha + \beta \left(Treat \times Post \right)_{jnhit} + \zeta \left(Treat \times Post5 \right)_{jnhit} + \mu X_{jnhit} + \omega VC_{jnhit} + f_j + \gamma_{ht} + \rho_{it} + \varepsilon_{jnhit} \tag{1}$$

In the above equation, Y represents the outcome variable, such as an indicator for having a VC investor in common with another startup within industry n. The observation unit is startup j that operates in industry n and is headquartered in state h and incorporated in state i in year t. $Treat$ is an indicator for startups that are incorporated in states with COW laws. $Treat \times Post$ is an indicator for startups that are incorporated in states with COW laws after the law change. β is the main coefficient of interest that isolates the change attributable to the law. $Post5$ is an indicator for any startup that has been treated for five years. Estimating ζ accounts for potential reversals that may occur outside the standard five-year window. f_j denotes the startup fixed effects that capture all of the startup-level time-invariant effects, γ_{ht} represents a headquarter-state-by-year fixed effect that attempts to control for local economic conditions (e.g., state funding initiatives for innovation), and ρ_{it} represents an industry-by-year fixed effect that accounts for industry level trends.

X_{jnhit} represents a vector of controls for (i) startup characteristics, specifically age, capital raised to date, and rounds raised to date, and (ii) the average of VC investor characteristics described in Section 3.6, such as age, size (assets under management), number of funds, total rounds of startup investment, the average number of investment rounds per year, and total IPOs of startups invested in. VC_{jnhit} represents a vector with indicator variables for each VC investor ranked in the top 250 for reputation based on the VC's average ranking in 2010-2015 under the Lee-Pollock-Jin's VC reputation index (Lee et al., 2011).[12]

We next evaluate the potential consequences of common VC investors. Because the difference-in-differences approach provides indirect evidence of a relationship between common VC investors and startup outcomes, we focus on an instrumental variable (IV) strategy for examining the direct effect of common VC investors on startup outcomes. We also run the difference-in-differences specifications (see Online Appendix G), as they represent the reduced form regression that, combined with the first stage results on changes in common VC investors, generate the IV coefficient of interest (Angrist and Pischke, 2008).

For the exclusion restriction to hold, it must be that conditional on controls, the law changes are uncorrelated with other drivers of startup growth. First, as mentioned above, these laws directly target the use of valuable business information in another venture, and do not affect other legal risks, as directors remain subject to all other fiduciary duties.[13] Second, as we show below, the event studies of both the first stage (see Section 6.1) and reduced form (see Online Appendix G) reinforce the notion that these law changes were sudden events with quasi-exogenous timing driven by the legal processes of each state, so that the difference between pre and post within each comparison group captures changes associated with the laws. Moreover, we have multiple events, so any selection concern must be about unobservable time-varying selection that occurs at the exact time of the law change in nine different states. This is unlikely given our survey results that show that COW laws do not affect incorporation decisions (see Section 2.2). Finally, our specifications also control for startup fixed effects and startup growth potential to account for the fixed and time-varying differences across startups as well as VC fixed effects and VC reputational characteristics, thereby mitigating concerns that the IV captures founders' or VC investors' private information

[12]We focus on the top 250 VCs in part because including more fixed effects is computationally challenging and because it is unlikely that VCs ranked below the top 250 have a meaningful role in monitoring and certifying the governance choice of startups.

[13]Rauterberg and Talley (2017) find a positive stock market reaction to adoption of waivers by 83 public firms. While this result may suggest that COWs benefit firms other than through facilitating common VC investment, all of the key examples of COWs adopted by public firms, such as Prosper and NetSuite, are focused on facilitating investments by VC investors. Thus, the adoption of COWs by public firms is fundamentally different than the adoption by startups given that public firm adoption is always bundled with a transaction or other board decisions, and thus the positive stock price effect for public firms may be the result of these concurrent events.

about the startup growth potential.

Accordingly, the resulting IV estimates can plausibly be interpreted as measuring the effects of common VC investors on startup performance. The first-stage equation for the IV analysis is:

$$CommonVC_{jnhit} = \alpha + \beta\,(Treat \times Post)_{jnhit} + \mu X_{jnhit} + \omega VC_{jnhit} + f_j + \gamma_{ht} + \rho_{it} + \nu_{jnhit} \quad (2)$$

where $CommonVC$ is one of the measures of common VC investors, primarily an indicator for whether the startup has any common VC investor and the log of the total number of common VC investors. The observation unit is startup j that operates in industry n and is headquartered in state h but incorporated in state i in year t. $Treat \times Post$ is an indicator for startups that are incorporated in states with COW laws after the law change. X_{jnhit} is a vector of startup and VC controls. VC_{jnhit} represents the vector of VC fixed effects for the top 250 VC investors, and f_j denotes the startup fixed effects. γ_{ht} represents a headquarter-state-by-year fixed effect that controls for local economic conditions (e.g., state funding initiatives for innovation), ρ_{it} represents an industry-by-year fixed effect that accounts for industry level trends, and ν_{jnhit} is the unobservable error component.

The IV approach uses the fitted values from the first stage to predict the outcome of interest as follows:

$$Y_{jnhit} = \alpha + \beta Comm\hat{o}nVC_{jnhit} + \mu X_{jnhit} + \omega VC_{jnhit} + f_j + \gamma_{ht} + \rho_{it} + \varepsilon_{jnhit} \quad (3)$$

In the above equation, Y again represents the outcome variable, such as raising a new round of VC financing. $Comm\hat{o}nVC$ is our first stage fitted value and X_{jnhit}, f_j, γ_{ht}, and ρ_{it} represent the same controls and fixed effects as in the first stage. For the specifications where the dependent variable is an exit by the startup (such as an IPO or failure), we do not include the startup fixed effects (f_j), because the dependent variable is equal to one at most one time in the life of the startup. We cluster the standard errors by startup j and adjust for small clusters.

6 Results

In this section, we present empirical evidence documenting an increase in common VC investors following the law changes. We next show that this increase in common VC investors is associated with an increase in VC directorships at other startups. Then, we assess whether the presence of common VC investors is associated with value creation by examining startups' ability to raise additional capital and their exits.

6.1 Common VC Investors

Panel A of Table 3 shows that the likelihood of a common VC investor increases for treated startups. As shown in column (2), on average, startups incorporated in treated states are 11.8 percentage points more likely to have a within-industry common VC investor after the law changes and this result is statistically significant at the 99th percentile. The economic magnitude is large in comparison to the baseline; for example, prior to the first law passage, only 10 percent of startups incorporated in Delaware had a common VC investor. This result holds across a variety of specifications including ones with startup, VC, industry-by-year, headquarter-state-by-year, and headquarter-state-by-industry-by-year fixed effects, as well as controls for startup growth potential and VC characteristics such as reputation, age, size, and total number of startups invested in that IPO. In each specification, the result is significant at the 1 percent level. Columns (3)-(6) include a control for late treatment and the coefficient on the control rejects the hypothesis that the initial increase in the likelihood of a common VC investor is short-lived as there is no evidence of reversal.

Columns (4) and (5) include headquarter-state-by-year fixed effects to ensure that the treatment is not confounded by local economic conditions such as the startup culture in Silicon Valley. Including these controls does not materially change the point estimate, but they do reduce the standard error by about 20 percent. This is consistent with local economic conditions reducing the precision of the estimates but being relatively orthogonal to the law change as described in our legal analysis.

Column (6) includes headquarter-state-by-industry-by-year fixed effects to control for shifts in the distribution of industries across states over time (e.g., more biotech) that could be correlated with the presence of particular VC investors. Even in this specification, startups incorporated in treated states are 8.0 percentage points more likely to have a within-industry common VC investor after the law changes.

Panel B of Table 3 evaluates the total number of common VC investors as the dependent variable. The coefficient estimate suggests that startups located in treated states experience a significant increase in the total number of common VC investors, on average. Focusing on the specifications with the most conservative fixed effects, the point estimate suggests a 10 percent increase in common VC investment following the law changes.

As an alternative approach, we explore within-industry investments on the extensive margin. As shown in Table G.1, for both extensive and intensive margins, VC investors are significantly more likely to make a within-industry investment after the passage of the COW laws. Again these results hold across all specifications. A comparison of the point estimates in column (2) shows that 24 percent of same-industry investment is on the extensive margin.

We have focused on the full sample of VC-funded startups so far, but this may ignore important heterogeneity across states of incorporation in terms of the availability of VC funding. A potential concern is that about 66 percent of the observations in our sample comes from startups incorporated in Delaware (see Table D.5). To evaluate the potentially disproportionate influence of Delaware, Table 4 examines two alternative samples for the regressions. First, we limit the sample to Delaware, California, Massachusetts, and New York, the states where most entrepreneurial startups are incorporated. Second, we exclude startups that are originally incorporated in Delaware from the analysis. After excluding these startups, 26 percent of the remaining observations are from control states and 8 percent are from the other treated states. In both cases, the results are similar, suggesting that the increase in common VC investors is not driven exclusively by one legal change or only part of the sample.

To further test the validity of the difference-in-differences approach, we run two placebo tests. First, we compare changes in the likelihood of a common VC investor using the same treated states but placebo treatment dates. In particular, we randomly assign a treatment date that is more than five years before or after its actual treatment. Second, we compare changes in the likelihood of a common VC investor using a placebo group of states that do not allow for COWs as treated states. For this exercise, we exclude treated startups. Then, among the remaining non-treated startups, we select a new set of eight states to serve as our placebo treatment group. The placebo treatment states include California, New York, Connecticut, Colorado, Indiana, Illinois, Arizona, Georgia, and Oregon. The placebo treatment group is deliberately similar to the actual treated states in terms of size and entrepreneurial activity. As shown in Table D.7, for both of these tests, we find insignificant changes in the likelihood of a common VC investor and total common VC investors. Moreover, the point estimates are small and close to zero. This provides further support for our identifying assumptions.

We next present visual evidence to assess whether the results are driven by preexisting differential trends or are biased due to effects that may be developing slowly over time. In Figure 1, we show the dynamic coefficient estimates equivalent to specification (2) with the startup and year fixed effects. The figures show a window spanning from five years before the law changes to ten years after the law changes. The straight lines represent 90% confidence intervals. The figures plot the coefficients when any common VC investor is the dependent variable. The figure on the left represents the full sample and the figure on the right focuses on the high entrepreneurship states. In both cases, in the five years prior to treatment the coefficient estimates are flat and close to zero while in the five years after treatment the coefficient estimates are positive and substantially higher. This means that there is little evidence of existing pre-trends.

In the Online Appendix, we show additional visual evidence consistent with a parallel trends assumption and meaningful jumps in the rates of startups with a common VC investor for treated states. Figure D.1 shows the raw trends in rates of startups with a common VC investor over time for the high entrepreneurship states without any controls. The figure reveals a meaningful jump for Delaware relative to California, Massachusetts, and New York following the law change. Figure D.2 plots the rates of startups with common VC investors in the years before and after the law change for different subgroups of startups: (i) Delaware, the first treated state, (ii) Texas, Washington and New Jersey, states that adopted the COW legislation in later years, (iii) California, Massachusetts, and New York, entrepreneurial states that did not adopt the legislation, and (iv) other control states. The treated states clearly experienced a greater growth in common VC investment. Moreover, the increase in common VC investment in startups incorporated in entrepreneurial states is lower than in those incorporated in Texas, Washington and New Jersey.

In addition, in Table D.8, we show robustness results that (i) exclude software startups after 2006 that may have attracted more VC capital due to the "spray and pray" investment that proliferated after the introduction of Cloud computing (Ewens et al., 2018), (ii) exclude the years of the bursting of the dotcom bubble, and (iii) exclude the years of the 2007-2009 financial crisis. In each case, the statistical inferences are similar to those based in our main specifications.

Next, in Table D.9, we address concerns that fixed effects and controls do not fully capture time-varying aspects of startup potential, and therefore, it could be the case that the results are driven by high-growth startups that choose to incorporate in Delaware. To mitigate these concerns, we re-run the empirical tests but include only the startups that are not headquartered in their state of incorporation and are not headquartered in Delaware. Even with this small sample size of 4,571 observations, our findings remain unchanged. This evidence suggests that our estimated relationship is similar across different periods and it is not driven by a particular set of law changes.

6.2 Directors as a Mechanism for Information Coordination

Having shown evidence consistent with the law changes increasing common VC investment, we next explore accompanying board seats, a key mechanism for facilitating information sharing among startups. We examine the relationship between common ownership and directorships using the IV regressions in which we instrument for common ownership using an indicator that equals one if the startup is incorporated in a treated state after the law change. As expected, the t-statistic on the instrument is highly significant and the F-statistic from the first stage of the IV regression exceeds the required threshold for making valid inferences (Lee et al., 2021). Panel A of Table 5 shows that startups with a common VC investor have 1.8 more VC directors on the board.

In Panel B and C of Table 5, we turn to our other measures of director thickness. These results suggest that common ownership is associated with having well-connected VC directors serve on a startup's board. For example, Column (1) of Panel B shows that the startups with a common VC investor have VC directors that sit on the board of 1.9 additional startups, on average, and 0.7 additional startup within the same industry. These results are statistically significant and robust to the inclusion of different fixed effects as shown in Columns (2) and (3). Columns (4) through (6) examine the total number of common VC investors to help understand incremental changes in common VC investment. These results suggest that a 50 percent increase in common VC investment is associated with 0.58-0.63 more VC directors, with a network that includes 0.65 more directorships, and 0.15-0.25 more directorships at startups within the same industry.

In conclusion, our analyses of VC directorships show that startups with greater common VC investors experience a rise in the number of well-connected VC board representatives. These findings uncover a mechanism through which VC investors acquire information on startups' aptitudes and may allocate business opportunities among their portfolio companies.

6.3 Startup Growth

We explore the relationship between common VC investment and startup growth using the IV strategy. As shown in Panel A of Table 6, greater common VC investment is associated with receiving an additional round of VC financing. The point estimate suggests that common VC investment is associated with about one additional round of financing. The increase in financing rounds is statistically significant at the 1 percent level and represents an economically meaningful effect given that the median number of financing rounds is two. In columns (4) through (6), the independent variable is the log of total number of common VC investors. For these tests, the point estimate suggests that a 10 percent increase in common VC investment is associated with between a 7.6 and 8.8 percent increase in deal volume. In each regression, the t-statistic on the instrument is highly significant and the F-statistic from the first stage of the IV regression is about 70 or above. Taken together, these initial results are consistent with positive economic outcomes associated with common VC investment rather than the alternative hypothesis that common VC investment merely facilitates expropriation of business opportunities or favoritism by the VC investor.

The next set of tests evaluates how common VC investment may influence the types of deals made. In particular, we explore the heterogeneity underlying our previous result by looking at whether early or later rounds of financing are associated with common VC investment. Given that later stage capital investments tend to be larger and have more investors in order to meet the higher capital needs of more mature entrepreneurial firms, we would expect to see more deals in later rounds and larger deals if there are positive economic effects. It is possible, however, that after early investments, the directors divert opportunities to other startups that they work with, thereby reducing the overall likelihood of a startup receiving late-stage financing and resulting in startups receiving smaller deals.

Panel B of Table 6 reports the results of the financing round tests. We find a positive relationship between common ownership and receiving later stage financing from VC firms. As shown in column (1), common VC investment is associated with 0.24 additional rounds of late-stage financing, on average. In this case, a 10 percent increase in common VC investment is associated with a 1.6 to 1.9 percent increase in late round financing as shown in columns (4) through (6). All results are significant at the 1 percent level when including the additional fixed effects.

In Panel C of Table 6, we consider deal size. The dependent variable is the log of one plus the deal value in millions of 2010 dollars. The coefficient estimates suggest that, on average, common VC investment is associated with larger deal volume. The elasticity estimated in column (4) suggests that a 10 percent increase in common VC investment would yield a 13.3 percent increase in deal size. Given that the increase in deal size could stem from these startups being forced to wait longer between rounds as part of a holdup by some VC investors, we evaluate the time between financing rounds. Because the sample size is limited since few startups receive multiple rounds of financing, we perform this test using the difference-in-differences framework. As reported in Table G.4, we find no evidence of delays in financing. Thus, overall, the results on deal size and timing are consistent with the notion that common ownership has positive economic benefits.

6.4 VC Funds' Returns and Startups' Exits

Although the startup growth results suggest that common VC investment could help create value for startups, they are also potentially consistent with VC investors with greater common ownership maximizing their overall portfolio return at the expense of individual startups. The VC investors might do this, for example, by providing advantages for one startup over another. To disentangle these two alternatives, we evaluate real effects by examining the performance of VC funds with greater common VC investment, and then analyzing startup exits and IPO valuations.

Table 7 examines the performance of VC portfolios as a whole. For these regressions, the dependent variable is the quartile of the VCs' returns in a given year and the focal explanatory variables are either the percentage of startups that are in treated states or the percentage of startups in the portfolio that are held by the same VC investor. In each case, we find a significant and positive relationship between attributes associated with common VC investment and greater VC portfolio returns. Note, however, that because most VC investors do not disclose the returns they achieve, the sample size is relatively small with only 381 unique VC investors and 3,452 VC-year observations. At any rate, the results are significant and suggest that common VC investment benefits VC investors.

We now turn to examine startup exits. Table 8 evaluates startup exits. Our results in Panel A of Table 8 suggest that common VC investment is associated with about 3.7 percentage point higher probability of IPOs, and this result is significant at the 99th percentile. Given that about 6% of the startups in the sample undergo an IPO, the economic magnitude is large. Next, we evaluate sales in Panel B of Table 8. We show that common VC investment is associated with a 2.1 percentage point higher probability of sale, and this result is significant at the 90th percentile. Finally, in Panel C of Table 8, we show a lower probability of failure for commonly held startups, which suggests that common VC investment even helps those firms at the bottom of the distribution. Having a common VC investor is associated with a 12.5 percentage point decrease in the probability of failure and is significant at the 99th percentile.

Thus, the overall results are consistent with common VC investment helping startups improve rather than common VC investment resulting in rent extraction from one startup to benefit another. The results also suggests that VC investors and startups tend to share information, and their relationship is broadly collaborative (Fisch and Sepe, 2020). This finding seems to contrast with norms in the banking industry, where previous research has found that competing firms are reluctant to disclose relevant information to a banker that also advises a competing firm (Asker and Ljungqvist, 2010).

We further investigate IPOs by examining IPO valuations. Given that a single IPO produces outsized returns for a fund, we examine the extent to which a treated startup receives a favorable valuation at its IPO in Table 9. While we do not have a large enough sample to pass weak instrument tests, we can still implement the difference-in-differences design. Using the standard difference-in-differences design, we examine three different valuation methods based on the IPO proceeds divided by three different accounting measures (sales, EBITDA and earnings) as compared to matched non-IPO firms. We find that the treated firms, on average, receive more favorable valuations. For example, as reported in Panel C, treated startups have a price-to-earnings ratio that is 6 percent higher than that of control firms.

In summary, while we find that VC funds that invest more in treated startups significantly outperform their benchmark index, startups benefit too. The rate of IPO exit for startups with higher rates of common VC investment is greater, and we have some suggestive evidence that their valuations are more favorable at the time they undergo an IPO. Similarly, we find that the VC investors with more common VC investments are able to shift some startups from failure and low-return multiples to higher multiples, likely via sales to would-be acquirers. Taken together, this evidence suggests that the accumulated information and expertise of VC investors who invest in startups within the same industry enables them to better allocate resources and opportunities among startups, and there is no evidence that they advantage one startup over another.

6.5 Informational Spillovers and Directors Networks

We document a pattern consistent with common VC investment facilitating startup growth, but it is also important to establish the mechanism through which common VC investment affects startup outcomes. As discussed above, prior studies of common VC investment do not provide evidence of a mechanism or a channel for information sharing (Lindsey, 2008; Gonzalez-Uribe, 2020). Incidentally, there is a debate as to whether common ownership of public firms is consequential for firm outcomes, in large part because there is skepticism about institutional investors' ability to affect firms' management (Lewellen and Lowry, 2021; Eldar and Grennan, 2021). In the context of startups, however, given the central role of VCs and their director representatives in managing startups, it is reasonable to hypothesize that common VC investment facilitates informational spillovers.

In Table 10, we consider a potential direct channel through which common VC investment generates informational exchanges, namely through sales to startups that have a common VC investor. As shown in column (1), there is a 0.8 percentage point higher likelihood of sale to a startup with a common VC investor. This result is significant at the 90th percentile and is conditional on the many controls for startup characteristics and VC reputation. The finding of higher likelihood of a sale is significant in three out of four specifications, but loses statistical significance when we add VC fixed effects. This is not surprising given the variation in the data (Griliches, 1986). To the extent that there are few such acquisitions (e.g., one such acquisition per VC), or if all of these are done by a small set of big VCs, one would expect the VC fixed effects to absorb the variation. Thus, we believe that the results suggest that common VC investment facilitates sales to commonly held startups.

We next inquire whether the informational benefits associated with common VC investment are driven by director networks. In Panels A and B of Table 11, we compare the characteristics of VC directors for startup growth and exits relative to those that do not raise additional financing or exit. We observe that having more VC directors, especially those with additional directorships, is associated with more growth and successful exits. Consistent with directors facilitating information flows, startups acquired by startups with a VC investor in common also have the most well-connected directors. While they do not have the largest number of VC directors, their VC directors have 5.7 additional directorships on average. The summary statistics are similarly higher for IPOs and sales.

In Panels C and D of Table 11, we run subsample tests based on director characteristics. Specifically, we restrict the sample to the startups without a VC director, with a VC director, and with a common VC director, which we define as having at least one additional VC directorship, and compare the startup exits. In Panel C, we find that common ownership facilitates more late-stage investment and greater deal amounts when the startup has a VC director (columns (2) and (4)) than startups with no VC directors (columns (1) and (3)), and these results are even stronger when the director is also a director of another startup in the same industry (columns (3) and (6)).

We find similar patterns when considering IPOs and failures. We find no relationship between common VC investment and an IPO or failure for startups without VC directors. In contrast, for the startups with VC directors, we find higher probabilities of IPOs and lower probabilities of failure. When we restrict the sample even further to common VC directors, the estimated relationships are even larger.

Overall, these findings support the hypothesis that directors are a mechanism through which common ownership generates positive outcomes for startups.

6.6 Robustness Tests and Extensions

In Online Appendix A, as part of the legal analysis, we run robustness tests that account for states' laws that permit firms to exempt directors from monetary liability for violating the duty of loyalty, which includes the corporate opportunity doctrine. Thus, it is possible that more states, such as Nevada and Virginia, should be included in our treatment group. In these tests, the treatment is defined as the earlier of two types of legislation: the passage of COW legislation or statutes that permit broader exemption from liability for violating the duty of loyalty. We also account for differences in states' COW legislation. For example, the COW legislation in

Nevada does not cover shareholders, and the legislation in Washington requires shareholder approval before adopting COWs (as opposed to merely board action as in other states). Thus, we examine regressions in which the treatment variable excludes Nevada and regressions in which the treatment variable excludes Washington. Across these tests, our estimates generate similar inferences to our main specifications.

In Online Appendix E, we present several variations of our IV estimates analyses for VC directorships, deals, and startup exits. We include robustness checks that limit the sample to high entrepreneurial states and specifications that exclude firms originally incorporated in Delaware. Further, we present evidence for the subsamples that exclude the "spray and pray" investment strategy documented in Ewens et al. (2018), the bursting of the dotcom bubble, and the financial crisis. We also create a subsample where VCs have stronger bargaining power and are arguably less likely to benefit startups.[14] In each of these sub-samples, the statistical inferences are similar to those based on our main specifications. In Table D.10, we extend directorship and deal outcome tests to the small sample of startups that are not headquartered in their state of incorporation and are not in Delaware, and we find similar results.

In Online Appendix F, we use two recent advances in the literature on IV estimation to test and relax the exclusion restriction. First, following the approach in Angrist et al. (2010), we find candidate subgroups of startups unlikely to comply with the instrument, and thus, for these subgroups, the first stage is very likely to be zero. Then, we examine whether the IV has a direct effect on startup outcomes for these zero-first stage groups. We use two candidate subgroups. The first candidate subgroup consists of startups that had not raised financing in over three years prior to the law change, and the second candidate subgroup consists of startups from regulated industries and/or industries where the government may influence the ownership structure. Overall, the evidence is consistent with the IV having insignificant effects on startup outcomes for these zero-first stage groups. This provides evidence in favor of the assumption that the exclusion restriction is satisfied. As a second step, we follow the literature on plausibly exogenous IV estimation (Conley

[14]We exclude startups where the common VC investor first invests in the startup in a later round when the startup is more mature and the founder may have more power.

et al., 2012; Imbens and Rubin, 2015; Kippersluis and Rietveld, 2018), and we relax the exclusion restriction assumption associated with the IV. We show that a plausibly exogenous IV still results in statistical inferences consistent with a positive relation between common VC investment and VC directorships, deal count, deal value, and probability of IPO and a negative relation with the probability of failure.

In Online Appendix G, we present parallel trend figures and the reduced-form difference-in-differences analyses for VC directorships, deals, and startup exits. The figures show no evidence of secular trends prior to treatment. For some of the outcomes, there is noise in the post-period, which suggests the need for strategies like the instrument to help isolate the quasi-random variation. The reduced-form evidence consistently shows a strong statistical relationship between the instrument and startup outcomes. In this Appendix, we also include robustness checks of the reduced form results, including those that limit the sample to high entrepreneurial states and that exclude firms originally incorporated in Delaware. Further, we present reduced-form evidence for the subsamples that exclude the "spray and pray" investment strategies (Ewens et al., 2018), the bursting of the dotcom bubble, and the financial crisis. In all cases, the reduced form results suggest that the instrument is statistically significantly related to these outcomes.

In Online Appendix H, we more fully consider the importance of potential pre-trends by extending our analysis to a matching framework. We present estimates from propensity score and Mahalanobis nearest neighbors matching techniques. We focus on startups incorporated in the high entrepreneurial states of Delaware, California, Massachusetts, and New York. The rationale is that startups in these states are likely to be a better match for one another. We evaluate the matching results based on a composite index of pre-treatment startup and VC characteristics. The matching exercises help to select the best comparison control group for the sample of treated startups. Among 18 alternative matching approaches, we find no evidence to contradict our main findings. In some cases, the economic magnitude of the point estimates is smaller, but in no cases are the inferences significantly different from those in the main results.

In Online Appendix I, we consider how our estimates may change conditional on the lifecycle of the startup by limiting the analysis to specific sub-samples based on the investment dollar values.

For example, as startups mature and scale, the impact of common VC investment is more likely to be related to consumer choice and quality, whereas issues of favoritism may be more salient early in the life of a startup. To address these concerns, we consider three thresholds for including startups in the sample based on the dollar value of VC investments ($10, $15 and $25 million) and three thresholds relating to the age of the startup (8, 10 and 15 years). We do not find any evidence that the dollar value meaningfully changes our inferences. We do see some evidence that only including young startups mutes the magnitude of the positive benefits from common VC investment, but this is likely mechanical as few startups exit early.

In Online Appendix J, we conduct several industry analyses. First, we address the concern that the main results are driven by VCs' industry specialization. We show that over the sample period, the average number of industries VCs invested in increased, suggesting that VCs actually became less specialized. Second, we evaluate whether there may be more favoritism in industries with strong IP protection because IP may be used to protect market share and discourage innovation by other startups. While we find a stronger likelihood of an IPO in industries that have strong intellectual property (IP) protection (e.g., pharmaceuticals), there are also positive outcomes in industries with low IP protection. Third, we examine the informativeness of the Preqin industry definition by creating randomized placebo industries, and recalculating the common VC investment using these placebo industries. We show that the increase in total common investment within industries was significantly larger than the increase in total common investments within the placebo industries following the adoption of COW laws.

Finally, in Online Appendix K, we explore the possibility that while common VC investment increases the average deal amounts and the number of deals, it may increase their variance.[15] We test the variance in outcomes for startups by re-casting our main indicator variables into an ordinal variable where values range from failure all the way up to IPO. Although our results are suggestive, we find no evidence of an increase in variance. Thus, the positive impact of common VC investment does not appear to come with greater risk.

[15]Note that it is not possible to examine the variance of outcomes variables that are dummy variables, and therefore this analysis is limited to continuous outcome variables.

7 Conclusion

We show that investment in startups within the same industry is pervasive in VC portfolios, and we investigate the extent to which such common industry investment may add value to startups beyond the funding provided by the VCs. Prior literature shows that startups in the same VC portfolio benefit from greater opportunities to form strategic alliances and innovation spillovers across startups held by the same VC investor (Lindsey, 2008; Gonzalez-Uribe, 2020).

Nonetheless, there are lingering concerns that VCs may disproportionately favor certain startups over others (Fried and Ganor, 2006; Somerville, 2015). The key economic tension centers on how opportunities for information sharing brought about by common VC investors are associated with startup performance. VCs can play favorites with their commonly held startups, maximizing the returns of one startup at the expense of another. On the other hand, information sharing creates opportunities for VCs to generate positive spillovers among startups.

Using a novel, quasi-experimental design based on legal changes that facilitate common VC investment, the findings in this study suggest that common VC investment across industries is associated with positive outcomes for startups. Examining a broad set of important outcomes, we find that startups with common VC investors raise more capital through more rounds of investment; they are more likely to exit through an IPO at a higher valuation; and they are less likely to fail. These results support the hypothesis that across a broad array of industries, common VC investments help the startups in which they invest.

Importantly, we show that common VC investment is associated with more directorships for VC investors and a thickening of those directors' overall networks, especially at startups in the same industry. Our evidence of a link between VC directorships at other startups and greater probability of IPOs and lower probability of failures suggests that directors are a key mechanism through which information spillovers could facilitate efficient allocation of resources among startups.

In summary, our identification strategy based on narrowly defined legal changes that facilitate common VC investment, coupled with an examination of a comprehensive set of startup outcomes, and evidence of a plausible mechanism for informational exchanges, suggest that common VC investment is beneficial to startup growth.

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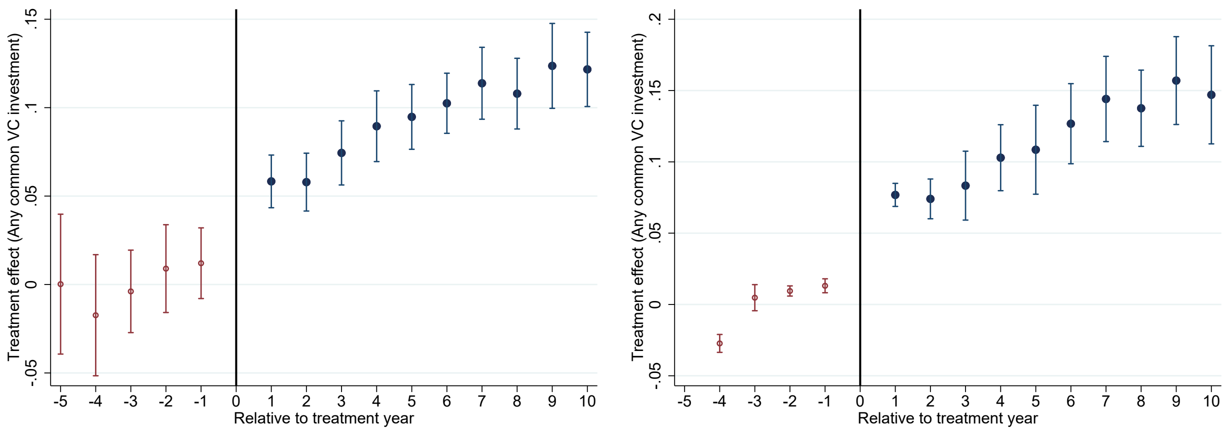


Figure 1. Dynamic effects of corporate opportunity waiver (COW) legislation. The figures plot the impact of COW legislation on common VC investments following the law changes by year relative to the law change. The figures show a window spanning from five years before the law changes to ten years after the changes. Coefficient estimates are normalized relative to the treatment years and inclusive of partially treated years. The straight lines represent 90% confidence intervals, adjusted for incorporation state-level clustering. The coefficients estimated are based on a difference-in-differences specification that includes startup and year fixed effects and controls for total capital previously raised, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. A full set of dummy variables for relative years are included in the regression, but only those in the window are plotted. The figure on the left plots the coefficients for the full sample and the figure on the right plots the coefficients for the the sample limited to startups incorporated in Delaware, California, Massachusetts, and New York.

Table 1.

Summary of Corporate Opportunity Waiver (COW) Legislation

This table offers an overview of the states that amended their corporate laws explicitly to allow COWs (Rauterberg and Talley, 2017). These waivers dilute aspects of the fiduciary duty of loyalty.

State of Inc.	Effective Date	By Charter	By Bylaws	By Board Action	Covers Directors	Covers Shareholders
DE	July 1, 2000	Yes	Possible	Yes	Yes	Yes
OK	November 1, 2001	Yes	Possible	Yes	Yes	Yes
MO	October 1, 2003	Yes	Possible	Yes	Yes	Yes
KS	January 1, 2005	Yes	Possible	Yes	Yes	Yes
TX	January 1, 2006	Yes	Possible	Yes	Yes	Yes
NV	October 1, 2007	Yes	Possible	Yes	Yes	Yes
NJ	March 11, 2011	Yes	Possible	Yes	Yes	Yes
MD	October 1, 2014	Yes	Possible	Yes	Yes	No
WA	January 1, 2016	Yes	No	No	Yes	Yes

Table 2.

Summary Statistics

This table provides summary statistics for observations at the startup-year level. Separate statistics are provided for the full sample of startups, for the treated sample of startups, and for the control sample of startups. Treatment is defined as being incorporated in a state that allows for COWs. Deal amounts are in millions of dollars and are inflation-adjusted. All variables are described in Section 3.

	All			Treated			Control		
	N	Mean	St. Dev.	N	Mean	St. Dev.	N	Mean	St. Dev.
Panel A: Common VC Investments									
Any common VC investment	142,174	0.51	0.50	100,970	0.60	0.49	41,204	0.32	0.46
Total common VC investment	142,174	0.99	1.20	100,970	1.18	1.24	41,204	0.54	0.95
Within-industry investment	142,174	0.18	0.38	100,970	0.20	0.40	41,204	0.11	0.31
Intensive margin investment	142,174	0.13	0.33	100,970	0.15	0.36	41,204	0.07	0.26
Extensive margin investment	142,174	0.05	0.22	100,970	0.06	0.23	41,204	0.03	0.18
Panel B: VC Directors									
VC directorships	100,894	1.72	2.05	73,889	1.99	2.14	27,005	1.00	1.58
Additional directorships	100,894	2.53	3.57	73,889	2.96	3.74	27,005	1.36	2.71
Additional within-industry directorships	100,894	0.61	1.22	73,889	0.73	1.33	27,005	0.29	0.80
Panel C: VC Reputation									
VC reputation (100=Best)	142,174	9.78	18.22	100,970	11.11	19.17	41,204	6.52	15.17
VC age	142,174	6.41	7.71	100,970	7.39	7.94	41,204	3.99	6.51
VC size (AUM)	142,174	747.04	1,441.03	100,970	882.47	1,563.74	41,204	415.17	1,008.93
VC fund number	142,174	2.63	3.24	100,970	3.04	3.36	41,204	1.62	2.68
VC total rounds of startup investment	142,174	71.08	128.53	100,970	86.44	139.94	41,204	33.46	83.79
VC total IPOs of startups invested in	142,174	3.71	8.64	100,970	4.47	9.48	41,204	1.85	5.66
VC same headquarter state as startup	142,174	0.29	0.38	100,970	0.33	0.39	41,204	0.21	0.35
VC same primary industry as startup	142,174	0.14	0.27	100,970	0.17	0.28	41,204	0.09	0.22
Panel D: VC Deals									
Count	142,174	0.28	0.51	100,970	0.31	0.53	41,204	0.19	0.44
Late Stage	142,174	0.10	0.32	100,970	0.11	0.34	41,204	0.07	0.27
Deal value (log(1+))	142,174	0.52	1.08	100,970	0.58	1.13	41,204	0.37	0.94
Time between deals (year)	28,047	0.56	1.13	22,734	0.56	1.12	5,313	0.55	1.18
Panel E: Exits									
IPO	142,174	0.01	0.08	100,970	0.01	0.08	41,204	0.00	0.06
Sale	142,174	0.03	0.17	100,970	0.03	0.18	41,204	0.02	0.14
Acquired by a common VC investment	142,174	0.01	0.07	100,970	0.01	0.08	41,204	0.00	0.05
Failure	142,174	0.04	0.20	100,970	0.05	0.21	41,204	0.03	0.17

Table 3.

Total Common Venture Capital Investments and Corporate Opportunity Waivers

This table presents the results from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). In Panel A, the dependent variable is an indicator variable for whether a startup has a VC investor that commonly owns another startup within the same industry. In Panel B, the dependent variable is the natural log of the total number of common VC owners that startup has. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. For the dependent variables, industry is defined based on Preqin's primary and sub industry classification. For the industry fixed effects, the adjusted primary industry classification, which is coarser than the primary industry classification, is used. Below the coefficient estimates are robust standard errors clustered by state of incorporation. ***, **, and * indicate *p*-values of 1%, 5%, and 10%, respectively.

Panel A: Any common VC investments	(1)	(2)	(3)	(4)	(5)	(6)
Treat	0.039**					
	(0.019)					
Treat × Post	0.070***	0.118***	0.078***	0.078***	0.075***	0.080***
	(0.018)	(0.015)	(0.010)	(0.008)	(0.007)	(0.008)
Treat × Post after five years			0.045***	0.050***	0.047***	0.047***
			(0.007)	(0.007)	(0.007)	(0.007)
Adjusted R^2	34.7%	66.0%	69.3%	69.4%	69.8%	70.0%
Panel B: Total common VC investments						
Treat	0.069					
	(0.056)					
Treat × Post	0.190***	0.203***	0.094***	0.087***	0.096***	0.103***
	(0.057)	(0.037)	(0.018)	(0.012)	(0.015)	(0.015)
Treat × Post after five years			0.116***	0.141***	0.105***	0.104***
			(0.020)	(0.011)	(0.010)	(0.010)
Adjusted R^2	35.3%	76.6%	81.4%	81.5%	82.9%	82.9%
Additional startup and VC controls	Yes	Yes	Yes	Yes	Yes	Yes
Startup fixed effects	No	Yes	Yes	Yes	Yes	Yes
VC fixed effects	No	No	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	No	No	No
HQ-state-by-year fixed effects	No	No	No	Yes	Yes	No
Industry-by-year fixed effects	No	No	No	No	Yes	No
HQ-state-by-industry-by-year fixed effects	No	No	No	No	No	Yes
Observations	142,174	142,174	142,174	141,296	141,296	139,559
Number of unique startups	14,991	14,991	14,991	14,896	14,896	14,794

Table 4.

Common Venture Capital (VC) Investments (Alternative Samples)

This table presents the results from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). In Panel A, the sample is limited to startups incorporated in Delaware, California, Massachusetts, and New York. In Panel B, the sample is limited to startups that do not first incorporate in Delaware. In columns (1) and (2), the dependent variable is an indicator variable for whether a startup has a VC investor that commonly invests in another startup within the same industry. In columns (3) and (4), the dependent variable is the natural log of the total number of common VC investments that startup has. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. For the dependent variables, industry is defined based on Preqin's primary and sub industry classification. For the industry fixed effects, the adjusted primary industry classification, which is coarser than the primary industry classification, is used. Below the coefficient estimates are robust standard errors clustered by state of incorporation. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	Any common VC investments		Total common VC investments	
Panel A: DE, CA, MA, and NY	(1)	(2)	(3)	(4)
Treat × Post	0.122***	0.091***	0.193**	0.128**
	(0.014)	(0.005)	(0.035)	(0.026)
Treat × Post after five years		0.053**		0.107***
		(0.013)		(0.010)
Adjusted R^2	69.0%	69.5%	81.6%	83.0%
Number of observations	123,327	122,435	123,327	122,435
Number of unique firms	13,512	13,421	13,512	13,421
Panel B: Non-DE	(1)	(2)	(3)	(4)
Treat × Post	0.121***	0.071***	0.199***	0.096**
	(0.020)	(0.024)	(0.051)	(0.041)
Treat × Post after five years		0.088***		0.179***
		(0.024)		(0.029)
Adjusted R^2	70.5%	71.4%	78.7%	80.8%
Additional controls	Yes	Yes	Yes	Yes
Startup fixed effects	Yes	Yes	Yes	Yes
VC investor fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	No	Yes	No
HQ-state-by-industry-by-year fixed effects	No	Yes	No	Yes
Industry-by-year fixed effects	No	Yes	No	Yes
Number of observations	47,739	47,350	47,739	47,350
Number of unique startups	4,109	4,083	4,109	4,083

Table 5.

Common Venture Capital Investments and the Board of Directors

This table presents results from instrumental variable (IV) regressions examining the type of directorships held at startups with common owners. The key explanatory variable is common venture capital (VC) investment and the instrument is an indicator variable for if a startup is incorporated in a state that permits corporate opportunity waivers (COWs). In columns (1) and (2), any common VC investment is an indicator variable for whether a startup has a VC investor that commonly invests in another startup within the same industry, and in columns (3) and (4) total common VC investments is defined as the natural log of the total number of common VC investors. In Panel A, the dependent variable is VC directorships, defined as the total number of directorships held by VC fund leaders in the startup. In Panel B, the dependent variable is the average number of other directorships that are held by VC fund leaders. In Panel C, the dependent variable is the average number of within-industry directorships held by VC fund leaders. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Industry for common ownership is defined based on Preqin's primary and sub industry classification. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. The first-stage F-statistic is the Kleibergen-Paap Wald statistic. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: VC director	(1)	(2)	(3)	(4)	(5)	(6)
Any common VC investment	1.792***	1.763***	1.645***			
	(0.363)	(0.392)	(0.384)			
Total common VC investments				1.258***	1.249***	1.160***
				(0.279)	(0.298)	(0.287)
Panel B: VC director with additional directorships						
Any common VC investment	1.850***	1.915***	1.730***			
	(0.592)	(0.629)	(0.615)			
Total common VC investments				1.299***	1.357***	1.220***
				(0.422)	(0.448)	(0.433)
Panel C: VC director with additional within-industry directorships						
Any common VC investment	0.702***	0.475**	0.416*			
	(0.240)	(0.236)	(0.232)			
Total common VC investments				0.493***	0.337**	0.293*
				(0.168)	(0.163)	(0.159)
First-stage F-statistic	130.4	127.7	136.0	48.7	49.2	52.1
t-statistic on instrument	11.42	11.30	11.66	6.98	7.01	7.22
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Startup fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
VC investor fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	No	No	Yes	No	No
HQ-state-by-year fixed effects	No	Yes	No	No	Yes	No
Industry-by-year fixed effects	No	Yes	No	No	Yes	No
HQ-state-by-industry-by-year fixed effects	No	No	Yes	No	No	Yes
Observations	100,893	100,310	98,639	100,893	100,310	98,639
Number of unique startups	10,264	10,206	10,119	10,264	10,206	10,119

Table 6.

Common Venture Capital (VC) Investments and Deals

This table presents results from instrumental variable (IV) regressions examining VC financing outcomes for startups with common VC investments. The key explanatory variable is common VC investment and the instrument is an indicator for if a startup is incorporated in a state that permits corporate opportunity waivers (COWs). In columns (1) through (3), any common VC investment is an indicator variable for whether a startup has a VC investor that commonly owns another startup within the same industry, and in columns (4) through (6) total common VC investment is defined as the natural log of the total number of common VC investments. In Panel A, the dependent variable is deal volume, which is defined as any VC equity financing deal a firm receives in a given year. In Panel B, the dependent variable is late-round VC deal volume, which is defined as a round of equity VC financing greater than the seed or first round. In Panel C, the dependent variable is deal value, defined as the natural log of one plus the deal value in millions of 2010 dollars. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Industry for common ownership is defined based on Preqin's primary and sub industry classification. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. The first-stage F-statistic is the Kleibergen-Paap Wald statistic. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: VC deal count	(1)	(2)	(3)	(4)	(5)	(6)
Any common VC investment	1.143***	1.307***	1.327***			
	(0.102)	(0.115)	(0.114)			
Total common VC investments				0.762***	0.866***	0.884***
				(0.093)	(0.103)	(0.104)
Panel B. Late stage deal count						
Any common VC investment	0.241***	0.270***	0.291***			
	(0.046)	(0.050)	(0.050)			
Total common VC investments				0.161***	0.179***	0.194***
				(0.033)	(0.036)	(0.036)
Panel C. Deal value						
Any common VC investment	1.987***	2.222***	2.268***			
	(0.201)	(0.220)	(0.220)			
Total common VC investments				1.325***	1.472***	1.512***
				(0.173)	(0.188)	(0.190)
First-stage F-statistic	154.8	147.7	155.1	69.5	71.4	72.3
t-statistic on instrument	12.44	12.15	12.45	8.33	8.45	8.50
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Startup fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
VC investor fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	No	No	Yes	No	No
HQ-state-by-year fixed effects	No	Yes	No	No	Yes	No
Industry-by-year fixed effects	No	Yes	No	No	Yes	No
HQ-state-by-industry-by-year fixed effects	No	No	Yes	No	No	Yes
Number of observations	142,174	141,296	139,559	142,174	141,296	139,559
Number of unique startups	14,991	14,896	14,794	14,991	14,896	14,794

Table 7.

Venture Capital (VC) Funds' Return Distributions

This table tests whether VC funds' portfolios of startups achieve higher returns when common ownership is greater. The dependent variable is the return quartile, where four indicates best returns. All returns are benchmarked against an appropriate index (e.g., early-stage, general venture, etc.). Common VC investment is proxied for using the percentage of portfolio companies that are treated. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Below the coefficient estimates are robust standard errors. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Dependent variable = return quartile, where 4 indicates best returns			
	(1)	(2)	(3)
Percent of portfolio startups that are treated	0.464***		
	(0.093)		
Percent of portfolio startups that have common VC investment		0.417***	
		(0.075)	
Average number of common VC investments across portfolio startups			0.116***
			(0.025)
Additional controls	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Adjusted R^2	1.9%	2.1%	1.8%
Number of observations	3,452	3,452	3,452
Number of unique VCs	381	381	381

Table 8.

Common Venture Capital Investments and Startup Exits

This table presents results from instrumental variable (IV) regressions examining startup exits and common ownership. The key explanatory variable is common VC investment and the instrument is an indicator for if a startup is incorporated in a state that permits corporate opportunity waivers (COWs). In columns (1) and (2), any common VC investment is an indicator variable for whether a startup has a VC investor that commonly owns another startup within the same industry, and in columns (3) and (4) total common VC investment is defined as the natural log of the total number of common VC investments. The dependent variable in Panel A is an indicator for whether a firm undergoes an IPO, in Panel B the dependent variable is an indicator for a sale, in Panel C the dependent variable is an indicator for whether the startup fails. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Industry for common ownership is defined based on Preqin's primary and sub industry classification. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. The first-stage F-statistic is the Kleibergen-Paap Wald statistic. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: Initial public offering (IPO)	(1)	(2)	(3)	(4)	(5)	(6)
Any common VC investment	0.037***	0.039***	0.042***			
	(0.005)	(0.006)	(0.007)			
Total common VC investments				0.018***	0.021***	0.023***
				(0.003)	(0.004)	(0.004)
Panel B: Sale						
Any common VC investment	0.021*	0.029**	0.027*			
	(0.012)	(0.014)	(0.014)			
Total common VC investments				0.010*	0.016**	0.015*
				(0.006)	(0.008)	(0.008)
Panel C: Failure						
Any common VC investment	-0.125***	-0.158***	-0.165***			
	(0.018)	(0.023)	(0.024)			
Total common VC investments				-0.061***	-0.087***	-0.090***
				(0.009)	(0.013)	(0.013)
First-stage F-statistic	185.1	144.7	140.4	153.4	101.6	99.5
t-statistic on instrument	13.60	12.03	11.85	12.38	10.08	9.97
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
VC investor fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	No	No	Yes	No	No
Industry-by-year fixed effects	No	Yes	No	No	Yes	No
HQ-state-by-year fixed effects	No	Yes	No	No	Yes	No
HQ-state-by-industry-by-year fixed effects	No	No	Yes	No	No	Yes
Number of observations	142,174	141,296	139,565	42,174	141,296	139,565
Number of unique startups	14,991	14,896	14,799	14,991	14,896	14,799

Table 9.

Initial Public Offering (IPO) Valuation

This table presents the results from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). In each panel, the dependent variable is the standardized ratio of the IPO offer price relative to the intrinsic value of the firm. The ratios are calculated using the method outlined in Appendix C that adjusts each ratio using an annual set of comparable firms. In Panel A, the multiple is the price-to-sales ratio, in Panel B the multiple is the price-to-earnings-before-interest-tax-depreciation-and-amortization ratio, and in Panel C the multiple is the price-to-earnings ratio. All specifications include year and two-digit SIC industry fixed effects. Below the coefficient estimates are robust standard errors clustered by state of incorporation. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: $\left(\frac{P}{V}\right)_{Sales}$	(1)
Treat	-0.024
	(0.026)
Treat × Post	0.044*
	(0.024)
R^2	7.84%
Number of observations	570
Panel B: $\left(\frac{P}{V}\right)_{EBITDA}$	
Treat	-0.008**
	(0.004)
Treat × Post	0.019**
	(0.008)
R^2	3.04%
Number of observations	577
Panel C: $\left(\frac{P}{V}\right)_{Earnings}$	
Treat	-0.006
	(0.007)
Treat × Post	0.064**
	(0.022)
R^2	2.13 %
Year fixed effects	Yes
Industry fixed effects	Yes
Number of observations	618

Table 10.

Common Venture Capital Investments and Common Acquisitions

This table presents results from instrumental variable (IV) regressions examining acquisitions by a firm with a common VC investor. The key explanatory variable is common VC investment and the instrument is an indicator for if a startup is incorporated in a state that permits corporate opportunity waivers (COWs). In Panel A the explanatory variable of interest is any common VC investment, an indicator variable for whether a startup has a VC investor that commonly owns another startup within the same industry. In Panel B, the explanatory variable of interest is total common VC investment, defined as the natural log of the total number of common VC investments. The dependent variable is an indicator for a sale to a firm with a common VC investment. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Industry for common ownership is defined based on Preqin's primary and sub industry classification. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. The first-stage F-statistic is the Kleibergen-Paap Wald statistic. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Dep. var. = Acquired by a common VC investment				
Panel A. Any common VC investment	(1)	(2)	(3)	(4)
Any common VC investment	0.008*	0.010*	0.009*	0.000
	(0.004)	(0.005)	(0.005)	(0.005)
First-stage F-statistic	272.6	213.2	202.1	185.1
t-statistic on instrument	16.51	14.60	14.22	13.60
Panel B. Total common VC investments				
Total common VC investments	0.003*	0.005*	0.004*	0.000
	(0.002)	(0.002)	(0.003)	(0.003)
First-stage F-statistic	262.3	176.8	165.8	153.4
t-statistic on instrument	16.20	13.30	12.87	12.38
Additional controls	Yes	Yes	Yes	Yes
VC investor fixed effects	No	No	No	Yes
Year fixed effects	Yes	No	No	Yes
Industry-by-year fixed effects	No	Yes	No	No
HQ-state-by-year fixed effects	No	Yes	No	No
HQ-state-by-industry-by-year fixed effects	No	No	Yes	No
Number of observations	142,174	141,296	139,565	142,174
Number of unique startups	14,991	14,896	14,799	14,991

Table 11.

Directors and Startup Exits

This table examines the relationship between VC directorships and startup growth. Panels A and B show summary statistics for characteristics of VC directors by VC deal rounds and startup exit type, respectively. Panels C and D presents IV regression evidence on the relationship between common ownership and startup growth for subsamples of startups categorized by VC director characteristics. In Panels C and D, columns (1) and (4) limit the sample to startups with no VC directors, columns (2) and (5) to startups with VC directors, and columns (3) and (6) to startups with common VC directors, defined as having at least one additional directorship. The focal explanatory variable is any common VC investment defined as an indicator variable for whether a startup has a VC investor that commonly owns another startup within the same industry. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	Any round		Late stage	
	No	Yes	No	Yes
Panel A: VC directors by VC deal outcomes	(1)	(2)	(3)	(4)
VC directors	1.56	2.16	1.57	2.87
Common VC directors	2.31	3.10	2.41	3.46

	IPOs		Sale		Common Acq.		Failures	
	No	Yes	No	Yes	No	Yes	No	Yes
Panel B: VC directors by startup exit	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VC directors	1.71	4.16	1.69	2.78	1.72	2.52	1.70	2.49
Common directors	2.53	3.41	2.48	4.42	2.59	5.69	2.48	4.00

	Dep. var = Late stage			Dep. var = Deal amount		
	No VC director	VC director	Common director	No VC director	VC director	Common director
Panel C: VC deal outcomes	(1)	(2)	(3)	(4)	(5)	(6)
Any common VC investment	0.106***	0.664***	0.864***	1.328***	3.194***	4.395***
	(0.037)	(0.179)	(0.322)	(0.156)	(0.778)	(1.503)
First-stage F-statistic	117.9	20.3	9.4	117.9	20.3	9.4
Number of observations	35,225	64,860	54,549	35,225	64,860	54,549
Includes additional controls, VC, industry-by-year and headquarter-state-by-year fixed effects						

	Dep var. = IPO			Dep var. = Failure		
	No VC director	VC director	Common director	No VC director	VC director	Common director
Panel D: Startup exits	(1)	(2)	(3)	(4)	(5)	(6)
Any common VC investment	0.013*	0.132***	0.205***	0.000	-0.423***	-0.639***
	(0.007)	(0.036)	(0.074)	(0.000)	(0.106)	(0.222)
First-stage F-statistic	144.0	27.1	13.5	144.0	27.1	13.5
Number of observations	76,280	64,860	54,549	76,280	64,860	54,549
Includes additional controls, VC, industry-by-year and headquarter-state-by-year fixed effects						

Online Appendix to "Common Venture Capital Investors and Startup Growth"

A Legal Analysis

All corporations are governed by the laws of the state in which they are incorporated. These laws dictate the type and scope of fiduciary duties. This background is intended to clarify the legal and institutional framework underlying states' corporate laws, the variation across states and time, and potential different interpretations of these laws.

The Duty of Loyalty: Managers owe two types of duties to the corporation, the duty of care and the duty of loyalty. Violation of the duty of care is rare due to the business judgment rule[16] and statutory provisions that permit exemptions from liability for such violations.[17] The duty of loyalty is the most important legal mechanism for disciplining managers as well as controlling shareholders, who are likewise subject to the duty. It is broadly defined as the duty to act in good faith to advance the best interests of the corporation (Strine et al., 2010). The duty of loyalty is relevant whenever managers or controlling shareholders face a conflict between the company's interests and their own. Broadly stated, the duty of loyalty regulates the following main circumstances that give rise to conflicts: (i) self-dealing transactions, (ii) duties of controlling shareholders to minority shareholders, (iii) fiduciary duties in hostile takeover transactions, and (iv) the corporate opportunity doctrine. When there is a potential conflict of interest, the courts generally review managers' decisions under the exacting "entire fairness" standard, which means that courts may evaluate the price of particular transactions and decide that they are unfair to the shareholders. Delaware and most other states do not allow broad exemptions from the duty of loyalty. Thus, subject to the discussion below, the duty of loyalty remains a mandatory feature of most states' corporate laws.

The Corporate Opportunity Doctrine: The corporate opportunity doctrine, the focus of this study, is a central aspect of the duty of loyalty. The seminal judicial statement of the duty

[16] According to the rule, courts do not second-guess the business judgment of corporate managers in the absence of conflicts of interest.

[17] Since 1986, Delaware, the most popular state for incorporations, has allowed firms to exempt directors from the duty of care (see section 102(b)(7) of the Delaware General Corporation Law) through a provision in the articles of incorporation. All states have largely followed Delaware, and virtually all firms exempt their directors from this duty.

of loyalty in *Meinhard v. Salmon* (249 N.Y. 458, 464 (1928)) by Chief Justice Benjamin Cardozo involved the appropriation of a business opportunity by a manager, and the doctrine governs what is perhaps the most common instance of a breach of directors' duty of loyalty (Rauterberg and Talley, 2017). In essence, the doctrine requires managers and controlling shareholders not to appropriate for themselves an opportunity that belongs to the corporation, unless they disclose it to the corporation and receive permission to pursue it. When a manager or a controlling shareholder seeks to expropriate an opportunity that belongs to the corporation, his or her interests are in direct conflict with those of the corporation.

In considering whether an opportunity belongs to the corporation, courts engage in a detailed fact-finding process to determine whether the pursuit of a business opportunity is impermissible. Courts consider multiple factors, including (i) whether the corporation is financially able to undertake the opportunity, (ii) whether the opportunity is in the corporation's line of business, (iii) whether the corporation has an interest or reasonable expectancy in it, and (iv) whether the pursuit of the opportunity will place the manager or controlling shareholder in a position inimical to his or her duties to the corporation (See *Guth v. Loft*, 23 Del. Ch. 255, 273 (1939), and *Broz v. Cellular Information Systems, Inc.*, 673 A.2d 148 (Del. 1996)). Applying these factors in specific instances has produced a great deal of complexity and unpredictability in how the doctrine is interpreted (Talley, 1998).

The corporate opportunity doctrine presents thorny problems for VC investors. When these firms make investments, they appoint their own representatives to the boards of these firms. These board members may be liable for violating the duty of loyalty to the corporation, and therefore also the corporate opportunity doctrine. The VC investors may likewise be liable for aiding and abetting the directors' breach of the duty of loyalty, or for violating their own fiduciary duties if they are deemed to be controlling shareholders (Boschner and Simmerman, 2016). This is a particular problem for VC investors that make investments in multiple companies in the same industry and whose board representatives serve on the boards of the multiple firms in which they invest. The VC investors and their board representatives may be required to share opportunities with the founders and other shareholders if the opportunity belongs to the firm they have invested in. Likewise, they may be especially vulnerable to liability risk when there is a perception that they have diverted opportunities from one firm to another. Despite these apparent conflicts of interest, VC investors may be best positioned to pursue new opportunities or to allocate such opportunities among their investments.

Waivers from the Corporate Opportunity Doctrine: In 2000, Delaware amended its corporate law statute by permitting firms incorporated in its jurisdiction to waive the corporate opportunity doctrine. Specifically, section 122(17) provides that a corporation may "renounce, in its certificate of incorporation or by action of its board of directors, any interest or expectancy of the corporation in, or in being offered an opportunity to participate in, specified business opportunities or specified classes or categories of business opportunities that are presented to the corporation or one or more of its officers, directors or stockholders." Thus, firms can ex ante permit their shareholders and managers to pursue any business opportunity that they learn about through their roles as fiduciaries on behalf of the corporation.

From the perspective of VC investors, this tool helps ensure that the VC fund, through its board representatives, can run its business smoothly without the need to engage in a difficult and imprecise legal analysis of the corporate opportunity doctrine when allocating business opportunities. Importantly, the statute permits startups to adopt the waiver without a charter amendment that would require shareholder approval, simply through a board resolution. Thus, a VC firm with control or at least substantial influence over the board can easily adopt such a waiver, or require a waiver from the board before making an investment. Moreover, the unequivocal advice of practitioners is to adopt a waiver in the charter, and the standard form certificate of incorporation provided by the National Venture Capital Association includes a COW provision.

Following the legislative change in Delaware, eight other states between 2000 and 2016 amended their corporate statutes similarly (see Table 1). These provisions are largely identical to those adopted in Delaware with a few minor differences, two of which are relevant for our study. First, the statute adopted by Nevada in 2007 does not cover shareholders, and therefore this provision does not protect the VC firm itself, although it does protect its board representatives. Second, the 2016 Washington statute does not permit firms to adopt the waiver by a board action, and requires a charter provision to this effect. We run relevant robustness tests that take these differences into account.

Political Economy Underlying the State Law Changes: When assessing the impetus for the state law changes, the states can be organized into three groups: Delaware, the most popular state for incorporations, states that specifically adopted COW statutes because of possible demand by parties within the state (New Jersey and Washington), and other states (Kansas, Oklahoma, Texas, Missouri, Nevada, and Maryland). Our review of lobbying transcripts and session notes in Delaware (available on Lexis Advance) indicates that the original motivation for the law was to eliminate uncertainty arising from a 1989 case, *Siegman v. Tri-Star Pictures*, regarding the power of a corporation to renounce corporate opportunities in advance. Lack of clarity in a ruling or conflicting rulings from different judges are often impetus for legislative action. No other forms of lobbying are explicitly mentioned in the Delaware session notes.

The second group includes only two states. New Jersey is the only state that explicitly mentions that the corporate opportunity doctrine may injure corporations, but there is no evidence of lobbying. The New Jersey Assembly Budget Committee Report states that "[the] corporate opportunity doctrine... operates as a disincentive and makes it difficult for New Jersey corporations to attract and retain businesspersons as board members." Washington is the only state where there is some evidence of lobbying, although the lobbying is by the Washington State Bar Association rather than by corporations. The session notes suggest that Washington's COW statute was adopted to prevent any further damage to the Washington corporate law industry. We emphasize though that both New Jersey's and Washington's shares of incorporations are very low throughout the sample period.

The third group of six states adopted COW statutes as part of a broad package of corporate reforms that were not directly aimed at corporate opportunities. It can be argued that the passage of COW statutes for these states, then, was close to exogenous.

Broader Exemptions from the Duty of Loyalty: Some states permit broader exemptions from managerial liability than that offered by Delaware. As early as 1987, states such as Nevada and Virginia allowed firms to exempt directors and/or officers from the duty of loyalty altogether

(Eldar and Magnolfi, 2020). Unlike section 102(b)(7) of the Delaware corporate law statute, the exemption provisions in these states do not require that the director or officer act in good faith, which is broadly interpreted as synonymous with the duty of loyalty (Strine et al., 2010). These exemptions appear to cover not only the corporate opportunity doctrine, but also other aspects of the duty of loyalty, such as self-dealing transactions and fiduciary duties in hostile takeovers. In addition, a few states exempt directors or officers by default without any charter provision or board action. Most notably, in 2001, Nevada changed its statute to make both directors and officers exempt from monetary liability for violating the duty of loyalty by default (Barzuza, 2012; Barzuza and Smith, 2014; Donelson and Yust, 2014; Eldar, 2018).

One could argue that in these states, directors and officers are already protected from liability under the corporate opportunity doctrine because they may already be exempted for monetary liability for violating the duty of loyalty. While we include robustness checks that account for broader exemptions from the duty of loyalty, we do not account for these broad exemptions in the main specifications for three main reasons. First, the statutes that exempt managers from the duty of loyalty do not cover controlling shareholders. In the context of VCs, it is not only necessary to exempt managers, but also to make sure that VC firms, which may be deemed to be controlling shareholders (Boschner and Simmerman, 2016), are not required to disclose and share business opportunities. Therefore, these broader exemptions may be insufficient to ensure that VC investors are not liable for appropriating business opportunities. Second, the actual scope of exemptions from the duty of loyalty and whether they apply to corporate opportunities is not conclusive, and it is not clear whether market participants interpret these laws as allowing COWs. Otherwise, it is hard to explain why Nevada enacted a statutory provision that permits COWs in 2007 (which does not cover controlling shareholders anyway), even though its directors and officers had already been exempt from the duty of loyalty since 2001. Thus, at the very least, the statutes permitting corporate opportunity waivers may have clarified the law for the market. Third, most statutes that exempt directors or officers do not do so by default; rather, they typically require a charter amendment and hence shareholder approval to make the exemption effective. In contrast, the statutes permitting corporate opportunity waivers typically only require board action. Accordingly, the firms in our sample were less likely to adopt general duty of loyalty exemptions, but very likely to adopt COWs, particularly in the context of VC investment. Accordingly, the assumption that most firms take advantage of permissible exemptions may be too strong when shareholder approval is required.

Robustness Checks Associated with State Law Changes: First, several states, such as Nevada and Virginia, permit all firms to exempt directors (although typically with shareholder approval) from monetary liability for violating the duty of loyalty, which includes the corporate opportunity doctrine. Thus, it is possible that more states should be included in our treatment group. Table A.1 and Table A.2 test this nuance by defining the treatment as the earlier of two types of legislation: the passage of COW legislation or statutes that permit broader exemption from liability for violating the duty of loyalty. For ease of interpretation, we only report the results from the regression specification with year and firm fixed effects. Doing so allows us to consolidate our reduced form and instrumental variable (IV) analyses into two tables and include all the dependent variables we examined for common ownership, investment, directors, deal outcomes, and startup exits. Next, we also account for differences in states' COW legislation. For example, the COW legislation in Nevada does not cover shareholders, and the legislation in Washington requires shareholder approval before adopting COWs (as opposed to merely board action as in other states). Table A.3 and Table A.4 show the results from regressions in which the treatment variable excludes Nevada and Table A.5 and Table A.6 show the results from regressions in which the treatment variable excludes Washington. Our results are similar to those in our main specifications.

Table A.1.

Reduced Form: Waivers from the Duty of Loyalty

This table presents a robustness test for our natural experiment that incorporates additional institutional and legal details that could change the efficacy of our treatment. In each panel, the results are from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). The definition of treatment is the earlier of COW legislation or the adoption of laws that permit firms to exempt directors from the duty of loyalty (including the corporate opportunity doctrine). Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. All panels include startup, year, and VC investor fixed effects. Below the coefficient estimates are robust standard errors clustered by state of incorporation. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: Common VC investments	Any VC common investment	Total VC common investments	
Treat × Post	0.097***	0.147***	
	(0.021)	(0.052)	
Adjusted R^2	69.2%	81.3%	
Number of observations	142,174	142,174	
Number of unique startups	14,991	14,991	
Panel B: Within-industry investment by VCs	Within-industry	Extensive margin	Intensive margin
Treat × Post	0.076***	0.021***	0.056***
	(0.026)	(0.007)	(0.021)
Adjusted R^2	15.8%	6.7%	13.4%
Number of observations	142,174	142,174	142,174
Number of unique startups	14,991	14,991	14,991
Panel C: VC directorships	VC directors	Other directorships held	Within-industry directorships held
Treat × Post	0.176***	0.225*	0.069
	(0.051)	(0.129)	(0.055)
Adjusted R^2	77.2%	67.3%	64.1%
Number of observations	100,893	100,893	100,893
Number of unique startups	10,264	10,264	10,264
Panel D: VC deals	Deal volume	Late round	Deal size
Treat × Post	0.116***	0.024**	0.199**
	(0.042)	(0.012)	(0.077)
Adjusted R^2	12.2%	4.4%	6.7%
Number of observations	142,174	142,174	142,174
Number of unique startups	14,991	14,991	14,991
Startup, Year, and VC investor fixed effects	Yes	Yes	Yes

Table A.2.

Instrumental Variable (IV): Waivers from the Duty of Loyalty

This table presents results from instrumental variable (IV) regressions examining the type of directorships held at startups with common VC investments where the instrumental variable is redefined as the earlier of COW legislation or the adoption of laws that permit firms to exempt directors from the duty of loyalty (including the corporate opportunity doctrine). Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Industry for common VC investment is defined based on Preqin's primary and sub industry classification. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. The first-stage F-statistic is the Kleibergen-Paap Wald statistic. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: VC directorships	VC directors	Additional directorships held	Same-industry directorships held
Any common VC investment	1.632***	2.082***	0.643**
	(0.390)	(0.637)	(0.262)
First-stage F-statistic	117.4	122.9	122.9
t-statistic on instrument	10.83	11.09	11.09
Number of observations	100,893	100,893	100,893
Includes additional controls, startup, year, and VC investor fixed effects			
Panel B: VC deal outcomes	Deal count	Late round	Deal value
Any common VC investment	1.192***	0.243***	2.046***
	(0.115)	(0.050)	(0.225)
First-stage F-statistic	132.8	132.8	132.8
t-statistic on instrument	11.52	11.52	11.52
Number of observations	142,174	142,174	142,174
Includes additional controls, startup, year, and VC investor fixed effects			
Panel C: Startup exits	IPO	Sale	Failure
Any common VC investment	0.047***	0.024	-0.123***
	(0.007)	(0.014)	(0.022)
First-stage F-statistic	115.8	115.8	115.8
t-statistic on instrument	10.76	10.76	10.76
Number of observations	142,174	142,174	142,174
Includes additional controls, year and VC investor fixed effects			

Table A.3.

Reduced Form: Nevada Treatment Changed

This table presents a robustness test for our natural experiment that incorporates additional institutional and legal details that could change the efficacy of our treatment. In each panel, the results are from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). The definition of treatment changes Nevada from treated to untreated because its COW legislation does not cover controlling shareholders. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Below the coefficient estimates are robust standard errors clustered by state of incorporation. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: Common VC investments	Any VC common investment	Total VC common investments	
Treat × Post	0.102***	0.153***	
	(0.020)	(0.048)	
Adjusted R^2	69.3%	81.3%	
Number of observations	142,174	142,174	
Number of unique startups	14,991	14,991	
Panel B: Within-industry investment by VCs	Within-industry	Extensive margin	Intensive margin
Treat × Post	0.074***	0.021***	0.055***
	(0.023)	(0.006)	(0.018)
Adjusted R^2	15.8%	6.7%	13.4%
Number of observations	142,174	142,174	142,174
Number of unique startups	14,991	14,991	14,991
Panel C: VC directorships	VC directors	Other directorships held	Within-industry directorships held
Treat × Post	0.201***	0.185	0.071
	(0.047)	(0.126)	(0.051)
Adjusted R^2	77.2%	67.3%	64.1%
Number of observations	100,893	100,893	100,893
Number of unique startups	10,264	10,264	10,264
Panel D: VC deals	Deal volume	Late round	Deal size
Treat × Post	0.116***	0.023**	0.199***
	(0.038)	(0.010)	(0.070)
Adjusted R^2	18.2%	9.0%	14.7%
Number of observations	142,174	142,174	142,174
Number of unique startups	14,991	14,991	14,991
Startup, Year, and VC investor fixed effects	Yes	Yes	Yes

Table A.4.

Instrumental Variable (IV): Nevada Treatment Changed

This table presents results from instrumental variable (IV) regressions examining the type of directorships held at startups with common VC investments where the instrumental variable is redefined to change Nevada from treated to untreated because its COW legislation does not cover controlling shareholders. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Industry for common VC investment is defined based on Preqin's primary and sub industry classification. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. The first-stage F-statistic is the Kleibergen-Paap Wald statistic. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: VC directorships	VC directors	Additional directorships held	Same-industry directorships held
Any common VC investment	1.787***	1.649***	0.634***
	(0.356)	(0.574)	(0.233)
First-stage F-statistic	136.2	136.2	136.2
t-statistic on instrument	11.67	11.67	11.67
Number of observations	100,893	100,893	100,893
Includes additional controls, startup, year, and VC investor fixed effects			
Panel B: VC deal outcomes	Deal count	Late round	Deal value
Any common VC investment	1.129***	0.227***	1.941***
	(0.099)	(0.044)	(0.194)
First-stage F-statistic	163.1	163.1	163.1
t-statistic on instrument	12.77	12.77	12.77
Number of observations	142,174	142,174	142,174
Includes additional controls, startup, year, and VC investor fixed effects			
Panel C: Startup exits	IPO	Sale	Failure
Any common VC investment	0.034***	0.021*	-0.132***
	(0.005)	(0.011)	(0.018)
First-stage F-statistic	194.3	194.3	194.3
t-statistic on instrument	13.94	13.94	13.94
Number of observations	142,174	142,174	142,174
Includes additional controls, year and VC investor fixed effects			

Table A.5.

Reduced Form: Washington Treatment Changed

This table presents a robustness test for our natural experiment that incorporates additional institutional and legal details that could change the efficacy of our treatment. In each panel, the results are from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). The definition of treatment changes Washington from treated to untreated because its COW legislation requires shareholder approval. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Below the coefficient estimates are test statistics from robust standard errors clustered by state of incorporation. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: Common VC investments	Any VC common investment	Total VC common investments	
Treat × Post	0.103***	0.157***	
	(0.020)	(0.049)	
Adjusted R^2	69.2%	81.4%	
Number of observations	142,174	142,174	
Number of unique startups	14,991	14,991	
Panel B: Within-industry investment by VCs	Within-industry	Extensive margin	Intensive margin
Treat × Post	0.077***	0.022***	0.056***
	(0.023)	(0.006)	(0.018)
Adjusted R^2	15.8%	6.8%	13.4%
Number of observations	142,174	142,174	142,174
Number of unique startups	14,991	14,991	14,991
Panel C: VC directorships	VC directors	Other directorships held	Within-industry directorships held
Treat × Post	0.209***	0.214*	0.080
	(0.048)	(0.126)	(0.053)
Adjusted R^2	77.2%	67.3%	64.1%
Number of observations	100,893	100,893	100,893
Number of unique startups	10,264	10,264	10,264
Panel D: VC deals	Deal volume	Late round	Deal size
Treat × Post	0.120***	0.026***	0.212***
	(0.037)	(0.010)	(0.068)
Adjusted R^2	18.2%	9.0%	14.7%
Number of observations	142,174	142,174	142,174
Number of unique firms	14,991	14,991	14,991
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes

Table A.6.

Instrumental Variable (IV): Washington Treatment Changed

This table presents results from instrumental variable (IV) regressions examining the type of directorships held at startups with common VC investments where the instrumental variable is redefined to change Washington from treated to untreated because its COW legislation requires shareholder approval. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Industry for common VC investment is defined based on Preqin's primary and sub industry classification. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. The first-stage F-statistic is the Kleibergen-Paap Wald statistic. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: VC directorships	VC directors	Additional directorships held	Same-industry directorships held
Any common VC investment	1.841***	1.885***	0.701***
	(0.359)	(0.577)	(0.235)
First-stage F-statistic	137.0	137.0	137.0
t-statistic on instrument	11.70	11.70	11.70
Number of observations	100,893	100,893	100,893
Includes additional controls, startup, year, and VC investor fixed effects			
Panel B: VC deal outcomes	Deal count	Late round	Deal value
Any common VC investment	1.169***	0.256***	2.063***
	(0.102)	(0.045)	(0.202)
First-stage F-statistic	159.4	159.4	159.4
t-statistic on instrument	12.89	12.89	12.89
Number of observations	142,174	142,174	142,174
Includes additional controls, startup, year, and VC investor fixed effects			
Panel C: Startup exits	IPO	Sale	Failure
Any common VC investment	0.036***	0.020*	-0.132***
	(0.005)	(0.011)	(0.018)
First-stage F-statistic	185.9	185.9	185.9
t-statistic on instrument	13.63	13.63	13.63
Number of observations	142,174	142,174	142,174
Includes additional controls, year and VC investor fixed effects			

B Survey Evidence

Reliable survey tools require careful design and sample planning. We consulted 3 experts to vet the survey design and administered 2 beta tests prior to launching the survey. After beta-testing and receiving feedback from survey experts, the final survey contains 7 questions and an optional set of demographic questions. The survey was run through Qualtrics and included both a mobile and internet version. The survey is anonymous and does not require subjects to disclose their names or their affiliation and is IRB approved by Duke University. One advantage of online/mobile administration is the ability to randomly scramble the order of choices within a question, so as to mitigate potential order-of-presentation effects. Specifically, the survey scrambles the order of answers in questions 1, 2, 3, 4, 5, and 7. For question 6 (the only question not scrambled), the order of choices is unlikely to be a first-order issue and we wanted to preserve "other" as the last option. Participants were always allowed to skip questions if they did not want to answer them, which is why the number of observations varies across questions.

The solicitation and consent preamble is: "The purpose of the study is to gain insights from entrepreneurs, lawyers, and venture capitalists (VCs) on the outlook entrepreneurs face when seeking to raise capital from VC investors. Participation in the study involves completing a survey and the total time for completing the survey should not exceed 5-7 minutes. If you choose to participate, the expertise you provide will never be associated with your name or any other identifying information. Although collected data may be made public or used for future research purposes, your identity will always remain confidential. This is integral to the research process as it allows other researchers to verify results and avoid duplicating research. The research will not benefit you personally. We know of no risks resulting from participating in the study. Your participation is voluntary. You may withdraw at any time, and you may choose not to answer any question. If you have any questions about us or the research, feel free to email either of us now. If you have questions about your rights as a research subject, contact Duke University's Institutional Review Board at campusirb@duke.edu or at 919-684-3030. If contacting the IRB, please reference protocol ID#2021-0297. If you are interested in taking part in this survey, please click to the next screen."

Table B.1.

Survey Results: State of Incorporation

This table presents descriptive statistics about the the process by which state of incorporation is determined for startups.

Q1: How often do VC firms and startups negotiate the state of incorporation in a VC deal?		
Choice	Percent	Frequency
Never	12%	3
Rarely	40%	10
Sometimes	16%	4
Most of the time	16%	4
Always	16%	4

Q2: Please rank the two most common reasons for startups to incorporate in Delaware (1=ranked in top two)		
Choice	Percent	Frequency
Most other companies incorporate in Delaware	12%	3
The familiarity with the law and the body of precedents	84%	21
The expertise of Delaware's judiciary on business law issues	40%	10
Investors will withhold investment if the startup does not incorporate in Delaware	40%	10
Specific law of Delaware that is favorable to business planners	12%	3
Other	12%	3

Q3: Some high-growth startups that seek VC investments choose not to incorporate in Delaware. Please rank the two most common reasons to not incorporate in Delaware. (1=ranked in top two)		
Choice	Percent	Frequency
Loyalty toward the headquarter state	36%	9
Personal preference of a founder and/or lawyer	68%	17
Personal preference of a VC investor	4%	1
The cost and time needed to incorporate in Delaware	16%	4
Delaware does not offer any material advantages over other states	20%	5
Specific provision of other states' laws that are more favorable to business planners.	20%	5
Other	8%	2

Table B.2.

Survey Results: Corporate Opportunity Waiver

This table presents descriptive statistics about waivers from the corporate opportunity doctrine.

Q4: The National Venture Capital Association's sample certificate of incorporation includes a waiver from the corporate opportunity doctrine, which is permitted under section 122(7) of the Delaware Corporate Law Code. Is this a standard provision in the charter or bylaws of startups that you observe in practice?

Choice	Percent	Frequency
Yes	74%	14
No	26%	5

Q5: If this provision is not included in the charter or bylaws, do you typically include it in another legal document (such as the investment agreement with a VC firm or an employment contract with a director)?

Choice	Percent	Frequency
Never	11%	2
Rarely	0%	0
Sometimes	28%	5
Most of the time	56%	10
Always	6%	1

Q6: Which best describes the rationale for waiving the corporate opportunity doctrine?

Choice	Percent	Frequency
(1) VC investors' frequently engage with founders and other investors, and they want to avoid the risk of unexpected litigation from holding board seats at these potentially related startups	85%	17
(2) Startups that seek financing from VC investors have no choice but to let VC investors pursue business opportunities that may belong to the startups	5%	1
(3) Startups that agree to the waiver of the corporate opportunity doctrine get in return a higher valuation from VC investors that invest in the company	10%	2
(4) Other	12%	3

Table B.3.

Survey Results: Benchmarking to Other Deal Terms

This table presents descriptive statistics about incorporation and the inclusion of a waiver from the corporate opportunity doctrine.

Q7: For each of the legal or contract terms below, please select how willing to compromise founders are when negotiating a new investment with VCs?
1 = Not willing to compromise (Founder-friendly), 5=Extremeley willing to compromise (Investor-friendly)

Term	Mean	Std. Dev.	Choice =	1	2	3	4	5
Board Control	2.8	0.8		0%	36%	50%	9%	5%
Corporate Opportunity Waiver	4.2	1.1		5%	0%	15%	30%	50%
Pro rata Clause	4.3	0.8		0%	0%	18%	36%	45%
Redemption Rights	2.8	1.1		9%	32%	32%	23%	5%
State of incorporation	4.5	1.0		0%	9%	5%	14%	73%

C IPO Valuations

For startups that undergo an IPO, we supplement the Preqin data with IPO valuation data. We follow the approach introduced by Purnanandam and Swaminathan (2004) to determine the IPO value. This procedure involves computing three multiples for each IPO firm:

$$\left(\frac{P}{S}\right)_{IPO} = \left(\frac{\text{Offer price} \times \text{Shares outstanding}}{\text{Prior fiscal year sales}}\right),$$

$$\left(\frac{P}{EBITDA}\right)_{IPO} = \left(\frac{\text{Offer price} \times \text{Shares outstanding}}{\text{Prior fiscal year EBITDA}}\right),$$

$$\left(\frac{P}{E}\right)_{IPO} = \left(\frac{\text{Offer price} \times \text{Shares outstanding}}{\text{Prior fiscal year earnings}}\right).$$

where the offer price comes from the Security Data Corporation (SDC) database, shares outstanding at the close of the offer date come from the Center for Research in Security Prices (CRSP), and all accounting data comes from Compustat.

Each IPO is matched to a public firm from Compustat that did not undergo an IPO in the previous three years. The matches are based on the Mahalanobis distance of the sale, EBITDA and net income figures for each of the respective multiples. In this way, the multiple captures the valuation of the firm for a given level of performance (e.g., sales). The matching algorithm also requires that the matched observation be from the same fiscal year and Fama-French 48 industry. We require EBITDA and net income to have the same sign.[18]

The advantage of this approach is that valuation ratios of the IPO startups are already adjusted for the typical valuation levels in the industry in a given year. For each matched firm, we compute the multiples in the same way as for the IPO firms, except that for offer price we use the market price from CRSP, and the shares outstanding refers to the number of shares outstanding at the close of the day immediately prior to the IPO offer date of the matching IPO firm. The final

[18] Unlike Purnanandam and Swaminathan (2004), we do not omit firms that have negative EBITDA or net income or if sales equal zero. The reason is that a high percentage of the firms in our sample are high-growth firms and have negative earnings.

valuation measures for each IPO firm are computed as follows:

$$\left(\frac{P}{V}\right)_{Sales} = \frac{\left(\frac{P}{S}\right)_{IPO}}{\left(\frac{P}{S}\right)_{Match}}, \tag{C.1}$$

$$\left(\frac{P}{V}\right)_{EBITDA} = \frac{exp\left(\left(\frac{P}{EBITDA}\right)_{IPO}\right)}{exp\left(\left(\frac{P}{EBITDA}\right)_{Match}\right)}, \tag{C.2}$$

$$\left(\frac{P}{V}\right)_{Earnings} = \frac{exp\left(\left(\frac{P}{E}\right)_{IPO}\right)}{exp\left(\left(\frac{P}{E}\right)_{Match}\right)}. \tag{C.3}$$

Note that in equations C.2 and C.3, we take the exponent of the EBITDA and net income multiples in order to account for the presence of negative figures.

D Additional Figures and Tables

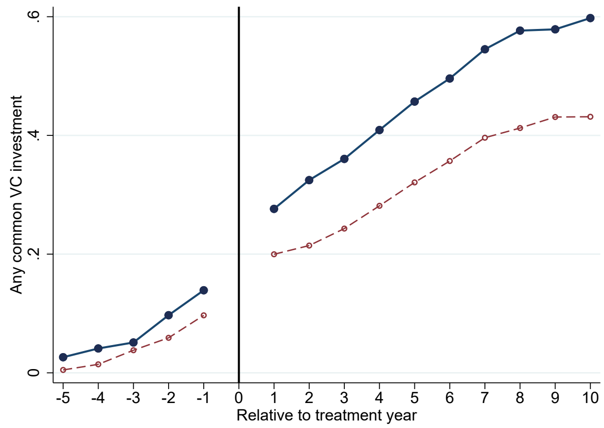


Figure D.1. Parallel trends in common VC investment for startups incorporated in states with a high concentration of VC funding. For this figure, the sample is limited to startups incorporated in Delaware, California, Massachusetts, and New York. The figure plots the parallel trend lines for Delaware and the control states by year relative to the law change in Delaware. The figure shows a window spanning from five years before the law change to ten years after the change. The solid navy line represents Delaware. The dashed maroon line represents the control states of California, Massachusetts, and New York.

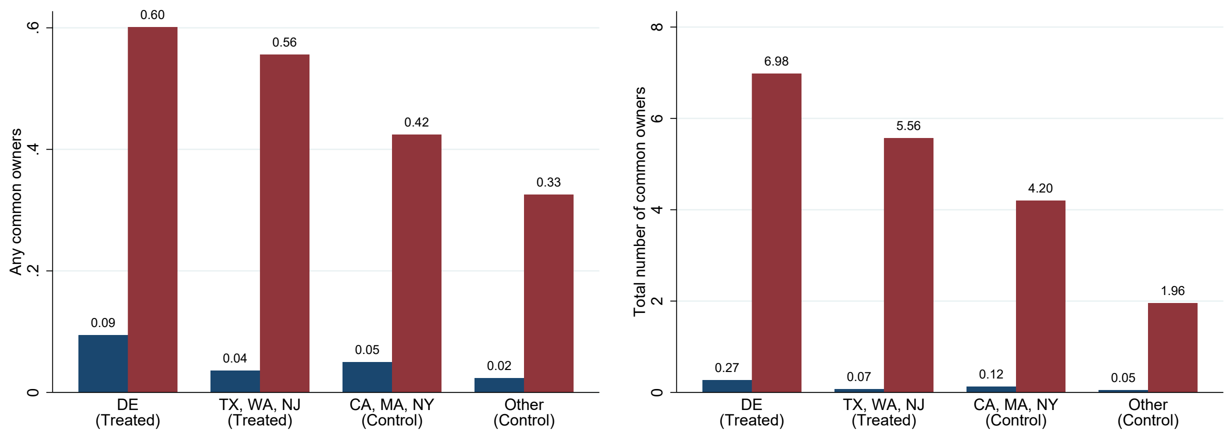


Figure D.2. Common ownership rates before and after the corporate opportunity waiver (COW) legislation. The figures plot common ownership rates in the years before and after the law change for different subgroups of startups categorized by their state of incorporation. The first set of bars represents startups incorporated in Delaware, the first state to be treated. The second set of bars represents startups incorporated in Texas, Washington, and New Jersey: these are treated states that adopted the COW legislation in years subsequent to Delaware. The third set of bars represent startups in California, Massachusetts, and New York: these are control states that have strong entrepreneurial ecosystems and a high concentration of startups with venture capital (VC) funding. The final set of bars represent the remaining control states. The figure on the left plots the percent of startups with any common ownership and the figure on the right plots the total number of common owners. The navy bar represents pre-treatment years and the maroon bar represents post-treatment years. The bars are labeled with their actual value.

Table D.1.

Summary Statistics on Industry (Broadly Defined)

This table presents summary statistics for the broadest industry definition applied to our sample of VC-funded startups.

	(1)	(2)	(3)	(4)
Broadest industry definition	Startup-year observations	Percent	Unique startups	Percent
Computer	41,049	28.9%	4,399	29.3%
IT	25,085	17.6%	2,605	17.4%
Internet	23,835	16.8%	2,717	18.1%
Medtech	22,064	15.5%	2,190	14.6%
Pharmaceuticals	8,472	6.0%	881	6.0%
Business	7,849	5.5%	785	5.2%
Cleantech	4,831	3.4%	439	2.9%
Other	4,621	3.3%	530	3.5%
Media	2,405	1.6%	253	1.7%
Industrial	1,963	1.3%	192	1.3%

Table D.2.

Summary Statistics on Industry (Narrowly Defined)

This table presents summary statistics for the narrowest industry definition applied to our sample of VC-funded startups. This table lists all industries with more than 1,300 startup-year observations.

Narrowest industry definition	(1) Startup-year observations	(2) Percent	(3) Unique startups	(4) Percent
Pharmaceuticals	8,472	6.0%	881	5.9%
Telecoms	7,816	5.5%	781	5.9%
Medical devices	7,213	5.1%	660	4.4%
Software – analytics	5,842	4.1%	679	4.5%
Biotechnology	4,879	3.4%	539	3.6%
Internet – business	4,318	3.0%	497	3.3%
Semiconductors	4,168	2.9%	364	2.4%
Internet – general	3,908	2.7%	442	2.9%
Software – general	3,882	2.7%	360	2.4%
IT	3,867	2.7%	389	2.6%
Clean technology	3,742	2.6%	342	2.3%
Technology	3,619	2.5%	385	2.6%
Healthcare IT	3,517	2.5%	370	2.5%
Healthcare	3,290	2.3%	322	2.1%
Software – cloud	2,860	2.0%	319	2.1%
Internet – communication	2,774	2.0%	312	2.1%
IT security	2,471	1.7%	280	1.9%
Medical technology	2,276	1.6%	214	1.4%
Software – security	1,997	1.4%	215	1.4%
Software – content management	1,904	1.3%	195	1.3%
Software – systems management	1,777	1.2%	180	1.2%
Software – internet	1,770	1.2%	186	1.2%
Software – financial	1,606	1.1%	173	1.1%
Internet – retail	1,605	1.1%	191	1.1%
Software – sales	1,546	1.1%	168	1.0%
Software – communication	1,531	1.1%	162	1.1%
Electronics	1,513	1.1%	151	1.0%
Internet – financial	1,429	1.0%	176	1.2%
Software – integration	1,385	1.0%	146	1.0%
Financial services	1,371	1.0%	151	1.0%
Gaming	1,364	1.0%	155	1.0%
Marketing	1,346	0.9%	135	0.9%

Table D.3.

Summary Statistics on Industry (Narrowly Defined) Continued

This table presents summary statistics for the narrowest industry definition applied to our sample of VC-funded startups. This table lists all industries with less than 1,300 and more than 500 startup-year observations.

Narrowest industry definition	(1) Startup-year observations	(2) Percent	(3) Unique startups	(4) Percent
Advertising	1,265	0.9%	119	0.8%
Digital media	1,233	0.9%	126	0.8%
Networks	1,221	0.9%	117	0.8%
Wireless	1,213	0.9%	114	0.8%
Communications	1,196	0.8%	108	0.7%
Internet – advertising	1,183	0.8%	119	0.8%
Software – technology	1,178	0.8%	136	0.9%
Software – advertising	1,163	0.8%	126	0.8%
Business services	1,150	0.8%	113	0.8%
Software – business	1,143	0.8%	124	0.8%
Internet – leisure	1,078	0.8%	112	0.7%
Software – mobile apps	1,022	0.7%	117	0.8%
Renewable energy	1,012	0.7%	90	0.6%
Internet – mobile apps	975	0.7%	116	0.8%
Software – labor	970	0.7%	113	0.8%
Software – telecom	967	0.7%	95	0.6%
Internet – education	919	0.7%	107	0.7%
Software – medical	882	0.6%	97	0.6%
Manufacturing	843	0.6%	78	0.5%
Hardware	838	0.6%	90	0.6%
Media	813	0.6%	74	0.5%
Food	753	0.5%	91	0.6%
Software IT	659	0.5%	77	0.5%
Software – billing	635	0.4%	58	0.4%
Consumer products	609	0.4%	77	0.5%
Software – education	569	0.4%	57	0.4%
Agriculture	544	0.4%	66	0.4%
Education	537	0.4%	65	0.4%
Software – industrial	520	0.4%	53	0.4%
Internet – telecom	511	0.3%	51	0.3%
Internet – equipment	501	0.3%	52	0.3%

Table D.4.

Summary Statistics on Industry (Narrowly Defined) Continued

This table presents summary statistics for the narrowest industry definition applied to our sample of VC-funded startups. This table lists all industries with less than 500 and more than 300 startup-year observations. Industries with fewer than 300 observations include: Energy, Internet – technology, Aerospace, Life sciences, Internet – analytics, Information services, Beverages, Software – retail, Entertainment, Biomedical, Internet – content management, Leisure, Telecom media, Internet – food and beverage, Outsourcing, Materials, Publishing, Oil and gas, Internet – labor, Internet – real estate, Software – transportation, Software – leisure, Internet – billing, Internet – medical, Construction, Property, Chemicals, Internet – sales, Computer services, Logistics, Power, Software – real estate, Internet – security, Internet – integration, Utilities, Distribution, Internet – systems management, Environmental services, Internet – supply chain, Internet – transportation, Shipping, Restaurants, Intellectual property, Engineering, Gambling, Infrastructure, Defense, Internet – industrial, Software – food and beverage, Mining, Hotels and offices, Predictive medicine, and Armaments.

	(1)	(2)	(3)	(4)
Narrowest industry definition	Startup-year observations	Percent	Unique startups	Percent
IT infrastructure	494	0.3%	54	0.4%
Internet – cloud	468	0.3%	57	0.4%
Software – supply chain	468	0.3%	44	0.3%
Industrial	461	0.3%	37	0.2%
Internet – software	459	0.3%	51	0.3%
Retail	449	0.3%	52	0.3%
Consumer services	439	0.3%	51	0.3%
Software – equipment	433	0.3%	42	0.3%
Nanotechnology	424	0.3%	37	0.2%
High technology	393	0.3%	65	0.4%
Medical Instruments	379	0.3%	39	0.3%
Internet IT	339	0.2%	39	0.3%
Transportation	312	0.2%	34	0.2%
Insurance	308	0.2%	42	0.3%

Table D.5.

Summary Statistics on State of Incorporation and Reincorporations

This table presents summary statistics for the sample of VC-backed startups for which we used Lexis Advance Public Records to identify the state of incorporation. Panel A summarizes the full sample. Panel B and Panel C summarize the subset of VC-backed startups in the sample that reincorporate into another state. Panel B characterizes the states from which the startups reincorporate and Panel C characterizes the states into which the startups reincorporate.

	(1)	(2)	(3)	(4)
Panel A: Full sample	Observations	Percent	Unique startups	Percent
Delaware	94,435	66.4%	10,882	72.6%
Non-Delaware treated states	9,667	6.8%	905	6.0%
Control states	38,072	26.8%	3,204	21.4%
Total	142,174	100.0%	14,991	100.0%
Panel B: Reincorporations from				
Delaware	1,912	9.7%	178	11.9%
Non-Delaware treated states	2,512	12.8%	199	13.3%
Control states	15,257	77.5%	1,124	74.9%
Total	19,681	100.0%	1,501	100.0%
Panel C: Reincorporations into				
Delaware	17,101	86.9%	1,284	85.5%
Non-Delaware treated states	753	3.8%	64	4.3%
California	600	3.1%	55	3.7%
Control states	1,227	6.2%	98	6.5%
Total	19,681	100.0%	1,501	100.0%

Table D.6.

Common VC Investments and Startup Growth

This table presents estimates of the change in startup outcomes from the endogenous ordinary least squares (OLS) regressions. The focal independent variable is common VC investments. In odd columns, any common VC investment is an indicator variable for whether a startup has a VC investor that commonly owns another startup within the same industry, and in even columns total common VC investments is defined as the natural log of the total number of common VC investments. In Panel A, the dependent variables focus on VC deals and in Panel B, the dependent variables focus on startup exits. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Deal count		Late stage		Deal value	
Panel A: VC deals	(1)	(2)	(3)	(4)	(5)	(6)
Any common VC investment	0.447***		0.123***		0.810***	
	(0.006)		(0.004)		(0.013)	
Total common VC investments		0.217***		0.070***		0.431***
		(0.003)		(0.002)		(0.007)
Additional control variables	Yes	Yes	Yes	Yes	Yes	Yes
Startup fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
VC investor fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
HQ-state-by-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-by-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	24.2%	22.9%	10.2%	10.3%	19.3%	19.0%
Number of observations	141,296	141,296	141,296	141,296	141,296	141,296
Number of unique startups	14,896	14,896	14,896	14,896	14,896	14,896

	IPO		Sale		Failure	
Panel B: Exits	(1)	(2)	(3)	(4)	(5)	(6)
Any common VC investment	0.000		0.011***		-0.002	
	(0.001)		(0.001)		(0.001)	
Total common VC investment		0.001***		0.006***		-0.005***
		(0.001)		(0.000)		(0.001)
Adjusted R^2	2.9%	2.9%	2.3%	2.4%	3.9%	4.0%
Additional startup controls	Yes	Yes	Yes	Yes	Yes	Yes
Headquarter-state-by-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-by-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	141,296	141,296	141,296	141,296	141,296	141,296
Number of unique startups	14,896	14,896	14,896	14,896	14,896	14,896

Table D.7.

Placebo Tests for Common VC Investment

This table presents the results from additional difference-in-differences regressions that use placebo treatments. In Panel A, the placebo treatment applies to the dates the treatment starts for actually treated states. We require the date to be at least 6 years before or after the actual treatment. In Panel B, the sample is restricted to untreated observations and then a placebo treatment is applied to a set of 8 untreated states of similar states in terms of size and entrepreneurial activity. The placebo treatment states include California, New York, Connecticut, Colorado, Indiana, Illinois, Arizona, Georgia, and Oregon. In columns (1) and (2), the dependent variable is an indicator variable for whether a startup has a VC investor that invests in another startup within the same industry. In columns (3) and (4), the dependent variable is the natural log of the total number of common VC investments that startup has. Additional control variables include total capital previously raised and the total number of rounds of capital previously raised. For the dependent variables, industry is defined based on Preqin's primary and sub industry classification. For the industry fixed effects, the adjusted primary industry classification, which is coarser than the primary industry classification, is used. Below the coefficient estimates are robust standard errors clustered by state of incorporation. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	Any common VC investment		Total common VC investment	
Panel A: Placebo treatment dates	(1)	(2)	(3)	(4)
Treat × Post	-0.010	-0.011	0.017	0.018
	(0.013)	(0.011)	(0.022)	(0.026)
Adjusted R^2	69.1%	69.6%	81.3%	82.8%
Number of observations	142,174	141,296	142,174	141,296
Number of unique firms	14,991	14,896	14,991	14,896
	Any common VC investment		Total common VC investment	
Panel B: Placebo treatment states				
Treat × Post	-0.019*	-0.030	0.006	-0.009
	(0.010)	(0.030)	(0.026)	(0.041)
Adjusted R^2	72.5%	73.3%	80.1%	81.9%
Additional controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	No	Yes	No
Startup fixed effects	Yes	Yes	Yes	Yes
VC investor fixed effects	Yes	Yes	Yes	Yes
Headquarter-state-by-year fixed effects	No	Yes	No	Yes
Industry-by-year fixed effects	No	Yes	No	Yes
Number of observations	40,516	40,082	40,516	40,082
Number of unique startups	5,029	4,977	5,029	4,977

Table D.8.

Common VC Investments (Alternative Samples)

This table presents robustness tests for the results from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). Panel A excludes startup-year observations associated with the "spray and pray" investment strategy adopted after the advent of cloud computing (i.e., software startups from 2006 to 2018). Panel B excludes startup-year observations associated with the dotcom bubble between 1999 and 2001. Panel C excludes startup-year observations associated with the financial crisis between 2007 and 2009. In column (1) the dependent variable is an indicator variable for whether a startup has a VC investor that invests in another startup within the same industry. In column (2), the dependent variable is the natural log of the total number of common VC investments that startup has. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Below the coefficient estimates are robust standard errors clustered by state of incorporation. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: Excluding "Spray and pray" investment	Any common VC investment (1)	Total common VC investment (2)
Treat × Post	0.092***	0.140***
	(0.013)	(0.030)
Adjusted R^2	70.5%	82.2%
Number of observations	114,045	114,045
Panel B: Excluding Dotcom bubble	Any common VC investment	Total common VC investment
Treat × Post	0.113***	0.171***
	(0.017)	(0.039)
Adjusted R^2	69.1%	82.0%
Number of observations	132,481	132,481
Panel C: Excluding financial crisis	Any common VC investment	Total common VC investment
Treat × Post	0.093***	0.142***
	(0.012)	(0.028)
Adjusted R^2	69.7%	81.5%
Number of observations	120,330	120,330
All regressions include additional controls, startup, VC investor, and year fixed effects.		

Table D.9.

Common VC Investment in Startups Incorporated Outside Headquarter State and Excluding Delaware

This table presents results from a restricted sample consisting only of startups incorporated in a state that is not their headquarter state and that is not Delaware. In each panel, the results are from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Below the coefficient estimates are robust standard errors clustered by state of incorporation. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: Common VC investments	Any common VC investment	Total common VC investments	
Treat × Post	0.100***	0.235***	
	(0.028)	(0.061)	
Adjusted R^2	71.2%	80.3%	
Number of observations	4,571	4,571	
Number of unique startups	390	390	
Panel B: Within-industry investment by VCs	Within-industry	Extensive margin	Intensive margin
Treat × Post	0.089***	0.052***	0.043*
	(0.028)	(0.011)	(0.022)
Adjusted R^2	16.3%	6.7%	14.7%
Number of observations	4,571	4,571	4,571
Number of unique startups	390	390	390
Additional controls	Yes	Yes	Yes
Startup fixed effects	Yes	Yes	Yes
VC investor fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes

Table D.10.

Startup Growth for Startups Incorporated Outside Headquarter State and Excluding Delaware

This table presents results from a restricted sample consisting only of startups incorporated in a state that is not their headquarter state and that is not Delaware. In each panel, the results are from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Below the coefficient estimates are robust standard errors clustered by state of incorporation. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: VC directorships	VC directors	Other directorships held	Within-industry directorships held
Treat × Post	0.056	0.320	0.263***
	(0.103)	(0.210)	(0.075)
Adjusted R^2	78.4%	63.2%	64.5%
Number of observations	2935	2935	2935
Number of unique startups	235	235	235
Panel B: VC deals	Deal volume	Late round	Deal size
Treat × Post	0.148***	0.042**	0.349***
	(0.035)	(0.016)	(0.062)
Adjusted R^2	22.3%	16.3%	19.6%
Number of observations	4,571	4,571	4,571
Number of unique startups	390	390	390
Additional controls	Yes	Yes	Yes
Startup fixed effects	Yes	Yes	Yes
VC investor fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes

E Additional Instrumental Variable Tests

Table E.1.

Instrumental Variable (IV): Entrepreneurial States

This table presents results from instrumental variable (IV) regressions examining the type of directorships held at startups with common VC investment using an alternative subsample of high entrepreneurial states. The sample is limited to startups incorporated in Delaware, California, Massachusetts, and New York. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Industry for common VC investment is defined based on Preqin's primary and sub industry classification. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. The first-stage F-statistic is the Kleibergen-Paap Wald statistic. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: VC directorships	VC directors	Additional directorships held	Same-industry directorships held
Any common VC investment	1.441***	2.427***	0.424
	(0.422)	(0.767)	(0.280)
First-stage F-statistic	111.6	111.6	111.6
t-statistic on instrument	10.56	10.56	10.56
Number of observations	89,125	89,125	89,125
Includes additional controls, startup, VC, industry-by-year and headquarter-state-by-year fixed effects			
Panel B: VC deal outcomes	Deal count	Late round	Deal value
Any common VC investment	1.425***	0.291***	2.317***
	(0.131)	(0.056)	(0.248)
First-stage F-statistic	136.2	136.2	136.2
t-statistic on instrument	11.67	11.67	11.67
Number of observations	122,435	122,435	122,435
Includes additional controls, startup, VC, industry-by-year and headquarter-state-by-year fixed effects			
Panel C: Startup exits	IPO	Sale	Failure
Any common VC investment	0.047***	0.028*	-0.187***
	(0.008)	(0.017)	(0.030)
First-stage F-statistic	97.8	97.8	97.8
t-statistic on instrument	9.89	9.89	9.89
Number of observations	122,438	122,438	122,438
Includes additional controls, VC, industry-by-year and headquarter-state-by-year fixed effects			

Table E.2.

Instrumental Variable (IV): No Startups Originally Incorporated in Delaware

This table presents robustness tests for the instrumental variable (IV) regressions examining the consequences of common VC investment using an alternative subsample that excludes startups originally incorporated in Delaware. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: VC directorships	VC directors	Additional directorships held	Same-industry directorships held
Any common VC investment	2.320***	2.723***	1.034***
	(0.391)	(0.710)	(0.233)
First-stage F-statistic	124.7	124.7	124.7
t-statistic on instrument	11.17	11.17	11.17
Number of observations	31,033	31,033	31,033
Includes additional controls, startup, VC, industry-by-year and headquarter-state-by-year fixed effects			

Panel B: VC deal outcomes	Deal count	Late round	Deal value
Any common VC investment	1.462***	0.371***	2.679***
	(0.130)	(0.054)	(0.253)
First-stage F-statistic	135.2	135.2	135.2
t-statistic on instrument	11.63	11.63	11.63
Number of observations	47,350	47,350	47,350
Includes additional controls, startup, VC, industry-by-year and headquarter-state-by-year fixed effects			

Panel C: Startup exits	IPO	Sale	Failure
Any common VC investment	0.141***	0.011	-0.084**
	(0.028)	(0.028)	(0.038)
First-stage F-statistic	40.6	40.6	40.6
t-statistic on instrument	6.37	6.37	6.37
Number of observations	47,350	47,350	47,350
Includes additional controls, VC, industry-by-year and fixed effects			

Table E.3.

Instrumental Variable (IV): "Spray and Pray" Investment

This table presents robustness tests for the instrumental variable (IV) regressions examining the consequences of common VC investment. These regressions exclude startup-year observations associated with the "spray and pray" investment strategy adopted after the advent of cloud computing (i.e., software startups from 2006 to 2018). Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: VC directorships	VC directors	Additional directorships held	Same-industry directorships held
Any common VC investment	2.069***	1.466*	0.226
	(0.498)	(0.748)	(0.222)
First-stage F-statistic	79.6	79.6	79.6
t-statistic on instrument	8.92	8.92	8.92
Number of observations	78,670	78,670	78,670
Includes additional controls, startup, VC, industry-by-year and headquarter-state-by-year fixed effects			

Panel B: VC deal outcomes	Deal count	Late round	Deal value
Any common VC investment	1.258***	0.230***	2.088***
	(0.133)	(0.060)	(0.256)
First-stage F-statistic	103.2	103.2	103.2
t-statistic on instrument	10.16	10.16	10.16
Number of observations	113,254	113,254	113,254
Includes additional controls, startup, VC, industry-by-year and headquarter-state-by-year fixed effects			

Panel C: Startup exits	IPO	Sale	Failure
Any common VC investment	0.046***	0.023	-0.188***
	(0.009)	(0.018)	(0.032)
First-stage F-statistic	86.2	86.2	86.2
t-statistic on instrument	9.29	9.29	9.29
Number of observations	113,254	113,254	113,254
Includes additional controls, VC, industry-by-year and headquarter-state-by-year fixed effects			

Table E.4.

Instrumental Variable (IV): Excluding Dotcom Bubble

This table presents robustness tests for the instrumental variable (IV) regressions examining the consequences of common VC investment. These regressions exclude startup-year observations associated with the bursting of the dotcom bubble between 1999 and 2001. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: VC directorships	VC directors	Additional directorships held	Same-industry directorships held
Any common VC investment	1.579***	1.870***	0.464*
	(0.391)	(0.632)	(0.241)
First-stage F-statistic	130.4	130.4	130.4
t-statistic on instrument	11.42	11.42	11.42
Number of observations	93,858	93,858	93,858
Includes additional controls, startup, VC, industry-by-year and headquarter-state-by-year fixed effects			
Panel B: VC deal outcomes	Deal count	Late round	Deal value
Any common VC investment	1.421***	0.321***	2.495***
	(0.118)	(0.048)	(0.223)
First-stage F-statistic	152.1	152.1	152.1
t-statistic on instrument	12.33	12.33	12.33
Number of observations	131,712	131,712	131,712
Includes additional controls, startup, VC, industry-by-year and headquarter-state-by-year fixed effects			
Panel C: Startup exits	IPO	Sale	Failure
Any common VC investment	0.041***	0.030**	-0.159***
	(0.006)	(0.014)	(0.023)
First-stage F-statistic	146.9	146.9	146.9
t-statistic on instrument	12.12	12.12	12.12
Number of observations	131,714	131,714	131,714
Includes additional controls, VC, industry-by-year and headquarter-state-by-year fixed effects			

Table E.5.

Instrumental Variable (IV): Excluding Financial Crisis

This table presents robustness tests for the instrumental variable (IV) regressions examining the consequences of common VC investment. These regressions exclude startup-year observations associated with the financial crisis period between 2007 and 2009. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: VC directorships	VC directors	Additional directorships held	Same-industry directorships held
Any common VC investment	1.735***	2.070***	0.569**
	(0.409)	(0.699)	(0.259)
First-stage F-statistic	115.2	115.2	115.2
t-statistic on instrument	10.73	10.73	10.73
Number of director observations	84,894	84,894	84,894
Includes additional controls, startup, VC, industry-by-year and headquarter-state-by-year fixed effects			

Panel B: VC deal outcomes	Deal volume	Late round	Deal value
Any common VC investment	1.343***	0.290***	2.293***
	(0.130)	(0.056)	(0.251)
First-stage F-statistic	128.3	128.3	128.3
t-statistic on instrument	11.33	11.33	11.33
Number of observations	119,549	119,549	119,549
Includes additional controls, startup, VC, industry-by-year and headquarter-state-by-year fixed effects			

Panel C: Startup exits	IPO	Sale	Failure
Any common VC investment	0.044***	0.029*	-0.187***
	(0.007)	(0.016)	(0.027)
First-stage F-statistic	144.9	144.9	144.9
t-statistic on instrument	12.04	12.04	12.04
Number of observations	119,549	119,549	119,549
Includes additional controls, VC, industry-by-year and headquarter-state-by-year fixed effects			

Table E.6.

Startup Exits and Venture Capital (VC) Bargaining Power

This table presents instrumental variable (IV) regression estimates for a subsample of startups where common VC investors have higher bargaining power with founders. Specifically, we exclude startups where the common VC investor first invests in the startup in a later round when the startup is more mature and the founder has more bargaining power. We present the results from instrumental variable (IV) regressions that explore the direct effect of common ownership on startup exits. Below the coefficient estimates are robust standard errors clustered startup and adjusted for small clusters. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	IPO	Sale	Failure
Panel A: Startup exits	(1)	(2)	(3)
Any common VC investment	0.042***	0.006	-0.162***
	(0.007)	(0.016)	(0.027)
Additional controls	Yes	Yes	Yes
VC investor fixed effects	Yes	Yes	Yes
Headquarter-state-by-year fixed effects	Yes	Yes	Yes
Industry-by-year fixed effects	Yes	Yes	Yes
First-stage F-statistic	113.9	113.9	113.9
t-statistic on instrument	10.67	10.67	10.67
Number of observations	102,280	102,280	102,280

F Exclusion Restriction Robustness Tests

In this Appendix, we use two recent advances in the literature on IV estimation to test and relax the exclusion restriction (Angrist et al., 2010; Conley et al., 2012; Kippersluis and Rietveld, 2018). First, we follow the approach emphasizing the identification of subgroups for which the IV is irrelevant as a test of the exclusion restriction. Second, we explore the consequences of relaxing the exclusion restriction assumption using a partial identification approach. If the IV correlates with some unobserved covariate affecting startup growth, then the point estimate for common ownership will be biased. We investigate the robustness of the IV estimator in relation to the potential bias from violating the exclusion restriction in our estimates. To achieve this goal, we estimate a bound for this potential bias by using an approach that produces a plausible estimate (a term proposed by Conley, Hansen, and Rossi (2012)).

The first approach we use to detect and investigate sensitivity to violations of the exclusion restriction is to estimate the direct effect of the IV on the outcome in a subsample for which the IV likely does not affect the treatment variable, which following the prior literature we refer to as the "zero-first-stage test" (Angrist et al., 2010). This test provides potential evidence for or against the exclusion restriction. The intuition for the test follows the logic of a placebo test. If the first stage is zero, then the reduced-form effect of the IV on the outcome variable should also be zero if the exclusion restriction is satisfied. In our setting, a zero-first-stage implies that being in a treated state has no effect on common VC investment. We consider a zero-first stage group consisting of startups that are in regulated, high trust industries (education, healthcare, and defense/aerospace). We limit this group to only those that are pure-play startups in the space as indicated by only having one subindustry. The rationale is that these high trust, regulated industries that are often subject to governmental and public scrutiny may be more likely to require common VC investors to strictly maintain their loyalty obligations and less likely to agree to the corporate opportunity waiver. We note that this group is limited to only 2,211 observations.

Panel A of Table F.1 summarizes these regression results for VC directorships. Columns (1) – (3) display the estimates for VC directorships, additional startup directorships, and within-industry directorships respectively. In Panel A, we see that the startups in the zero-first-stage group have no relationship with the IV. In contrast, for the remaining sample, which we expect to be related to the IV, we see positive, statistically significant relationships. Thus, these subsample tests provide support for the proposition that the exclusion restriction is satisfied, and the results are consistent with the legal analysis provided in Online Appendix A.

Similarly, Panel A of Table F.2 and Panel A of Table F.3 summarize these regression results for VC deal outcomes and startup exits respectively. Again, we see contrasts between the zero-first-stage group and the remaining sample. In each case, the zero-first-stage group is statistically insignificant (except where the dependent variable is sale, where it is negative and significant at the 10 percent level) while the remaining sample is not. For most outcomes, the point estimate is close to zero, but the smaller sample size lends itself to noisy estimates On the other hand, the estimates are positive and significant in the remaining sample.

Next, we explore the importance of the exclusion restriction by following the local-to-zero method outlined in Conley et al. (2012) that estimates different degrees of exclusion restriction violation to generate a plausibly exogenous IV estimate. Their method involves estimating the following equation:

$$Y_{jnhit} = \alpha + \beta Common\hat{O}wnership_{jnhit} + \mu X_{jnhit} + \zeta Z_{jnhit} + f_j + \gamma_{ht} + \rho_{it} + \varepsilon_{jnhit} \qquad \text{(F.1)}$$

where ζ is a parameter measuring the plausibility of the exclusion restriction and Z_{jnhit} is the IV, that is, being in a treated state after the law change. The difference between the primary IV specification (i.e., Equation 3 in the main paper) is the presence of the term ζZ_{jnhit}. In the main specification, we assume the exclusion restriction holds and $\zeta = 0$. If the exclusion restriction is not violated, then this method produces the same confidence interval as a traditional IV regression. If the exclusion restriction is violated, then different degrees of violation can be incorporated into the original estimate.

The estimates that allow for exclusion restriction violation are Bayesian. The estimates are based on updating a prior without exclusion restriction violation to a posterior with violations. To see this more clearly, let the distribution for β be approximated as follows:

$$\hat{\beta} \sim N\left(\beta, Var_{2SLS}\right) + A\zeta$$

$$Prior = \zeta \sim N\left(0,\Omega_{\zeta}\right) \tag{F.2}$$

$$\hat{\beta} \sim N\left(\beta, Var_{2SLS} + A\Omega_{\zeta}A'\right)$$

where Var_{2SLS} is the variance-covariance matrix and A is the projection matrix from estimating the two-stage least-squares estimator from Equation F.1. Exclusion restriction violations are represented by ζ. In contrast to the traditional IV approach, where ζ is assumed to be zero, we replace that assumption with the assumption that ζ is close to, but not necessarily equal to, zero. We do this by specifying a distribution for ζ. This can be symmetrical around zero, for example, by specifying a normal distribution centered at zero as shown in the equation above. If the underlying economic arguments suggest that the direction of potential exclusion restriction violations are ambiguous, an uninformative prior of 0 is reasonable.

Alternatively, Kippersluis and Rietveld (2018) develop a method for estimating a sensible prior distribution to use as an input when generating plausibly exogenous IV estimates. The intuition for their approach to generating an informative prior is to use observable changes in coefficient estimates and standard errors associated with the IV stemming from different subsamples of the data. They suggest using the zero-first-stage test discussed above in conjunction with a formula based on Imbens and Rubin (2015) for estimating the variance. Specifically, $\mathit{\Omega}_{\zeta} = \left(0.125\sqrt{S_Z^2 + S_R^2}\right)$ where S_Z is the standard error for the zero-first-stage test and S_R is the standard error from the remaining sample.

Panel B of Table F.1 summarizes the plausibly exogenous IV estimates for directorships. The results in the first row assume exclusion restriction violations are symmetric around zero but introduce uncertainty using the Imbens and Rubin formula. While the size of the standard errors is larger than in the main specifications, the results are statistically significant. The results in the second row allow for a positive violation of the exclusion restriction, where the mean is 0.05 and the variance continues to be that from the Imbens and Rubin formula. To put the size of the selected potential positive violation into context, we can compare it with our point estimate for common VC investment. Doing so suggests we are allowing for a meaningful unobservable factor that is between 4% to 10% of the magnitude of the estimate on common VC investment. For the VC directorships, the results from the plausibly exogenous IV estimates reveal that common VC investment remains positively and significantly associated even if the exclusion restriction does not hold precisely.

Panel B of Table F.2 summarizes the plausibly exogenous tests for the VC deals. First, we see that adding the Imbens and Rubin assumption of symmetric uncertainty does not materially change any of the inferences for deal volume, late round deals, or deal amount. Put another way, even with larger standard errors, all estimates remain statistically significant. Second, when we add meaningful violations of the exclusion restriction (ranging from 3% to 21% of the magnitude of the estimate on common VC investment), we observe no change in the statistical significance and no change in the materiality of deal volume or deal amount. The economic magnitude of late stage deal volume is no longer positive with a 21% violation of the exclusion restriction.

Panel B of Table F.3 summarizes the plausibly exogenous tests for startup exits. We see that adding the Imbens and Rubin uncertainty does not materially change the inferences for either IPOs or failures, but it does eliminate statistical significance for sales. While the precision of the estimate is smaller for sales, the economic magnitude of the point estimate remains the same.

Thus, these analyses lead us to conclude that while there may be minor violations of the exclusion restriction, the IV approach provides a reasonable way for identifying the effect of common VC investment on each of the major outcomes: directorships, deals, and startup exits.

Table F.1.

Assessing the Exclusion Restriction Assumption for Directorships

This table summarizes tests related to potential violations of the exclusion restriction assumption. Panel A examines the direct effect of the IV on directorship outcomes for the candidate zero-first-stage and remaining group. The zero-first-stage is defined as startups in regulated industries and do not indicate additional subindustries. Panel B reports the IV and plausibly exogenous IV estimates. The plausibly exogenous IV is estimated using the procedure in Conley et al. (2012) and the variance for the prior distribution is the Imbens and Rubin uncertainty based on the procedure in Kippersluis and Rietveld (2018). Common ownership is an indicator for having a common owner. Controls and fixed effects are noted in the bottom row. Robust standard errors clustered by startup are reported below the coefficient estimates. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A. The effect of COW on directorships	VC directorships (1)	Additional directorships (2)	Same-industry directorships (3)
Zero-first stage group #1	-0.022	0.835	-0.072
	(0.084)	(0.674)	(0.046)
Observations	2,211	2,211	2,211
Remaining group	0.119***	0.122***	0.050***
	(0.031)	(0.050)	(0.019)
Observations	139,963	139,963	139,963
Panel B. The effect of common VC investment on directorships	VC directorships (1)	Additional directorships (2)	Same-industry directorships (3)
IV	1.203***	1.345***	0.502***
	(0.314)	(0.510)	(0.192)
Plausibly Exogenous IV, $\zeta \sim(0.00, \Omega_\zeta)$	1.159***	1.539*	0.575***
	(0.307)	(0.920)	(0.190)
Plausibly Exogenous IV, $\zeta \sim(0.05, \Omega_\zeta)$	0.698**	0.838	-0.004
	(0.334)	(0.996)	(0.202)
Additional startup and VC controls	Yes	Yes	Yes
Startup fixed effects	Yes	Yes	Yes
VC fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	142,174	142,174	142,174

Table F.2.

Assessing the Exclusion Restriction Assumption for Deal Outcomes

This table summarizes tests related to potential violations of the exclusion restriction assumption. Panel A examines the direct effect of the IV on venture capital (VC) deals for the zero-first-stage and remaining group. The zero-first-stage is defined as those startups in regulated industries and do not indicate additional subindustries.. Panel B reports the IV and plausibly exogenous IV estimates. The plausibly exogenous IV is estimated using the procedure in Conley et al. (2012) and the variance for the prior distribution is the Imbens and Rubin uncertainty based on the procedure in Kippersluis and Rietveld (2018). Common ownership is an indicator for having a common owner. Controls and fixed effects are noted in the bottom rows. Standard errors clustered by startup are reported below the coefficient estimates. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Deal count	Late stage	Deal value
Panel A. The effect of COW on deal outcomes	(1)	(2)	(3)
Zero-first stage group #1	0.032	0.023	0.041
	(0.046)	(0.023)	(0.099)
Observations	2,211	2,211	2,211
Remaining group	0.114***	0.024***	0.198***
	(0.008)	(0.005)	(0.017)
Observations	139,963	139,963	139,963
Panel B. The effect of common VC investment on deal outcomes	(1)	(2)	(3)
IV	1.143***	0.241***	1.987***
	(0.102)	(0.046)	(0.201)
Plausibly Exogenous IV, $\zeta \sim(0.00, \Omega_\zeta)$	1.143****	0.241***	1.987***
	(0.018)	(0.055)	(0.238)
Plausibly Exogenous IV, $\zeta \sim(0.05, \Omega_\zeta)$	0.637***	-0.265***	1.480***
	(0.118)	(0.055)	(0.238)
Additional startup and VC controls	Yes	Yes	Yes
Startup fixed effects	Yes	Yes	Yes
VC fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	142,174	142,174	142,174

Table F.3.

Assessing the Exclusion Restriction Assumption for Startup Exits

This table summarizes tests related to potential violations of the exclusion restriction assumption. Panel A examines the direct effect of the IV on startup exits for the zero-first-stage and remaining group. The zero-first-stage is defined as startups that in regulated industries and do not indicate additional subindustries. Panel B reports the IV and plausibly exogenous IV estimates. The plausibly exogenous IV is estimated using Conley et al. (2012) and the prior distribution with Imbens and Rubin uncertainty follows the procedure in Kippersluis and Rietveld (2018). Common VC investment is an indicator for having a common owner. Controls and fixed effects are noted in the bottom rows. Standard errors clustered by startup are reported below the coefficient estimates. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	IPO	Sale	Failure
Panel A. The effect of COW on startup exits	(1)	(2)	(3)
Zero-first stage group #1	0.0029	-0.0165*	0.0062
	(0.0021)	(0.0097)	(0.0137)
Observations	2,197	2,197	2,197
Remaining group	0.0026***	0.0020**	-0.0100***
	(0.0004)	(0.0009)	(0.0012)
Observations	139,172	139,172	139,172
Panel B. The effect of common VC investment on startup exits	(1)	(2)	(3)
IV	0.0378***	0.0258*	-0.1413***
	(0.0062)	(0.0136)	(0.0218)
Plausibly Exogenous IV, $\zeta \sim (0.0000, \Omega_\zeta)$	0.0378***	0.0259	-0.1413***
	(0.0072)	(0.0221)	(0.0328)
Plausibly Exogenous IV, $\zeta \sim (0.0001, \Omega_\zeta)$	0.0363***	0.0245	-0.1430***
	(0.0072)	(0.0221)	(0.0328)
Additional startup and VC controls	Yes	Yes	Yes
VC fixed effects	Yes	Yes	Yes
Industry-by-year fixed effects	Yes	Yes	Yes
Observations	141,371	141,371	141,371

G Additional Difference-in-differences Tests

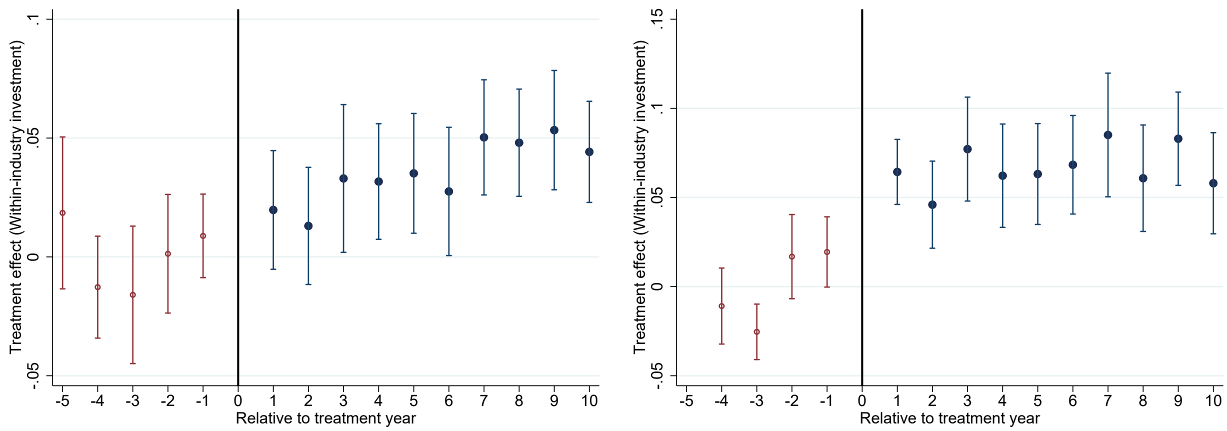


Figure G.1. Dynamic effects of corporate opportunity waiver (COW) legislation on within-industry investment. The figures plot the impact of COW legislation on VC deal volume following the law changes by year relative to the law change. The figures show a window spanning from five years before the law changes to ten years after the changes. Coefficient estimates are normalized relative to the treatment years and inclusive of partially treated years. The straight lines represent 90% confidence intervals, adjusted for incorporation state-level clustering. The coefficients estimated are based on a difference-in-differences specification that includes startup, VC investor, and year fixed effects and includes controls for startup and VC characteristics including total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, the number of funds, total rounds of startup investment, total IPOs of startups invested in, percent of investments in same headquarter state, and percent of investments in same primary industry. A full set of dummy variables for relative years are included in the regression, but only those in the window are plotted. The figure on the left plots the coefficients for the full sample and the figure on the right plots the coefficients for the sample limited to startups incorporated in Delaware, California, Massachusetts, and New York.

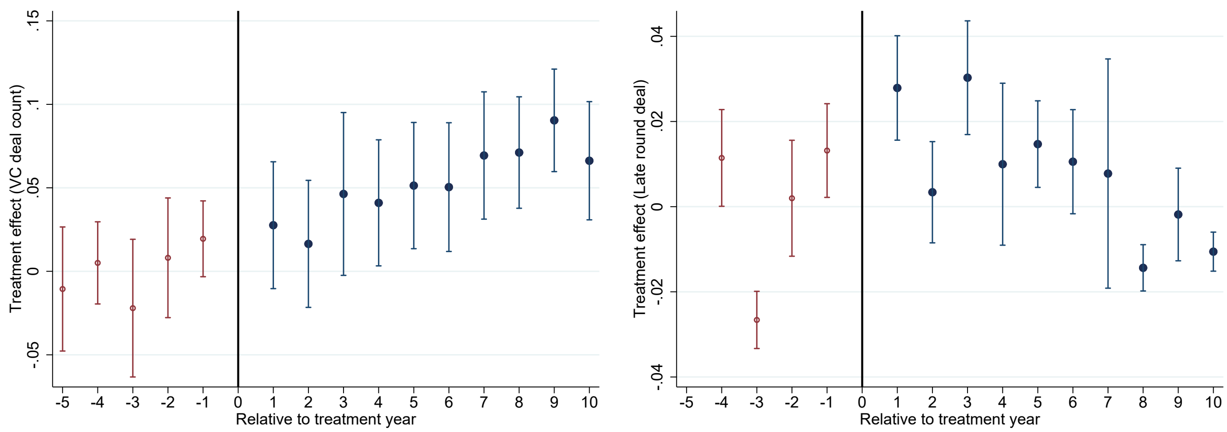


Figure G.2. Dynamic effects of corporate opportunity waiver (COW) legislation on deal volume. The figures plot the impact of COW legislation on VC deal volume following the law changes by year relative to the law change. The figures show a window spanning from five years before the law changes to ten years after the changes. Coefficient estimates are normalized relative to the treatment years and inclusive of partially treated years. The straight lines represent 90% confidence intervals, adjusted for incorporation state-level clustering. The coefficients estimated are based on a difference-in-differences specification that includes startup, VC investor, and year fixed effects and includes controls for startup and VC characteristics including total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, percent of investments in same headquarter state, and percent of investments in same primary industry. A full set of dummy variables for relative years are included in the regression, but only those in the window are plotted. The figure on the left plots the coefficients for the full sample and the figure on the right plots the coefficients for the sample limited to startups incorporated in Delaware, California, Massachusetts, and New York.

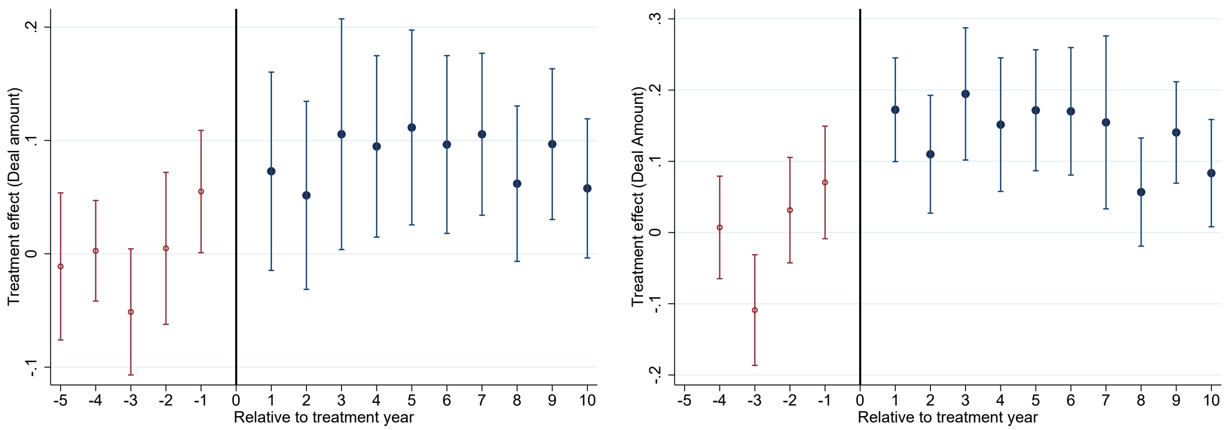


Figure G.3. Dynamic effects of corporate opportunity waiver (COW) legislation on deal amount. The figures plot the impact of COW legislation on VC deal volume following the law changes by year relative to the law change. The figures show a window spanning from five years before the law changes to ten years after the changes. Coefficient estimates are normalized relative to the treatment years and inclusive of partially treated years. The straight lines represent 90% confidence intervals, adjusted for incorporation state-level clustering. The coefficients estimated are based on a difference-in-differences specification that includes startup, VC investor, and year fixed effects and includes controls for startup and VC characteristics including total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, percent of investments in same headquarter state, and percent of investments in same primary industry. A full set of dummy variables for relative years are included in the regression, but only those in the window are plotted. The figure on the left plots the coefficients for the full sample and the figure on the right plots the coefficients for the sample limited to startups incorporated in Delaware, California, Massachusetts, and New York.

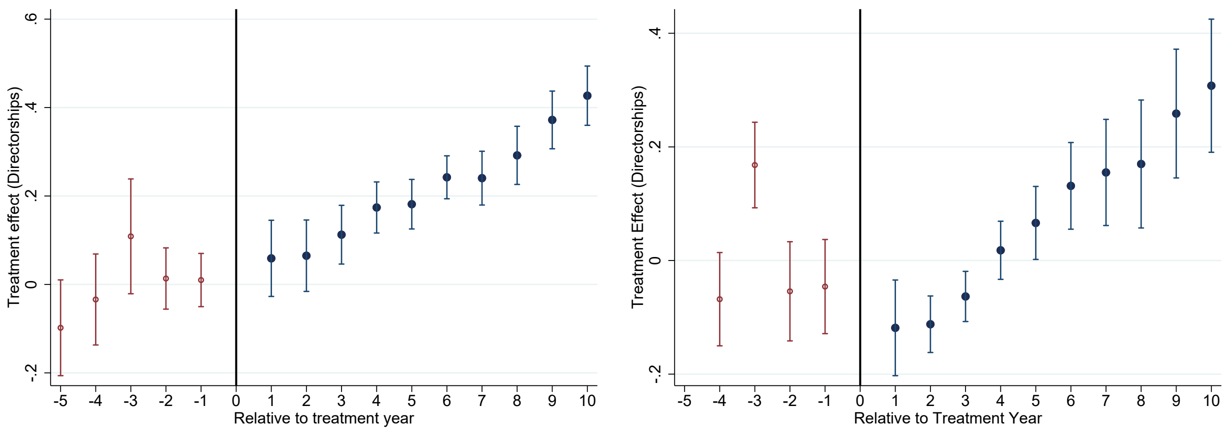


Figure G.4. Dynamic effects of corporate opportunity waiver (COW) legislation on VC directorships. The figures plot the impact of COW legislation on VC directorships following the law changes by year relative to the law change. The figures show a window spanning from five years before the law changes to ten years after the changes. Coefficient estimates are normalized relative to the treatment years and inclusive of partially treated years. The straight lines represent 90% confidence intervals, adjusted for incorporation state-level clustering. The coefficients estimated are based on a difference-in-differences specification that includes startup and year fixed effects and controls for total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry.. A full set of dummy variables for relative years are included in the regression, but only those in the window are plotted. The figure on the left plots the coefficients for the full sample and the figure on the right plots the coefficients for the sample limited to startups incorporated in Delaware, California, Massachusetts, and New York.

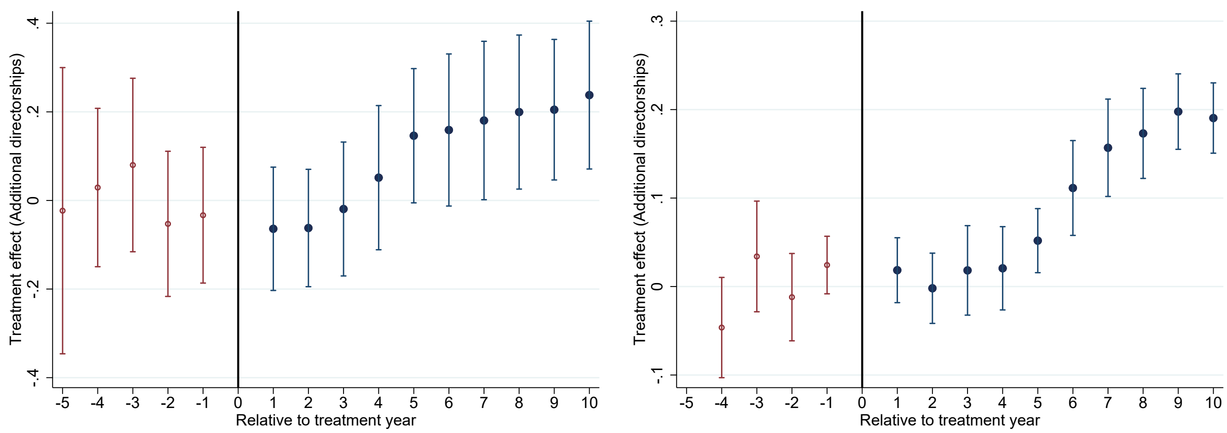


Figure G.5. Dynamic effects of corporate opportunity waiver (COW) legislation on VC directorships. The figures plot the impact of COW legislation on VC directorships following the law changes by year relative to the law change. The figures show a window spanning from five years before the law changes to ten years after the changes. Coefficient estimates are normalized relative to the treatment years and inclusive of partially treated years. The straight lines represent 90% confidence intervals, adjusted for incorporation state-level clustering. The coefficients estimated are based on a difference-in-differences specification that includes startup and year fixed effects and controls for total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry.. A full set of dummy variables for relative years are included in the regression, but only those in the window are plotted. The figure on the left plots the coefficients for the full sample and the figure on the right plots the coefficients for the sample limited to startups incorporated in Delaware, California, Massachusetts, and New York.

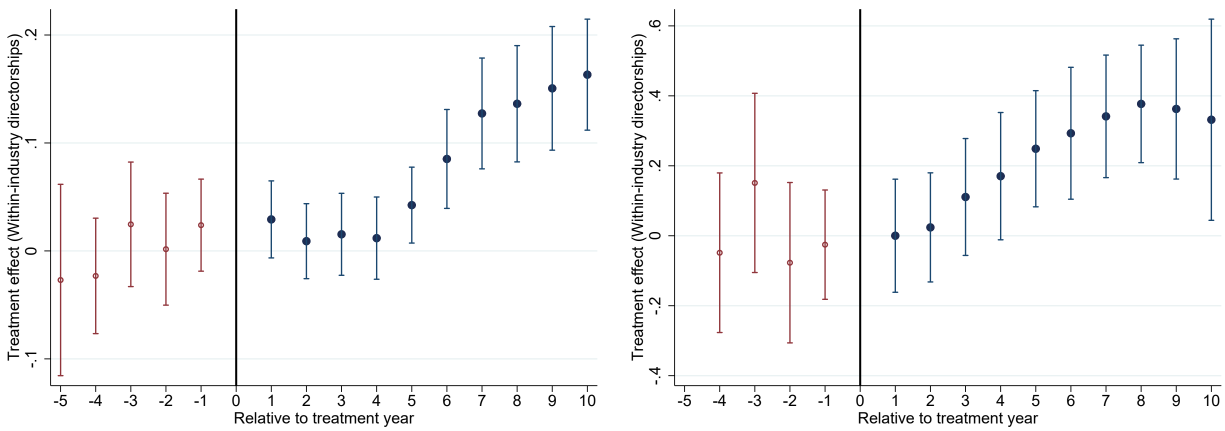


Figure G.6. Dynamic effects of corporate opportunity waiver (COW) legislation on within-industry VC directorships. The figures plot the impact of COW legislation on same-industry VC directorships following the law changes by year relative to the law change. The figures show a window spanning from five years before the law changes to ten years after the changes. Coefficient estimates are normalized relative to the treatment years and inclusive of partially treated years. The straight lines represent 90% confidence intervals, adjusted for incorporation state-level clustering. The coefficients estimated are based on a difference-in-differences specification that includes startup and year fixed effects and includes controls for total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. A full set of dummy variables for relative years are included in the regression, but only those in the window are plotted. The figure on the left plots the coefficients for the full sample and the figure on the right plots the coefficients for the sample limited to startups incorporated in Delaware, California, Massachusetts, and New York.

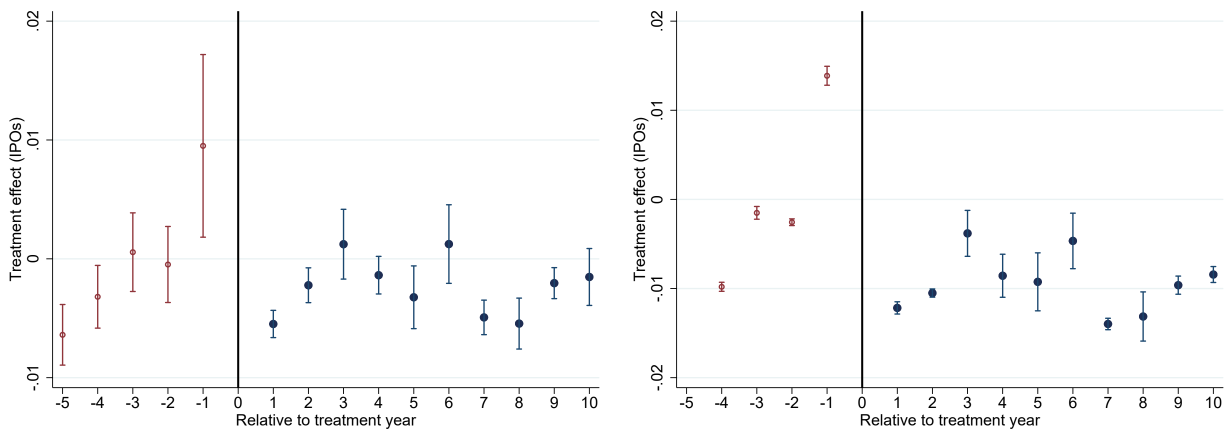


Figure G.7. Dynamic effects of corporate opportunity waiver (COW) legislation on startup IPOs. The figures plot the impact of COW legislation on same-industry VC directorships following the law changes by year relative to the law change. The figures show a window spanning from five years before the law changes to ten years after the changes. Coefficient estimates are normalized relative to the treatment years and inclusive of partially treated years. The straight lines represent 90% confidence intervals, adjusted for incorporation state-level clustering. The coefficients estimated are based on a difference-in-differences specification that includes startup and year fixed effects and includes controls for total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. A full set of dummy variables for relative years are included in the regression, but only those in the window are plotted. The figure on the left plots the coefficients for the full sample and the figure on the right plots the coefficients for the sample limited to startups incorporated in Delaware, California, Massachusetts, and New York.

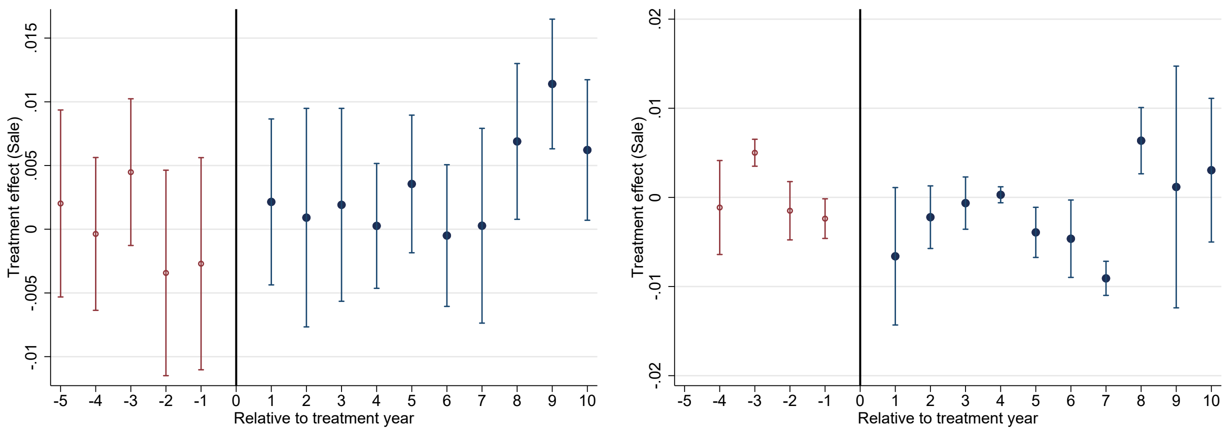


Figure G.8. Dynamic effects of corporate opportunity waiver (COW) legislation on sales. The figures plot the impact of COW legislation on same-industry VC directorships following the law changes by year relative to the law change. The figures show a window spanning from five years before the law changes to ten years after the changes. Coefficient estimates are normalized relative to the treatment years and inclusive of partially treated years. The straight lines represent 90% confidence intervals, adjusted for incorporation state-level clustering. The coefficients estimated are based on a difference-in-differences specification that includes startup and year fixed effects and includes controls for total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. A full set of dummy variables for relative years are included in the regression, but only those in the window are plotted. The figure on the left plots the coefficients for the full sample and the figure on the right plots the coefficients for the sample limited to startups incorporated in Delaware, California, Massachusetts, and New York.

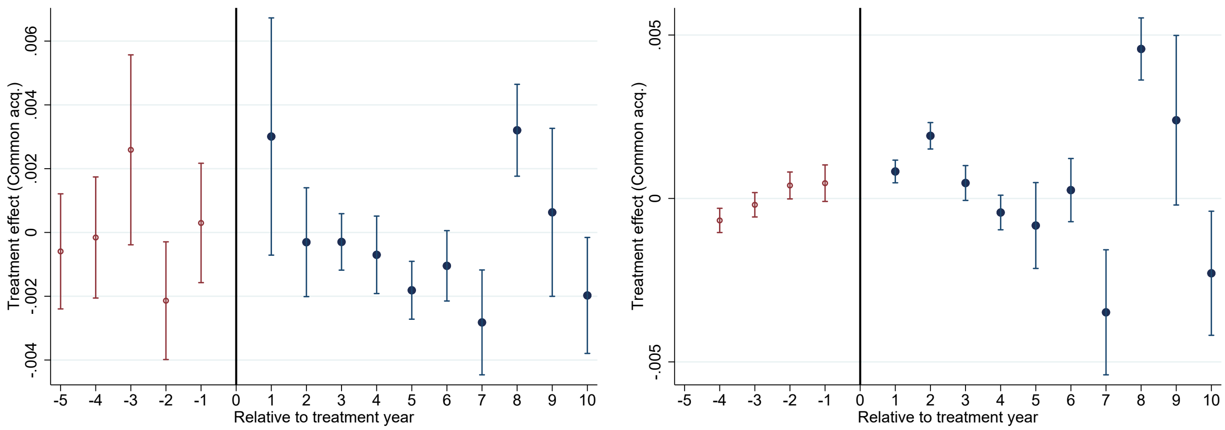
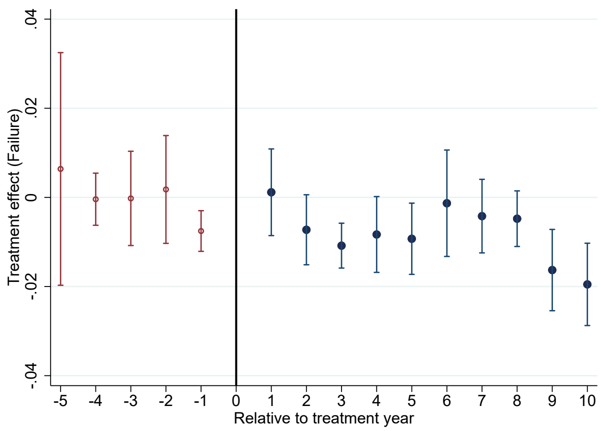


Figure G.9. Dynamic effects of corporate opportunity waiver (COW) legislation on startup being aquired by a firm with a common VC investor. The figures plot the impact of COW legislation on same-industry VC directorships following the law changes by year relative to the law change. The figures show a window spanning from five years before the law changes to ten years after the changes. Coefficient estimates are normalized relative to the treatment years and inclusive of partially treated years. The straight lines represent 90% confidence intervals, adjusted for incorporation state-level clustering. The coefficients estimated are based on a difference-in-differences specification that includes startup and year fixed effects and includes controls for total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. A full set of dummy variables for relative years are included in the regression, but only those in the window are plotted. The figure on the left plots the coefficients for the full sample and the figure on the right plots the coefficients for the sample limited to startups incorporated in Delaware, California, Massachusetts, and New York.



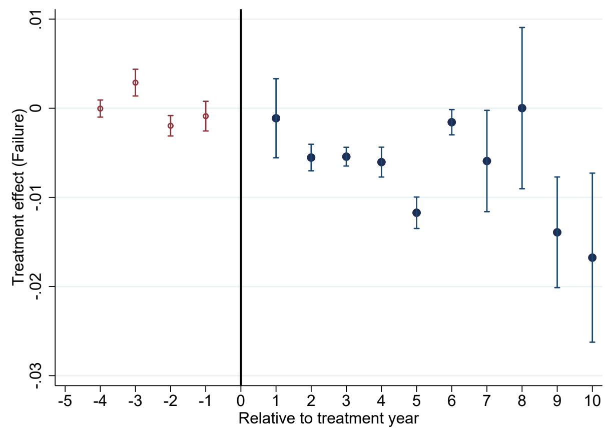


Figure G.10. Dynamic effects of corporate opportunity waiver (COW) legislation on startup failure. The figures plot the impact of COW legislation on same-industry VC directorships following the law changes by year relative to the law change. The figures show a window spanning from five years before the law changes to ten years after the changes. Coefficient estimates are normalized relative to the treatment years and inclusive of partially treated years. The straight lines represent 90% confidence intervals, adjusted for incorporation state-level clustering. The coefficients estimated are based on a difference-in-differences specification that includes startup and year fixed effects and includes controls for total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. A full set of dummy variables for relative years are included in the regression, but only those in the window are plotted. The figure on the left plots the coefficients for the full sample and the figure on the right plots the coefficients for the sample limited to startups incorporated in Delaware, California, Massachusetts, and New York.

Table G.1.

Reduced Form: Within-industry Investment by Venture Capital (VC) Investors

This table presents the results from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). In Panel A, the dependent variable is an indicator variable for whether a startup receives financing from a VC with a previous investment in the same industry. In Panel B and C, this common VC investment is broken down into extensive margin (Panel B) and invensive margin (Panel C). Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. For the dependent variables, industry is defined based on Preqin's primary industry classification. For the industry fixed effects, the adjusted primary industry classification, which is coarser than the primary industry classification, is used. Below the coefficient estimates are robust standard errors clustered by state of incorporation. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: Within-industry investment	(1)	(2)	(3)	(4)	(5)	(6)
Treat	0.016*					
	(0.009)					
Treat × Post	0.055***	0.089***	0.061***	0.075***	0.071***	0.076***
	(0.010)	(0.023)	(0.023)	(0.014)	(0.013)	(0.013)
Treat × Post after five years			0.026***	0.021***	0.021***	0.023***
			(0.008)	(0.005)	(0.006)	(0.006)
Adjusted R^2	3.3%	10.9%	15.8%	15.8%	16.1%	15.6%
Panel B: Extensive margin						
Treat	0.006*					
	(0.003)					
Treat × Post	0.015***	0.025***	0.018***	0.023***	0.021***	0.023***
	(0.003)	(0.006)	(0.006)	(0.003)	(0.003)	(0.003)
Treat × Post after five years			0.005*	0.003*	0.005**	0.004*
			(0.003)	(0.002)	(0.002)	(0.002)
Adjusted R^2	1.6%	5.2%	6.8%	6.6%	6.7%	6.0%
Panel C: Intensive margin						
Treat	0.010					
	(0.007)					
Treat × Post	0.042***	0.067***	0.044**	0.053***	0.051***	0.055***
	(0.008)	(0.018)	(0.018)	(0.011)	(0.011)	(0.011)
Treat × Post after five years			0.022***	0.019***	0.018***	0.020***
			(0.006)	(0.004)	(0.004)	(0.005)
Adjusted R^2	3.1%	10.2%	13.4%	13.3%	13.6%	13.0%
Additional variables	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	No	No	No
Startup fixed effects	No	Yes	Yes	Yes	Yes	yes
VC investor fixed effects	No	No	Yes	Yes	Yes	Yes
HQ-state-by-year fixed effects	No	No	No	Yes	Yes	No
Industry-by-year fixed effects	No	No	No	No	Yes	No
HQ-state-by-industry-by-year fixed effects	No	No	No	No	No	Yes
Number of observations	142,174	142,174	142,174	141,296	141,296	139,559
Number of unique startups	14,991	14,991	14,991	14,896	14,896	14,794

Table G.2.

Reduced Form: Directorships

This table presents the results from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). In Panel A, the dependent variable is venture capital (VC) directorships, defined as the total number of directorships held by VC fund leaders in the startup. In Panel B, the dependent variable is the average number of other directorships that are held by VC fund leaders. In Panel C, the dependent variable is the average number of within-industry directorships held by VC fund leaders. For the dependent variables, industry is defined based on Preqin's primary industry classification. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. For the industry fixed effects, the adjusted primary industry classification, which is coarser than the primary industry classification, is used. Below the coefficient estimates are robust standard errors clustered by state of incorporation and adjusted for small clusters. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: VC directors	(1)	(2)	(3)	(4)	(5)	(6)
Treat	0.200^{***}					
	(0.051)					
Treat × Post	0.130^{***}	0.254^{***}	0.081^{**}	0.050^{*}	0.067^{***}	0.064^{**}
	(0.047)	(0.055)	(0.035)	(0.026)	(0.024)	(0.031)
Treat × Post after five years			0.231^{***}	0.262^{***}	0.239^{***}	0.243^{***}
			(0.061)	(0.044)	(0.044)	(0.046)
Adjusted R^2	39.6%	74.1%	77.3%	77.3%	77.4%	77.6%
Panel B: Additional directorships held						
Treat	0.162^{***}					
	(0.050)					
Treat × Post	0.209^{***}	0.258^{***}	0.089	0.104^{***}	0.078^{**}	0.053
	(0.041)	(0.084)	(0.059)	(0.038)	(0.034)	(0.035)
Treat × Post after five years			0.230^{***}	0.255^{***}	0.249^{***}	0.284^{***}
			(0.046)	(0.051)	(0.050)	(0.045)
Adjusted R^2	28.6%	65.8%	67.3%	67.4%	67.8%	67.5%
Panel C: Within-industry directorships held						
Treat	0.039^{**}					
	(0.016)					
Treat × Post	0.105^{***}	0.094^{***}	0.020^{**}	0.003	-0.014^{*}	-0.019^{*}
	(0.019)	(0.028)	(0.009)	(0.009)	(0.008)	(0.011)
Treat × Post after five years			0.115^{***}	0.142^{***}	0.127^{***}	0.130^{***}
			(0.028)	(0.020)	(0.020)	(0.019)
Adjusted R^2	17.3%	62.8%	64.1%	64.1%	66.8%	66.5%
Additional variables	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	No	No	No
Startup fixed effects	No	Yes	Yes	Yes	Yes	yes
VC investor fixed effects	No	No	Yes	Yes	Yes	Yes
HQ-state-by-year fixed effects	No	No	No	Yes	Yes	No
Industry-by-year fixed effects	No	No	No	No	Yes	No
HQ-state-by-industry-by-year fixed effects	No	No	No	No	No	Yes
Number of observations	100,894	100,894	100,894	100,310	100,310	98,639
Number of unique startups	10,264	10,264	10,264	10,206	10,206	10,119

Table G.3.

Reduced Form: Venture Capital (VC) Deals

This table presents the results from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). In Panel A, the dependent variable is deal volume, which is defined as any VC equity financing deal a firm receives in a given year. In Panel B, the dependent variable is late-round VC deal volume, which is defined as a round of equity VC financing greater than the seed or first round. In Panel C, the dependent variable is deal value, defined as the natural log of one plus the deal value in millions of 2010 dollars. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Below the coefficient estimates are robust standard errors clustered by state of incorporation. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: VC deal volume	(1)	(2)	(3)	(4)	(5)	(6)
Treat	0.040**					
	(0.016)					
Treat × Post	0.067***	0.137***	0.092***	0.114***	0.109***	0.116***
	(0.014)	(0.033)	(0.034)	(0.020)	(0.021)	(0.021)
Treat × Post after five years			0.045***	0.037***	0.040***	0.042***
			(0.015)	(0.010)	(0.010)	(0.011)
Adjusted R^2	4.1%	12.3%	18.3%	18.3%	18.6%	18.4%
Panel B: Late round deal volume						
Treat	0.013*					
	(0.007)					
Treat × Post	0.025***	0.035***	0.026**	0.034***	0.032***	0.035***
	(0.005)	(0.008)	(0.011)	(0.007)	(0.007)	(0.008)
Treat × Post after five years			-0.005	-0.011*	-0.010	-0.010
			(0.009)	(0.006)	(0.006)	(0.007)
Adjusted R^2	3.0%	6.1%	9.0%	9.0%	9.2%	8.8%
Panel C: Deal value						
Treat	0.079**					
	(0.038)					
Treat × Post	0.121***	0.253***	0.180***	0.222***	0.206***	0.218***
	(0.033)	(0.062)	(0.068)	(0.044)	(0.043)	(0.043)
Treat × Post after five years			0.034	0.018	0.025	0.033
			(0.031)	(0.024)	(0.025)	(0.026)
Adjusted R^2	2.7%	7.5%	14.7%	14.7%	15.2%	14.9%
Additional variables	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	No	No	No
Startup fixed effects	No	Yes	Yes	Yes	Yes	yes
VC investor fixed effects	No	No	Yes	Yes	Yes	Yes
HQ-state-by-year fixed effects	No	No	No	Yes	Yes	No
Industry-by-year fixed effects	No	No	No	No	Yes	No
HQ-state-by-industry-by-year fixed effects	No	No	No	No	No	Yes
Number of observations	142,174	142,174	142,174	141,296	141,296	139,559
Number of unique startups	14,991	14,991	14,991	14,896	14,896	14,794

Table G.4.

Reduced Form: Time Between Venture Capital (VC) Deals

This table presents the results from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). The dependent variable is the time between VC deal rounds, defined in years. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Below the coefficient estimates are robust standard errors clustered by state of incorporation. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Dependent variable = Time between deals (years)			
	(1)	(2)	(3)
Treat	0.065*		
	(0.035)		
Treat × Post	-0.161***	-0.064	-0.026
	(0.033)	(0.070)	(0.076)
Treat × Post after five years			-0.063
			(0.116)
Additional controls	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Startup fixed effects	No	Yes	Yes
VC fixed effects	No	Yes	Yes
Adjusted R^2	2.8%	17.0%	18.3%
Number of observations	28,047	23,164	23,164
Number of unique startups	12,737	7,854	7,854

Table G.5.

Reduced Form: High Entrepreneurship States

This table presents the results from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). For these regressions, the sample is limited to startups incorporated in Delaware, California, Massachusetts, and New York. Below the coefficient estimates are robust standard errors clustered by state of incorporation. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: Within-industry investment by VCs	Within-industry	Extensive margin	Intensive margin
Treat × Post	0.112***	0.028***	0.086**
	(0.018)	(0.004)	(0.015)
Adjusted R^2	15.8%	6.7%	13.3%
Number of observations	123,327	123,327	123,327
Number of unique startups	13,512	13,512	13,512
Includes additional controls, startup, VC, and year fixed effects			

Panel B: VC directorships	VC directors	Other directorships held	Within-industry directorships held
Treat × Post	0.164*	0.367**	0.111***
	(0.056)	(0.082)	(0.014)
Adjusted R^2	77.6%	66.1%	67.0%
Number of director observations	89,703	89,703	89,703
Number of unique startups	9,470	9,470	9,470
Includes additional controls, startup, VC, and year fixed effects			

Panel C: VC deals	Deal volume	Late round	Deal size
Treat × Post	0.169**	0.036***	0.284***
	(0.030)	(0.005)	(0.048)
Adjusted R^2	18.4%	8.9%	14.5%
Number of observations	123,327	123,327	123,327
Number of unique startups	13,512	13,512	13,512
Includes additional controls, startup, VC, and year fixed effects			

Panel D: Exits	IPO	Sale	Failure
Treat × Post	0.004**	0.003*	-0.012**
	(0.001)	(0.001)	(0.003)
Adjusted R^2	2.4%	2.5%	3.5%
Number of observations	123,328	123,328	123,328
Number of unique startups	13,512	13,512	13,512
Includes additional controls, VC, and year fixed effects			

Table G.6.

Reduced Form: Excluding Delaware

This table presents the results from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). For these regressions, the sample is limited to startups not originally incorporated in Delaware. Below the coefficient estimates are robust standard errors clustered by state of incorporation. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: Within-industry investment by VCs	Within-industry	Extensive margin	Intensive margin
Treat × Post	0.087***	0.027***	0.062**
	(0.030)	(0.006)	(0.025)
Adjusted R^2	14.2%	6.2%	11.9%
Number of observations	47,739	47,739	47,739
Number of unique startups	4,109	4,109	4,109
Includes additional controls, startup, VC, and year fixed effects			

Panel B: VC directorships	VC directors	Other directorships held	Within-industry directorships held
Treat × Post	0.333***	0.332***	0.171***
	(0.036)	(0.106)	(0.034)
Adjusted R^2	77.3%	63.4%	58.8%
Number of director observations	31,396	31,396	31,396
Number of unique startups	2,569	2,569	2,569
Includes additional controls, startup, VC, and year fixed effects			

Panel C: VC deals	Deal volume	Late round	Deal size
Treat × Post	0.146***	0.037***	0.276***
	(0.042)	(0.011)	(0.068)
Adjusted R^2	16.8%	9.1%	15.9%
Number of observations	47,739	47,739	47,739
Number of unique startups	4,109	4,109	4,109
Includes additional controls, startup, VC, and year fixed effects			

Panel D: Exits	IPO	Sale	Failure
Treat × Post	0.008***	0.002	0.002
	(0.003)	(0.002)	(0.007)
Adjusted R^2	2.4%	2.9%	3.2%
Number of observations	47,739	47,739	47,739
Number of unique startups	4,109	4,109	4,109
Includes additional controls, VC and year fixed effects			

Table G.7.

Reduced Form: "Spray and Pray" Investment

This table presents the results from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). These regressions exclude startup-year observations associated with the "spray and pray" investment strategy adopted after the advent of cloud computing (i.e., software startups from 2006 to 2018). Below the coefficient estimates are robust standard errors clustered by state of incorporation. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: Within-industry investment by VCs	Within-industry	Extensive margin	Intensive margin
Treat × Post	0.058***	0.020***	0.039***
	(0.017)	(0.005)	(0.013)
Adjusted R^2	25.1%	7.3%	12.5%
Number of observations	114,045	114,045	114,045
Number of unique startups	12,458	12,458	12,458
Includes additional controls, startup, VC, and year fixed effects			

Panel B: VC directorships	VC directors	Other directorships held	Within-industry directorships held
Treat × Post	0.194***	0.149**	0.032
	(0.046)	(0.073)	(0.020)
Adjusted R^2	77.7%	72.3%	66.6%
Number of director observations	79,183	79,183	79,183
Number of unique startups	8,356	8,356	8,356
Includes additional controls, startup, VC, and year fixed effects			

Panel C: VC deals	Deal volume	Late round	Deal size
Treat × Post	0.097***	0.019***	0.168***
	(0.028)	(0.006)	(0.052)
Adjusted R^2	27.8%	9.0%	15.5%
Number of observations	114,045	114,045	114,045
Number of unique startups	12,458	12,458	12,458
Includes additional controls, startup, VC, and year fixed effects			

Panel D: Exits	IPO	Sale	Failure
Treat × Post	0.003***	0.001	-0.010***
	(0.001)	(0.001)	(0.002)
Adjusted R^2	2.4%	2.5%	3.9%
Number of observations	114,045	114,045	114,045
Number of unique startups	12,458	12,458	12,458
Includes additional controls, VC and year fixed effects			

Table G.8.

Reduced Form: Excluding Dotcom Bubble

This table presents the results from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). These regressions exclude startup-year observations associated with the bursting of the dotcom bubble between 1999 and 2001. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Below the coefficient estimates are robust standard errors clustered by state of incorporation. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: Within-industry investment by VCs	Within-industry	Extensive margin	Intensive margin
Treat × Post	0.089***	0.025***	0.067***
	(0.028)	(0.007)	(0.021)
Adjusted R^2	15.5%	6.4%	13.1%
Number of observations	132,481	132,481	132,481
Number of unique startups	14,972	14,972	14,972
Includes additional startup controls, startup and year fixed effects			

Panel B: VC directorships	VC directors	Other directorships held	Within-industry directorships held
Treat × Post	0.206***	0.231**	0.090***
	(0.042)	(0.107)	(0.032)
Adjusted R^2	78.0%	67.2%	64.7%
Number of director observations	94,370	94,370	94,370
Number of unique startups	10,253	10,253	10,253
Includes additional startup controls, startup and year fixed effects			

Panel C: VC deals	Deal volume	Late round	Deal size
Treat × Post	0.144***	0.032***	0.260***
	(0.041)	(0.009)	(0.075)
Adjusted R^2	18.0%	9.0%	8.0%
Number of observations	132,481	132,481	132,481
Number of unique startups	14,972	14,972	14,972
Includes additional startup controls, startup and year fixed effects			

Panel D: Exits	IPO	Sale	Failure
Treat × Post	0.003***	0.002*	-0.011***
	(0.001)	(0.001)	(0.002)
Adjusted R^2	2.2%	2.3%	3.5%
Number of observations	132,481	132,481	132,481
Number of unique startups	14,972	14,972	14,972
Includes additional startup controls and year fixed effects			

Table G.9.

Reduced Form: Excluding Financial Crisis

This table presents the results from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). These regressions exclude startup-year observations associated with the financial crisis period between 2007 and 2009. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Below the coefficient estimates are robust standard errors clustered by state of incorporation. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: Within-industry investment by VCs	Within-industry	Extensive margin	Intensive margin
Treat × Post	0.070***	0.018***	0.054***
	(0.019)	(0.005)	(0.015)
Adjusted R^2	16.7%	7.0%	11.2%
Number of observations	120,330	120,330	120,330
Number of unique startups	14,983	14,983	14,983
Includes additional controls, startup, VC investor, and year fixed effects			

Panel B: VC directorships	VC directors	Other directorships held	Within-industry directorships held
Treat × Post	0.181***	0.212***	0.081***
	(0.040)	(0.065)	(0.027)
Adjusted R^2	78.8%	68.5%	65.6%
Number of director observations	85,411	85,411	85,411
Number of unique startups	10,260	10,260	10,260
Includes additional controls, startup, VC investor, and year fixed effects			

Panel C: VC deals	Deal volume	Late round	Deal size
Treat × Post	0.108***	0.024***	0.189***
	(0.028)	(0.007)	(0.053)
Adjusted R^2	19.1%	9.6%	15.4%
Number of observations	120,330	120,330	120,330
Number of unique startups	14,983	14,983	14,983
Includes additional controls, startup, VC investor, and year fixed effects			

Panel D: Exits	IPO	Sale	Failure
Treat × Post	0.003***	0.002*	-0.012***
	(0.001)	(0.001)	(0.002)
Adjusted R^2	2.5%	2.6%	3.7%
Number of observations	120,330	120,330	120,330
Number of unique startups	14,983	14,983	14,983
Includes additional controls, VC investor, and year fixed effects			

H Matching Treated and Control Startups

We evaluate various matching approaches for our sample of startups in high venture capital (VC) states: Delaware, California, Massachusetts, and New York. We focus on these states as we believe they are the best potential pool for finding matches with overlapping characteristics. Table H.1 compares the performance of alternative matching approaches. The metric of interest is an index of the difference in startup and VC investor characteristics X between treated and non-treated startups in the pre-treatment period. We call this metric the composite distance. It summarizes performance across characteristics κ by the average standardized difference.

$$\sum_{k \epsilon \kappa} \frac{1}{K} \frac{\bar{X}_{ctrl,k} - \bar{X}_{treat,k}}{\sigma_k}$$

where $\bar{X}_{ctrl,k}$ is the mean of the characteristics k in the matched control group, and $\bar{X}_{treat,k}$ is the mean of characteristics k in the treated group, and σ_k is the standard deviation of characteristic k among treated startups. To select potential matching algorithms, we use M-to-1 optimal Mahalanobis matching algorithms and propensity score matching algorithms for a vector of startup and VC investor characteristics. These include startup age, capital raised to date, rounds raised to date, industry, and year, as well as the characteristics of previous VC investors in the startup, averaged over the number of VC investors. The VC characteristics include reputation, age, size (assets under management), fund number, total rounds of startup investment, the average number of startups invested in per year, total IPOs of startups invested in, percent of investments in startups in the same headquarter state, and percent of investments in startups in same primary industry.

We consider 1, 5, and 10 potential matches using the optimal Mahalanobis and propensity score techniques to determine overlap. Given that propensity score matching techniques impute the counterfactual potential outcome for each observation by using an average of the outcomes of similar observations that do not receive treatment, both the number of matches and the minimum distance between the matches (i.e., the caliper) can influence the estimate. As such, we also vary the caliper to be as small as 0.025 but no more than 0.01 distance in match overlap and any distance for match overlap.

The results in Table H.1 are in descending order of the matching algorithms performance. Based on the composite distance in column (1), the propensity score matching based on a single match and the caliper of 0.01 performs the best. In general, for both the Mahalanobis and the propensity score matching algorithms, fewer matches perform better than averaging over more matches. For this reason, when evaluating the Mahalnobis and the propensity score matching techniques we added additional iterations to assess the sensitivity of the best match. Specifically, for the 1-to-1 optimal Mahalanobis algorithm, we varied the exact matching criteria across exact match for industry, exact match for year, or an exact match for both industry and year. The best performing Mahalnobis algorithm matches exactly for year but flexibly for the industry. Despite the gains from exact matching for year, it is clear that the propensity score-matching algorithm outperforms the Mahalnobis algorithm.

The composite distance in characteristics underlying treated and control startups using the best Mahalanobis algorithm is almost twice as large as the distance when using the best propensity-score matching algorithm. In terms of statistical inference, in each case the null hypothesis of no change in common VC investment is easily rejected as shown by the t-statistics reported in column (4). The point estimates do fluctuate in terms of economic magnitude depending on the algorithm. For example, the point estimate is 0.068 for the best propensity-score algorithm and 0.053 for the best Mahalnobis algorithm.

Table H.2 shows the results for all of our main outcome variables using our best performing matching algorithm, the propensity score matching algorithm with a single match and 0.01 caliper. For each of the various outcomes, we evaluate in the paper, common VC investment, within-industry investment, VC directorships, and, startup growth, the point estimates remain positive and statistically significant. Using the preferred matching procedure, the economic magnitude is about half that based on the standard difference-in-differences regression. For example, the point estimate for common VC investment in high entrepreneurial states is 0.122 with a standard error of 0.014 as shown in Table 4. Using the preferred propensity-score matching procedure, the point estimate is 0.065 with a standard error of 0.005.

In Table H.3, we run main difference-in-differences regressions based on the matched sample alone. The results are robust to this specification as well.

Table H.1.

Comparison of Matching Results

This table compares the performance of alternative matching approaches when the sample of startups is limited to high venture capital (VC) states, which are defined as Delaware, California, Massachusetts, and New York. Column (1) presents the Euclidean distance of mean characteristics between treatment and control startups (i.e., the sum of mean differences between the treated and relevant control group based on the matching approach, divided by the standard deviation in the treated group). The results are shown in descending order of match performance based on the composite distance. The matching procedure is applied to the following pre-treatment characteristics: startup age, capital raised to date, rounds raised to date, and averages for VC investor characteristics including reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, indicator for same headquarter state, as well as industry and year. For the Mahalanobis distance matching procedure, exact industry and/or year matches are required and a linear function of the remaining characteristics is used to correct for a large-sample bias that exists when matching on more than one continuous variable. Columns (2)–(4) show additional statistics for the given matching approach. Column (2) lists the number of observations given that some observations are excluded when they lack match support. Column (3) shows the coefficient estimate when common ownership is the dependent variable. Column (4) shows the associated test statistics from bias-corrected standard errors that match on the estimated treatment probabilities described in Abadie and Imbens (2011).

Sample limited to DE, CA, MA, and NY	(1) Composite Distance	(2) Number of observations	(3) Common VC investment β	(4) t-statistic
Probit (1 match), 0.010 caliper	0.470	123,014	0.068	12.39
Probit (1 match), 0.025 caliper	0.489	123,156	0.071	13.14
Probit (1 match), 0.050 caliper	0.536	123,326	0.062	13.42
Probit (1 match), no caliper	0.558	123,328	0.062	13.35
Probit (1 match), 0.100 caliper	0.558	123,328	0.062	13.35
Probit (5 matches), 0.025 caliper	0.580	122,985	0.068	15.71
Probit (5 matches), 0.050 caliper	0.597	123,175	0.064	15.00
Probit (5 matches), no caliper	0.631	123,328	0.065	15.90
Probit (5 matches), 0.100 caliper	0.631	123,328	0.065	15.90
Probit (10 matches), 0.025 caliper	0.636	122,413	0.065	16.38
Probit (10 matches), 0.050 caliper	0.653	123,018	0.067	16.63
Probit (10 matches), 0.100 caliper	0.697	123,306	0.067	16.63
Probit (10 matches), no caliper	0.702	123,328	0.066	16.55
Mahalanobis (1 match), exact for year	0.944	114,792	0.053	11.94
Mahalanobis (1 match), exact for industry	1.260	123,163	0.022	6.00
Mahalanobis (1 match), exact for industry and year	1.404	110,724	0.041	8.71
Mahalanobis (5 match), exact for industry and year	2.476	108,118	0.048	10.61
Mahalanobis (10 match), exact for industry and year	3.077	100,523	0.054	11.80

Table H.2.

Propensity Score Matching Matching

This table presents the results from using our preferred matching procedure for the sample of startups limited to high venture capital (VC) states, which are defined as Delaware, California, Massachusetts, and New York. Based on the composite distance measure, our preferred matching procedure is the 1-to-1 optimal Mahalanobis matching based on the pre-treatment startup characteristics of startup age, capital raised to date, and rounds raised to date as well as exact match for industry and year. Below the coefficient estimates are test statistics from bias-corrected standard errors that match on the estimated treatment probabilities described in Abadie and Imbens (2011). ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: Common VC investment	Any common VC investment	Total common VC investment	
Treat × Post	0.065***	0.152***	
	(0.005)	(0.012)	
Number of observations	123,012	123,012	

Panel B: Within-industry investment by VCs	Within-industry	Extensive margin	Intensive margin
Treat × Post	0.052***	0.014***	0.039***
	(0.006)	(0.004)	(0.004)
Number of observations	123,012	123,012	123,012

Panel C: VC directorships	VC directors	Other directorships held	Within-industry directorships held
Treat × Post	0.208***	0.129***	0.222***
	(0.024)	(0.019)	(0.046)
Number of director observations	89,263	89,263	89,263

Panel D: VC deals	Deal volume	Late round	Deal size
Treat × Post	0.066***	0.027***	0.111***
	(0.007)	(0.004)	(0.013)
Number of observations	123,012	123,012	123,012

Table H.3.

Robustness Test: Difference-in-Differences with Matched Sample

This table presents the results from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). These regressions use our preferred matching procedure sample. First, the sample is limited to high venture capital (VC) states, which are defined as Delaware, California, Massachusetts, and New York. Then, the sample is limited to the propensity score matched sample based on the pre-treatment startup and VC investor characteristics. Below the coefficient estimates are test statistics from robust standard errors clustered by state of incorporation. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: Common VC investment	Any common VC investment	Total common VC investment	
Treat × Post	0.121***	0.194***	
	(0.015)	(0.037)	
Adjusted R^2	69.0%	81.6%	
Number of observations	123,012	123,012	
Panel B: Within-industry investment by VCs	Within-industry	Extensive margin	Intensive margin
Treat × Post	0.113***	0.028***	0.087***
	(0.019)	(0.004)	(0.008)
Adjusted R^2	15.7%	6.7%	13.3%
Number of observations	123,012	123,012	123,012
Panel C: VC directorships	VC directors	Other directorships held	Within-industry directorships held
Treat × Post	0.153**	0.396***	0.116***
	(0.057)	(0.094)	(0.014)
Adjusted R^2	77.6%	67.7%	64.4%
Number of observations	89,262	89,262	89,262
Panel D: VC deals	Deal volume	Late round	Deal size
Treat × Post	0.171***	0.037***	0.288***
	(0.032)	(0.006)	(0.052)
Adjusted R^2	18.3%	8.9%	14.4%
Number of observations	123,012	123,012	123,012
Additional startup controls	Yes	Yes	Yes
Startup fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes

I Life Cycle Tests

In this appendix, we examine whether the influence of common VC investors varies over the lifecycle stage of the startup. As discussed above, our sample mostly consists of firms that raised at least $10 million in VC funding. Although we supplement this sample with some firms that raised a smaller sum (see Section 3.1), it is still possible that our main results are driven by large startups that raised large amounts of VC late-stage funding, and that common VC investment is detrimental to smaller or early-stage startups. In particular, there is a concern that VC investors' strategy is to support a few different startups as a seed or angel investor and dispense of the others once one is successful.

To address this concern, we first examine a few perturbations around the threshold. Specifically, we first drop the startups below the $10 million threshold included in the sample (i.e., Series A low dollar rounds, Massachusetts low dollar startups, and certain industries with low dollar startups). Second, we drop the startups below a $15 million threshold, and third, we raise it to a $25 million threshold. If the startups that raise smaller amounts of VC investment are less likely to raise financing and more likely to fail, we would expect the results at higher thresholds to be stronger.

Table I.1, Table I.2, and Table I.3 provide comparisons across the sample inclusion thresholds. In each table, the point estimates are stable across the different thresholds. This suggests that the observations near the threshold are not pivotal to the main inferences of this study. For example, in Panel A of Table I.2, we see no evidence that common VC investors help startups to raise even more rounds at the higher thresholds. Similarly, in Panel D of Table I.3 we see no evidence that the magnitude of the startup failure coefficient moves as the threshold increases to $25 million. This suggests that the influence of common VC investors is similar across the life of the startup rather than only at certain cut-off points early in the lifecycle.

In Table I.4, Table I.5, and Table I.6, we evaluate more directly whether late-stage VC rounds are driving the main results. We exclude startups in the upper quartile (8 years), quantile (10 years), and decile (15 years) of the age distribution and re-estimate each of our main outcomes: board membership, deal outcomes, and startup exits. The results are similar to those in the main analysis, except with respect to sales. The differences in economic magnitude from the main results are small and in many cases within the standard error of the original estimate. In conclusion, this set of analyses is consistent with startup lifecycle considerations not being a primary determinant of the impact of common VC investment on startup performance.

Table I.1.

Threshold Tests: Common VC Investments and the Board of Directors

This table presents a robustness check for director appointments by comparing our instrumental variable (IV) regression estimates from excluding startups with less than \$10, \$15, and \$25 million in VC investments. In each panel, the key explanatory variable is common VC investment and the instrument is an indicator for if a startup is incorporated in a state that permits corporate opportunity waivers (COWs). In columns (1) through (3), any common VC investment is an indicator variable for whether a startup has a VC investor that commonly owns another startup within the same industry, and in columns (4) through (6) total common VC investment is defined as the natural log of the total number of common VC investments. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Industry for common ownership is defined based on Preqin's primary and sub industry classification. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. The first-stage F-statistic is the Kleibergen-Paap Wald statistic. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively..

Panel A: VC director	(1)	(2)	(3)	(4)	(5)	(6)
Any common VC investment	1.687***	1.509***	1.847***			
	(0.370)	(0.469)	(0.553)			
Total common VC investments				1.162***	1.034***	1.149***
				(0.274)	(0.333)	(0.358)
Panel B: VC director with additional directorships						
Any common VC investment	2.000***	2.728***	2.806***			
	(0.600)	(0.725)	(0.805)			
Total common VC investments				1.378***	1.869***	1.745***
				(0.423)	(0.532)	(0.530)
Panel C: VC director with same-industry directorships						
Any common VC investment	0.808***	0.731**	0.590*			
	(0.244)	(0.295)	(0.315)			
Total common VC investments				0.557***	0.501**	0.367*
				(0.170)	(0.204)	(0.194)
First-stage F-statistic	129.6	93.8	79.5	46.3	33.1	32.2
t-statistic on instrument	11.39	9.68	8.91	6.97	5.75	5.68
Exclude startups with less than \$10 million VC investment	Yes	No	No	Yes	No	No
Exclude startups with less than \$15 million VC investment	No	Yes	No	No	Yes	No
Exclude startups with less than \$25 million VC investment	No	No	Yes	No	No	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Startup, VC investor, and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	93,904	67,690	53,454	93,904	67,690	53,454
Number of unique startups	9,439	6,251	4,768	9,439	6,251	4,768

Table I.2.

Threshold Tests: Common Venture Capital (VC) Investments and Deals

This table presents a robustness check for startup growth by comparing our instrumental variable (IV) regression estimates from excluding startups with less than $10, $15, and $25 million in VC investments. In each panel, the key explanatory variable is common VC investment and the instrument is an indicator for if a startup is incorporated in a state that permits corporate opportunity waivers (COWs). In columns (1) through (3), any common VC investment is an indicator variable for whether a startup has a VC investor that commonly owns another startup within the same industry, and in columns (4) through (6) total common VC investment is defined as the natural log of the total number of common VC investments. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Industry for common ownership is defined based on Preqin's primary and sub industry classification. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. The first-stage F-statistic is the Kleibergen-Paap Wald statistic. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively..

Panel A: VC deal count	(1)	(2)	(3)	(4)	(5)	(6)
Any common VC investment	1.111***	1.021***	0.960***			
	(0.106)	(0.123)	(0.130)			
Total common VC investments				0.722***	0.659***	0.551***
				(0.093)	(0.106)	(0.093)
Panel B. Late stage deal count						
Any common VC investment	0.262***	0.311***	0.319***			
	(0.051)	(0.069)	(0.079)			
Total common VC investments				0.170***	0.201***	0.183***
				(0.036)	(0.049)	(0.049)
Panel C. Deal value						
Any common VC investment	2.055***	2.167***	2.094***			
	(0.217)	(0.282)	(0.308)			
Total common VC investments				1.336***	1.399***	1.202***
				(0.183)	(0.236)	(0.215)
First-stage F-statistic	141.4	100.9	84.1	63.9	42.3	42.0
t-statistic on instrument	11.89	10.04	9.17	7.99	6.50	6.48
Exclude startups with less than $10 mil. VC investment	Yes	No	No	Yes	No	No
Exclude startups with less than $15 mil. VC investment	No	Yes	No	No	Yes	No
Exclude startups with less than $25 mil. VC investment	No	No	Yes	No	No	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Startup, VC investor, and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	126,871	83,259	63,355	126,871	83,259	63,355
Number of unique startups	13,151	7,796	5,722	13,151	7,796	5,722

Table I.3.

Threshold Tests: Common Venture Capital (VC) Investments and Exits

This table presents a robustness check for startup exits by comparing our instrumental variable (IV) regression estimates from excluding startups with less than \$10, \$15, and \$25 million in VC investments. In each panel, the key explanatory variable is common VC investment and the instrument is an indicator for if a startup is incorporated in a state that permits corporate opportunity waivers (COWs). In columns (1) through (3), any common VC investment is an indicator variable for whether a startup has a VC investor that commonly owns another startup within the same industry, and in columns (4) through (6) total common VC investment is defined as the natural log of the total number of common VC investments. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Industry for common ownership is defined based on Preqin's primary and sub industry classification. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. The first-stage F-statistic is the Kleibergen-Paap Wald statistic. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: IPO	(1)	(2)	(3)	(4)	(5)	(6)
Any common VC investment	0.043***	0.066***	0.076***			
	(0.006)	(0.012)	(0.015)			
Total common VC investments				0.020***	0.030***	0.032***
				(0.003)	(0.005)	(0.006)
Panel B. Sale						
Any common VC investment	0.013	0.026	-0.004			
	(0.013)	(0.018)	(0.021)			
Total common VC investments				0.006	0.012	-0.002
				(0.006)	(0.008)	(0.009)
Panel C. Failure						
Any common VC investment	-0.125***	-0.131***	-0.131***			
	(0.020)	(0.028)	(0.031)			
Total common VC investments				-0.059***	-0.059***	-0.054***
				(0.009)	(0.013)	(0.013)
First-stage F-statistic	149.2	77.6	58.9	121.7	59.6	49.1
t-statistic on instrument	12.21	8.81	7.67	11.03	7.72	7.01
Exclude startups with less than \$10 mil. VC investment	Yes	No	No	Yes	No	No
Exclude startups with less than \$15 mil. VC investment	No	Yes	No	No	Yes	No
Exclude startups with less than \$25 mil. VC investment	No	No	Yes	No	No	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
VC investor and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	126,871	83,259	63,355	126,871	83,259	63,355
Number of unique startups	13,151	7,796	5,722	13,151	7,796	5,722

Table I.4.

Lifecycle Tests: Common VC Investments and the Board of Directors

This table presents a robustness check for director appointments by examining instrumental variable (IV) regression estimates that exclude startups based on age. In each panel, the key explanatory variable is common VC investment and the instrument is an indicator for if a startup is incorporated in a state that permits corporate opportunity waivers (COWs). In columns (1) through (3), any common VC investment is an indicator variable for whether a startup has a VC investor that commonly owns another startup within the same industry, and in columns (4) through (6) total common VC investment is defined as the natural log of the total number of common VC investments. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Industry for common ownership is defined based on Preqin's primary and sub industry classification. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. The first-stage F-statistic is the Kleibergen-Paap Wald statistic. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: VC director	(1)	(2)	(3)	(4)	(5)	(6)
Any common VC investment	1.291***	1.761***	2.144***			
	(0.450)	(0.444)	(0.435)			
Total common VC investments				1.734**	1.743***	1.684***
				(0.772)	(0.522)	(0.391)
Panel B: VC director with additional directorships						
Any common VC investment	-0.079	0.722	1.482**			
	(0.796)	(0.674)	(0.619)			
Total common VC investments				-0.106	0.714	1.164**
				(1.071)	(0.675)	(0.502)
Panel C: VC director with same-industry directorships						
Any common VC investment	-0.261	0.108	0.536**			
	(0.329)	(0.281)	(0.265)			
Total common VC investments				-0.351	0.107	0.421**
				(0.468)	(0.276)	(0.207)
First-stage F-statistic	95.3	130.7	184.4	8.3	18.6	32.6
t-statistic on instrument	7.27	8.54	9.67	2.88	4.31	5.71
Exclude startups older than 8 years	Yes	No	No	Yes	No	No
Exclude startups older than 10 years	No	Yes	No	No	Yes	No
Exclude startups older than 15 years	No	No	Yes	No	No	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Startup, VC investor, and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	68,644	78,512	91,824	68,644	78,512	91,824
Number of unique startups	10,017	10,060	10,152	10,017	10,060	10,152

Table I.5.

Lifecycle Tests: Common Venture Capital (VC) Investments and Deals

This table presents a robustness check for startup growth by examining instrumental variable (IV) regression estimates that exclude startups based on age. In each panel, the key explanatory variable is common VC investment and the instrument is an indicator for if a startup is incorporated in a state that permits corporate opportunity waivers (COWs). In columns (1) through (3), any common VC investment is an indicator variable for whether a startup has a VC investor that commonly owns another startup within the same industry, and in columns (4) through (6) total common VC investment is defined as the natural log of the total number of common VC investments. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Industry for common ownership is defined based on Preqin's primary and sub industry classification. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. The first-stage F-statistic is the Kleibergen-Paap Wald statistic. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: VC deal count	(1)	(2)	(3)	(4)	(5)	(6)
Any common VC investment	1.422***	1.337***	1.172***			
	(0.226)	(0.178)	(0.132)			
Total common VC investments				2.039***	1.307***	0.894***
				(0.742)	(0.295)	(0.145)
Panel B: Early stage deal count	(1)	(2)	(3)	(4)	(5)	(6)
Any common VC investment	0.955***	0.812***	0.679***			
	(0.154)	(0.114)	(0.084)			
Total common VC investments				1.283***	0.804***	0.534***
				(0.458)	(0.196)	(0.099)
Panel C. Late stage deal count						
Any common VC investment	0.206**	0.306***	0.278***			
	(0.101)	(0.083)	(0.062)			
Total common VC investments				0.296*	0.299***	0.212***
				(0.170)	(0.096)	(0.052)
Panel D. Deal value						
Any common VC investment	2.120***	2.219***	2.106***			
	(0.406)	(0.336)	(0.266)			
Total common VC investments				3.040***	2.170***	1.607***
				(1.133)	(0.504)	(0.272)
First-stage F-statistic	51.4	74.2	102.4	7.5	20.5	40.6
t-statistic on instrument	7.17	8.61	10.12	2.74	4.83	6.37
Exclude startups older than 8 years	Yes	No	No	Yes	No	No
Exclude startups older than 10 years	No	Yes	No	No	Yes	No
Exclude startups older than 15 years	No	No	Yes	No	No	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Startup, VC investor, and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	98,381	111,586	129,126	98,381	111,586	129,126
Number of unique startups	14,592	14,658	14,810	14,592	14,658	14,810

Table I.6.

Lifecycle Tests: Common Venture Capital (VC) Investments and Exits

This table presents a robustness check for startup exits by examining instrumental variable (IV) regression estimates that exclude startups based on age. In each panel, the key explanatory variable is common VC investment and the instrument is an indicator for if a startup is incorporated in a state that permits corporate opportunity waivers (COWs). In columns (1) through (3), any common VC investment is an indicator variable for whether a startup has a VC investor that commonly owns another startup within the same industry, and in columns (4) through (6) total common VC investment is defined as the natural log of the total number of common VC investments. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Industry for common ownership is defined based on Preqin's primary and sub industry classification. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. The first-stage F-statistic is the Kleibergen-Paap Wald statistic. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: IPO	(1)	(2)	(3)	(4)	(5)	(6)
Any common VC investment	0.020***	0.030***	0.037***			
	(0.006)	(0.006)	(0.006)			
Total common VC investments				0.010***	0.014***	0.017***
				(0.003)	(0.003)	(0.003)
Panel B. Sale						
Any common VC investment	-0.000	0.000	0.012			
	(0.017)	(0.016)	(0.015)			
Total common VC investments				-0.000	0.000	0.006
				(0.009)	(0.008)	(0.007)
Panel C. Failure						
Any common VC investment	-0.105***	-0.115***	-0.150***			
	(0.020)	(0.022)	(0.023)			
Total common VC investments				-0.053***	-0.055***	-0.069***
				(0.010)	(0.010)	(0.010)
First-stage F-statistic	104.1	105.9	120.8	92.3	97.4	118.0
t-statistic on instrument	10.20	10.29	10.99	9.61	9.87	10.86
Exclude startups older than 8 years	Yes	No	No	Yes	No	No
Exclude startups older than 10 years	No	Yes	No	No	Yes	No
Exclude startups older than 15 years	No	No	Yes	No	No	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Startup, VC investor, and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	98,416	111,621	129,143	98,416	111,621	129,143
Number of unique startups	14,627	14,693	14,827	14,627	14,693	14,827

J Industry Analysis

1. Industry Specialization

One concern with the results is that they are driven by VCs developing industry specialization over time. That is, following the adoption of the laws, VCs chose to concentrate on specific industries. With greater industry specialization, VC investors may be able to make better investment decisions. Thus, startup growth may be driven by industry specialization rather than common ownership. In Table J.1, we depict the average industries VCs invested in over time. We find that the average number of industries that VCs invest in has increased over the years, suggesting that VC specialization has decreased. Thus, it is unlikely that trends in VC specialization are driving our results.

2. Industries with High versus Low Intellectual Property Protection

Our results show that common VC investment is associated with startup growth and exits, but this may not be the case across all industries (Cunningham et al., 2021; Li et al., 2023). To the extent that there is a nuanced relationship between common VC investment and startup growth across industries, our estimates reflect a weighted combination of these relationships.

To evaluate the potential for different patterns across industries, we split our analyses into subsamples of industries with strong and weak IP protection. For example, industries such as pharmaceuticals have strong IP protection. Given that IP may be used strategically to protect market share and discourage follow-on innovations (Abrams et al., 2020), a common VC investor would arguably have relatively higher incentives to pursue a winner-take-all approach as opposed to a synergistic approach for maximizing returns across commonly held startups. To test this hypothesis, we use the industrial classification of IP protection provided in Hall et al. (2014). We then match their industry names to the Preqin industry names.

Specifically, Table 3 in Hall et al. (2014) summarizes 8 research papers examining the choice of IP protection methods across industrial sectors and identifies those sectors that place a high and low emphasis on IP protection as well as those sectors that place a high and low emphasis on trade secrets. For example, pharmaceuticals, biotech, medical equipment, petroleum and chemicals are identified as the industrial sectors that emphasize IP protection. We match this to 16 Preqin industries, which include biomedical, biotechnology, chemicals, clean technology, energy, healthcare, high-tech, life sciences, medical devices, medical instruments, medical technologies, nanotechnology, oil and gas, pharmaceuticals, predictive medicine and renewable energy. Similarly, Table 3 in Hall et al. (2014) identifies construction, electronics, food, publishing, forestry, and software as the industries focused on trade secrets. We again match these industries to Preqin. For any Preqin industry that does not closely match an industry described as either high IP protection or high trade secret, we code it as neither high nor low IP protection.

Table J.2 shows evidence consistent with stronger IP protection supporting a winner-take-all approach to portfolio maximization by common VC investors. In strong IP industries, the likelihood of undergoing an IPO is 2.8 times that of weak IP industries. Further supporting this finding, we see relatively fewer investment rounds for strong IP industries, especially on the extensive margin. This suggests that VC investors in industries with high IP protection may end additional investments once a favorite startup has been identified. Finally, we find little evidence that the weaker of two startups may be killed off in some industries with strong IP protection so that the favorite may succeed. Rather, across the board we see that common VC investment is improving efficiency, consistent with Li et al. (2023).

3. Industry Placebo Tests

To evaluate whether our results are driven by within industry common VC investment or any common investment, we test the industry definitions by creating two randomized "placebo" industry definitions. The first definition randomly assigns industry based on a uniform distribution that results in our approximately 15,000 startups being allocated to 150 unique industries each with approximately 100 startups (calibrated to reflect the industry size in our data which ranges from 2 startups to 895 startups and has a median of 53 and a mean of 115). Second, we randomly assign industries based on a gamma distribution that results in 92 unique industries (again calibrated to our data). By definition, a uniform distribution will result in a roughly equal number of startups within an industry. The gamma distribution with its two adjustable parameters allows us to better capture the fact that the number of startups in a given industry is characterized by a distribution with a long tail (i.e., the industry with 895 startups). Using our two random definitions of industry, we then re-calculate our common VC investment measures. The logic behind this exercise is that the random industry allocations offer a baseline pattern of common investment we would expect even if there were no factors driving correlations across firms within the Preqin industries.

Table J.3 showcases the difference in the measures of total common VC investment across the three samples. We then evaluate the changes in the three different samples relative to the series of COW law adoptions. As shown in the table, the relative change in total common VC investment after the law changes is greater in the main sample than in the placebo samples. In fact, almost all of the common VC investments that we observe in the samples are attributable to observations after the law changes.

We further show that including the full set of fixed effects and controls does not alter this conclusion. To test this, we combine the placebo and the main sample. We then run the same specification for establishing that common VC investment increases after the law change, but this time we run it as a triple difference-in-differences estimator. This allows us to isolate how much more common VC investment is associated with the Preqin definition of industry relative to the placebo definition. As Table J.4 shows the triple interaction term is statistically significant and economically large both for any common VC investment and for total common VC investment.

Table J.1.

Industrial Specialization by Venture Capital (VC) Investors

This table presents summary statistics on industrial specialization by venture capital (VC) investors. Columns (1) and (2) summarize the mean and standard deviation for industrial specialization, which is defined as the number of unique industries in which a VC fund invests in a given year. Industry is defined based on Preqin's primary industry classification.

Number of Industries Invested In		
	Mean	Std. Dev.
Year	(1)	(2)
1995	1.4	1.03
1996	1.5	1.18
1997	1.5	1.13
1998	1.6	1.26
1999	1.7	1.42
2000	1.9	1.69
2001	1.8	1.54
2002	1.8	1.54
2003	1.9	1.71
2004	2.1	1.99
2005	2.2	2.24
2006	2.2	2.33
2007	2.3	2.47
2008	2.2	2.39
2009	2.1	2.21
2010	2.1	2.28
2011	2.2	2.42
2012	2.2	2.34
2013	2.1	2.25
2014	2.1	2.30
2015	2.1	2.29
2016	2.0	2.12
2017	1.9	2.02
2018	1.9	1.99

Table J.2.

Startup Growth by Strength of Intellectual Property Protection

This table presents regression estimates for subsamples of startups that historically have strong or weak intellectual property (IP) protection in the form of patents. In columns (1), (3), and (5) the sample is restricted to strong IP industries, and in columns (2), (4), and (6), to weak IP industries. In Panel A, the focus is on common venture capital (VC) investments and same-industry investment, in Panel B, the focus is on VC deals, and in Panel C, the focus is on startup exits. In Panel A, we present the results from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). In Panel B and C, we present the results from instrumental variable (IV) regressions that explore the direct effect of common ownership on the outcomes exploiting the same variation. Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. For Panel A, below the coefficient estimates are robust standard errors clustered by state of incorporation, and for Panels B and C, the robust standard errors are clustered by startup and adjusted for small clusters. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	Any common VC investment		Total common VC investment		Extensive margin investments	
	Strong	Weak	Strong	Weak	Strong	Weak
Panel A: Common VC investments	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	0.114***	0.073***	0.194***	0.128***	0.018***	0.030***
	(0.011)	(0.015)	(0.046)	(0.031)	(0.003)	(0.005)
Adjusted R^2	70.4%	70.2%	85.1%	81.5%	5.8%	7.1%
Number of observations	32,988	37,714	32,988	37,714	32,988	37,714
Includes additional controls, startup, VC, HQ-state-by-year, and industry-by-year fixed effects.						

	Deal volume		Late stage		Deal value	
	Strong	Weak	Strong	Weak	Strong	Weak
Panel B: VC deals	(1)	(2)	(3)	(4)	(5)	(6)
Any common VC investments	0.830***	1.962***	0.203**	0.090	1.525***	2.801***
	(0.162)	(0.423)	(0.093)	(0.125)	(0.355)	(0.698)
First-stage F-statistic	37.8	21.2	37.8	21.2	37.8	21.2
t-statistic on instrument	6.14	4.61	6.14	4.61	6.14	4.61
Number of observations	32,988	37,714	32,988	37,714	32,988	37,714
Includes additional controls, startup, VC, HQ-state-by-year, and industry-by-year fixed effects.						

	IPOs		Sales		Failures	
	Strong	Weak	Strong	Weak	Strong	Weak
Panel C: Startup exits	(1)	(2)	(3)	(4)	(5)	(6)
Any common VC investments	0.086***	0.030***	-0.056*	0.030	-0.157***	-0.164***
	(0.026)	(0.009)	(0.033)	(0.027)	(0.054)	(0.047)
First-stage F-statistic	27.6	35.4	27.6	35.4	27.6	35.4
t-statistic on instrument	5.25	5.95	5.25	5.95	5.25	5.95
Number of observations	33,234	38,221	33,234	38,221	33,234	38,221
Includes additional controls, VC, HQ-state-by-year, and industry-by-year fixed effects.						

Table J.3.

Industry Definition Placebo Tests

This table provides summary statistics for measures of common venture capital (VC) investment for three different samples. Column (1) reports the observation level, and column (2) the combined sample size. The mean for main sample in the paper is shown in column (3). Then, in column (4) the mean of a placebo sample in which industry has been randomized is shown. Column (5) reports the difference as well as the statistical significance for total common VC investment and the change in total common VC investment associated with the corporate opportunity waiver. The main sample is compared with samples defined by placebo industries where industry is randomly assigned to firms according to a uniform distribution (Panel A) and gamma distribution (Panel B). ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively for univariate differences in the mean.

Panel A. Placebo industries using uniform distribution	Obs.-level (1)	Combined N (2)	Main sample Mean (3)	Placebo sample Mean (4)	Diff. (5)
Total common VC investment	Firm-year-sample	286,734	5.59	0.78	4.81***
Difference-in-differences parts					
Pre-COW total common VC investors	Firm-year-sample, pre vs. post	32,4267	1.04	0.15	0.89
Post-COW total common VC investors	Firm-year-sample, pre vs. post	58,724	6.62	0.84	5.78
Change in total common VC investors	Firm-sample, pre vs. post	6,636	5.51	0.72	4.79***
Panel B. Placebo industry using gamma distribution					
Total common VC investment	Firm-year-sample	286,734	5.59	2.54	3.04***
Difference-in-differences parts					
Pre-COW total common VC investors	Firm-year-sample, pre vs. post	32,4267	1.04	0.45	0.59
Post-COW total common VC investors	Firm-year-sample, pre vs. post	58,724	6.62	2.73	3.88
Change in total common VC investors	Firm-sample, pre vs. post	6,636	5.51	2.36	3.15***

Table J.4.

Total Common Venture Capital Investment and Corporate Opportunity Waivers (Placebo)

This table presents the results from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs) where the main sample is combined with the placebo sample to assess the relative change in common VC investment. In columns (1) through (3), the dependent variable is an indicator variable for whether a startup has a VC investor that commonly owns another startup within the same industry. In columns (4) to (6), the dependent variable is the natural log of the total number of common VC owners that startup has. For the dependent variables for the placebo samples, the industry for firms is defined either based on a randomly assigned uniform distribution (Panel A) or a randomly assigned gamma distribution (Panel B). Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. For the dependent variables for the main sample, industry is defined based on Preqin's primary and sub industry classification. For the industry fixed effects, the industry definition from the respective samples are used. Below the coefficient estimates are robust standard errors clustered by state of incorporation. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	Any common VC investment			Total common VC investments		
Panel A: Placebo industries using uniform distribution	(1)	(2)	(3)	(4)	(5)	(6)
Treat	0.039**			0.044		
	(0.019)			(0.035)		
Treat × Post	0.020	0.054***	0.017**	-0.081**	-0.070**	-0.143***
	(0.013)	(0.011)	(0.007)	(0.035)	(0.031)	(0.020)
Treat × Post × Main sample	0.074***	0.074***	0.074***	0.395***	0.395***	0.395***
	(0.008)	(0.008)	(0.008)	(0.047)	(0.048)	(0.048)
Main sample	0.115***	0.115***	0.115***	0.338***	0.338***	0.338***
	(0.008)	(0.008)	(0.008)	(0.041)	(0.042)	(0.042)
Treat × Post after five years			0.040***			0.077***
			(0.006)			(0.014)
Adjusted R^2	34.2%	54.5%	57.4%	38.5%	60.6%	63.8%
Panel B: Placebo industries using gamma distribution						
Treat	0.030*			0.044		
	(0.017)			(0.035)		
Treat × Post	0.060***	0.101***	0.062***	0.075**	0.087***	-0.005
	(0.017)	(0.016)	(0.013)	(0.035)	(0.032)	(0.016)
Treat × Post × Main sample	0.015*	0.015*	0.015*	0.158***	0.158***	0.158***
	(0.008)	(0.008)	(0.008)	(0.017)	(0.017)	(0.017)
Main sample	0.017**	0.017**	0.017**	0.125***	0.125***	0.125***
	(0.008)	(0.008)	(0.008)	(0.016)	(0.017)	(0.017)
Treat × Post after five years			0.043***			0.101***
			(0.006)			(0.015)
Adjusted R^2	35.6%	56.9%	60.0%	38.8%	64.3%	68.3%
Additional startup and VC controls	Yes	Yes	Yes	Yes	Yes	Yes
Startup fixed effects	No	Yes	Yes	No	Yes	Yes
VC fixed effects	No	No	Yes	No	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	284,666	284,666	284,666	284,666	284,666	284,666

K Do Common Venture Capital (VC) Investors Increase the Variance in Outcomes?

Our main regressions look at the average impact of common VC investment on startups, which is overall positive. In this appendix, we explore the variance in outcomes. While some startups may benefit from common VC investment, some may be worse off. Evaluating the average treatment effect for many individual indicator variables as we do may overlook shifts in the distribution of outcomes. For our purposes, because most of the outcomes that we look at are indicator variables, we cannot evaluate the variance separately from the mean in most specifications. We thus focus our analysis of the variance of the continuous outcomes, such as deal value and deal count. One challenge with examining the variance is defining the reference group for calculating the variance. We create the simple difference-in-differences analysis (i.e., 2 x 2 box) where one dimension is treatment and the other dimension is time. Instead of calculating the mean, we calculate the standard deviation of each of the two outcomes: deal amount and deal count.

Table K.1 presents the simple difference-in-differences estimate for the mean and standard deviation. The estimate for the mean in Panels A and C indicates an increase in the deal amount and deal count, which is consistent with the main findings in the paper. On the other hand, the simple difference-in-differences estimate for the standard deviations in Panels B and D is small and close to 0. We test the null hypothesis of equivalence of the standard deviations using bootstrapped standard errors, and we fail to reject the null hypothesis. This analysis suggests that common VC investment is not associated with a shift in the variance of outcomes.

In addition, we re-cast our main outcome variables into an ordinal variable that captures the variance in the full spectrum of outcomes startups can achieve. That is, we create a new variable where failure is assigned a value of -1, no VC investment is assigned a value of 0, early-round investment is assigned a value of 1, late-round investment is assigned a value of 2, non-IPO exit is assigned a value of 3, and IPO is assigned a value of 4. We then re-estimate the IV tests using this ordinal outcome. We also create an alternative ordinal variable that leaves a startup at its previous highest value rather than moving back to 0 if nothing happens in a given year. If our main results were driven by variance rather than a shift of the full distribution, we would expect the association between common VC investment and these ordinal proxies to be statistically insignificant. As shown in Table K.2, the results indicate that startup outcomes experience a positive shift in the distribution rather than an increase in variance.

Taken together, our analysis suggests that the overall distribution of startup outcomes experienced a positive shift in the distribution rather than an increase in variance.

Table K.1.

Simple Difference-in-differences: Continuous Variables

This table presents the mean and standard deviations for two continuous variable outcomes – deal amount and deal count – when the observations are grouped into simplified categories by treatment and time. Entries in the differences column and row represent simple differences except for the lower right column entry which represents the difference-in-differences estimate. No controls and fixed effects are used in these calculations. For Panel B and D, we test the null hypothesis of equivalence using bootstrapped standard errors, we fail to reject the null.

Panel A: Deal Amount (Mean)

Treatment/Time	Before	After	Difference
No	0.141	0.365	0.224
Yes	0.265	0.579	0.315
Difference	0.124	0.214	0.090

Panel B: Deal Amount (Standard Deviation)

Treatment/Time	Before	After	Difference
No	0.617	0.918	0.301
Yes	0.838	1.128	0.290
Difference	0.221	0.210	-0.011

Panel C: Deal Count (Mean)

Treatment/Time	Before	After	Difference
No	0.071	0.201	0.130
Yes	0.136	0.314	0.178
Difference	0.065	0.113	0.048

Panel D: Deal Count (Standard Deviation)

Treatment/Time	Before	After	Difference
No	0.280	0.446	0.166
Yes	0.379	0.534	0.156
Difference	0.098	0.088	-0.010

Table K.2.

Ordinal Rank Test: Common VC Investments and Startup Growth

This table presents an alternative, aggregated version of the individual startup outcomes examined in the main tables. The dependent variable combines the various outcomes startups may experience into one variable. Specifically, we create a new ordinal rank variable where failure is assigned a value of -1, no VC investment is assigned a value of 0, early-round investment is assigned a value of 1, late-round investment is assigned a value of 2, non-IPO exit is assigned a value of 3, and IPO is assigned a value of 4. We then re-estimate the instrumental variable tests using this ordinal rank variable. We also check an alternative proxy construction in columns (3) and (4) that leaves the startup at its previous highest value rather than moving it back to 0 if nothing happens in a given startup-year. The key explanatory variable is common VC investment and the instrument is an indicator for if a startup is incorporated in a state that permits corporate opportunity waivers (COWs). Additional control variables include total capital previously raised by the startup, the total number of rounds of capital previously raised, and averages for VC investors' reputation, age, size, fund number, total rounds of startup investment, total IPOs of startups invested in, same headquarter state, and same primary industry. Industry for common ownership is defined based on Preqin's primary and sub industry classification. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. The first-stage F-statistic is the Kleibergen-Paap Wald statistic. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

	Ordinal Proxy #1		Ordinal Proxy #2	
Ordinal Rank of Startup Performance	(1)	(2)	(3)	(4)
Any common VC investment	1.832***		1.623***	
	(0.185)		(0.170)	
Total common VC investments		1.286***		1.139***
		(0.188)		(0.167)
First-stage F-statistic	129.6	48.7	130.4	48.7
t-statistic on instrument	11.42	6.98	11.42	6.98
Additional controls	Yes	Yes	Yes	Yes
Startup, VC investor, and year fixed effects	Yes	Yes	Yes	Yes
Number of observations	100,893	100,893	100,893	100,893
Number of unique startups	10,264	10,264	10,264	10,264

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