Common Ownership and Startup Growth*

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ABSTRACT

Is common ownership anticompetitive or do firms benefit when the same investors hold stakes in competing firms? We exploit a quasi-natural experiment in the venture capital (VC) industry – the staggered introduction of exemptions from liability when investors pursue conflicting business opportunities – as a shock to common ownership. We find increases in same-industry investment and directorships held at competing startups. Despite potential conflicts from information sharing, commonly held startups benefit by raising more capital through more investment rounds. Evidence from VC funds' returns and startups' exits suggests common ownership helps weaker startups improve rather than biasing competition toward winners.

JEL classification: G32, G24, G28.

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I. Introduction

Entrepreneurship is a key element in the process of creative destruction (Schumpeter, 1942), and startups play a vital role in generating economic growth (Aghion and Howitt, 1992; King and Levine, 1993) and productivity gains (Foster, Haltiwanger, and Syverson, 2008). Venture capital (VC) financing meaningfully shapes these outcomes, and therefore, the VC industry disproportionately impacts the economy given its size (Kortum and Lerner, 2000; Gompers and Lerner, 2000, 2004; Kerr, Nanda, and Rhodes-Kropf, 2014; Ewens, Nanda, and Rhodes-Kropf, 2018). Yet what happens to startups when the same VC investor holds stakes in multiple startups competing to disrupt an industry?

While a growing number of studies have evaluated and debated the effects of common ownership in public equity markets (Bebchuk, Cohen, and Hirst, 2017; Azar, Schmalz, and Tecu, 2018), no studies examine VC specifically. However, common ownership of private firms by VC investors is plausibly more relevant for firm performance than common ownership is for public firms (Gilje, Gormley, and Levit, 2019; Backus, Conlon, and Sinkinson, 2019; Dennis, Gerardi, and Schenone, 2019). High-growth startups often raise capital from a small set of overlapping VC funds. Even firms that aggressively compete against one another, such as Uber and Lyft, may raise capital from the same VC funds.[1] This phenomenon is likely to accelerate in coming years as SoftBank's $100 billion dollar Vision Fund encourages rivals like Sequoia Capital to raise even larger funds (Chernova, 2018). Moreover, unlike institutional ownership in public companies, VC firms contract for substantial control rights over startups' management that often exceed their equity stakes (Fried and Ganor, 2006; Fisch, forthcoming; Pollman, forthcoming).

In this study, we test whether common ownership by VC investors affects startup performance. Evaluating the economic consequences of common ownership for startups is not trivial because different factors militate in different directions. While the classical model of the firm assumes that firms maximize profits, this assumption may not hold under common ownership by VC investors because VC investors may have incentives to divert valuable competitive information from one

[1]For example, VC funds, 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.

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startup to another. But conflicts of interest and information-shifting may be in tension with other countervailing advantages, particularly accumulated industry expertise and experience (Asker and Ljungqvist, 2010). In fact, in the VC context, these factors may be interrelated in a way that could result in net positive gains for social welfare. Thus, to comprehensively assess the economic consequences of common ownership for startups, it is necessary to consider the different paths VC firms could follow to maximize returns under common ownership.

On one hand, portfolio maximization by VC funds could mean encouraging the weaker of two common industry startups to quickly go out of business in order to increase the likelihood of outsized success for the remaining startup. VC investors might also simply put less pressure on founders to aggressively compete against their rivals when they also hold stakes in those rivals. On the other hand, especially for startups, only a small set of expert VC investors deeply understand the issues they face. Thus, the information flows from one startup to another through the common owners, and the allocation of business opportunities by expert VC investors to startups in their portfolios may actually boost startups that are far from reaching first best outcome to much better results, even if only second best. In this sense, the benefits from shared investor expertise, potential synergy gains, and reduced information asymmetry may dominate any welfare losses from imperfect competition.

We face two main challenges in estimating the economic consequences of common ownership for startups. First, we need to identify a mechanism through which common ownership might affect performance (Anton, Ederer, Giné, and Schmalz, 2018; Hemphill and Kahan, 2018; Lewellen and Lowry, 2019). This can be problematic when managers' incentives (Harford, Jenter, and Li, 2011; Gilje, Gormley, and Levit, 2019) or the dynamics of competition are unknown (Backus, Conlon, and Sinkinson, 2019). To address this challenge, we exploit an important feature of the VC industry. VC investments are typically accompanied by a board seat for a partner from the VC firm (Kaplan and Stromberg, 2003; Fried and Ganor, 2006; Bengtsson and Sensoy, 2015). As capital-raising rounds progress, startup boards transition from founder-led to investor-controlled. These VC board members are privy to information that may bolster both the ability of VC investors to favor startups at the expense of others, but also their expertise and effectiveness in managing

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the startups in their portfolio.

Second, we need a shock that plausibly shifts the levels of common ownership by changing the institutional framework for VC investment (Kennedy, O'Brien, Song, and Waehrer, 2017; Lewellen and Lewellen, 2018; Backus, Conlon, and Sinkinson, 2018; Dennis, Gerardi, and Schenone, 2019). To this end, we exploit variation in the ability of VC investors and board members to utilize information. This variation stems from the staggered adoption of laws across eight states from 2000 to 2016 that enable corporations to adopt corporate opportunity waivers (COWs) (Rauterberg and Talley, 2017). These waivers exempt directors 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 financial conflicts of interest and requires fiduciaries to act in the best interest of the corporation. Although the duty of loyalty may seem immutable,[2] many corporations are electing to dilute it by waiving the business or corporate opportunity doctrine (Rauterberg and Talley, 2017).

To help understand the importance of the legal change, consider the following example. GoDaddy Inc., a firm that provides domain name registration services worldwide, adopted COWs for its directors.[3] At present, GoDaddy has five VC partners sitting on its board. Those five directors sit on 31 other boards, including another web domain company. Prior to the law change, these board seats would have subjected them 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 additional investment opportunities once they signal their interest in a space. Without the law change, they faced potential financial conflicts of interest if they invested in two different but closely related businesses. The effectiveness of COWs is further demonstrated by a recent case, in which the Delaware Chancery

[2]The duty of loyalty was not even one of the governance provisions considered when constructing the Gompers, Ishii, and Metrick index or entrenchment index, which are popular in corporate governance research (Gompers, Ishii, and Metrick, 2003; Bebchuk, Cohen, and Ferrell, 2009), presumably because it was assumed that it is not possible to waive it.

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

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Court dismissed a claim by a company against an investor that allegedly misappropriated confidential information, acquired through its representative board member, by investing in a competitor company. The court reasoned that the COW in the company's charter precluded such a claim.[4] Thus, there is ample evidence that legal changes permitting COWs are consequential.

Figure 1 demonstrates the importance of these legal changes for common ownership. After the first legal change in Delaware where most startups are incorporated, same-industry investment by VC investors doubles. The likelihood of making a second investment within an industry increases from 40% to 80%. While the visual evidence is compelling, we test these findings using 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 novel panel dataset of startups' states of incorporation across time from 1995 to 2018. To develop this dataset, we create an algorithm that reads incorporation filings, which are sourced from Lexis Advance, for startups that receive VC financing and are included in the Preqin Venture Deals dataset. The final sample includes almost 130,000 observations and 13,500 startup firms. To the best of our knowledge, this is the most comprehensive dataset documenting the state of incorporation for private firms.

Using this dataset, we find significant increases in same-industry investment both on the extensive margin (VC funds investing in competitor startups) and intensive margin (VC funds investing in the same startup in later rounds). Our results hold across a variety of specifications including ones that include firm and year fixed effects, industry-by-year fixed effects, and controls for local economic conditions. We interpret these findings as consistent with the remarks of one legal expert who stated that the corporate opportunity doctrine could easily dissuade investors "if they 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).

Next, we establish that the legal changes are also associated with more VC director cross-appointments, a key mechanism through which common ownership may affect startups. Using the

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

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staggered adoption of the same laws in a difference-in-differences setting, we find strong evidence that the firms incorporated in treated states place more VC directors on their boards. We also find that the VC directors sitting on the boards of firms incorporated in treated states have thicker networks, meaning that they sit on the boards of multiple startups, especially same-industry startups. Taken together, this evidence suggests that waiving the corporate opportunity doctrine is materially associated with cross ownership and appointments in entrepreneurial firms. Given the unique expertise and influence VC directors have on startups (Lerner, 1995; Kaplan and Stromberg, 2001; Hellmann and Puri, 2002; Robinson and Sensoy, 2013; Bernstein, Giroud, and Townsend, 2015), our results suggest that this plausible shock to common ownership may also have meaningful economic consequences.

To evaluate potential economic consequences, we first examine startup growth. We find evidence that the firms incorporated in treated states are significantly more likely to receive an additional round of VC funding: firms in our sample incorporated in treated states are between 5.9 and 14.2 percentage points more likely to raise an additional round of VC financing. We also find that deal sizes are larger, that the time between deal rounds is shorter, and that much of the increased funding is coming in later rounds. These findings are all economically meaningful given the very low baseline rates for raising capital.

We note that in all of our difference-in-differences specifications, the point estimates may reflect intent to treat rather than treatment on the treated. Private firms are not required to disclose information about the adoption of a COW. Public firms, however, are required to disclose this information, and Rauterberg and Talley (2017) show that the majority of companies do adopt it. Given the hurdles that the corporate opportunity doctrine poses for VC investment, 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. Thus, scaling by the proportion of actual private firm adopters would likely not change the main inferences from our statistical tests.

While additional VC funding could suggest a positive effect of common ownership, the startup outcomes are also potentially consistent with VC funds with greater common ownership maximizing

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their overall portfolio returns at the expense of individual startups' success. To disentangle these two alternatives, we evaluate real effects. Specifically, we look at the performance of VC funds with greater common ownership and the effects of common ownership on startup exits.

The evidence suggests that VC funds with greater common ownership significantly outperform their benchmark index. The VC funds' outsized returns appear to stem from three factors: (i) higher valuations when startups undergo IPOs, (ii) a higher probability of trade sale, and (iii) a lower probability of failure for startups incorporated in treated states. Thus, by shifting startups from failure or low-return multiples to higher multiples, primarily through trade sales, and focusing on successful IPOs that are likely to raise more capital, the VC funds are able to maximize their overall portfolio returns and improve individual startups' success. This fact pattern is consistent with the VC investors with superior expertise and information allocating opportunities efficiently among startups rather than stealing opportunities or advantaging one startup at the expense of another.

We emphasize that this study does not seek to evaluate effects on consumer welfare. For example, improved performance by otherwise less successful startups could be caused by collusive behavior that softens competition and increases the profits for the industry at the expense of consumers. Examining this hypothesis is outside the scope of this paper as it would require detailed product and price information that is unavailable for private firms.

We conduct several robustness tests to verify our results. First, we evaluate year-by-year coefficient estimates and find evidence consistent with the "parallel trends" assumption necessary for causal inference in this setting. Second, we consider nuances in the legal interpretation of directors' fiduciary duties and find that our results are not driven by such nuance. In particular, the results are robust when controlling for broader exemptions from the duty of loyalty which are available under some states' laws (Eldar and Magnolfi, forthcoming). Third, given that the majority of startup firms are incorporated in Delaware and it is a treated state, we exclude firms initially incorporated in Delaware and obtain similar results. Similarly, given that firms can reincorporate into another state, one may argue that our only real treatment is the first state law change. As such, we consider a single difference-in-differences framework where the first state law change is the treatment and

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limited liability corporation startups (LLCs), which are not subject to the law change, are the control startups. We find similar results. Finally, we assess states' political economy when the law changes were enacted by reviewing states' legislative records, and find no evidence to suggest that startup firms were lobbying for the change.

Our study makes several contributions to the literature. We contribute to the entrepreneurship research by identifying institutional features that affect the efficiency of capital allocation (Lerner, 2009; Acemoglu, Akcigit, Hanley, and Kerr, 2016; Howell, 2017) and business decisions by small firms (Hurst and Pugsley, 2011). Our findings suggest that common ownership affects startups' behavior (Germán and Philippon, 2017; Azar, Schmalz, and Tecu, 2018; Azar, Raina, and Schmalz, 2018; Gilje, Gormley, and Levit, 2019) and that VC funds with stakes in multiple startups in the same industry accumulate valuable expertise and information (Azoulay, 2004; Baccara, 2007; Asker and Ljungqvist, 2010; He and Huang, 2017; Kostovetsky and Manconi, 2018) which enables them to achieve higher returns (Moskowitz and Vissing-Jørgensen, 2002; Cochrane, 2005). These findings complement earlier research indicating that institutions and industries shape startup growth and innovation (Lindsey, 2008; Hellman and Puri, 2000; Furman and Stern, 2011). Overall, our study adds to the 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, Nanda, and Rhodes-Kropf, 2018; Gompers et al., forthcoming), especially studies emphasizing relationships and networks (Hochberg, Ljungqvist, and Lu, 2007, 2010; Hochberg, Lindsey, and Westerfield, 2015).

We find that a key mechanism through which common ownership affects outcomes is the cross appointment of directors. This finding thus sheds light on existing research on common ownership that faces challenges in uncovering a clear channel through which common owners could influence firm policy (Anton, Ederer, Giné, and Schmalz, 2018; Hemphill and Kahan, 2018; Dennis, Gerardi, and Schenone, 2019). This finding also supports a large body of research examining how directors influence corporate policy (see Adams (2017) for a recent review), particularly, how they add value. For example, directors may add value through their industry expertise (Güner, Malmendier, and Tate, 2008; Dass et al., 2014), social connections (Fracassi and Tate, 2012), reputation (Fich and

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Shivdasani, 2006; Fahlenbrach, Low, and Stulz, 2010) or diversity in terms of gender, culture, educational background (Adams and Ferreira, 2009; Bernile, Bhagwat, and Yonker, 2018), and geographic location (Masulis, Wang, and Xie, 2012; Knyazeva, Knyazeva, and Masulis, 2013). More specifically, it is consistent with research that shows that director networks tend to have greater expertise and knowledge (Bouwman, 2011; Barzuza and Curtis, 2017), which in turn could benefit startups.

Finally, we contribute to the law and economics literature on what constitutes sound practice in corporate governance (Shleifer and Vishny, 1997). Our results are consistent with a view that governance cannot be one-size-fits-all, and in some cases what might be considered bad governance can be beneficial (Cremers, Litov, and Sepe, 2017; Grennan, 2018; Eldar, 2018). The fact that the startups initially adopt weaker governance provides a potential rationale for why IPO firms tend to have weaker governance as compared to mature public firms (Daines and Klausner, 2001; Karpoff and Field, 2002; Baker and Gompers, 2003; Hochberg, 2012; Johnson, Karpoff, and Yi, 2015; Fields and Lowry, 2018). That weaker governance is associated with a greater supply of VC investment for startups also relates to the ongoing debate on decisions to go public and the increasing number of unicorn valuations (Lowry, 2003; Doidge, Karolyi, and Stulz, 2013; Gao, Ritter, and Zhu, 2013; Doidge, Karolyi, and Stulz, 2017; Ewens and Farre-Mensa, 2018; Gornall and Strebulaev, forthcoming). While factors such as increased accounting burdens for public firms or the deregulation of the private equity industry have affected this multifaceted decision, we offer another element that may be contributing to the phenomenon. Identifying such factors informs policymakers evaluating the decline in public firms (Grullon, Larkin, and Michaely, forthcoming; Kahle and Stulz, 2017) and its broader effect on productivity (Syverson, 2017).

II. Institutional Background

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

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potential different interpretations of these laws.

A. *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[5] and statutory provisions that permit exemptions from liability for such violations.[6] 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.

B. *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 of loyalty in *Meinhard v. Salmon* (249 N.Y. 458, 464 (1928)) by Chief Justice Benjamin Cardozo involved the appropriation of a business opportunity

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

[6]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.

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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 tend to acquire a controlling stake in the firms in which they invest, and they often appoint their own representatives to the boards of these firms. The VC investors, as controlling shareholders, and their board representatives are subject to the duty of loyalty, and therefore also the corporate opportunity doctrine. 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.

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C. 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 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 investor with control or at least substantial influence over the board can easily adopt such a waiver. 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 moved to amend 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.

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D. 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, forthcoming). 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 already benefit from exemptions for monetary liability for violating the duty of loyalty. While we include robustness checks that account for broader exemptions from the duty of loyalty, our main specifications, we do not account for these broad exemptions 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 the VC firm as a controlling shareholder is not required to disclose and share business opportunities. Therefore, these broader exemptions are likely 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

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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 permit COWs, particularly in the context of VC investment. Accordingly, the assumption that all firms take advantage of permissible exemptions may be too strong when shareholder approval is required.

E. 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, states that mimic Delaware for all of their corporate laws (Kansas, Oklahoma, Texas, Missouri, Nevada, and Maryland), and states that specifically adopted COW statutes because of possible demand by parties within the state (New Jersey and Washington). Our review of lobbying transcripts and session notes available on Lexis Advance indicate that the original motivation for the law was to eliminate uncertainty regarding corporate opportunities arising from a 1989 case, *Siegman v. Tri-Star Pictures*. A lack of clarity in a ruling or conflicting rulings from different judges is often an impetus for legislative action. No other forms of lobbying are explicitly mentioned in the Delaware session notes. The six states in the second group that mimic Delaware law generally 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.

The last 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

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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 share of incorporations is very low throughout the sample period.

III. Data and Sample

A. Panel Data of Startup Incorporations

Our data on startups and VC funds is sourced from Preqin Venture Deals and our sample horizon extends from 1995 through 2018. We keep in the sample only startups for which there is sufficient data on VC investments. In order to pursue our identification strategy we need to identify the incorporation state across time for each startup in our sample. Firms incorporated in states that adopted laws that permit COWs are the treated firms, and those incorporated in states that do not are the control firms. Table 1 reports the states and the dates when firms were first permitted to adopt the COW in treated states. States that are not listed are the control states.

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 for a sample of approximately 14,000 startups. To select this sample, we aggregate deal value at the firm level and select the startups that receive at least 10 million in funding. For firms headquartered in a treated state other than Delaware, we relax the deal value restriction in order to increase the sample size for our analyses that exclude Delaware. The nationwide business locator tool on Lexis allows for searches by name and location. It then presents match results ordered by likelihood. 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 startups, we are able to identify about 92% of firms using the nationwide business locator. We then download all corporate records from the secretary of state of the state in which the firm is headquartered. We develop an algorithm that examines the filings and de-

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termines 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 manually code firms for which no filings are available on Lexis using data obtained from the Delaware and California secretaries of state.

We exclude startups not located in the United States. We also exclude nonprofit startups, those that have "LLC" in their names, and those that are not corporations as they would not be subject to the state legislation changes. For state of headquarters, we use the state of location, and for deal count we add the number of Preqin deals per firm-year.

Our final sample of startups with state of incorporation data comprises 13,428 unique startups. While this sample represents only approximately 52% of the unique startups in the full sample, it represents 99% 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, and the only one that includes a panel as opposed to one cross-section of firms.

B. VC Investments

We create three measures of within-industry VC investments. We focus primarily on VC investment as it is thought to be beneficial to startups whereas PE funding may not be (Eaton, Howell, and Yannelis, 2018). Our observations are at the portfolio-company-year level and the three measures are indicator variables for (i) whether any VC investor made a within-industry investment in that portfolio company, (ii) whether any VC investor made a within-industry investment in that portfolio company on the extensive margin, and (iii) whether any VC investor made a within-industry investment in that portfolio company on the intensive margin. We use the Preqin variable corresponding to a VC investor, such as Benchmark Capital or Sequoia Capital, to define the investor level.

Our indicator variable for within-industry extensive margin investment is equal to one when

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the VC investor invests in a new portfolio company in a given year that is in the same industry as a portfolio company that it currently holds in its portfolio. Similarly, we create an indicator variable for within-industry intensive margin investment when the VC investor invests more money in a portfolio company in a given year that it has previously invested in. We define any within-industry investment as an indicator variable equal to one if at the portfolio company investor level either an intensive or extensive margin investment is made. When aggregating to the portfolio-company-year level, we require that at least one VC investor in a given year made a within-industry investment.

We define late-stage deals 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.

C. Industry Classification

Our analysis uses multiple industry classifications. We use Preqin's primary industry description for determining common ownership, within-industry investments, and within-industry directorships. Preqin defines 78 unique industries. Given that the industry descriptions can be very detailed (for example, information technology (IT) is divided into healthcare IT, IT security, IT infrastructure, and general IT), we also define our own industry classifications that allow us to control for industry fixed effects at a more aggregate level. The resulting classification consists of 11 industries. The five largest industry groups are computer software and hardware (29%), IT (18%), medical technology (16%), internet (16%), and pharmaceuticals (6%). The remaining 15% of firms are classified as business, clean technology, industrial, media, or other.

D. Founding Dates and Exit Dates

We must exclude firm-year observations from the analysis for years prior to the firm's founding and after the firm's exit. We define year founded as the earliest of the following three variables: year of first incorporation (from Lexis Advance), year of first deal (from Preqin), and founding year (from Preqin or VentureXpert when missing). We gather exit data from Preqin's Exit data

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set, which includes IPOs, mergers, and trade sales and we code each as an indicator variable. We supplement the Preqin Exit data with data from VentureXpert and manual searches in Crunchbase, because Preqin does not have a variable to indicate when a portfolio company goes out of business or fails. We define the year of any exit as the year that the firm either experienced an IPO or a merger, according to Preqin. For firms that we can match to VentureXpert and Crunchbase, we define a firm 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.

E. Board Members

We gather data on directors from Preqin to construct three measures related to board members. First, we calculate the total number of VC directors per firm-year. Next, we are interested in measuring the thickness of the VC director networks, defined as the number of additional board seats on which directors sit. The first measure of director thickness is the average number of additional board appointments that the VC directors hold per firm-year. Other board appointments include all directorships that year within the full universe of Preqin firms. Our second measure of director thickness is the average number of other board appointments per firm-year that are within the same industry as the portfolio company. For example, consider firm x that had two VC directors on its board in year y. The first director held no other board positions in year y, whereas the second director held two other board positions in year y for VC-backed firms in this same industry. This measure of director thickness for firm x in year y would then be $\frac{0+2}{2} = 1$.

F. 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 benchmarked quartiles at an

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annual frequency to assess portfolio returns.

G. *IPO valuations*

For startups that undergo an IPO, we supplement the Preqin data with IPO valuation data. We follow the method 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 comes from the Center for Research in Security Prices (CRSP), and all accounting data comes from Compustat.[7]

Each IPO is matched to a public firm from Compustat that did not undergo an IPO in the previous three years. The Compustat firms are grouped into fiscal years and industries based on the Fama-French 48 industry classification, and then divided into 3×3 portfolios (or 3×2 or 2×2 if there are not enough firms) based on past sales and EBITDA profit margin (defined as EBITDA/sales). Each IPO is then matched to the appropriate fiscal year-industry-sales-EBITDA margin portfolio. From this portfolio, we find a matching firm that is closest in sales to the IPO firm. If the previous fiscal year sales of the IPO firm equal zero, and therefore profit-margin cannot be computed, we simply match the IPO firm to the nearest match from the same fiscal year-industry-sale portfolio based on Mahanabolis distance computed using both sales and EBITDA. For each matched firm, we compute the multiples in the same way, except that for offer price we use the market price from

[7]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 because most firms in our sample are high-growth firms that would require us to omit most firms in our sample.

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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 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. The final valuation measures for each IPO firm are computed as follows:

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

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

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

H. Limited Liability Companies (LLCs)

To identify startups that are LLCs, we search the text of the Preqin name for "LLC." The textual approach identifies 3,239 startups as LLCs. For the 14,000 startups for which we identify state of incorporation using Lexis Advance Public Records, we use the startups' actual legal status. In total, we find that 4,018 startups are LLCs in the Preqin Venture Deals database for our sample horizon that extends from 1995 through 2018.

I. Summary Statistics

Table 2 provides summary statistics for the 129,492 firm-years belonging to 13,428 Preqin firms with data on their states of incorporation. The panels show summary statistics for VC investment, VC directors, VC deals, and startup exits. Separate statistics are provided for the full sample as well as the treated and control samples. Firm-years are only included in the treated sample for any year after the treatment. Control firm-years consist of firm-year observations for the never-treated group as well as firm-year observations for years prior to the treatment for the treated group. About 71% of all firm-years are in the treated sample. This is because about 66% of our firm-year

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observations come from startups incorporated in Delaware (see Appendix Table A.1).[8] Naturally, the large proportion of Delaware firms in our data may give rise to concerns that the results are driven by Delaware startups. As discussed below, we use several robustness tests to address this concern, including specifications that exclude Delaware firms.

Panel A of Table 2 focuses on our measure of within-industry investment. The statistics are consistent with Figure 1, which reveals a meaningful increase in common ownership after the law change. Among the treated firms, within-industry investment almost doubles relative to the control firms at the mean. The doubling 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 1.77 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 firm-year. Across all measures of director thickness, we observe higher values for the treated sample.

Panel C of Table 2 summarizes the VC deal variables. We observe 0.29 deals per firm-year. Late-stage deals occur less frequently and constitute only 0.11 deals per firm-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 deal amounts than the treated sample. Panel D of Table 2 summarizes the exits. Here we see that treated firms undergo IPOs and exit at similar rates to control firms, but they have a higher rate of involvement in trade sales. Startup failure rates also differ between treated and control firms.

IV. Empirical Design

Firms incorporated in the states that allow for COWs can dilute the duty of loyalty. We exploit these incorporation state-level shocks as natural experiments to establish the link between a firm's

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

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ability to customize liability risk, common ownership, and startup growth. This setting has several appealing empirical features that facilitate a valid difference-in-differences analysis. First, the variation in governance generated by the staggered adoption of COWs is arguably exogenous to firm-level attributes. Second, because variation is at the state of incorporation level, we can compare firms that are headquartered in the same state but are subject to different legislation. Firms incorporated in states with COWs are the treated firms, and those incorporated in states without COWs are the control firms. This empirical design significantly mitigates the confounding effects resulting from regional economic shocks or conglomeration effects in entrepreneurial hubs.

In our difference-in-differences design, we compare treated firms with control firms pre- and post-law change. We use firm-year panel regressions to estimate the average treatment effect of being able to adopt COWs. We estimate the following five regressions:

$$Y_{jit} = \alpha + \gamma Treat_{jit} + \beta \left(Treat \times Post\right)_{jit} + \delta_t + \varepsilon_{jit} \tag{4}$$

$$Y_{jit} = \alpha + \beta \left(Treat \times Post\right)_{jit} + f_j + \delta_t + \varepsilon_{jit} \tag{5}$$

$$Y_{jit} = \alpha + \beta \left(Treat \times Post\right)_{jit} + \zeta \left(Treat \times Post5\right)_{jit} + f_j + \delta_t + \varepsilon_{jit} \tag{6}$$

$$Y_{jnhit} = \alpha + \beta \left(Treat \times Post\right)_{jnhit} + \zeta \left(Treat \times Post5\right)_{jnhit} + f_j + \gamma_{ht} + \varepsilon_{jnhit} \tag{7}$$

$$Y_{jnhit} = \alpha + \beta \left(Treat \times Post\right)_{jnhit} + \zeta \left(Treat \times Post5\right)_{jnhit} + f_j + \gamma_{ht} + \rho_{it} + \varepsilon_{jnhit} \tag{8}$$

In the above equations, Y represents the outcome variable, such as an indicator for receiving VC funding for startup j that operates in industry n and is headquartered in state h but incorporated in state i in year t. $Treat$ is an indicator for firms that are incorporated in states with COW laws. $Treat \times Post$ is an indicator for firms that are incorporated in states with COW laws after the law change. β is the main coefficient of interest to identify the effect of the law change. $Post5$ is an indicator for five years after the law change. ζ captures treatment effects or reversals that may occur outside a standard five-year window. Using this regression specification rather than truncating the sample to only a five-year window around the transitions from control to treated state helps to

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ensure greater precision in our estimate of the time trend. f_j denotes the firm fixed effects that capture all of the firm-level time invariant effects, δ_t denotes year fixed 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. We deliberately do not control for any time-varying accounting variables in our regressions. Because these variables may be simultaneously affected by the law changes, they could confound the estimates of the effect of such changes.

V. Results

In this section, we first present evidence documenting the increase in common ownership measured as within-industry investment following the law change. We next show that this increase in common ownership relates to an increase in directorships at related startups for VC directors. Then, we evaluate startup growth, specifically the startups' ability to raise capital. Finally, we examine whether the evidence we find is consistent with common ownership generating positive or negative economic benefits. We conclude by presenting multiple robustness tests.

A. Common Ownership

In general, we consider five regression specifications throughout this study. Specification (1), always reported in column (1), is a standard difference-in-differences specification with year fixed effects. Specification (2) also includes firm fixed effects. Specification (3) includes a control for late treatment. Specification (4) includes firm and headquarter-state-by-year fixed effects, and column (5) augments that specification with industry-by-year fixed effects. All of our standard errors are clustered at the incorporation state level, as that is our level of treatment.

Panel A of Table 3 shows that treated firms experience a statistically significant increase in within-industry investment. On average, firms incorporated in treated states are between 5.7 and 9.9 percentage points more likely to raise capital from a VC with a previous investment in the same

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industry. The result is significant, generally at the 1% level, across all five specifications. Panel B and C of Table 3 report separate coefficient estimates for within-industry investment at the extensive and intensive margins, respectively. For both margins, VC investors are significantly more likely to make a within-industry investment, and again these results hold across all specifications. A comparison of the point estimates shows that 20% of same-industry investment is on the extensive margin and the remaining 80% is on the intensive margin. Taken together, this statistical evidence confirms what we observed in Figure 1, that after the law change, common ownership meaningfully increases. Given that large VC investments are typically accompanied by a board seat, a key mechanism for facilitating any potential competitive or anticompetitive effects among competitors, we next explore this outcome.

B. Director Networks

We now explore the relationship between the ability to customize directors' fiduciary duties and actual directorships at startups. Panel A of Table 4 shows that compared to firms incorporated in states without COWs, the treated firms, on average, are much more likely to have a VC director on their board. The point estimate of 0.259 in column (2) indicates about a quarter of treated firms end up with a VC board member. This result is statistically significant at the 1% or 10% level depending on the fixed effect specification. As expected when we look at the loading on the coefficient after five years, it is quite strong. This is likely because directors are typically appointed in later rounds of VC financing and because directorships often last for multiple years. Overall, the basic result that startups saw a rise in VC directors after the law changes is consistent with VC investors favoring startups with COWs or insisting that startups sign such waivers as a closing condition for their capital contributions.

Next, we more thoroughly evaluate economic channels that may explain the increase in VC directorships. For example, a relevant null hypothesis is that the increase in VC directorships is associated with the waivers but VC investors still generally maintain a single board seat within an industry. If this were the case, VC investors might not have a clear mechanism for acquiring

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information on startups' aptitude and allocating business opportunities among their portfolio companies. Alternatively, if the VC partners are less concerned about conflicts of interests that arise from holding multiple investments, they may desire board representation at firms that operate in the same space.

To test this hypothesis, we examine the thickness of directors' networks in two ways. First, in Panel B of Table 4, we examine VC board members with other directorships at startups and in Panel C, we examine VC board members with other directorships at startups within the same industry. As shown in column (2) of Panel B, the newly appointed VC directors often also already hold board seats at other startups. We find that compared to firms incorporated in states without COW legislation, the treated firms, on average, have VC directors who hold 0.352 more directorships at startups. Importantly, as shown in column (2) of Panel C, about one-third of the additional directorships are at directly competing startups. These results are robust to the inclusion of headquarter-state-by-year and industry-by-year fixed effects and are statistically significant at the 1% level.

Overall, these results support our previous results on common ownership. In recent years, the speed of deal-making is critical, and VC investors are more likely to invest in a firm if that investment will not hold up future investments by requiring the constant review of financial conflicts of interest. When startups customize their governance and adopt the COWs, they attract well-connected VC investors. However, as highlighted in the introduction, well-connected VC investors can be a double edged-sword. On one hand, only a small set of expert VC investors deeply understand the issues startups face, and their expertise could help these firms succeed more than they otherwise would. On the other hand, these VC investors have their own incentives and may use the information that they are privy to for the benefit of their overall portfolio rather than doing what is in the corporations' best interest. To begin to evaluate these competing hypotheses, we next examine startup growth.

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C. *Startup Growth*

We explore the relationship between firms' ability to adopt COWs and startups' ability to raise capital. We first examine the effect of the law changes on the number of rounds of financing these startups receive. As shown Panel A of Table 5, the treated firms, on average, are between 5.9 and 14.2 percentage points more likely to receive an additional round of VC financing than firms incorporated in states without COW legislation. The increase is statistically significant at the 1% level and represents an economically meaningful effect given the low baseline rate for VC investment. When we include headquarter-state-by-year fixed effects and industry-by-year fixed effects in the regressions, the relevant comparison group is firms in the same industry and headquarter state. The empirical results are also highly robust when we include these fixed effects, suggesting that industry time trends do not drive the results. When we compare the treatment effect of the COW legislation in the first five years with the effect several years later, we see that all of the effect occurs within the narrow window around the treatment. We find no significant effect five years after the treatment, indicating no reversal in the effect of the treatment. Taken together, these initial results are consistent with a positive economic impact from common ownership rather than the alternative hypothesis that COWs merely facilitate expropriation of business opportunities by common owners.

Our next set of tests evaluate how changes in state laws 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 the law changes. 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 from permitting COWs. It is possible, however, that after early investments, the directors who are subject to weaker governance divert opportunities to other firms that they work with, thereby reducing the overall likelihood of a firm receiving late-stage financing and resulting in firms receiving smaller deals.

Panel B of Table 5 reports the results of the financing round tests. We find a positive relationship

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between the ability to adopt COWs and receiving later stage financing from VC firms. As shown in column (1), compared to firms incorporated in states without COW legislation, the treated firms, on average, are 4.1 percentage points more likely to receive late round VC financing. Depending on specification, this point estimate can be as large as 5.2 percentage points more likely to receive a late round financing deal. All results are significant at the 1% level when including the additional fixed effects. In terms of timing, our results suggest that the majority of the average benefits for startups in treated states come from these later rounds of financing.

In Panel C of Table 5, we consider deal size. Our dependent variable is the log of one plus the deal value in millions of 2010 dollars. The coefficient estimates suggest that, on average, the firms in treated states are able to attract more capital, with the average deal size being 15 to 28 percent larger. 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. To test this alternative hypothesis, we evaluate the time between financing rounds. As reported in Table A.2, we find no evidence of delays in financing. Rather, our results suggest that these startups are able to speed up financing rounds by about one month. Thus, overall, the results on deal size and timing are consistent with the notion that common ownership has positive economic benefits.

D. VC Funds' Returns and Startups' Exits

Although the startup growth evidence suggests that common ownership by VC investors could have a positive effect, the startup outcomes 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. Specifically, we look first at the performance of VC funds with greater common ownership, and then analyze startup exits and IPO valuations.

Table 6 examines the performance of VC portfolios as a whole. We use a simple ordinary least-squares regression where the dependent variable is the quartile of the VCs' returns in a given

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year, and the primary explanatory variable is the percentage of portfolio companies in the VC portfolio that are treated. The results are significant and suggest that common ownership is linked to greater portfolio returns for the VC investors. 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 result suggests that common ownership benefits the VC investors. We now turn to examine the effects on startups.

Table 7 evaluates startup firm exits. Most of the exits stem from trade sales, with fewer IPOs and exits via merger. The results suggest that common ownership does not have a meaningful economic effect on the probability of exit through an IPO or a merger. While the coefficients are negative, the magnitudes are very small. We do find, however, that compared to firms incorporated in states without COW legislation, the treated firms are more likely to be part of a trade sale. Importantly, these startups are also 1.3 percentage points less likely to fail.

Thus, the overall results are consistent with common ownership helping weaker startups to improve rather than common onwership distorting competition. The results also suggests that VC investors and startups tend to share information, and their relationship is broadly collaborative (Fisch, forthcoming). 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).

Next, we take a deeper look at 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 8. As discussed in Section III, following Purnanandam and Swaminathan (2004), 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 firms have a price-to-earnings ratio that is 0.33 standard deviations higher than that of control firms. The relative increase is statistically significant at the 5% level.

In summary, while we find that VC funds that invest more in treated startups significantly

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outperform their benchmark index, startups benefit too. While the rates of IPO exit for treated startups are not larger, their valuations are more favorable at the time they undergo an IPO. Similarly, we find that the VC investors with greater common ownership are able to shift some startups from failure and low-return multiples to higher multiples via trade sale exits. Taken together, this evidence suggests that the accumulated information and expertise of VC investors who are common owners of startups enables them to better allocate resources and opportunities among startups rather than advantage one startup over another. Ultimately, this suggests that most startups benefit from common ownership through lower failure risk and higher IPO valuations.

E. Robustness

A potential concern when using a staggered difference-in-differences specification is that a preexisting trend in the outcome variable will confound the point estimate. We therefore examine our identifying assumption, namely that the treatment and control groups would have evolved along parallel paths had it not been for the law changes. We assess the validity of this assumption in two ways. First, Figure 2 plots the parallel trend lines for deal characteristics for the first law change in the staggered event study, which occurred in Delaware, the most popular state for firm incorporations. In regard to average within-industry investment, within-industry directorships, deal counts and deal size, Delaware firms trend higher than their control firms after the law change. Further, we see no evidence of a reversal in this upward trend for Delaware firms even five years after the law change.

Next, we plot the yearly coefficients and the confidence intervals for a window spanning from three years before the law change to the five years after the change. Figure 3 provides further evidence that the parallel trends assumption holds even when additional fixed effects are considered. Moreover, the figure shows that the treated firms display parallel trends prior to the law changes; however, after the law changes, the trends diverge. Given that we find no violation of the parallel trends assumption, and the results appear not to be merely the product of mean reversion as they get stronger every year post reform, we conclude that the identifying assumptions for our

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difference-in-differences research design are satisfied.

As discussed in Section II, several states, such as Nevada and Virginia, permit 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. In Appendix Table A.3, we conduct additional robustness tests in which 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. For ease of interpretation, we only report the results from the regression specification with year and firm fixed effects. Doing so allows us to show the results for all the dependent variables we examined in Tables 3, 4, and 5. Our main results are all robust to this specification.

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). Appendix Table A.4 shows the results from regressions in which the treatment variable excludes Nevada and Table A.5 shows the results from regressions in which the treatment variable excludes Washington. Again, our results are materially the same as in our main specifications.

While our review of the political economy underlying the state law changes did not suggest that lobbying by startups was an important factor, and even if the parallel trend assumption holds, it is still possible that some unobservable factors could contribute to the passage of COW legislation. One key concern is that about 66% of our firm-year observations come from startups incorporated in Delaware (see Appendix Table A.1), where the first legal change took place in 2000. To evaluate the potentially disproportionate influence of Delaware, Table A.6 excludes startups that are originally incorporated in Delaware from the analysis. After excluding these startups, of the remaining observations, 26% are from control states and 8% are from the other treated states. Even with the much smaller sample size, the results still hold and are statistically significant at the 1% level. These results suggest that the main findings are not driven exclusively by the legal change in Delaware or Delaware startups.

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Another potential concern is that firms can reincorporate into other states. Thus, even if a startup appears to be untreated to the econometrician because it is incorporated in a control state, it could in fact receive treatment relatively quickly if it chose to reincorporate into a treated state. Appendix Table A.1 shows that approximately 9.6% of the observations, or 1,291 startups, in our sample reincorporate into another state. Moreover, 74% of the startups that reincorporate in our sample change their incorporation from a control state to a treated state.

To address the reincorporation concern, we consider an alternative single difference-in-differences framework. We use the first state law change (July 1, 2000) as the date of treatment. Our control group is LLCs, which are not subject to the law change. While we do not have accounting data to show that size and other financial variables are similar between LLC and non-LLC startups, it is important to note that several large VC-backed startups are structured as LLCs. For example, the popular peer-to-peer lending firm Prosper is an LLC. Table A.7 provides summary statistics comparing LLC and non-LLC startups for our main outcome variables. As with the staggered difference-in-difference sample, treated startups appear to have meaningful increases in common ownership after the law change relative to this alternative set of control observations. Table A.8 replicates Table 3 of our main analysis but uses the LLC sample. Similarly, Table A.9 replicates Table 4 and Table A.10 replicates Table 5. Even with this alternative research design, we see nearly identical patterns for within-industry investment, directorships, and capital raising among treated startups. In general, the results are statistically significant across all specifications at the 1% level.

Overall, our various robustness checks suggest that our finding of positive economic benefits from allowing firms to customize the duty of loyalty holds more broadly.

VI. Conclusion

VC investors with cash to invest provide a valuable resource to capital markets, yet financing frictions arising from asymmetric information between entrepreneurs and VC investors can force the abandonment of high-potential startups. This study suggests that it is easier to raise capital from VC investors when startups have common VC investors. However, since common ownership

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of competing firms by the same investors creates opportunities for information sharing across the firms, there is a concern that VC investors might misappropriate information to benefit competitor firms. But alternatively, there is also the potential for positive spillovers that might improve capital allocation and efficient decision-making.

We use the staggered adoption of laws that reduce liability risk for VC investors when they pursue conflicting business opportunities as a shock to common ownership of startups. We find these laws are followed by a substantial increase in common ownership among startups with about 80% of VC firms investing in competing startups. The investment is accompanied by more directorships for VC investors and a thickening of those directors' overall networks. After the law changes, VC investors hold more directorships at startups and one-third of those additional directorships are at a directly competing startups.

While this director mechanism could potentially be used to orchestrate anticompetitive practices, our evidence is more consistent with directors using their positions to reduce information asymmetries and facilitate synergy gains. We find the effects of common ownership are mostly positive for startups: They raise more capital through more rounds of investment and at shorter intervals. Our analysis of VCs' returns and startups' exits supports the idea that the economic benefits of common VC owners come from shifting startups from failure or low-return multiples to higher multiples, primarily through trade sales, and focusing on IPOs that are likely to raise more capital. Thus, the VC funds are able to both maximize their overall portfolio returns and improve individual startups' success.

That common ownership by VC investors improves startup performance has important implications for our understanding of various policy debates concerning entrepreneurship, industrial organization, corporate governance, the rise of private financing and the decline of IPOs. First, common ownership may explain why firms conducting IPOs do not necessarily choose stronger governance mechanisms. Second, our study highlights the importance of liability risk in facilitating the process of raising private capital. The new equilibrium, in which the number and economic significance of public firms has been declining, appears to be driven in part by firms' ability to attract private capital. As policymakers continue to consider interventions to spur a return to

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public markets, the role of common ownership by VC investors deserves attention.

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Figure 1

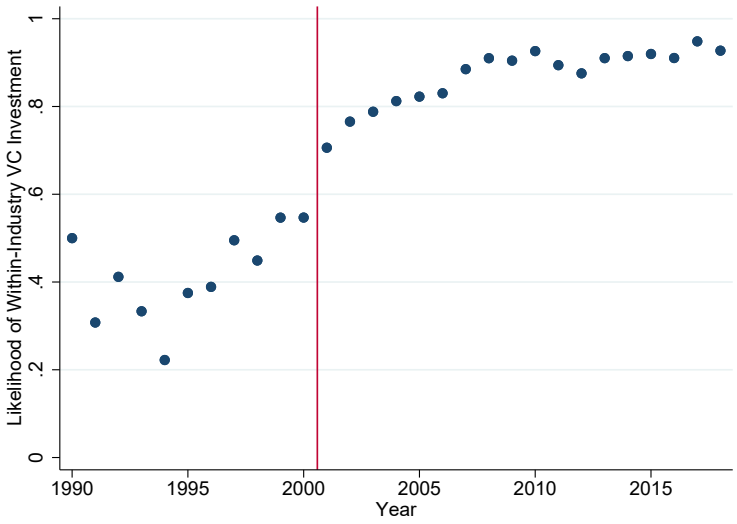


FIG. 1.—Within-industry investment rates relative to the corporate opportunity waiver (COW) legislation. The figure plots the annual rate of within-industry investment for venture capital (VC) funds. The red vertical line represents when the first state, Delaware, amended its laws to allow corporate boards to adopt COWs that exempt directors, officers, and shareholders from liability arising from pursuing new business opportunities that may be in direct financial conflict with the given firm's interest.

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Figure 2

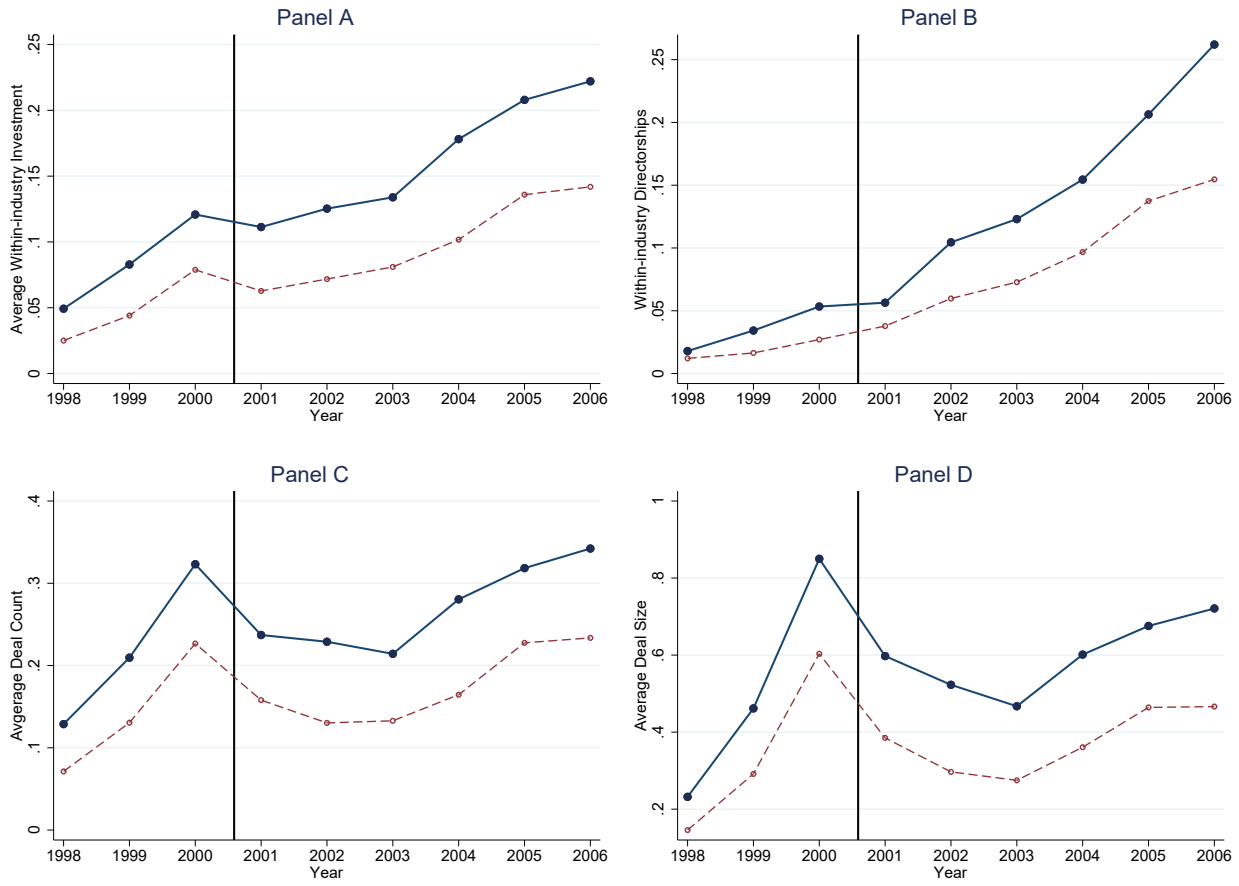


FIG. 2.—Parallel trends of corporate opportunity waiver (COW) legislation. The figures plot the parallel trend lines for Delaware and control states. We consider a window spanning from three years before the law change to the year of the change and the five years after the change. The solid navy line represents Delaware. The dashed maroon line represents the control states. Panel A plots within-industry investment, Panel B plots within-industry directorships, Panel C plots the average deal count, and Panel D plots the average deal size.

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Figure 3

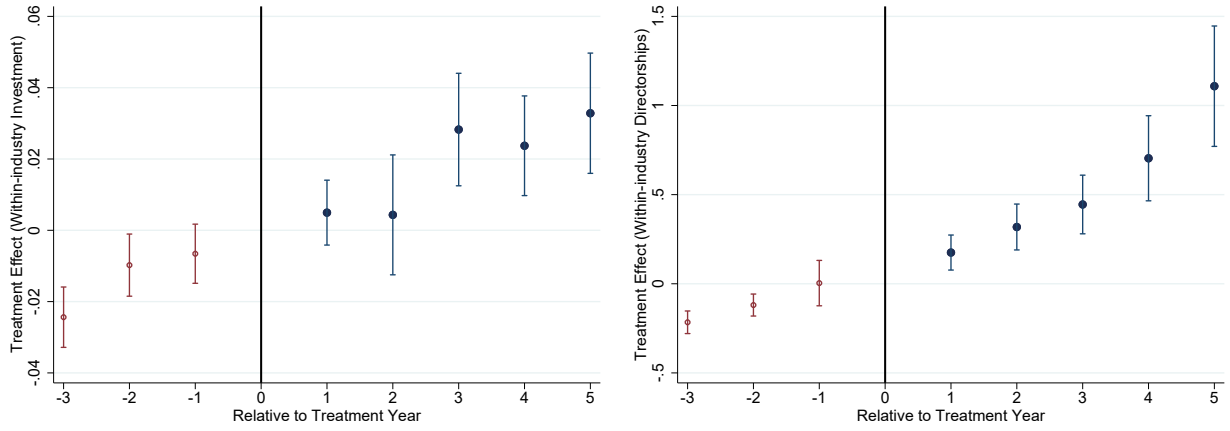


FIG. 3.—Dynamic effects of corporate opportunity waiver (COW) legislation. The figures plot the impact of Delaware's COW law on deal characteristics following the law change. We consider a window spanning from three years before the law change to the year of the change and the five years after the change. Coefficient estimates are normalized relative to the treatment year. 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 year fixed effects. A full set of dummy variables for relative years are included in the regression, but only those in the narrow window are plotted. Panel A plots within-industry investment and Panel B plots within-industry directorships.

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Table 1

Summary of corporate opportunity waiver (COW) legislation

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	No
NJ	March 11, 2011	Yes	Possible	Yes	Yes	Yes
MD	October 1, 2014	Yes	Possible	Yes	Yes	Yes
WA	January 1, 2016	Yes	No	No	Yes	Yes

Note—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.

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Table 2

SUMMARY STATISTICS

	All			Treated			Control		
	N	Mean	St. Dev.	N	Mean	St. Dev.	N	Mean	St. Dev.
Panel A: VC Investment									
Within-industry investment	129,492	0.18	0.39	91,485	0.21	0.41	38,007	0.11	0.31
Intensive margin investment	129,492	0.14	0.34	91,485	0.16	0.37	38,007	0.08	0.27
Extensive margin investment	129,492	0.05	0.22	91,485	0.06	0.23	38,007	0.03	0.18
Panel B: VC Directors									
VC directorships	95,188	1.77	2.09	69,252	2.04	2.18	25,936	1.03	1.63
Additional directorships	95,188	2.53	3.52	69,252	2.97	3.69	25,936	1.36	2.67
Additional within-industry directorships	95,188	0.61	1.22	69,252	0.73	1.32	25,936	0.29	0.80
Panel C: VC Deals									
Count	129,492	0.29	0.52	91,485	0.32	0.54	38,007	0.20	0.44
Late stage	129,492	0.11	0.33	91,485	0.12	0.35	38,007	0.08	0.28
Amount (log(1+$mn))	129,492	0.55	1.12	91,485	0.61	1.16	38,007	0.39	0.98
Time between deals (year))	33,233	0.47	1.06	26,362	0.48	1.06	6,871	0.42	1.04
Panel D: Exits									
IPO	129,492	0.006	0.079	91,485	0.007	0.083	38,007	0.005	0.069
IPO or merger	129,492	0.009	0.096	91,485	0.010	0.099	38,007	0.008	0.088
Trade sale	129,492	0.030	0.171	91,485	0.034	0.181	38,007	0.022	0.146
Failure	129,492	0.040	0.195	91,485	0.044	0.205	38,007	0.029	0.168

NOTE—This table provides summary statistics for observations at the firm-year level. Separate statistics are provided for the full sample, for the treated sample, and for the control sample. 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 III.

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Table 3

Within-industry Investment by Venture Capital (VC) Investors

Panel A: Within-industry investment	(1)	(2)	(3)	(4)	(5)
Treat	0.013				
	(1.19)				
Treat × Post	0.057	0.088	0.078	0.099	0.094
	(5.39)***	(3.07)***	(2.58)***	(5.65)***	(5.41)***
Treat × Post after five years			0.023	0.013	0.014
			(1.63)	(1.80)*	(1.78)*
Adjusted R^2	2.3%	8.7%	8.7%	8.7%	9.0%
Panel B: Extensive margin					
Treat	0.005				
	(1.41)				
Treat × Post	0.012	0.024	0.023	0.028	0.026
	(4.02)***	(2.80)***	(2.68)***	(4.89)***	(4.62)***
Treat × Post after five years			0.002	-0.001	0.001
			(0.36)	(0.15)	(0.22)
Adjusted R^2	0.4%	3.4%	3.4%	3.2%	3.3%
Panel C: Intensive margin					
Treat	0.008				
	(0.92)				
Treat × Post	0.048	0.067	0.057	0.073	0.070
	(5.30)***	(3.07)***	(2.49)**	(5.70)***	(5.47)***
Treat × Post after five years			0.022	0.015	0.015
			(2.27)**	(3.21)***	(2.85)***
Adjusted R^2	2.0%	8.6%	8.6%	8.6%	8.9%
Year fixed effects	Yes	Yes	Yes	No	No
Firm fixed effects	No	Yes	Yes	Yes	Yes
Headquarter-state-by-year fixed effects	No	No	No	Yes	Yes
Industry-by-year fixed effects	No	No	No	No	Yes
Number of observations	129,492	129,492	129,492	128,631	128,490
Number of unique firms	13,428	13,428	13,428	13,337	13,320

Note—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 firm receives financing from a VC with a previous investment in the same industry. In Panel B, the dependent variable is an indicator variable for whether the firm receives VC investment from a VC fund that already invested in a startup in the same industry (extensive margin), and in Panel C, the dependent variable is an indicator variable for whether the firm receives VC investment in later rounds from the same VC fund that already invested in the firm (intensive margin). In all panels, industry is defined based on Prequin's primary industry classification adjusted to account for similar industries. 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.

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Table 4

Directorships for Venture Capital (VC) Investors

Panel A: VC directors	(1)	(2)	(3)	(4)	(5)
Treat	0.164				
	(1.80)*				
Treat × Post	0.250	0.259	0.042	-0.002	0.010
	(2.98)***	(1.85)*	(0.37)	(0.02)	(0.09)
Treat × Post after five years			0.452	0.530	0.489
			(4.38)***	(7.27)***	(7.01)***
Adjusted R^2	13.2%	69.2%	69.4%	69.7%	70.1%
Panel B: Additional directorships held					
Treat	0.071				
	(0.73)				
Treat × Post	0.441	0.352	0.102	0.134	0.096
	(4.88)***	(2.62)***	(1.18)	(2.18)**	(1.38)
Treat × Post after five years			0.521	0.585	0.585
			(5.62)***	(6.64)***	(6.64)***
Adjusted R^2	16.4%	61.0%	61.0%	61.4%	62.1%
Panel C: Within-industry directorships held					
Treat	0.046				
	(1.49)				
Treat × Post	0.149	0.130	0.038	0.028	0.009
	(5.22)***	(2.84)***	(1.71)*	(1.66)*	(0.41)
Treat × Post after five years			0.192	0.232	0.215
			(5.64)***	(8.21)***	(7.15)***
Adjusted R^2	8.6%	59.6%	59.6%	59.8%	63.1%
Year fixed effects	Yes	Yes	Yes	No	No
Firm fixed effects	No	Yes	Yes	Yes	Yes
Headquarter-state-by-year fixed effects	No	No	No	Yes	Yes
Industry-by-year fixed effects	No	No	No	No	Yes
Number of observations	95,188	95,188	95,188	94,614	94,520
Number of unique firms	9,572	9,572	9,572	9,519	9,507

Note—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. 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.

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Table 5

Venture Capital (VC) Deals

Panel A: VC deal volume	(1)	(2)	(3)	(4)	(5)
Treat	0.039				
	(2.53)**				
Treat × Post	0.059	0.122	0.111	0.142	0.137
	(4.58)***	(2.63)***	(2.21)**	(4.59)***	(4.42)***
Treat × Post after five years			0.025	0.011	0.014
			(0.98)	(0.74)	(0.95)
Adjusted R^2	2.1%	7.4%	7.4%	7.5%	8.0%
Panel B: Late-round VC deal volume					
Treat	0.003				
	(0.40)				
Treat × Post	0.041	0.041	0.038	0.052	0.049
	(5.89)***	(3.32)***	(2.48)**	(6.51)***	(6.46)***
Treat × Post after five years			0.005	-0.003	-0.002
			(0.59)	(0.25)	(0.14)
Adjusted R^2	0.9%	4.2%	4.2%	4.1%	4.4%
Panel C: Log(1+deal value)					
Treat	0.052				
	(1.17)				
Treat × Post	0.151	0.241	0.222	0.283	0.266
	(3.64)***	(3.01)***	(2.49)**	(5.44)***	(5.27)***
Treat × Post after five years			0.041	0.017	0.025
			(1.03)	(0.78)	(1.15)
Adjusted R^2	1.9%	5.5%	5.5%	5.6%	6.2%
Year fixed effects	Yes	Yes	Yes	No	No
Firm fixed effects	No	Yes	Yes	Yes	Yes
Headquarter-state-by-year fixed effects	No	No	No	Yes	Yes
Industry-by-year fixed effects	No	No	No	No	Yes
Number of observations	129,492	129,492	129,492	128,631	128,490
Number of unique firms	13,428	13,428	13,428	13,337	13,320

Note—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 log of one plus the deal value in millions of 2010 dollars. 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.

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Table 6

Venture Capital (VC) Funds' Return Distributions

Dependent variable = return quartile, where 4 indicates best returns	(1)
Percent of portfolio companies that are treated	0.464
	(4.98)***
Year fixed effects	Yes
Adjusted R^2	1.9%
Number of observations	3,452
Number of unique VCs	381

Note—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 ownership is proxied for using the percentage of portfolio companies that are treated. Below the coefficient estimates are test statistics from robust standard errors. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

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Table 7

Entrepreneurial Firm Exits

Panel A: IPO					
Treat	0.005	0.005	0.002	0.002	0.002
	(2.01)**	(2.00) **	(2.01)**	(3.17)***	(3.08)***
Treat × Post	-0.001	-0.001	-0.001	-0.002	-0.003
	(0.33)	(0.31)	(0.53)	(1.22)	(1.41)
Treat × Post after five years		-0.000	-0.000	-0.000	-0.000
		(0.17)	(0.05)	(0.42)	(0.62)
Adjusted R^2	0.3%	0.3%	1.4%	0.3%	1.3%
Panel B: IPO or merger					
Treat	0.005	0.005	0.005	0.007	0.007
	(1.91)*	(1.92) *	(2.02)**	(3.34)***	(3.25)***
Treat × Post	-0.003	-0.001	-0.002	-0.003	-0.003
	(1.07)	(0.61)	(0.86)	(1.50)	(1.70)*
Treat × Post after five years		-0.002	-0.002	-0.003	-0.003
		(1.56)	(2.01)**	(3.19)***	(3.79)***
Adjusted R^2	0.2%	0.2%	1.2%	0.3%	1.2%
Panel C: Trade sale					
Treat	0.001	0.001	0.002	0.000	0.001
	(0.51)	(0.49)	(0.74)	(0.18)	(0.43)
Treat × Post	0.004	0.004	0.004	0.006	0.005
	(2.43)**	(3.12)***	(2.65)***	(3.89)***	(3.49)***
Treat × Post after five years		0.000	0.001	-0.000	-0.000
		(0.13)	(0.23)	(0.20)	(0.01)
Adjusted R^2	0.3%	0.3%	0.5%	0.1%	0.3%
Panel D: Failure					
Treat	-0.001	-0.001	0.000	-0.003	-0.002
	(0.59)	(0.26)	(0.07)	(1.39)	(1.14)
Treat × Post	-0.005	0.004	0.004	0.002	0.002
	(1.76)*	(1.06)	(1.13)	(0.69)	(0.68)
Treat × Post after five years		-0.013	-0.013	-0.009	-0.008
		(2.50)**	(2.40)**	(2.43)**	(2.33)**
Adjusted R^2	1.4%	1.4%	1.8%	1.5%	1.9%
Year fixed effects	Yes	Yes	No	No	No
Industry-by-year fixed effects	No	No	Yes	No	Yes
Headquarter-state-by-year fixed effects	No	No	No	Yes	Yes
Number of observations	129,492	129,492	129,492	128,631	128,490
Number of unique firms	13,428	13,428	13,428	13,337	13,320

Note—This table presents the results for startup exits that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). The dependent variable in Panel A is an indicator for whether a firm undergoes an IPO, the dependent variable in Panel B is an indicator for whether a firm undergoes an IPO or is acquired, in Panel C the dependent variable is an indicator for a trade sale, and in Panel D the dependent variable is an indicator for whether a firm fails. 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.

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Table 8

Initial Public Offering (IPO) Valuation

Panel A: $\left(\frac{P}{V}\right)_{Sales}$	(1)
Treat	0.018
	(5.03)***
Treat × Post	0.011
	(2.43)**
Adjusted R^2	2.6%
Number of observations	563
Panel B: $\left(\frac{P}{V}\right)_{EBITDA}$	
Treat	-0.019
	(1.66)*
Treat × Post	0.024
	(1.41)
Adjusted R^2	2.3%
Number of observations	686
Panel C: $\left(\frac{P}{V}\right)_{Earnings}$	
Treat	0.054
	(0.38)
Treat × Post	0.330
	(2.10)**
Adjusted R^2	0.1%
Year fixed effects	Yes
Number of observations	686

Note—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 by Purnanandam and Swaminathan (2004) 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. 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.

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Appendix A. Internet Appendix

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Table A.1

Summary statistics on State of Incorporation and Reincorporations

	(1)	(2)	(3)	(4)
Panel A: Full sample	Observations	Percent	Unique firms	Percent
Delaware	85,868	66.3%	9,729	72.4%
Non-Delaware treated states	9,833	7.6%	924	6.9%
Control states	33,791	26.1%	2,775	20.7%
Total	129,492	100.0%	13,428	100.0%
Panel B: Reincorporations from				
Delaware	1,700	9.7%	155	12.0%
Non-Delaware treated states	2,272	13.0%	178	13.8%
Control states	13,519	77.3%	958	74.2%
Total	17,491	100.0%	1,291	100.0%
Panel C: Reincorporations into				
Delaware	15,149	86.6%	1,100	85.2%
Non-Delaware treated states	736	4.2%	59	4.6%
California	506	2.9%	44	3.4%
Control states	1,100	6.3%	88	6.8%
Total	17,491	100.0%	1,291	100.0%

Note–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.

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Table A.2

Time Between Venture Capital (VC) Deals

	Dependent variable = Time between deals (years)				
	(1)	(2)	(3)	(4)	(5)
Treat	0.035				
	(2.53)**				
Treat × Post	-0.081	-0.047	-0.045	-0.028	-0.042
	(6.61)***	(0.55)	(0.51)	(0.43)	(0.67)
Treat × Post after five years			-0.003	0.011	0.018
			(0.05)	(0.22)	(0.39)
Year fixed effects	Yes	Yes	Yes	No	No
Firm fixed effects	No	Yes	Yes	Yes	Yes
Headquarter-state-by-year fixed effects	No	No	No	Yes	Yes
Industry-by-year fixed effects	No	No	No	No	Yes
Adjusted R^2	1.3%	15.2%	15.2%	15.8%	16.0%
Number of observations	33,233	29,306	29,306	28,999	28,968
Number of unique firms	13,428	9,501	9,501	9,455	9,446

Note—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. 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.

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Table A.3

Robustness Test: Waivers from the Duty of Loyalty

	(1)	(2)	(3)
Panel A: Within-industry investment by VCs	Within-industry	Extensive margin	Intensive margin
Treat × Post	0.085	0.022	0.066
	(2.60)***	(2.69)***	(2.48)**
Adjusted R^2	8.6%	3.4%	8.6%
Number of observations	129,492	129,492	129,492
Number of unique firms	13,428	13,428	13,428
Panel B: VC directorships	VC directors	Other directorships held	Within-industry directorships held
Treat × Post	0.300	0.417	0.140
	(4.43)***	(2.19)**	(2.02)**
Adjusted R^2	69.3%	61.0%	59.6%
Number of observations	95,188	95,188	95,188
Number of unique firms	9,572	9,572	9,572
Panel C: VC deals	Deal volume	Late round	Deal size
Treat × Post	0.120	0.043	0.242
	(2.35)**	(2.58)***	(2.41)**
Adjusted R^2	7.4%	4.2%	5.4%
Number of observations	129,492	129,492	129,492
Number of unique firms	13,428	13,428	13,428
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes

Note—This table presents a robustness test for our natural experiment that incorporates additional institutional and legal details that can 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). In panels A, B, and C, the dependent variables are the same as in Tables 3, 4, and 5, respectively. 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.

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Table A.4

Robustness Test: Nevada

	(1)	(2)	(3)
Panel A: Within-industry investment by VCs	Within-industry	Extensive margin	Intensive margin
Treat × Post	0.089	0.024	0.068
	(3.27)***	(3.75)***	(3.03)***
Adjusted R^2	8.7%	3.4%	8.6%
Number of observations	129,492	129,492	129,492
Number of unique firms	13,428	13,428	13,428
Panel B: VC directorships	VC directors	Other directorships held	Within-industry directorships held
Treat × Post	0.276	0.354	0.133
	(3.36)***	(1.84)*	(1.94)*
Adjusted R^2	72.4%	61.0%	59.6%
Number of observations	95,188	95,188	95,188
Number of unique firms	9,572	9,572	9,572
Panel C: VC deals	Deal volume	Late round	Deal size
Treat × Post	0.124	0.041	0.244
	(3.00)***	(2.73)***	(2.79)***
Adjusted R^2	7.4%	4.2%	5.5%
Number of observations	129,492	129,492	129,492
Number of unique firms	13,428	13,428	13,428
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes

Note—This table presents a robustness test for our natural experiment that incorporates additional institutional and legal details that can 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. In Panels A, B, and C, the dependent variables are the same as in Tables 3, 4, and 5, respectively. 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.

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Table A.5

Robustness Test: Washington

	(1)	(2)	(3)
Panel A: Within-industry investment by VCs	Same industry	Extensive margin	Intensive margin
Treat × Post	0.093	0.025	0.070
	(3.40)***	(3.82)***	(3.08)***
Adjusted R^2	8.7%	3.4%	8.6%
Number of observations	129,492	129,492	129,492
Number of unique firms	13,428	13,428	13,428
Panel B: VC directorships	VC directors	Other directorships held	Within-industry directorships held
Treat × Post	0.286	0.384	0.140
	(3.94)***	(2.02)**	(2.06)**
Adjusted R^2	69.3%	61.0%	59.6%
Number of observations	95,188	95,188	95,188
Number of unique firms	9,572	9,572	9,572
Panel C: VC Deals	Deal volume	Late round	Deal size
Treat × Post	0.129	0.045	0.259
	(3.07)***	(3.19)***	(2.99)***
Adjusted R^2	7.4%	4.2%	5.5%
Number of observations	129,492	129,492	129,492
Number of unique firms	13,428	13,428	13,428
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes

Note—This table presents a robustness test for our natural experiment that incorporates additional institutional and legal details that can 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. In Panels A, B, and C, the dependent variables are the same as in in Tables 3, 4, and 5, respectively. 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.

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Table A.6

Robustness Test: Excluding Delaware

	(1)	(2)	(3)
Panel A: Within-industry investment by VCs	Within-industry	Extensive margin	Intensive margin
Treat × Post	0.100	0.029	0.075
	(3.02)***	(4.78)***	(2.57)**
Adjusted R^2	6.4%	1.6%	6.9%
Number of observations	43,624	43,624	43,624
Number of unique firms	3,699	3,699	3,699
Panel B: VC directorships	VC directors	Other directorships held	Within-industry directorships held
Treat × Post	0.522	0.612	0.243
	(7.74)***	(3.49)***	(4.14)***
Adjusted R^2	67.9%	55.2%	53.5%
Number of director observations	29,730	29,730	29,730
Number of unique firms	2,401	2,401	2,401
Panel C: VC deals	Deal volume	Late round	Deal size
Treat × Post	0.141	0.052	0.307
	(2.94)***	(3.00)***	(3.25)***
Adjusted R^2	4.9%	4.3%	4.4%
Number of observations	43,624	43,624	43,624
Number of unique firms	3,699	3,699	3,699
Firm fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Excludes Delaware	Yes	Yes	Yes

Note—This table presents the results from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). In Panels A, B, and C, the dependent variables are the same as in Tables 3, 4, and 5, respectively. 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.

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Table A.7

Summary Statistics: Sample of LLCs

	All			Treated			Control		
	N	Mean	St. Dev.	N	Mean	St. Dev.	N	Mean	St. Dev.
Panel A: VC investment									
Within-industry investment	571,859	0.07	0.25	381,078	0.09	0.29	190,781	0.01	0.13
Intensive margin investment	571,859	0.05	0.22	381,078	0.07	0.26	190,781	0.01	0.11
Extensive margin investment	571,859	0.03	0.18	381,078	0.05	0.21	190,781	0.01	0.10
Panel B: VC Directors									
VC directorships	298,431	0.67	1.49	200,064	0.94	1.69	98,367	0.12	0.64
Additional directorships	298,431	1.07	2.81	200,064	1.52	3.26	98,367	0.17	1.13
Additional within-industry directorships	298,431	0.25	0.86	200,064	0.35	1.01	98,367	0.04	0.32
Panel C: VC Deals									
Count	571,859	0.26	1.12	381,078	0.35	1.29	190,781	0.07	0.63
Late stage	571,859	0.09	0.69	381,078	0.13	0.80	190,781	0.03	0.38
Amount (log(1+$mn))	562,047	0.20	0.83	372,278	0.26	0.95	189,769	0.06	0.49
Time between deals	49,032	0.84	1.28	44,172	0.87	1.29	4,860	0.59	1.10

Note—This table provides summary statistics for observations at the firm-year level. Separate statistics are provided for the full sample, for the treated sample, and for the control sample. Treatment is defined as the first adoption of state legislation permitting corporate opportunity waivers (COWs), and the control group comprises limited liability company (LLCs) startups. Deal amounts are in millions of dollars and are inflation-adjusted. All variables are described in Section III.

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Table A.8

Robustness Test of Within-industry Investment: Sample of LLCs

Panel A: Within-industry investment	(1)	(2)	(3)	(4)	(5)
Treat	0.004				
	(7.59)***				
Treat × Post	0.024	0.027	0.014	0.015	0.015
	(16.06)***	(17.31)***	(9.84)***	(10.16)***	(10.16)***
Treat × Post after five years			0.019	0.020	0.020
			(8.34)***	(8.61)***	(8.62)***
Adjusted R^2	5.9%	8.1%	8.1%	8.2%	8.2%
Panel B: Extensive margin					
Treat	0.002				
	(4.97)***				
Treat × Post	0.011	0.013	0.007	0.008	0.008
	(9.60)***	(10.94)***	(6.90)***	(7.17)***	(7.17)***
Treat × Post after five years			0.007	0.008	0.008
			(4.43)***	(4.51)***	(4.52)***
Adjusted R^2	3.3%	3.5%	3.5%	3.6%	3.6%
Panel C: Intensive margin					
Treat	0.003				
	(7.32)***				
Treat × Post	0.025	0.027	0.012	0.012	0.013
	(18.93)***	(19.48)***	(9.43)***	(9.68)***	(9.69)***
Treat × Post after five years			0.021	0.022	0.023
			(10.96)***	(11.33)***	(11.34)***
Adjusted R^2	4.7%	7.8%	7.8%	7.9%	7.9%
Year fixed effects	Yes	Yes	Yes	No	No
Firm fixed effects	No	Yes	Yes	Yes	Yes
Headquarter-state-by-year fixed effects	No	No	No	Yes	Yes
Industry-by-year fixed effects	No	No	No	No	Yes
Number of observations	571,859	571,852	571,852	552,095	551,280
Number of unique firms	26,502	26,495	26,495	25,597	25,560

Note—This table presents the results from a single event difference-in-differences regression design. The event is defined as the first adoption of state legislation permitting corporate opportunity waivers (COWs). The control group comprises LLC startups, and the treated group is made up of all other startups. In Panel A, the dependent variable is an indicator variable for whether a startup firm receives financing from a VC with a previous investment in the same industry. In Panel B, the dependent variable is an indicator variable for whether the firm receives VC investment from a VC fund that already invested in a startup in the same industry (extensive margin), and in Panel C, the dependent variable is an indicator variable for whether the firm receives VC investment in later rounds from the same VC fund that already invested in the firm (intensive margin). In all panels, industry is defined based on Prequin's primary industry classification adjusted to account for similar industries. Below the coefficient estimates are test statistics from robust standard errors. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

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Table A.9

Robustness Test of Venture Capital (VC) Directorships: Sample of LLCs

Panel A: VC directorships	(1)	(2)	(3)	(4)	(5)
Treat	0.014				
	(7.53)***				
Treat × Post	0.305	0.327	0.082	0.082	0.082
	(27.03)***	(24.67)***	(5.51)***	(5.45)***	(5.45)***
Treat × Post after five years			0.359	0.364	0.364
			(23.79)***	(23.93)***	(23.92)***
Adjusted R^2	22.2%	51.4%	51.5%	51.5%	51.6%
Panel B: Additional directorships					
Treat	0.010				
	(3.77)***				
Treat × Post	0.596	0.616	0.087	0.094	0.094
	(28.39)***	(26.68)***	(3.99)***	(4.30)***	(4.30)***
Treat × Post after five years			0.775	0.765	0.765
			(26.38)***	(25.86)***	(25.82)***
Adjusted R^2	23.1%	38.6%	38.6%	38.6%	38.6% *Panel C: Within-industry directorships*
Treat	0.003				
	(5.39)***				
Treat × Post	0.160	0.168	0.025	0.025	0.026
	(25.55)***	(24.09)***	(3.78)***	(3.85)***	(3.85)***
Treat × Post after five years			0.209	0.208	0.209
			(23.47)***	(23.26)***	(23.28)***
Adjusted R^2	13.4%	32.5%	32.6%	32.6%	32.6%
Year fixed effects	Yes	Yes	Yes	No	No
Firm fixed effects	No	Yes	Yes	Yes	Yes
Headquarter-state-by-year fixed effects	No	No	No	Yes	Yes
Industry-by-year fixed effects	No	No	No	No	Yes
Number of observations	298,431	298,430	298,430	294,586	294,180
Number of unique firms	13,820	13,819	13,819	13,641	13,623

Note—This table presents the results from a single event difference-in-differences regression design. The event is defined as the first adoption of state legislation permitting corporate opportunity waivers (COWs). The control group comprises LLC startups, and the treated group is made up of all other startups. 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. Industry is defined based on Prequin's primary industry classification adjusted to account for similar industries. Below the coefficient estimates are test statistics from robust standard errors. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

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Table A.10

Robustness Test of Venture Capital (VC) Deals: Sample of LLCs

Panel A: VC deal volume	(1)	(2)	(3)	(4)	(5)
Treat	0.022				
	(6.55)***				
Treat × Post	0.084	0.100	0.056	0.058	0.058
	(11.64)***	(13.81)***	(8.14)***	(8.27)***	(8.28)***
Treat × Post after five years			0.063	0.067	0.067
			(6.21)***	(6.53)***	(6.53)***
Adjusted R^2	4.6%	7.4%	7.4%	7.5%	7.6%
Number of observations	571,859	571,852	571,852	552,095	551,280
Number of unique firms	26,502	26,495	26,495	25,597	25,560
Panel B: Late-round VC deal volume					
Treat	0.009				
	(3.31)***				
Treat × Post	0.057	0.065	0.037	0.038	0.038
	(13.67)***	(15.72)***	(7.12)***	(7.21)***	(7.21)***
Treat × Post after five years			0.040	0.043	0.043
			(6.94)***	(7.42)***	(7.42)***
Adjusted R^2	0.9%	5.9%	5.9%	5.9%	5.9%
Number of observations	571,859	571,852	571,852	552,095	551,280
Number of unique firms	26,502	26,495	26,495	25,597	25,560
Panel C: Log(1+deal value)					
Treat	0.019				
	(7.03)***				
Treat × Post	0.042	0.055	0.048	0.050	0.050
	(7.64)***	(9.82)***	(8.11)***	(8.33)***	(8.34)***
Treat × Post after five years			0.010	0.017	0.017
			(1.31)	(2.11)**	(2.10)**
Adjusted R^2	4.1%	9.3%	9.3%	9.4%	9.4%
Year fixed effects	Yes	Yes	Yes	No	No
Firm fixed effects	No	Yes	Yes	Yes	Yes
Headquarter-state-by-year fixed effects	No	No	No	Yes	Yes
Industry-by-year fixed effects	No	No	No	No	Yes
Number of observations	562,047	562,037	562,037	542,776	541,984
Number of unique firms	26,500	26,490	26,490	25,592	25,555

Note—This table presents the results from a single event difference-in-differences regression design. The event is defined as the first adoption of state legislation permitting corporate opportunity waivers (COWs). The control group comprises LLC startups, and the treated group is made up of all other startups. 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 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 log of one plus the deal value in millions of 2010 dollars. Below the coefficient estimates are test statistics from robust standard errors. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

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