Common Ownership and Startup Growth*

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ABSTRACT

Many startups are commonly-held by the same venture capital (VC) investors. We exploit the staggered introduction of liability exemptions when investors hold stakes in conflicting business opportunities as a shock to common ownership. We find increases in common ownership and directors serving on rivals' boards after the law changes. Despite the potential for rent-extraction, commonly-held startups benefit by raising more capital through more investment rounds. Moreover, they are less likely to fail and exit more successfully through IPOs or acquisitions by another commonly-held startup. These successful startup outcomes are linked to VC directors serving on other startup boards.

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Common ownership of firms that compete with one another is increasingly the norm in many industries. From a theoretical standpoint, common ownership is a crucial issue because it could undermine firms' incentives to compete with one another (Bresnahan and Salop, 1986; O'Brien and Salop, 2000; Azar, 2017) or improve outcomes through large informational spillovers (Lopez and Vives, 2019). The literature on common ownership has mostly focused on institutional ownership of public corporations (He and Huang, 2017; Azar et al., 2018; Gilje et al., 2019; Lewellen and Lowry, 2019). In this study, we examine common ownership of startups by venture capital ("VC") investors, and its impact on startup growth.

There are several reasons why common ownership of startups is likely to be consequential. First, there is a clear mechanism through which VC investors can influence management. VC investors typically have substantial control rights (Kaplan and Stromberg, 2001; Gompers et al., 2020), and they play an active role in startups' management through appointment of board representatives (Amornsiripanitch et al., 2019). By comparison, a mechanism through which passive, arms-length institutional investors may influence public firms is more difficult to observe (Anton et al., 2018). Second, common ownership of startups by VC investors is pervasive. Even startups that directly compete against one another, such as Uber and Lyft, often raise capital from the same VC investors.[1] Finally, more broadly, startups play a vital role in generating economic growth, innovation and productivity gains (Aghion and Howitt, 1992; Foster, Haltiwanger, and Syverson, 2008), and the VC industry disproportionately impacts the economy given its size (Kortum and Lerner, 2000; Gompers and Lerner, 2000, 2004; Kerr, Nanda, and Rhodes-Kropf, 2014a).

What happens to startups when the same VC investor holds stakes in multiple competing startups? On the one hand, VC investors could play favorites, killing off one startup so another can succeed or diverting valuable competitive information from one startup to another (Fried and Ganor, 2006; Fisch, forthcoming; Pollman, forthcoming). To use an example from a recent court case, a common investor might use confidential information acquired through a board representative to benefit another company in its portfolio.[2] On the other hand, VC investors can act as incubators

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

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

for valuable information and expertise. The expertise acquired through common ownership could benefit all portfolio companies as well as maximize VC investors' returns. While startups may learn from one another via observable events (Kostovetsky and Manconi, 2020; Yang, 2020), a common VC investor would be privy to hard-to-observe information.[3] Thus, common ownership could improve prospects for startups, for example, through innovation spillovers (Gonzalez-Uribe, 2020).

We evaluate the economic consequences of common ownership for startups using a quasi-natural experiment: the staggered adoption of laws across eight states from 2000 to 2016 that enable corporations incorporated in these states to adopt corporate opportunity waivers (COWs). These waivers exempt investors and directors from litigation risk if they usurp a business opportunity in a way that conflicts with the firm's best interest (Talley, 1998; Rauterberg and Talley, 2017; Licht, 2018; Velasco, 2018). VC investments are typically accompanied by a board seat for a VC investor (Kaplan and Stromberg, 2003; Fried and Ganor, 2006; Bengtsson and Sensoy, 2015). These VC board members are privy to information that may bolster their expertise and effectiveness in managing the startups in their portfolios, but could also expose them to liability if it enables them to divert business opportunities from one startup to another. COWs are necessary to relieve VC investors and their director appointees from such potential liability. Since the passage of the legal changes, adoptions of COWs have been standard practice in the VC industry given the high likelihood of conflicts due to common ownership and the potential harmful impact of litigation on VCs' reputations (Atanasov et al., 2012).

The variation in liability risk generated by the staggered adoption of COWs is arguably exogenous. The impetus for the state law changes was to eliminate uncertainty arising from case law, and there was no apparent evidence of lobbying prior to the passage of these laws. Moreover, unlike other law changes that may affect the scope of managerial discretion in multiple ways (e.g., constituency statutes), these law changes had a very narrow application to the liability that arises from commonly owning competing ventures (see Section 2 and Appendix A). Moreover, because

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

these laws apply at the state of incorporation level, we can compare startups that are headquartered in the same state but are subject to different legislation, and in this way control for potential economic shocks in entrepreneurial hubs that could affect common ownership.

To test the importance of the legal changes for common ownership, we use a difference-in-differences estimator with the staggered adoption of the state laws permitting COWs serving as the treatment. In order to implement this strategy, we construct a 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 startups. To the best of our knowledge, this is the most comprehensive dataset documenting the state of incorporation for private firms, and it represents virtually the full value of VC investments in the sample period.

Using this dataset, we find that on average, startups incorporated in treated states are 11.8 percentage points more likely to have a within-industry common owner after the law change. In comparison, prior to the first law passage in Delaware, only 10% of startups in Delaware had a common owner. These results hold across a variety of specifications including ones with startup, industry-by-year and headquarter-state-by-year fixed effects as well as controls for startup growth. In addition, when we evaluate year-by-year coefficient estimates, we find evidence consistent with the parallel trends assumption. Moreover, the results are robust to (i) limiting the analysis to startups incorporated in states with entrepreneurial ecosystems, (ii) employing matching estimators based on pre-treatment characteristics of startups, such as age and capital raised, (iii) excluding startups initially incorporated in Delaware, where a majority of startups are incorporated, (iv) placebo tests using different event dates for treated states, (v) placebo tests removing actual treated states and using false treated states, (vi) excluding different time periods, such as the dotcom bubble and financial crisis, (vii) excluding industries where the "spray and pray" VC investment strategy is common (Ewens et al., 2018), and (viii) restricting the analysis to non-Delaware startups incorporated outside their headquarter state. Taken together, the evidence indicates that COW legislation is followed by a significant and sudden shift in common ownership by VC investors.

We examine the potential consequences of common ownership on startup outcomes using the legal changes as an instrumental variable (IV) for common ownership. For the IV to be valid, it needs to satisfy the exclusion restriction, that is, conditional on startup characteristics, the legal changes are related to startup outcomes only through common ownership. We show such a relationship by extending the difference-in-difference analysis to startup outcomes (see Appendix F), which is the reduced form for any regression that uses the law changes as IV. While the reduced-form analysis shows that the IV significantly explains startup growth, it may also affect startup growth through other channels. To address this concern, following Angrist et al. (2010), we show that for a subsample in which the IV does not affect common ownership, the IV is not significantly associated with startup outcomes (see Appendix E). These placebo tests indicate that the legal changes do not affect outcomes other than through common ownership. The validity of the exclusion restriction is further supported by our legal analysis. The laws that permit COWs specifically target the use of valuable business information in other ventures and have no effect whatsoever on other aspects of investors' and directors' legal duties. Thus, the exclusion restriction is likely satisfied because the legal changes apply narrowly to the risk of liability that would result from common ownership.

Using the IV approach, we first examine the association between common ownership and VC director networks. We hypothesize that directors are the key mechanism through which information can be exchanged and coordination among commonly held startups can occur. We find evidence that startups with a common VC owner have 1.8 more VC directors on their boards, on average. Further, we find that the VC directors who sit on the boards of the commonly held startups also have thicker networks, meaning that they sit on the boards of multiple startups, especially same-industry startups.

Next, we examine startup growth. We find that common ownership is associated with greater likelihood of receiving an additional round of VC funding. Having a common VC owner is associated with raising 0.99 additional rounds of financing. This estimate represents a meaningful growth given that the median startup in our sample raises only two rounds of financing. The point estimate in the IV regression is about 2.1 times that of the endogenous OLS estimate. This is plausible as common

ownership likely involves expert VCs investing in more risky startups (Nanda and Rhodes-Kropf, 2013), and therefore the endogenous OLS estimates are likely negatively biased.

While additional VC funding suggests that common ownership creates value for all startups, the positive relationship is also potentially consistent with VC funds with greater common ownership maximizing their overall portfolio returns at the expense of individual startups' success. Indeed, we do find that VC funds with greater common ownership significantly outperform their benchmark index. To disentangle value creation from value shifting, we evaluate the effects of common ownership on startup exits. Our findings suggest that common ownership generates real benefits for startups. Specifically, we find that common ownership is associated with (i) a higher probability of an exit through an IPO, (ii) higher valuations when startups undergo IPOs, (iii) a higher probability of trade sale, (iv) a higher probability of a sale to a commonly held firm, and (v) a lower probability of failure. This fact pattern is consistent with common ownership by VC investors creating value for startups rather than advantaging one startup at the expense of another. These results suggest that common ownership enables startups to increase profits through the sharing of valuable information, and efficient allocation of opportunities among startups due to accumulated expertise.

We further link the results on director networks to startup growth and exits. If cross-appointment of directors is the channel through which information flows benefit commonly held startups, we would expect startup outcomes to be stronger for commonly held startups with thicker director networks. Thus, we re-run the startup growth and exit tests for different subsamples of startups: those without a VC director, those with a VC director, and those with a connected VC director that sits on the board of at least one additional startup. The evidence is consistent with little to no effect on startup growth and exits for startups without a VC director but increasing effects for those with VC directors and well-connected ones. This is consistent with the idea that information flows through VC directors drive positive outcomes for startups.

We also reconcile our findings with research from the pharmaceutical industry showing that common ownership inhibits startup growth (Li et al., 2020). We conduct an analysis for industries with strong intellectual property (IP) protection, such as pharmaceuticals. These industries have

high experimentation costs and rely on well-proven business models, and therefore the benefits from information spillovers might be limited. We find that the benefits associated with common ownership in high IP industries, such as raising additional financing and lower failure rates, are weaker than in other industries, though the probability of IPOs is higher. This suggests that in high IP industries, VC investors are more inclined to pursue a winner-take-all strategy for maximizing returns across commonly held startups. Thus, the lower benefits of common ownership in the pharmaceutical industry are consistent with a dominant positive relationship between common ownership and startup growth in other industries, and averaged across all industries.

To conclude, we examine the robustness of our findings in a series of additional tests. All the IV results are robust to alternative specifications, including evaluating subsamples in high entrepreneurial states, non-Delaware startups, non-Delaware startups incorporated in a different state than their headquarter state, excluding "spray and pray" industries, the bursting of the dotcom bubble, and the financial crisis. Moreover, though we have found qualitative and quantitative evidence that the exclusion restriction for our IV holds, we also consider sensitivity analyses (Conley et al., 2012) that show that even if the exclusion restriction is partially violated, our IV estimates still remain qualitatively similar (see Appendix E). Finally, we show that our results are robust to nuances in the legal interpretation of the state law changes (see Appendix A).

1. Contribution to the Literature and Hypothesis Development

1.1. Contribution to the Literature

Our study contributes to the literature on common ownership by extending it to the VC industry and startup governance. Theoretical research on common ownership predicts that it can be anticompetitive (Bresnahan and Salop, 1986; Gilo et al., 2006), and recent empirical studies support anticompetitive effects (He and Huang, 2017; Azar et al., 2018, 2020). Yet subsequent re-

search questions both the robustness of these findings and the logic of the conclusions (Backus et al., 2019; Dennis et al., 2019; Gilje et al., 2019; Lewellen and Lowry, 2019; Koch et al., 2020; Backus et al., 2020). One concern is that researchers have yet to uncover a clear channel through which common owners could influence firm policy (Anton, Ederer, Giné, and Schmalz, 2018; Hemphill and Kahan, 2018). A related concern is that theoretically, common ownership could improve social welfare (Kerr et al., 2014b; Lopez and Vives, 2019), for example through innovation gains (Allen and Phillips, 2002; Kostovetsky and Manconi, 2020; Anton et al., 2020; Borochin et al., 2020).

Our study speaks to both of these issues. First, we find that a key mechanism through which common ownership is linked to market competition is the cross-appointment of directors, thus providing evidence of an active channel through which common owners can influence firm policy. This finding complements a rich literature that shows that directors add value to firms (Adams, 2017) through their expertise (Güner et al., 2008; Bouwman, 2011; Dass et al., 2014; Barzuza and Curtis, 2017), social connections (Fracassi and Tate, 2012), reputations (Fich and Shivdasani, 2006; Fahlenbrach et al., 2010) and diversity (Adams and Ferreira, 2009; Bernile et al., 2018; Masulis et al., 2012; Knyazeva et al., 2013). Second, we provide detailed evidence supporting a relationship between common ownership and startup financing and exits. Our study complements earlier research documenting strategic alliances and innovation spillovers among startups owned by the same VCs (Lindsey, 2008; Gonzalez-Uribe, 2020). In particular, Lindsey (2008) documents a higher IPO likelihood for startups in alliances with other portfolio startups, and Gonzalez-Uribe (2020) shows a higher rate of acquisitions with other portfolio startups. Our findings also accommodate recent work that suggests that VCs may stifle competition among pharmaceutical startups (Li et al., 2020) by showing that startup outcomes are weaker in industries with high intellectual property protection. Importantly, we contribute to the emerging literature on common ownership in the VC industry by linking the evidence on startup growth to VC director networks. This is consistent with the view that information flows among directors generate positive spillovers (Bouwman, 2011).

More broadly, we contribute to entrepreneurship research by identifying institutional features associated with the efficiency of capital allocation (Lerner, 2009; Acemoglu, Akcigit, Hanley, and Kerr, 2016; Howell, 2017). Our findings suggest 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, 2020) 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 et al., 2018; Gompers et al., 2020), especially studies emphasizing relationships (Hochberg, Ljungqvist, and Lu, 2007, 2010; Hochberg, Lindsey, and Westerfield, 2015).

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

1.2. Hypothesis Development

From a theoretical perspective, common ownership implies that VC investors may not seek to maximize the profits of each startup, but rather will maximize the aggregate returns on their joint holdings. In the VC industry, this could mean that VC investors will not necessarily use the traditional approach of intensive monitoring through board representatives, provision of value added services, and exercise of their control rights to help each of their startups to maximize value (Kerr et al., 2014b). Rather, VC investors may use their control rights and board appointees to acquire information about individual startups, and use that information in a way that maximizes their portfolio returns. To this end, VC investors could transfer knowledge or shift business opportunities from one startup to another, an approach consistent with favoritism and exploitation (Hellmann, 2002). Or, the VC investor could identify synergies among startups and opportunities for startups to coordinate their business.

While we have focused on the VC investors' incentives thus far, it is also important to consider why entrepreneurial founders would accept the risk that VC investors would appropriate information to benefit another startup when founders can control who their shareholders are. First, VC investors often back entrepreneurs who were not successful in their first endeavor, or bring them on as part of the leadership team for other business ventures (Kupor, 2019). This suggests that receiving VC financing benefits an entrepreneur with career concerns regardless of the success of his or her startup. Second, frictions in raising early-stage financing mean that the timing of such financing is important to an entrepreneur. Raising a round of capital earlier enables an entrepreneur to have first-mover advantage, which is especially important because entrepreneurial ideas are often not unique (Bhide, 2001; Hellman and Puri, 2000). Third, VC firms have strong reputational incentives already to help startups by guiding them to a successful exit (Krishnan and Masulis, 2012). Finally, entrepreneurs may know that common ownership could benefit their prospects given the potential synergies and informational advantages that it may bring.

Thus, in our empirical analyses, we test for both good and bad outcomes for startups by examining exits. We focus on the bottom of the distribution by studying whether commonly-held startups fail at the same rate as other startups. Then, we turn to the startups that survive and examine their ability to raise additional capital. Finally, we look to the upper end of the distribution by examining startup exits via IPOs, trade sales, and mergers. For example, we examine startups' IPO valuations relative to public market peers to evaluate whether common ownership enables VC firms to improve the performance of the stronger startups that exit through an IPO. When we examine mergers, we focus on acquisitions by commonly-owned startups, thereby identifying a potential information channel and synergy gains from common ownership.

Finally, we emphasize that this study does not seek to evaluate the effects of common ownership on consumer welfare. Positive outcomes for startups are consistent with common ownership raising consumer welfare through improved product quality, but also increasing coordinated behavior that hurts consumers. Examining this hypothesis is outside the scope of this paper as it would require detailed product and price information that is unavailable for most startups and industries.

2. Institutional Background

In this section, we provide brief institutional background explaining the legal changes that we exploit in our analysis, and why they mattered for common ownership of startups. We include a more detailed legal analysis and evidence of the motivation for these legal changes from the legislative debates in Appendix A.

The specific law changes that we use are the staggered adoption of laws across eight states from 2000 to 2016 that enable corporations to adopt waivers from the corporate opportunity doctrine, or COWs (Rauterberg and Talley, 2017). Table 1 reports the states and the dates when firms were first permitted to adopt the COW in treated states. These waivers exempt directors, officers and shareholders from litigation risk if they usurp a business opportunity in a way that conflicts with the firm's best interest (Talley, 1998; Rauterberg and Talley, 2017; Licht, 2018; Velasco, 2018). The waivers are an element of the duty of loyalty, which more broadly regulates conflicts of interest and requires fiduciaries to act in the best interest of the corporation.

To better 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.[4] As of 2019, GoDaddy had five VC partners sitting on its board. Those five directors sat on 31 other boards, including another web domain company. Prior to the law change, these board seats would have subjected the directors to substantial liability risk. As Little and Orien (2014) detail, general partners serving on multiple company boards had to strategically manage liability landmines, which occur because VC investors are often approached with 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. In support of the view that these waivers meaningfully reduce liability risk, one legal expert stated that if investors "...will need to worry that all their subsequent private investments in other possibly related firms will be attacked as usurped opportunities of the first company they bought into, they will justifiably think twice before committing their capital; hence the need for

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

waiver of the doctrine" (Grossman, 2009).

We emphasize that we do not observe whether or not the startups in our sample actually adopt COWs because private startups are not required to disclose information about the adoption of COWs. Thus, in theory, using the legal changes as a shock may reflect intent to treat rather than treatment on the treated. Although the duty of loyalty (including the corporate opportunity doctrine) may seem immutable,[5] a majority of public corporations are electing to dilute it by adopting COWs (Rauterberg and Talley, 2017). Given the hurdles that the corporate opportunity doctrine poses for VC investment, and the negative impact of litigation on VC firms' reputations (Atanasov et al., 2012), it is likely that the rates of COW adoption in startups are significantly higher. Consistent with this view, the standard form certificate of incorporation provided as a model legal document by the National Venture Capital Association includes a COW provision. In any case, COWs do not even require amendments to the certificate of incorporation (which entails shareholder approval); rather, they can be easily adopted ad hoc through any contractual instrument in the context of specific transactions.

Finally, we emphasize that not only are the waivers an important shock to investors' and directors' liability risk ex ante, but the waivers have also been found to be effective in practice ex post. The effectiveness of COWs is demonstrated by a recent case already mentioned above, Alarm.Com Holdings Inc. v. ABS Capital Partners Inc., C.A. 2017-0583-JTL (June 15, 2018). In this case, the Delaware Chancery 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. Thus, there is evidence that legal changes permitting COWs are consequential.

[5]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 et al., 2003; Bebchuk et al., 2009) presumably because it was assumed that it is not possible to waive it.

3. Data and Sample

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

3.1. VC-funded Startups

The data on startups and VC funds is sourced from Preqin and the sample period extends from 1995 through 2018. To determine startup founding year, we use the earliest of the following three variables: year of first incorporation (from Lexis Advance), year of first deal (from Preqin), and founding year (from Preqin or VentureXpert when missing). To determine startup exit year, we supplement the Preqin data on exits with data from VentureXpert and manual searches in Crunchbase, because Preqin does not have a variable to indicate whether a portfolio company goes out of business or fails. We label a startup as having failed if it is listed as defunct, out of business, or in bankruptcy. Finally, we conservatively code as failed any startup that has not raised capital in five years.

We exclude startups not located in the United States. We also exclude nonprofit startups and those that are not incorporated (such as LLCs), as they would not be subject to the state legislation changes. For state of headquarters, we use the state of location. Given that determining state of incorporation is a complicated and time-consuming process, we limit the total number of startups in the sample. To select this sample, we aggregate deal value at the startup level and select the startups that receive at least 10 million in VC funding. This results in a sample size of approximately 13,500 startups. For startups 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 startups.

3.2. State of Incorporation

A major challenge in identifying the state of incorporation is that Preqin does not provide this information. We use Lexis Advance Public Records to manually identify this variable. The nationwide business locator tool on Lexis allows for searches by name and location. The source records include all corporate filings collected from secretaries of state, Uniform Commercial Code filings, and Experian business records. Of the initial set of VC-funded startups, we are able to identify about 92% of the startups using the nationwide business locator. We then download all corporate records from the secretary of state of the state in which the startup is headquartered. We develop an algorithm that examines the filings and determines 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 startups for which no filings are available on Lexis using data obtained from the Delaware and California secretaries of state. Our final sample of startups with state of incorporation data comprises 13,428 unique startups. While this sample represents only approximately 52% of the unique startups in Preqin's database, 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.

3.3. Industry Classification

For each VC-funded startup, Preqin provides two descriptions of industry: primary industry and subindustries. The subindustries are listed in order of relevance, so we focus on the first subindustry. As examples, Zocdoc's primary industry is Healthcare IT and its subindustry is web applications. Lending Club's primary industry is financial services and its subindustry is e-financial.

The narrowest definition of industry uses both primary industry and subindustry and includes 130 unique industries. Using only the primary industry produces 78 unique industries, and the coarsest definition includes 10 unique industries. Appendix Table C.1, Table C.2, Table C.3, and Table C.4 provide additional details on these definitions.

3.4. Common Ownership

We create three measures of common ownership at the startup-year level. The three measures are (i) an indicator for whether any VC investor is a common owner at the within-industry startup-pair level as in (Azar, 2017), (ii) a count of the total number of VC investors at the startup-pair level that are common owners as in He and Huang (2017), and (iii) the GGL common ownership measure, which is a directional pairwise measure as in Gilje et al. (2019). We use the Preqin variable corresponding to a VC investor, such as Benchmark Capital or Sequoia Capital, to define the investor level. We use the Preqin variable of primary industry to define the industry. In the main specifications we use the simple measures of common ownership, because data limitations require making multiple assumptions and likely create measurement error when calculating the GGL measure for startups. We describe the construction of the GGL measure in Appendix B.

3.5. Within-industry Investments

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

within-industry investment as an indicator variable equal to one if at the startup-investor level either an intensive or extensive margin investment is made.

3.6. Within-industry Directorships

We gather data on directors and VC partners from Preqin to construct three measures related to board members. First, we calculate the total number of VC directors per startup-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 and partners sit. The first measure of director thickness is the average number of additional board appointments that the VC directors hold per startup-year. Other board appointments include all directorships that year within the full universe of Preqin startups. Our second measure of director thickness is the average number of other board appointments per startup-year that are within the same industry as the startup.

3.7. Startup Deal Outcomes and Exits

We use Preqin to determine the VC-funding raised by startups. For deal count, we add the number of rounds of financing a startup raises per year. Late-stage rounds are defined as those that are either at the Series B-J stage of financing or are described as "Pre-IPO." We define deal amount as the log of U.S. dollar deal size reported by Preqin, adjusted for inflation. We define the time between deal rounds on an annual basis. We gather exit data from Preqin, which includes IPOs, acquisitions, and trade sales. When the startup is acquired, Preqin provides the name of the acquirer. Based on this name matching, we then determine if the acquirer and the target have a common VC investor and create an indicator variable for whether a startup is acquired by a commonly-owned startup.

3.8. VC Fund Performance

We gather VC fund performance data from Preqin's Cash Flow data set. The Cash Flow data set provides periodic snapshots of fund performance relative to a benchmark. It only covers a limited

number of VC funds, so coverage is incomplete. However, to approximate complete coverage, Preqin benchmarks the performance to established indexes (e.g., early stage, general venture, etc.) and ranks each funds' performance relative to the benchmark. We use the benchmarked quartiles at an annual frequency to assess portfolio returns.

3.9. 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 based on Sales, EBITDA and Earnings divided by the equivalent multiple for a matched public firm. We describe each of the three measures: $\left(\frac{P}{V}\right)_{Sales}$,$\left(\frac{P}{V}\right)_{EBITDA}$, and $\left(\frac{P}{V}\right)_{Earnings}$ in Appendix B.

4. Descriptive Statistics

Due to the merging of various datasets, our main startup-year sample includes 129,448 startup-years belonging to 13,428 Preqin startups with data on their states of incorporation. Table 2 displays summary statistics for common ownership, 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. Startup-years are only included in the treated sample for years after the treatment. Control startup-years consist of startup-year observations for the never-treated group as well as startup-year observations for years prior to the treatment for the treated group. About 71% of all startup-years are in the treated sample. This is because about 66% of the startup-year observations come from startups incorporated in Delaware (see Appendix Table C.5).[6] Naturally, the large proportion of Delaware firms in the 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 startups.

[6]This is consistent with Broughman et al. (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.

Panel A of Table 2 focuses on the measures of common ownership. The statistics reveal a meaningful increase in common ownership after the law changes. Among the treated startups, 60 percent have a common owner while only 32 percent of control startups have a common owner. Both the total number of common owners and the GGL measure of common ownership follow a similar pattern. Panel B shows that within-industry investment almost doubles relative to the control startups at the mean. The doubling holds for both the intensive and extensive margin. Panel C 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 startup-year. Across all measures of director thickness, we observe higher values for the treated sample.

Panel D of Table 2 summarizes the VC deal variables. We observe 0.29 deals per startup-year. Late-stage deals occur less frequently and constitute only 0.11 deals per startup-year. The average inflation-adjusted dollar deal volume per year is $4.7 million, although this amount is averaged across years that do and do not include deals. The treated sample has a higher average deal count, including late-stage deals, and higher deal amounts than the untreated sample. Panel E of Table 2 summarizes the exits. Here we see that treated startups undergo IPOs at higher rates than control startups and they fail at lower rates. Treated startups also have a higher rate of trade sales and are more frequently acquired by a commonly-owned startup.

We use this startup-year data to describe the sample correlation between common ownership and startup growth. Appendix Table C.6 reports the associations based on the endogenous OLS regressions. The table shows that having a common owner is associated with raising 0.45 additional rounds of financing in a given year. This positive relationship is a conditional correlation after including startup, headquarter-state-by-year, and industry-by-year fixed effects as well as controls for startup growth potential. Next, the table includes the partial correlations associated with the total number of common owners. It reveals a correlation of 0.22 for raising additional rounds of financing. Similarly, when we shift our attention to late stage financing and deal value, we observe positive correlations. Finally, the table illustrates that having a common owner is also correlated

with successful startup exits. We observe correlations with a higher probability of IPO, trade sale, and acquisition by a commonly held startup.

5. Empirical Design

Identifying the effects of common ownership on startup outcomes in general is challenging due to inherent selection issues that arise from VC investment decisions. For example, endogeneity may arise if VC investors choose to commonly own startups with higher risk or at times when the price of risk is higher. In this case, the underlying economics suggest that the sample correlation between common ownership and startup growth is negative because risky startups may be less likely to succeed. While endogeneity arising from selection matters, other potential sources of endogeneity are also problematic. For example, it is plausible that changes in a startup's growth trajectory may cause a VC investor to become a common owner. This simultaneity may be the source of a positive association between common ownership and growth rather than the other way around. To the extent that these potential sources of endogeneity are important drivers of the real world process that generates the data, they can lead to OLS point estimates that are inconsistent for the true relationship between common ownership and startup growth.

To get closer to the ideal of a random shock that generates exogenous variation in common ownership, we use the set of quasi-natural experiments stemming from the law changes that permit COWs. Specifically, we exploit the incorporation state-level shocks as quasi-natural experiments to establish a link between a startup's ability to adopt COWs for VC investors and common ownership. This setting has two main appealing features. First, the variation in liability risk generated by the staggered adoption of COWs is arguably exogenous to startup-level attributes. As discussed above, the impetus for the state law changes was to eliminate uncertainty arising from case law, and there was no apparent evidence of lobbying prior to the law. Moreover, unlike other law changes that may affects the scope of managerial discretion in multiple ways (e.g., constituency statutes), these law changes had a very narrow application to liability that arises from commonly owning competing ventures. Second, because variation is at the state of incorporation level, we can compare startups

that are headquartered in the same state but are subject to different legislation. This empirical design significantly mitigates the confounding effects resulting from regional economic shocks or conglomeration effects in entrepreneurial hubs.

To understand whether these laws did unlock a sudden and significant shift in the extent to which VCs hold stakes in competing startups, we estimate five regressions. Specification (1) is a standard difference-in-differences specification with year fixed effects. Specification (2) also includes startup 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. Below we show the most conservative difference-in-differences regression, specification (5).

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

In the above equation, Y represents the outcome variable, such as an indicator for having a VC investor that commonly owns another startup within industry n. The observation unit is startup j that operates in industry n and is headquartered in state h and incorporated in state i in year t. $Treat$ is an indicator for startups that are incorporated in states with COW laws. $Treat \times Post$ is an indicator for startups that are incorporated in states with COW laws after the law change. β is the main coefficient of interest that isolates the change attributable to the law. $Post5$ is an indicator for any startup that has been treated for five years. ζ captures potential 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 ensure greater precision in our estimate of the time trend. f_j denotes the startup fixed effects that capture all of the startup-level time invariant effects, γ_{ht} represents a headquarter-state-by-year fixed effect that attempts to control for local economic conditions (e.g., state funding initiatives for innovation), and ρ_{it} represents an industry-by-year fixed effect that accounts for industry-level trends. X_{jnhit} represents controls for startup growth potential including total capital previously raised and the total number of rounds of capital previously raised. Standard errors are clustered

at the incorporation state level, as that is our level of treatment.

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

The key assumption required for this IV strategy to generate a consistent estimate of the consequences of common ownership for startups is that conditional on controls, the law changes are uncorrelated with other drivers of startup growth. There are several reasons to believe that the exclusion restriction assumption holds. First, as mentioned earlier, these laws changes were targeted. They regulate very specific conduct around the use of valuable business information in another venture, and directors remain subject to all other fiduciary duties.[7] Second, the event studies of both the first stage and reduced form reinforce the notion that these law changes were sudden events with quasi-exogenous timing driven by the legal processes of each state, so that the difference between pre and post within each comparison group captures changes in the laws. Our specifications also control for startup fixed effects and startup growth potential to account for the fixed and time varying differences across startups, thereby mitigating concerns about selection of high-growth firms into treated states. For these reasons, we believe the resulting IV estimates can plausibly be interpreted as measuring the effects of VC common ownership on startup performance.

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

The first-stage equation for the IV analysis is:

$$CommonOwnership_{jnhit} = \alpha + \beta \left(Treat \times Post\right)_{jnhit} + \mu X_{jnhit} + f_j + \gamma_{ht} + \rho_{it} + \nu_{jnhit} \qquad (2)$$

where $CommonOwnership$ is one of the measures of common ownership, primarily an indicator for whether the startup has any common owners and the log of the total number of common owners. The observation unit is startup j that operates in industry n and is headquartered in state h but incorporated in state i in year t. $Treat \times Post$ is an indicator for startups that are incorporated in states with COW laws after the law change. X_{jnhit} is a vector of controls including startup age, startup size, and number of rounds of financing raised. f_j denotes the startup fixed effects that capture all of the startup-level time invariant effects. γ_{ht} represents a headquarter-state-by-year fixed effect that attempts to control for local economic conditions (e.g., state funding initiatives for innovation), and ρ_{it} represents an industry-by-year fixed effect that accounts for industry-level trends, and ν_{jnhit} is the unobservable error component.

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

$$Y_{jnhit} = \alpha + \beta \hat{CommonOwnership}_{jnhit} + \mu X_{jnhit} + f_j + \gamma_{ht} + \rho_{it} + \varepsilon_{jnhit} \qquad (3)$$

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

6. Results

In this section, we present empirical evidence documenting an increase in common ownership following the law changes. We next show that this increase in common ownership is associated with an increase in VC directorships at other startups. This evidence supports directorships as a potential mechanism through which common ownership could be linked to outcomes at the startup level. Then, we assess whether common ownership is associated with value creation by examining startups' ability to raise additional capital and their exits. The results are consistent with the hypothesis that common ownership generates value for startups.

6.1. Common Ownership

Panel A of Table 3 shows that common ownership rates increase for treated startups. As shown in column (2), on average, startups incorporated in treated states are 11.8 percentage points more likely to have a within-industry common owner after the law change and the 95% confidence interval is 7.2 to 16.5 percentage points. This is large in comparison to the baseline; for example, prior to the first law passage, only 10% of startups incorporated in Delaware had a common owner. Our results hold across a variety of specifications including ones with startup, industry-by-year, and headquarter-state-by-year fixed effects, as well as controls for startup growth potential. In each specification, the result is significant at the 1% level. Specifications (3)-(5) include a control for late treatment and the coefficient on the control rejects the hypothesis that the initial increase in common ownership rates is short-lived as there is no evidence of reversal.

Specifications (4) and (5) include headquarter-state-by-year fixed effects to ensure that the treatment is not confounded by local economic conditions such as the startup culture in Silicon Valley. Including these controls does not change the point estimate, but they do reduce the standard error by half. This is consistent with local economic conditions reducing the precision of the estimates but being relatively orthogonal to the law change as described in our legal analysis. Panel B of Table 3 evaluates the total number of common owners as the dependent variable. The coefficient estimate suggests that startups located in treated states observe a 30 percent increase

in the total number of common owners, on average. The results in Panel B further show that the number of common owners increases following the law changes. Again, these results are significant across specifications.

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

To further test the validity of the difference-in-differences approach, which relies on the assumption that no time-varying differences exist between the treatment and control groups, we run two placebo tests. First, we compare changes in common ownership rates using the same treated states but placebo treatment dates. In particular, we randomly assign a treatment date that is more than five years before or after its actual treatment. Second, we compare changes in common ownership rates using a placebo group of states that do not allow for COWs as treated states. For this exercise, we exclude treated startups. Then, among the remaining non-treated startups, we select a new set of eight states to serve as our placebo treatment group. The placebo treatment states include California, New York, Connecticut, Colorado, Indiana, Illinois, Arizona, Georgia, and Oregon. The placebo treatment group is deliberately similar to the actual treated states in terms of size and entrepreneurial activity. For both of these placebo tests, we find insignificant changes in common ownership and total common ownership. Moreover, the point estimates are small and close to zero. This provides further support for our identifying assumptions. The results of these tests are included in Internet Appendix Table C.7.

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

In the Internet Appendix, we show additional visual evidence consistent with a parallel trends assumption and meaningful jumps in common ownership for treated states. Figure C.1 shows the raw trends in common ownership over time for the high entrepreneurship states without any controls. The figure reveals a meaningful jump in levels of common ownership for Delaware relative to California, Massachusetts, and New York following the law change. Appendix Figure C.2 plots common ownership rates in the years before and after the law change for different subgroups of startups categorized by both entrepreneurial activity and timing of treatment (first to be treated or not). As expected, California, Massachusetts, and New York are different from the other control states in terms of common ownership rates, but even in these states with strong entrepreneurial ecosystems, common ownership rates remains lower than in either the first treated state or states that are treated later, such as Texas, Washington and New Jersey.

We also consider the GGL measure of common ownership developed by Gilje et al. (2019) in Appendix Table C.8. We evaluate this measure for the full sample of startups, the sample of startups in Delaware, California, Massachusetts, and New York, and finally the sample of startups that excludes those originally incorporated in Delaware from the analysis. The point estimates show a similar positive increase in common ownership after the law changes but the results are statistically weaker (four out of six of the regressions are significant, while the remaining two only

show significance after five years). As explained above, however, the GGL measure likely suffers from a measurement error in the VC context.

In addition, in Internet Appendix Table C.9, we show robustness results that exclude software startups after 2006 that benefited from Cloud computing (because they are characterized by the "spray and pray" investment strategy documented in Ewens et al. (2018)), the bursting of the dotcom bubble, and the financial crisis. In each case, the statistical inferences are similar to those based in our main specifications.

Finally, we explore within-industry investments on the extensive margin to get a better understanding of the patterns of investment following the law changes. Consistent with the common ownership findings, treated startups experience a statistically significant increase in within-industry investment. For both extensive and intensive 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. These results are included in Internet Appendix Table F.1.

6.2. Directors as a Mechanism for Information Coordination

Having shown evidence consistent with the law changes increasing common ownership, we next explore accompanying board seats, a key mechanism for facilitating information sharing among startups. We examine the relationship between common ownership and directorships using the IV regressions in which we instrument for common ownership using an indicator that equals one if the startup is incorporated in a treated state after the law change. Panel A of Table 5 shows that startups with a common VC owner have 2.1 more VC directors on the board, on average. This result is statistically significant at the 1% level and robust to the inclusion of startup, headquarter-state-by-year, and industry-by-year fixed effects. As expected, the t-statistic on the instrument is highly significant and the F-statistic from the first stage of the IV regression exceeds the requisite 10 to ensure minimal bias of the point estimate.

In Panel B and C of Table 5, we turn to our other measures of director thickness. These results

suggest that common ownership is associated with having well-connected VC directors serve on a startup's board. For example, Column (1) of Panel B shows that the startups with a common VC owner have VC directors that sit on the board of 2.5 additional startups, on average, and 1.0 additional startup within the same industry. These results are statistically significant at the 1% level and robust to the inclusion of different fixed effects. Columns (3) and (4) examine the total number of common owners to help understand incremental changes in common ownership. These results suggest that a 50% increase in common ownership is associated with 0.61 more VC directors, with a network that includes 0.75 more directorships, and 0.28 more directorships at startups within the same industry.

In conclusion, our analyses of VC directorships shows that startups with greater common ownership or startups in treated states experienced a rise in the number of well-connected VC representatives willing to serve on the board of directors. That these VC investors sat on multiple boards and boards within the same industry uncovers a mechanism through which VC investors acquire information on startups' aptitudes and may allocate business opportunities among their portfolio companies.

6.3. Startup Growth

We explore the relationship between common ownership and startup growth using the IV strategy. As shown in Panel A of Table 6, greater common ownership is associated with receiving an additional round of VC financing. The point estimate suggests that common ownership is associated with about one additional round of financing. The increase in financing rounds is statistically significant at the 1% level and represents an economically meaningful effect given that the median number of financing rounds is two. In columns (3) and (4), the independent variable is the log of total number of common owners. For these tests, the point estimate suggests that a 10 percent increase in common ownership is associated with between a 5.5 and 6.2 percent increase in deal volume. In each regression, the t-statistic on the instrument is highly significant and the F-statistic from the first stage of the IV regression ranges from 83.5 to 138.9. Taken together, these initial

results are consistent with positive economic outcomes associated with common ownership rather than the alternative hypothesis that common ownership merely facilitates expropriation of business opportunities by common owners.

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

Panel B of Table 6 reports the results of the financing round tests. We find a positive relationship between common ownership and receiving later stage financing from VC firms. As shown in column (1), common ownership is associated with 0.28 and 0.34 additional rounds of late-stage financing, on average. In this case, a 10 percent increase in common ownership is associated with a 1.6 to 1.9 percent increase in late round financing. All results are significant at the 1% level when including the additional fixed effects.

In Panel C of Table 6, we consider deal size. The dependent variable is the log of one plus the deal value in millions of 2010 dollars. The coefficient estimates suggest that, on average, common ownership is associated with larger deal volume. The elasticity estimated in columns (3) and (4) suggests that a 10 percent increase in common ownership would yield a 11.8 to 21.8 percent increase in deal size. Given that the increase in deal size could stem from these startups being forced to wait longer between rounds as part of a holdup by some VC investors, we evaluate the time between financing rounds. Because the sample size is limited since few startups receive multiple rounds of financing, we perform this test using the difference-in-differences framework. As reported in Appendix Table F.4, we find no evidence of delays in financing. Thus, overall, the results on deal size and timing are consistent with the notion that common ownership has positive economic

benefits.

So far our results are consistent with common ownership helping startups to grow, but lingering concerns remain. For example, one might worry that our startup fixed effects and control variables do not fully capture time-varying aspects of startup potential, and therefore, it could be the case that the results are driven by high-growth startups that choose to incorporate in Delaware. To mitigate these concerns, we re-run the empirical tests but including only the startups that are not headquartered in their state of incorporation and are not headquartered in Delaware. As shown in Table 7, our findings remain unchanged using this restricted sample of startups. This evidence is very compelling: it shows that our estimated relationship is similar across different time periods and it is not driven by a particular set of law changes.

6.4. VC Funds' Returns and Startups' Exits

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

Table 8 examines the performance of VC portfolios as a whole. For these regressions, the dependent variable is the quartile of the VCs' returns in a given year and the focal explanatory variables are the percentage of startups that are either in treated states or the percentage of startups in the portfolio that are commonly held. In each case, we find a significant and positive relationship between attributes associated with common ownership and greater VC portfolio returns. 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 startup exits. Table 9 evaluates startup exits. Our results in Panel A of Table 9 suggest that common ownership is associated with about 2.7% higher probability of IPOs. Given that about 6% of the startups in the sample undergo an IPO, the economic magnitude is large. As IPOs are invariably viewed as a good outcome, this result supports the hypothesis that common ownership benefits startups.

Next we evaluate acquisitions. We first show in Panel B of Table 9 that common ownership is associated with a higher probability of trade sales. More importantly, Panel C of Table 9 shows a direct channel through which common ownership is associated with exits. Having a common owner is associated with a 0.8 to 0.9 percentage point increase in the likelihood of being acquired by a commonly-owned startup. This evidence suggests that common ownership facilitates synergy gains through the sharing of information among VC directors. To be sure, it is possible that the main beneficiaries in these deals are the VC investors who typically have a liquidation preference. While we cannot overrule the possibility that the non-VC investors do not get their fair share of these deals, the evidence to date supports the view that such merger activity tends to be value-maximizing (Gompers and Xuan, 2012).

Finally, to study the potential for common ownership to create value for the full distribution of startups, we next examine startup failures. Panel D of Table 9 shows a lower probability of failure for commonly held startups, which suggests that common ownership even helps those firms at the bottom of the distribution. Having a common VC owner is associated with 5.7 to 7.0 percentage point decrease in the probability of failure depending on which fixed effects are included in the specification.

Thus, the overall results are consistent with common ownership helping weaker startups improve rather than common ownership resulting in rent extraction from one startup to benefit another. The results also suggests that VC investors and startups tend to share information, and their relationship is broadly collaborative (Fisch, 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).

We further 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 10. While we do not have a large enough sample to pass weak instrument tests, we can still implement the difference-in-differences design. Using the standard difference-in-differences design, we examine three different valuation methods based on the IPO proceeds divided by three different accounting measures (sales, EBITDA and earnings) as compared to matched non-IPO firms. We find that the treated firms, on average, receive more favorable valuations. For example, as reported in Panel C, treated 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 outperform their benchmark index, startups benefit too. The rates of IPO exit for startups with higher rates of common ownership are larger, and we have some suggestive evidence that their valuations are more favorable at the time they undergo an IPO. Similarly, we find that the VC investors with greater common ownership are able to shift some startups from failure and low-return multiples, to higher multiples likely via sales to would-be acquirers. When we look at acquirers with a common VC owner, we find that the merger is much more likely to be to a commonly held startup. 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, and there is no evidence that they advantage one startup over another. Thus, for most startups, common owners create value by reducing failure risk, facilitating sales, and ultimately, guiding startups to higher IPO valuations.

6.5. Linking Growth to Directors Networks

We document a pattern consistent with common ownership facilitating startup growth, but it is also important to establish that the director mechanism that we conjecture is meaningfully linked to these changes in startup trajectory. This is especially important since the emerging research on common ownership is fiercely debated, in part, because there is a lingering concern that the arms-

length relationship between institutional investors and management in public companies generates relatively few channels for influence (Lewellen and Lowry, 2019).

In Panel A and B of Table 11, we compare characteristics of VC directors for startup growth and exits relative to those that do not raise additional financing or exit. We observe that having more VC directors, especially those with additional directorships, are associated with more growth and successful exits. Consistent with directors facilitating information flows, startups being acquired by a commonly-owned startup have the most well-connected directors. While they do not have the largest number of VC directors, their VC directors have 5.7 additional directorships on average. The summary statistics are similar for IPOs and trade sales.

In Panel C and D of Table 11, we run subsample tests based on director characteristics. Specifically, we restrict the sample to the startups without a VC director, with a VC director, and with a common VC director, which we define as having at least one additional VC directorship, and compare the startup exits. We find no relationship between common ownership and an IPO or failure for startups without VC directors. In contrast, for the startups with VC directors, we find higher probabilities of IPOs and lower probabilities of failure. When we restrict the sample even further to common VC directors, the estimated relationships are even larger. These findings support the hypothesis that directors are a mechanism through which common ownership is consequential.

6.6. Industries with High versus Low Intellectual Property Protection

Our results show that common ownership is associated with startup growth and exits, but this may not be the case across all industries. Recent evidence from the pharmaceutical industry suggests that common owners play favorites to the detriment of weaker startups in their portfolios (Cunningham et al., 2020; Li et al., 2020). These studies are consistent with our findings to the extent that there is a nuanced relationship between common ownership and startup growth across industries, and our estimates are a weighted combination of these relationships.[8]

[8]The literature on entrepreneurship suggests many potential economic channels that vary in strength across industry for linking common ownership and startup growth. For example, in the biotechnology industry, cooperation between startups and more established firms in the form of strategic alliances is the norm (Lerner and Merges, 1998; Robinson and Stuart, 2007). Yet in other industries, especially those with

To evaluate the potential for different patterns across industries, we split our analyses into subsamples of industries with strong and weak IP protection. For example, industries such as pharmaceuticals have strong IP protection. Given that IP is not only used to extend scientific ideas, but also strategically to protect market share and discourage follow-on innovations (Abrams et al., 2020), this suggests that a common VC investor would have relatively higher incentives to pursue a winner-take-all approach as opposed to a synergistic approach for maximizing returns across commonly held startups. To test this hypothesis, we use the industrial classification of IP protection provided in Hall et al. (2014). We then match their industry names to the Preqin industry names.

Internet Appendix Table C.10 shows evidence consistent with stronger IP protection supporting a winner-take-all approach to portfolio maximization by common VC investors. In strong IP industries, the likelihood of undergoing an IPO is 1.8 times that of weak IP industries. Further supporting this finding, we see relatively fewer investment rounds, especially on the extensive margin. The point estimate on extensive margin investment for strong IP protection is 0.3 times the magnitude of the point estimate for weak IP protection. This suggests that VC investors in industries with high IPO protection may end additional investments once a favorite startup has been identified. Finally, we find relatively more failures when IP protection is strong, but we still see relatively fewer failures overall. This suggests that the weaker of two startups may be killed off in some industries with strong IP protection so that the favorite may succeed, but this phenomenon is not the norm. Taken together, these results help reconcile our findings with Cunningham et al. (2020) and Li et al. (2020).

6.7. Additional Robustness Checks

In Appendix A, we provide additional details on our legal analyses as well as several robustness checks. For example, we assess the impetus for the state law changes, and show that the original

an imperfect "market for ideas," strategic alliances are rare (Gans et al., 2002). While strategic alliances are not common ownership, they have been linked to VC investors' common portfolio holdings (Lindsey, 2008; Gonzalez-Uribe, 2020).

motivation for the law change was to eliminate uncertainty arising from a 1989 case and therefore was likely unrelated to startup lobbying. Next, we focus on states, such as Nevada and Virginia, that 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. 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. We also account for differences in states' COW legislation. For example, the COW legislation in Nevada does not cover shareholders, and the legislation in Washington requires shareholder approval before adopting COWs (as opposed to merely board action as in other states). Thus, we examine regressions in which the treatment variable excludes Nevada and regressions in which the treatment variable excludes Washington. Across this battery of robustness tests, our estimates generate very similar inferences to our main specifications.

In Appendix Table C.11, we consider the possibility that common ownership may be harmful to entrepreneurs with weaker bargaining power. While founders may have good reasons to accept the informational risks associated with investments by common VC investors (see Section 1.2), it is possible that VC investors exploit weaker founders to benefit other startups. We find that even when VCs' bargaining power is high, startups achieve the full set of benefits from having common VC investors such as higher likelihood of IPOs and lower failure rates.

In Appendix Table C.12, we evaluate VC specialization. It is possible that the law changes could have increased industry specialization rather than facilitating common ownership. With greater industry specialization, VC investors may be able to make better investment decisions. Thus, startup growth may be driven by industry specialization rather than common ownership. We find that VC investor specialization is decreasing over our sample period, suggesting that VC specialization is not driving our results.

In Appendix D, we present several variations of our IV estimates analyses for VC directorships, deals, and startup exits. We include robustness checks that limit the sample to high entrepreneurial states and specifications that exclude firms originally incorporated in Delaware. Further, we present

evidence for the subsamples that exclude the "spray and pray" investment strategy documented in Ewens et al. (2018), the bursting of the dotcom bubble, and the financial crisis. In each case, the statistical inferences are similar to those based on our main specifications.

In Appendix E, we use two recent advances in the literature on IV estimation to test and relax the exclusion restriction. First, following the approach in Angrist et al. (2010), we find candidate subgroups of startups unlikely to comply with the instrument, and thus, for these subgroups, the first stage is very likely to be zero. Then, we examine whether the IV has a direct effect on startup outcomes for these zero-first stage groups. We use two candidate subgroups. The first candidate subgroup consists of startups that had not raised financing in over three years prior to the law change, and the second candidate subgroup consists of startups from regulated industries and/or industries where the government may influence the ownership structure. Overall, the evidence is consistent with the IV having insignificant effects on startup outcomes for these zero-first stage groups. This provides evidence in favor of the assumption that the exclusion restriction is satisfied. As a second step, we follow the literature on plausibly exogenous IV estimation (Conley et al., 2012; Imbens and Rubin, 2015; Kippersluis and Rietveld, 2018), and we relax the exclusion restriction assumption associated with the IV. We show that a plausibly exogenous IV still results in statistical inferences consistent with a positive relationship between common ownership and directorships, startup growth, and exits.

In Appendix F, we present the reduced-form difference-in-differences analyses for VC directorships, deals, and startup exits. The reduced-form evidence consistently shows a strong statistical relationship between the instrument and startup outcomes. We also include robustness checks of the reduced form results, including those that limit the sample to high entrepreneurial states and that exclude Delaware firms. Further, we present reduced-form evidence for the subsamples that exclude the "spray and pray" investment strategy (Ewens et al., 2018), the bursting of the dotcom bubble, and the financial crisis. In all cases, the reduced form results suggest that the instrument is statistically significantly related to these outcomes. We also present figures for the dynamic coefficient estimates associated with these outcomes over a window spanning from five years before the law changes to ten years after the law changes. Virtually all figures are consistent with no

pre-existing differential trends and meaningful jumps at the time of the law changes, except perhaps VC directorships. To more fully consider the importance of potential trends, we extend our analysis to a matching framework.

In Appendix G, we present estimates from different matching approaches. The matching exercise helps to select the best comparison control group for the sample of treated startups to ensure internal validity. We consider several alternative matching approaches and they all generate similar results. In some cases, the economic magnitude of the point estimates are smaller, but none of the inferences are significantly different from those in the main results. This indicates that observed compositional differences across states do not generate large differences in common ownership for the control and treated startups.

7. Conclusion

The literature on common ownership has thus far largely focused on institutional ownership in public firms, where ownership stakes tend to be small, and the ability of diversified and often passive owners to affect firm policy is debatable. In this study, we examine common ownership of entrepreneurial startups. Common ownership of startups is pervasive, and the channel for potential coordination of policies, namely through the appointment of VC representative directors, is apparent. The growing economic significance of entrepreneurial startups in the economy further underlies the need to examine the VC industry. Doing so helps to develop a broader understanding of the theoretical and practical implications of common ownership and startup growth.

An important policy concern surrounding common ownership in the VC industry is that it would stifle competition among startups held by common owners. The key economic tension centers on how the opportunities for information sharing brought about by common ownership affect startup performance. VCs can play favorites with their commonly held startups, maximizing the returns of one startup at the expense of another. On the other hand, information sharing creates opportunities for VCs to generate positive spillovers among startups.

The findings in this study reveal that common ownership is associated with positive outcomes

for startups. Commonly-held startups raise more capital through more rounds of investment; they are more likely to exit through an IPO at a higher valuation; they are more likely to merge with other commonly-held startups; and they are less likely to fail. These results support the hypothesis that across a broad array of industries, common VC owners help the startups in which they invest.

Importantly, our analysis also uncovers a mechanism through which common ownership is linked to startup success. First, we show that common ownership is associated with more directorships for VC investors and a thickening of those directors' overall networks, especially at startups in the same industry. Moreover, we find a link between VC directorships at other startups and greater probability of IPOs and lower probability of failures. This evidence suggests that directors are a key mechanism through which information spillovers could facilitate efficient allocation of resources among startups.

These results suggest that policies aimed at eliminating common ownership by VC investors could be at odds with the goals of stimulating startup growth, an important driver of competition. Our analysis also suggests broadening the scope of analysis for anticompetitive effects beyond price measures. For example, our results indicate that common ownership enhances startup performance and reduces failure, which is consistent with more startups getting their products to consumers. Such entry may enhance welfare through increasing choice and increasing competition. Further, we find that common ownership alters the information environment for startups, which could improve quality or lower costs. Taken together, our findings suggest that an important question for future research is to establish the broader social implications of common ownership by VC investors.

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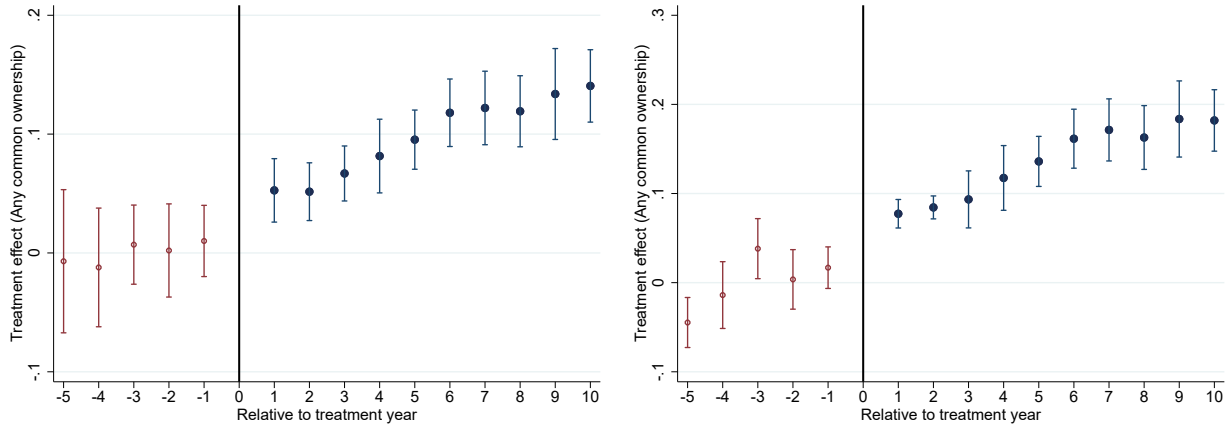

Fig. 1. Dynamic effects of corporate opportunity waiver (COW) legislation. The figures plot the impact of COW legislation on common ownership following the law changes by year relative to the law change. The figures show a window spanning from five years before the law changes to ten years after the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

Table 1:
Summary of Corporate Opportunity Waiver (COW) Legislation

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

State of Inc.	Effective Date	By Charter	By Bylaws	By Board Action	Covers Directors	Covers Shareholders
DE	July 1, 2000	Yes	Possible	Yes	Yes	Yes
OK	November 1, 2001	Yes	Possible	Yes	Yes	Yes
MO	October 1, 2003	Yes	Possible	Yes	Yes	Yes
KS	January 1, 2005	Yes	Possible	Yes	Yes	Yes
TX	January 1, 2006	Yes	Possible	Yes	Yes	Yes
NV	October 1, 2007	Yes	Possible	Yes	Yes	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

Table 2:
Summary Statistics

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

	All			Treated			Control		
	N	Mean	St. Dev.	N	Mean	St. Dev.	N	Mean	St. Dev.
Panel A: Common Ownership									
Any common ownership	129,448	0.52	0.50	91,479	0.60	0.49	37,969	0.32	0.47
Total common owners	129,448	1.01	1.21	91,479	1.20	1.25	37,969	0.57	0.98
Common ownership (GGL)	129,448	0.29	1.00	91,479	0.34	1.03	37,969	0.19	0.90
Panel B: VC Investment									
Within-industry investment	129,448	0.18	0.39	91,479	0.21	0.41	37,969	0.11	0.31
Intensive margin investment	129,448	0.14	0.34	91,479	0.16	0.37	37,969	0.08	0.27
Extensive margin investment	129,448	0.05	0.22	91,479	0.06	0.23	37,969	0.03	0.18
Panel C: VC Directors									
VC directorships	95,144	1.77	2.09	69,246	2.04	2.18	25,936	1.03	1.63
Additional directorships	95,144	2.53	3.52	69,246	2.97	3.69	25,936	1.36	2.67
Additional within-industry directorships	95,144	0.61	1.22	69,246	0.73	1.32	25,936	0.29	0.80
Panel D: VC Deals									
Count	129,448	0.29	0.52	91,479	0.32	0.54	37,969	0.20	0.44
Late stage	129,448	0.11	0.33	91,479	0.12	0.35	37,969	0.08	0.28
Deal value (log(1+$mn))	129,448	0.55	1.12	91,479	0.61	1.16	37,969	0.39	0.98
Time between deals (year)	26,767	0.55	1.12	21,573	0.56	1.11	5,194	0.52	1.13
Panel E: Exits									
IPO	129,448	0.006	0.079	91,479	0.007	0.083	37,969	0.005	0.069
Trade sale	129,448	0.030	0.171	91,479	0.034	0.181	37,969	0.022	0.146
Acquired by a commonly-owned startup	129,448	0.005	0.096	91,479	0.006	0.099	37,969	0.003	0.054
Failure	129,448	0.040	0.195	91,479	0.044	0.205	37,969	0.029	0.168

Table 3:
Common Ownership and Corporate Opportunity Waivers

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

Panel A: Any common owners	(1)	(2)	(3)	(4)	(5)
Treat	0.035				
	(0.025)				
Treat × Post	0.090***	0.118***	0.086***	0.090***	0.084***
	(0.024)	(0.023)	(0.015)	(0.009)	(0.009)
Treat × Post after five years			0.073***	0.082***	0.078***
			(0.012)	(0.011)	(0.012)
Adjusted R^2	25.4%	62.9%	63.0%	63.2%	63.7%
Panel B: Total common owners					
Treat	0.041				
	(0.063)				
Treat × Post	0.234***	0.201***	0.118***	0.128***	0.131***
	(0.064)	(0.045)	(0.024)	(0.013)	(0.012)
Treat × Post after five years			0.186***	0.220***	0.182***
			(0.034)	(0.013)	(0.024)
Adjusted R^2	26.2%	73.7%	73.7%	74.2%	75.7%
Additional startup controls	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	No	No
Startup 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	129,448	129,448	129,448	128,587	128,587
Number of unique startups	13,428	13,428	13,428	13,336	13,336

Table 4:
Common Ownership (Alternative Samples)

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

	Any common owners		Total common owners	
Panel A: DE, CA, MA, and NY	(1)	(2)	(3)	(4)
Treat × Post	0.159***	0.101***	0.293***	0.152***
	(0.017)	(0.004)	(0.032)	(0.004)
Treat × Post after five years		0.098***		0.226***
		(0.010)		(0.014)
Adjusted R^2	62.7%	63.4%	74.0%	75.8%
Number of observations	112,106	111,253	112,106	111,253
Number of unique firms	12,056	11,969	12,056	11,969
Panel B: Non-DE	(1)	(2)	(3)	(4)
Treat × Post	0.157***	0.092***	0.278***	0.178***
	(0.034)	(0.023)	(0.073)	(0.043)
Treat × Post after five years		0.133***		0.238***
		(0.021)		(0.038)
Adjusted R^2	65.6%	64.0%	68.7%	71.5%
Additional startup controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	No	Yes	No
Startup fixed effects	Yes	Yes	Yes	Yes
Headquarter-state-by-year fixed effects	No	Yes	No	Yes
Industry-by-year fixed effects	No	Yes	No	Yes
Number of observations	43,592	43,179	43,592	43,179
Number of unique startups	3,699	3,671	3,699	3,671

Table 5:
Common Ownership and the Board of Directors

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

Panel A: VC director	(1)	(2)	(3)	(4)
Any common owners	2.06***	2.02***		
	(0.35)	(0.36)		
Total common owners			1.21***	1.16***
			(0.22)	(0.22)
Panel B: VC director with additional directorships				
Any common owners	2.54***	2.87***		
	(0.56)	(0.57)		
Total common owners			1.49***	1.66***
			(0.33)	(0.33)
Panel C: VC director with additional within-industry directorships				
Any common owners	0.95***	0.84***		
	(0.21)	(0.20)		
Total common owners			0.56***	0.49***
			(0.13)	(0.11)
First-stage F-statistic	122.6	132.2	68.7	78.4
t-statistic on instrument	11.07	11.50	8.29	8.85
Additional startup controls	Yes	Yes	Yes	Yes
Startup fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	No	Yes	No
Headquarter-state-by-year fixed effects	No	Yes	No	Yes
Industry-by-year fixed effects	No	Yes	No	Yes
Number of observations	95,144	94,570	95,144	94,570
Number of unique startups	9,572	9,516	9,572	9,516

Table 6:
Common Ownership and Venture Capital (VC) Deals

This table presents results from instrumental variable (IV) regressions examining VC financing outcomes for startups with common owners. The key explanatory variable is common ownership and the instrument for common ownership is an indicator for if a startup is incorporated in a state that permits corporate opportunity waivers (COWs). In columns (1) and (2), common ownership is an indicator variable for whether a startup has a VC investor that commonly owns another startup within the same industry, and in columns (3) and (4) common ownership is defined as the natural log of the total number of common VC owners. In Panel A, the dependent variable is deal volume, which is defined as any VC equity financing deal a firm receives in a given year. In Panel B, the dependent variable is late-round VC deal volume, which is defined as a round of equity VC financing greater than the seed or first round. In Panel C, the dependent variable is deal value, defined as the natural log of one plus the deal value in millions of 2010 dollars. Additional control variables include total capital previously raised and the total number of rounds of capital previously raised. Industry for common ownership is defined based on Preqin's primary and sub industry classification. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. The first-stage F-statistic is the Kleibergen-Paap Wald statistic. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: VC deal count	(1)	(2)	(3)	(4)
Any common owners	0.987***	1.144***		
	(0.092)	(0.100)		
Total common owners			0.558***	0.618***
			(0.063)	(0.063)
Panel B. Late stage deal count				
Any common owners	0.279***	0.344***		
	(0.046)	(0.048)		
Total number of common owners			0.157***	0.186***
			(0.028)	(0.028)
Panel C. Deal value				
Any common owners	1.972***	2.255***		
	(0.196)	(0.209)		
Total common owners			1.114***	1.218***
			(0.131)	(0.130)
First-stage F-statistic	132.2	138.9	83.5	99.6
t-statistic on instrument	11.50	11.79	9.14	9.98
Additional startup controls	Yes	Yes	Yes	Yes
Startup fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	No	Yes	No
Headquarter-state-by-year fixed effects	No	Yes	No	Yes
Industry-by-year fixed effects	No	Yes	No	Yes
Number of observations	129,448	128,587	129,448	128,587
Number of unique startups	13,428	13,336	13,428	13,336

Table 7:
Startups Incorporated Outside Headquarter State and Excluding Delaware

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

	Any	Total	
Panel A: Common ownership	common owner	common owners	
Treat × Post	0.106***	0.242***	
	(0.028)	(0.071)	
Adjusted R^2	60.0%	64.2%	
Number of observations	3,661	3,661	
Number of unique startups	307	307	
Panel B: Within-industry investment by VCs	Within-industry	Extensive margin	Intensive margin
Treat × Post	0.108***	0.056***	0.056**
	(0.030)	(0.013)	(0.022)
Adjusted R^2	6.4%	4.4%	5.2%
Number of observations	3,661	3,661	3,661
Number of unique startups	307	307	307
Panel C: VC directorships	VC directors	Other directorships held	Within-industry directorships held
Treat × Post	0.241	0.467*	0.321***
	(0.168)	(0.250)	(0.096)
Adjusted R^2	67.7%	51.8%	51.4%
Number of observations	2,380	2,380	2,380
Number of unique startups	186	186	186
Panel D: VC deals	Deal volume	Late round	Deal size
Treat × Post	0.158***	0.050***	0.387***
	(0.028)	(0.015)	(0.066)
Adjusted R^2	11.5%	4.5%	5.8%
Number of observations	3,661	3,661	3,661
Number of unique startups	307	307	307
Additional startup controls	Yes	Yes	Yes
Startup fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes

Table 8:
Venture Capital (VC) Funds' Return Distributions

This table tests whether VC funds' portfolios of startups achieve higher returns when common ownership is greater. The dependent variable is the return quartile, where four indicates best returns. All returns are benchmarked against an appropriate index (e.g., early-stage, general venture, etc.). Common ownership is proxied for using the percentage of portfolio companies that are treated. Below the coefficient estimates are robust standard errors. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

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

Table 9:
Common Ownership and Startup Exits

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

Panel A: Initial public offering (IPO)	(1)	(2)	(3)	(4)
Any common owners	0.027***	0.023***		
	(0.004)	(0.005)		
Total common owners			0.012***	0.011***
			(0.002)	(0.002)
Panel B: Trade sale				
Any common owners	0.035***	0.045***		
	(0.008)	(0.010)		
Total common owners			0.016***	0.021***
			(0.004)	(0.004)
Panel C: Acquired by a commonly-owned startup				
Any common owners	0.008**	0.009**		
	(0.004)	(0.004)		
Total common owners			0.003**	0.004**
			(0.002)	(0.002)
Panel D: Failure				
Any common owners	-0.057***	-0.070***		
	(0.012)	(0.014)		
Total common owners			-0.032***	-0.032***
			(0.006)	(0.006)
First-stage F-statistic	256.4	214.5	247.9	205.9
t-statistic on instrument	16.01	14.65	15.74	14.35
Additional startup controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	No	Yes	No
Industry-by-year fixed effects	No	Yes	No	Yes
Headquarter-state-by-year fixed effects	No	Yes	No	Yes
Number of observations	129,448	128,587	129,448	128,587
Number of unique startups	13,428	13,336	13,428	13,336

Table 10:
Initial Public Offering (IPO) Valuation

This table presents the results from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). In each panel, the dependent variable is the standardized ratio of the IPO offer price relative to the intrinsic value of the firm. The ratios are calculated using the method outlined 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 robust standard errors clustered by state of incorporation. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: $\left(\frac{P}{V}\right)_{Sales}$	(1)
Treat	0.018***
	(0.004)
Treat × Post	0.011**
	(0.005)
Adjusted R^2	2.6%
Number of observations	563
Panel B: $\left(\frac{P}{V}\right)_{EBITDA}$	
Treat	-0.020*
	(0.012)
Treat × Post	0.024
	(0.017)
Adjusted R^2	2.3%
Number of observations	686
Panel C: $\left(\frac{P}{V}\right)_{Earnings}$	
Treat	0.054
	(0.140)
Treat × Post	0.330**
	(0.157)
Adjusted R^2	0.1%
Year fixed effects	Yes
Number of observations	686

Table 11:
Directors and Startup Exits

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

	Any round		Late stage	
	No	Yes	No	Yes
Panel A: VC directors by VC deal outcomes	(1)	(2)	(3)	(4)
VC directors	1.60	2.20	1.61	2.88
Common directors	2.31	3.12	2.41	3.46

	IPOs		Trade Sale		Common Acq.		Failures	
	No	Yes	No	Yes	No	Yes	No	Yes
Panel B: VC directors by startup exit	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VC directors	1.75	4.22	1.73	2.85	1.76	2.69	1.74	2.59
Common directors	2.53	3.44	2.48	4.36	2.51	5.67	2.48	3.96

	Dep. var = Late stage			Dep. var = Deal amount		
	No VC director	VC director	Common director	No VC director	VC director	Common director
Panel C: VC deal outcomes	(1)	(2)	(3)	(4)	(5)	(6)
Any common owners	0.182***	0.748***	0.881***	1.190***	3.441***	4.252***
	(0.023)	(0.183)	(0.275)	(0.086)	(0.759)	(1.244)
First-stage F-statistic	186.9	22.8	13.2	186.9	22.8	13.2
Number of observations	33,400	60,963	51,563	33,400	60,963	51,563
Includes additional startup controls, industry-by-year and headquarter-state-by-year fixed effects						

	Dep var. = IPO			Dep var. = Failure		
	No VC director	VC director	Common director	No VC director	VC director	Common director
Panel D: Startup exits	(1)	(2)	(3)	(4)	(5)	(6)
Any common owners	0.002	0.087***	0.128***	0.000	-0.372***	-0.508***
	(0.002)	(0.026)	(0.044)	(0.000)	(0.090)	(0.154)
First-stage F-statistic	186.9	22.8	13.2	186.9	22.8	13.2
Number of observations	33,400	60,963	51,563	33,400	60,963	51,563
Includes additional startup controls, industry-by-year and headquarter-state-by-year fixed effects						

Internet Appendix

Appendix A. Legal Analysis

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

The Duty of Loyalty: Managers owe two types of duties to the corporation, the duty of care and the duty of loyalty. Violation of the duty of care is rare due to the business judgment rule[9] and statutory provisions that permit exemptions from liability for such violations.[10] The duty of loyalty is the most important legal mechanism for disciplining managers as well as controlling shareholders, who are likewise subject to the duty. It is broadly defined as the duty to act in good faith to advance the best interests of the corporation (Strine et al., 2010). The duty of loyalty is relevant whenever managers or controlling shareholders face a conflict between the company's interests and their own. Broadly stated, the duty of loyalty regulates the following main circumstances that give rise to conflicts: (i) self-dealing transactions, (ii) duties of controlling shareholders to minority shareholders, (iii) fiduciary duties in hostile takeover transactions, and (iv) the corporate opportunity doctrine. When there is a potential conflict of interest, the courts generally review managers' decisions under the exacting "entire fairness" standard, which means that courts may evaluate the price of particular transactions and decide that they are unfair to the shareholders. Delaware and most other states do not allow broad exemptions from the duty of loyalty. Thus, subject to the discussion below, the duty of loyalty remains a mandatory feature of most states' corporate laws.

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

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

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

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

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

The corporate opportunity doctrine presents thorny problems for VC investors. When these firms make investments, they appoint their own representatives to the boards of these firms. These board members may be liable for violating the duty of loyalty to the corporation, and therefore also the corporate opportunity doctrine. The VC investors may likewise be liable for aiding and abetting the directors' breach of the duty of loyalty, or for violating their own fiduciary duties if they are deemed to be controlling shareholders (Boschner and Simmerman, 2016). This is a particular problem for VC investors that make investments in multiple companies in the same industry and whose board representatives serve on the boards of the multiple firms in which they invest. The VC investors and their board representatives may be required to share opportunities with the founders and other shareholders if the opportunity belongs to the firm they have invested in. Likewise, they

may be especially vulnerable to liability risk when there is a perception that they have diverted opportunities from one firm to another. Despite these apparent conflicts of interest, VC investors may be best positioned to pursue new opportunities or to allocate such opportunities among their investments.

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

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

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

a charter provision to this effect. We run relevant robustness tests that take these differences into account.

Broader Exemptions from the Duty of Loyalty: Some states permit broader exemptions from managerial liability than that offered by Delaware. As early as 1987, states such as Nevada and Virginia allowed firms to exempt directors and/or officers from the duty of loyalty altogether (Eldar and Magnolfi, 2020). Unlike section 102(b)(7) of the Delaware corporate law statute, the exemption provisions in these states do not require that the director or officer act in good faith, which is broadly interpreted as synonymous with the duty of loyalty (Strine et al., 2010). These exemptions appear to cover not only the corporate opportunity doctrine, but also other aspects of the duty of loyalty, such as self-dealing transactions and fiduciary duties in hostile takeovers. In addition, a few states exempt directors or officers by default without any charter provision or board action. Most notably, in 2001, Nevada changed its statute to make both directors and officers exempt from monetary liability for violating the duty of loyalty by default (Barzuza, 2012; Barzuza and Smith, 2014; Donelson and Yust, 2014; Eldar, 2018).

One could argue that in these states, directors and officers are already protected from liability under the corporate opportunity doctrine because they may already be exempted for monetary liability for violating the duty of loyalty. While we include robustness checks that account for broader exemptions from the duty of loyalty, we do not account for these broad exemptions in the main specifications for three main reasons. First, the statutes that exempt managers from the duty of loyalty do not cover controlling shareholders. In the context of VCs, it is not only necessary to exempt managers, but also to make sure that VC firms, which may be deemed to be controlling shareholders (Boschner and Simmerman, 2016), are not required to disclose and share business opportunities. Therefore, these broader exemptions may be insufficient to ensure that VC investors are not liable for appropriating business opportunities. Second, the actual scope of exemptions from the duty of loyalty and whether they apply to corporate opportunities is not conclusive, and it is not clear whether market participants interpret these laws as allowing COWs. Otherwise, it is hard to explain why Nevada enacted a statutory provision that permits COWs in 2007 (which does not cover controlling shareholders anyway), even though its directors and officers had already been exempt

from the duty of loyalty since 2001. Thus, at the very least, the statutes permitting corporate opportunity waivers may have clarified the law for the market. Third, most statutes that exempt directors or officers do not do so by default; rather, they typically require a charter amendment and hence shareholder approval to make the exemption effective. In contrast, the statutes permitting corporate opportunity waivers typically only require board action. Accordingly, the firms in our sample were less likely to adopt general duty of loyalty exemptions, but very likely to adopt COWs, particularly in the context of VC investment. Accordingly, the assumption that most firms take advantage of permissible exemptions may be too strong when shareholder approval is required.

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

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

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

Robustness Checks Associated with State Law Changes: First, several states, such as Nevada and Virginia, permit all firms to exempt directors (although typically with shareholder approval) from monetary liability for violating the duty of loyalty, which includes the corporate opportunity doctrine. Thus, it is possible that more states should be included in our treatment group. Table A.1 and Table A.2 test this nuance by defining the treatment as the earlier of two types of legislation: the passage of COW legislation or statutes that permit broader exemption from liability for violating the duty of loyalty. For ease of interpretation, we only report the results from the regression specification with year and firm fixed effects. Doing so allows us to consolidate our reduced form and instrumental variable (IV) analyses into two tables and include all the dependent variables we examined for common ownership, investment, directors, deal outcomes, and startup exits. 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). Table A.3 and Table A.4 show the results from regressions in which the treatment variable excludes Nevada and Table A.5 and Table A.6 show the results from regressions in which the treatment variable excludes Washington. Our results are materially the same as in our main specifications, although statistically weaker for acquisitions by commonly held startups.

Table A.1:
Reduced Form: Waivers from the Duty of Loyalty

This table presents a robustness test for our natural experiment that incorporates additional institutional and legal details that could change the efficacy of our treatment. In each panel, the results are from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). The definition of treatment is the earlier of COW legislation or the adoption of laws that permit firms to exempt directors from the duty of loyalty (including the corporate opportunity doctrine). Below the coefficient estimates are robust standard errors clustered by state of incorporation. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: Common ownership	Any common owner	Total number of common owners	
Treat × Post	0.118***	0.210***	
	(0.034)	(0.074)	
Adjusted R^2	62.9%	73.6%	
Number of observations	129,492	129,492	
Number of unique startups	13,428	13,428	
Panel B: Within-industry investment by VCs	Within-industry	Extensive margin	Intensive margin
Treat × Post	0.088***	0.017***	0.074***
	(0.031)	(0.005)	(0.027)
Adjusted R^2	10.6%	4.5%	9.8%
Number of observations	129,492	129,492	129,492
Number of unique startups	13,428	13,428	13,428
Panel C: VC directorships	VC directors	Other directorships held	Within-industry directorships held
Treat × Post	0.261***	0.377*	0.129*
	(0.081)	(0.214)	(0.075)
Adjusted R^2	73.3%	62.9%	60.5%
Number of observations	95,188	95,188	95,188
Number of unique startups	9,572	9,572	9,572
Panel D: VC deals	Deal volume	Late round	Deal size
Treat × Post	0.125***	0.042**	0.256***
	(0.047)	(0.017)	(0.097)
Adjusted R^2	12.2%	4.4%	6.7%
Number of observations	129,492	129,492	129,492
Number of unique startups	13,428	13,428	13,428
Startup fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes

Table A.2:
Instrumental Variable (IV): Waivers from the Duty of Loyalty

This table presents results from instrumental variable (IV) regressions examining the type of directorships held at startups with common owners where the instrumental variable is redefined as the earlier of COW legislation or the adoption of laws that permit firms to exempt directors from the duty of loyalty (including the corporate opportunity doctrine). Industry for common ownership is defined based on Preqin's primary and sub industry classification. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. The first-stage F-statistic is the Kleibergen-Paap Wald statistic. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: VC directorships	VC directors	Additional directorships held	Same-industry directorships held
Any common owners	1.995***	2.878***	0.985***
	(0.384)	(0.571)	(0.224)
First-stage F-statistic	122.9	122.9	122.9
t-statistic on instrument	11.09	11.09	11.09
Number of observations	95,144	95,144	95,144
Includes additional startup controls, startup and year fixed effects			

Panel B: VC deal outcomes	Deal count	Late round	Deal value
Any common owners	1.063***	0.353***	2.172***
	(0.095)	(0.045)	(0.204)
First-stage F-statistic	136.7	136.7	136.7
t-statistic on instrument	11.69	11.69	11.69
Number of observations	129,448	129,448	129,448
Includes additional startup controls, startup and year fixed effects			

Panel C: Startup exits	IPO	Trade sale	Common acq.	Failure
Any common owners	0.035***	0.037***	0.005	-0.070***
	(0.006)	(0.011)	(0.005)	(0.017)
First-stage F-statistic	133.5	133.5	133.5	133.5
t-statistic on instrument	11.56	11.56	11.56	11.56
Number of observations	129,448	129,448	129,448	129,448
Includes additional startup controls and year fixed effects				

Table A.3:
Reduced Form: Nevada Treatment Changed

This table presents a robustness test for our natural experiment that incorporates additional institutional and legal details that could change the efficacy of our treatment. In each panel, the results are from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). The definition of treatment changes Nevada from treated to untreated because its COW legislation does not cover controlling shareholders. Below the coefficient estimates are robust standard errors clustered by state of incorporation. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: Common ownership	Any common owner	Total number of common owners	
Treat × Post	0.123***	0.210***	
	(0.033)	(0.069)	
Adjusted R^2	62.9%	73.7%	
Number of observations	129,492	129,492	
Number of unique startups	13,428	13,428	
Panel B: Within-industry investment by VCs	Within-industry	Extensive margin	Intensive margin
Treat × Post	0.088***	0.017***	0.073***
	(0.027)	(0.005)	(0.023)
Adjusted R^2	10.6%	4.5%	9.8%
Number of observations	129,492	129,492	129,492
Number of unique startups	13,428	13,428	13,428
Panel C: VC directorships	VC directors	Other directorships held	Within-industry directorships held
Treat × Post	0.265***	0.345*	0.129*
	(0.085)	(0.199)	(0.069)
Adjusted R^2	73.3%	62.9%	60.5%
Number of observations	95,188	95,188	95,188
Number of unique startups	9,572	9,572	9,572
Panel D: VC deals	Deal volume	Late round	Deal size
Treat × Post	0.124***	0.040***	0.249***
	(0.041)	(0.015)	(0.088)
Adjusted R^2	12.2%	4.4%	4.4%
Number of observations	129,492	129,492	129,492
Number of unique startups	13,428	13,428	13,428
Startup fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes

Table A.4:
Instrumental Variable (IV): Nevada Treatment Changed

This table presents results from instrumental variable (IV) regressions examining the type of directorships held at startups with common owners where the instrumental variable is redefined to change Nevada from treated to untreated because its COW legislation does not cover controlling shareholders. Industry for common ownership is defined based on Preqin's primary and sub industry classification. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. The first-stage F-statistic is the Kleibergen-Paap Wald statistic. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: VC directorships	VC directors	Additional directorships held	Same-industry directorships held	
Any common owners	1.896***	2.473***	0.926***	
	(0.317)	(0.504)	(0.194)	
First-stage F-statistic	155.0	155.0	155.0	
t-statistic on instrument	12.45	12.45	12.45	
Number of observations	95,144	95,144	95,144	
Includes additional startup controls, startup and year fixed effects				
Panel B: VC deal outcomes	Deal count	Late round	Deal value	
Any common owners	1.002***	0.322***	2.020***	
	(0.082)	(0.040)	(0.175)	
First-stage F-statistic	170.4	170.4	170.4	
t-statistic on instrument	13.05	13.05	13.05	
Number of observations	129,448	129,448	129,448	
Includes additional startup controls, startup and year fixed effects				
Panel C: Startup exits	IPO	Trade sale	Common acq.	Failure
Any common owners	0.025***	0.035***	0.008**	-0.062***
	(0.004)	(0.008)	(0.004)	(0.012)
First-stage F-statistic	273.0	273.0	273.0	273.0
t-statistic on instrument	16.52	16.52	16.52	16.52
Number of observations	129,448	129,448	129,448	129,448
Includes additional startup controls and year fixed effects				

Table A.5:
Reduced Form: Washington Treatment Changed

This table presents a robustness test for our natural experiment that incorporates additional institutional and legal details that could change the efficacy of our treatment. In each panel, the results are from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). The definition of treatment changes Washington from treated to untreated because its COW legislation requires shareholder approval. Below the coefficient estimates are test statistics from robust standard errors clustered by state of incorporation. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: Common ownership	Any common owner	Total number of common owners	
Treat × Post	0.125***	0.215***	
	(0.032)	(0.068)	
Adjusted R^2	62.9%	73.7%	
Number of observations	129,492	129,492	
Number of unique startups	13,428	13,428	
Panel B: Within-industry investment by VCs	Within-industry	Extensive margin	Intensive margin
Treat × Post	0.093***	0.019***	0.075***
	(0.027)	(0.005)	(0.023)
Adjusted R^2	10.7%	4.5%	9.8%
Number of observations	129,492	129,492	129,492
Number of unique startups	13,428	13,428	13,428
Panel C: VC directorships	VC directors	Other directorships held	Within-industry directorships held
Treat × Post	0.274***	0.375*	0.136**
	(0.078)	(0.200)	(0.070)
Adjusted R^2	73.3%	62.9%	60.5%
Number of observations	95,188	95,188	95,188
Number of unique startups	9,572	9,572	9,572
Panel D: VC deals	Deal volume	Late round	Deal size
Treat × Post	0.130***	0.044***	0.266***
	(0.041)	(0.014)	(0.086)
Adjusted R^2	12.2%	4.4%	6.7%
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

Table A.6:
Instrumental Variable (IV): Washington Treatment Changed

This table presents results from instrumental variable (IV) regressions examining the type of directorships held at startups with common owners where the instrumental variable is redefined to change Washington from treated to untreated because its COW legislation requires shareholder approval. Industry for common ownership is defined based on Preqin's primary and sub industry classification. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. The first-stage F-statistic is the Kleibergen-Paap Wald statistic. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: VC directorships	VC directors	Additional directorships held	Same-industry directorships held
Any common owners	1.948***	2.663***	0.967***
	(0.321)	(0.508)	(0.196)
First-stage F-statistic	153.3	153.3	153.3
t-statistic on instrument	12.38	12.38	12.38
Number of observations	95,144	95,144	95,144
Includes additional startup controls, startup and year fixed effects			

Panel B: VC deal outcomes	Deal count	Late round	Deal value
Any common owners	1.039***	0.351***	2.135***
	(0.085)	(0.041)	(0.183)
First-stage F-statistic	166.1	166.1	166.1
t-statistic on instrument	12.89	12.89	12.89
Number of observations	129,448	129,448	129,448
Includes additional startup controls, startup and year fixed effects			

Panel C: Startup exits	IPO	Trade sale	Common acq.	Failure
Any common owners	0.026***	0.034***	0.006	-0.065***
	(0.004)	(0.008)	(0.004)	(0.012)
First-stage F-statistic	259.0	259.0	259.0	259.0
t-statistic on instrument	16.09	16.09	16.09	16.09
Number of observations	129,448	129,448	129,448	129,448
Includes additional startup controls and year fixed effects				

Appendix B. Data Construction

A. *The GGL Measure*

As an additional measure of common ownership we construct the GGL common ownership measure, which is a directional pairwise measure as in Gilje et al. (2019). For a single VC investor i, the directional pairwise measure from startup A to startup B is: $GGL\left(A,B\right)=\sum_{i=1}^{I}\alpha_{i,A}\beta_{i,A}\alpha_{i,B}$, where $\alpha_{i,A}$ is the fraction of shares held by VC investor i in startup A, $\beta_{i,A}$ is the weight of startup A in VC investor i's portfolio, and $\alpha_{i,B}$ is the fraction of shares held by VC investor i in startup B. To approximate, $\alpha_{i,A}$, we take the amount invested in startup A relative to the overall capital raised by startup A. When only the total capital raised in a round is reported but there is more than one VC investor in that round (as is often the case), we assume proportional investment. To calculate, $\beta_{i,A}$, we use all capital contributed to date by VC investor i to determine the overall value of the portfolio. Because we do not have data on market valuations of VC portfolios, we use the total capital contributed by each VC investor. For each startup, we average the pairwise measures for each year. Inevitably, the assumptions made in this computation make the GGL measure less reliable in the VC context. Accordingly, as discussed above, we use simpler measures in our main specifications.

A. *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.[11]

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 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:

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

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

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

[11] 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.

Appendix C. Additional Figures and Tables

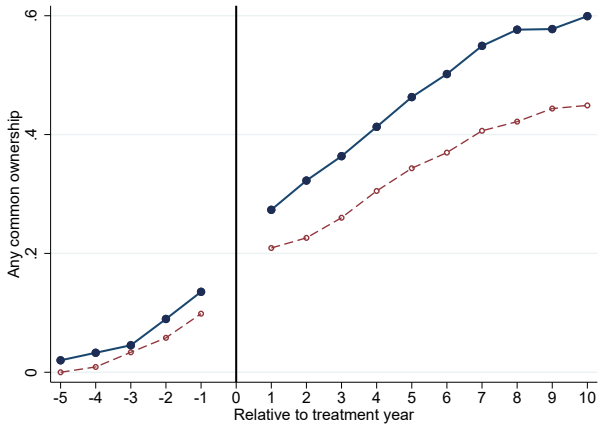

Fig. C.1. Parallel trends in common ownership for startups incorporated in states with a high concentration of venture capital (VC) funding. For this figure, the sample is limited to startups incorporated in Delaware, California, Massachusetts, and New York. The figure plots the parallel trend lines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

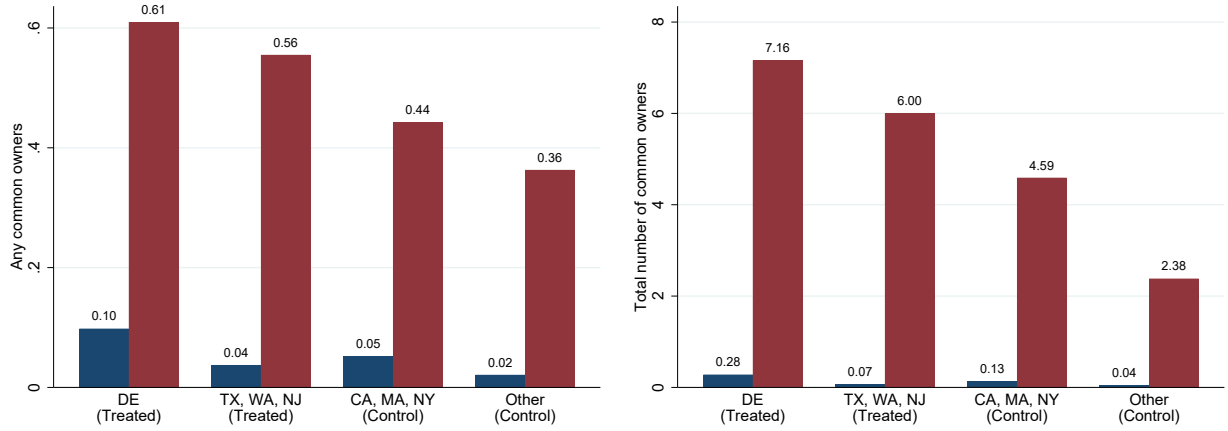

Fig. C.2. Common ownership rates before and after the corporate opportunity waiver (COW) legislation. The figures plot common ownership rates in the years before and after the law change for different subgroups of startups categorized by their state of incorporation. The first set of bars represents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

Table C.1:
Summary Statistics on Industry (Broadly Defined)

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

	(1)	(2)	(3)	(4)
	Startup-year		Unique	
Broadest industry definition	observations	Percent	startups	Percent
Computer	37,676	29.1%	3,979	29.6%
IT	23,445	18.1%	2,394	17.8%
Medtech	20,573	15.9%	2,007	14.9%
Internet	20,330	15.7%	2,271	16.9%
Pharmaceuticals	7,759	6.0%	814	6.1%
Business	7,214	5.6%	709	5.3%
Cleantech	4,460	3.4%	398	3.0%
Other	4,227	3.3%	475	3.5%
Media	2,041	1.6%	211	1.6%
Industrial	1,743	1.3%	170	1.3%

Table C.2:
Summary Statistics on Industry (Narrowly Defined)

This table presents summary statistics for the narrowest industry definition applied to our sample of VC-funded startups. This table lists all industries with more than 1,250 startup-year observations.

Narrowest industry definition	(1) Startup-year observations	(2) Percent	(3) Unique startups	(4) Percent
Pharmaceuticals	7,759	6.0%	814	6.1%
Telecoms	7,118	5.5%	785	5.8%
Medical devices	6,652	5.1%	596	4.4%
Software – analytics	5,393	4.2%	614	4.6%
Biotechnology	4,592	3.5%	502	3.7%
Semiconductors	3,939	3.0%	340	2.5%
IT	3,579	2.8%	355	2.6%
Software – general	3,536	2.7%	324	2.4%
Clean technology	3,497	2.7%	313	2.3%
Internet – general	3,397	2.6%	379	2.8%
Internet – business	3,348	2.6%	371	2.8%
Technology	3,266	2.5%	343	2.6%
Healthcare IT	3,245	2.5%	336	2.5%
Healthcare	3,112	2.4%	297	2.2%
Software – cloud	2,737	2.1%	300	2.2%
IT security	2,473	1.9%	275	2.0%
Internet – communication	2,345	1.8%	269	1.9%
Medical technology	2,168	1.7%	201	1.5%
Software – content management	1,801	1.4%	181	1.3%
Software – security	1,798	1.4%	193	1.4%
Software – systems management	1,643	1.3%	161	1.2%
Software – internet	1,599	1.2%	167	1.2%
Software – financial	1,416	1.1%	148	1.1%
Software – sales	1,410	1.1%	154	1.1%
Software – communication	1,409	1.1%	147	1.1%
Electronics	1,298	1.0%	126	0.9%
Software – integration	1,290	1.0%	138	1.0%
Internet – financial	1,273	1.0%	156	1.2%

Table C.3:
Summary Statistics on Industry (Narrowly Defined) Continued

This table presents summary statistics for the narrowest industry definition applied to our sample of VC-funded startups. This table lists all industries with less than 1,250 and more than 500 startup-year observations.

	(1)	(2)	(3)	(4)
	Startup-year		Unique	
Narrowest industry definition	observations	Percent	startups	Percent
Financial services	1,242	1.0%	137	1.0%
Internet – retail	1,237	1.0%	143	1.1%
Advertising	1,209	0.9%	112	0.8%
Marketing	1,206	0.9%	117	0.9%
Networks	1,204	0.9%	113	0.8%
Communications	1,204	0.9%	107	0.8%
Wireless	1,190	0.9%	112	0.8%
Gaming	1,135	0.9%	126	0.9%
Software – technology	1,129	0.9%	128	1.0%
Digital media	1,113	0.9%	111	0.8%
Software – advertising	1,086	0.8%	117	0.9%
Software – business	1,071	0.8%	114	0.8%
Internet – advertising	1,021	0.8%	99	0.7%
Business services	1,013	0.8%	97	0.7%
Internet – leisure	989	0.8%	101	0.8%
Renewable energy	917	0.7%	80	0.6%
Software – telecom	909	0.7%	87	0.6%
Software – mobile apps	905	0.7%	104	0.8%
Internet – mobile apps	905	0.7%	107	0.8%
Software – labor	881	0.7%	102	0.8%
Software – medical	813	0.6%	88	0.7%
Internet – education	803	0.6%	92	0.7%
Hardware	770	0.6%	81	0.6%
Manufacturing	708	0.5%	61	0.5%
Media	700	0.5%	63	0.5%
Food	683	0.5%	82	0.6%
Software IT	602	0.5%	69	0.5%
Software – billing	571	0.4%	52	0.4%
Consumer products	540	0.4%	65	0.5%
Agriculture	514	0.4%	61	0.5%
Education	504	0.4%	60	0.4%

Table C.4:
Summary Statistics on Industry (Narrowly Defined) Continued

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

	(1)	(2)	(3)	(4)
	Startup-year		Unique	
Narrowest industry definition	observations	Percent	startups	Percent
IT infrastructure	498	0.4%	53	0.4%
Software – industrial	495	0.4%	50	0.4%
Software – education	476	0.4%	47	0.4%
Internet – equipment	453	0.3%	46	0.3%
Software – supply chain	449	0.3%	43	0.3%
Internet – telecom	446	0.3%	44	0.3%
Internet – cloud	437	0.3%	52	0.4%
Industrial	434	0.3%	34	0.3%
Software – equipment	418	0.3%	40	0.3%
Consumer services	399	0.3%	46	0.3%
Robotics	385	0.3%	62	0.5%
Retail	383	0.3%	44	0.3%
Internet – software	381	0.3%	42	0.3%
Nanotechnology	352	0.3%	29	0.2%
Medical Instruments	349	0.2%	35	0.3%
Internet IT	304	0.2%	36	0.3%

Table C.5:
Summary Statistics on State of Incorporation and Reincorporations

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

	(1)	(2)	(3)	(4)
Panel A: Full sample	Observations	Percent	Unique startups	Percent
Delaware	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%

Table C.6:
Common Ownership and Startup Growth

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

	Deal count		Late stage		Deal value	
Panel A: VC deals	(1)	(2)	(3)	(4)	(5)	(6)
Any common owners	0.469***		0.193***		0.973***	
	(0.006)		(0.004)		(0.012)	
Total common owners		0.229***		0.108***		0.522***
		(0.003)		(0.002)		(0.007)
Adjusted R^2	19.9%	18.6%	7.6%	7.9%	14.0%	14.1%
Additional control variables	Yes	Yes	Yes	Yes	Yes	Yes
Startup fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Headquarter-state-by-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-by-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	128,587	128,587	128,587	128,587	128,587	128,587
Number of unique startups	13,336	13,336	13,336	13,336	13,336	13,336

	IPO		Trade sale		Common acq.		Failure	
Panel B: Exits	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Any common owners	0.0032***		0.0226***		0.0066***		0.0109***	
	(0.0002)		(0.0010)		(0.0004)		(0.0012)	
Total common owners		0.0022***		0.0104***		0.0052***		0.0003
		(0.0003)		(0.0005)		(0.0003)		(0.0006)
Adjusted R^2	2.1%	2.1%	1.8%	1.8%	0.2%	0.5%	2.8%	2.7%
Additional startup controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Headquarter-state-by-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry-by-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	128,587	128,587	128,587	128,587	128,587	128,587	128,587	128,587
Number of unique startups	13,336	13,336	13,336	13,336	13,336	13,336	13,336	13,336

Table C.7:
Placebo Tests for Common Ownership

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

	Any common owners		Total common owners	
Panel A: Placebo treatment dates	(1)	(2)	(3)	(4)
Treat × Post	-0.019	-0.012	-0.013	0.021
	(0.020)	(0.021)	(0.035)	(0.046)
Adjusted R^2	62.7%	67.6%	73.5%	75.5%
Number of observations	129,448	129,448	129,448	129,448
Number of unique firms	13,428	13,428	13,428	13,428
Panel B: Placebo treatment states	Any common owners		Total common owners	
Treat × Post	-0.020	-0.027	0.060	-0.026
	(0.019)	(0.042)	(0.042)	(0.073)
Adjusted R^2	65.6%	64.0%	68.7%	71.5%
Additional startup controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	No	Yes	No
Startup fixed effects	Yes	Yes	Yes	Yes
Headquarter-state-by-year fixed effects	No	Yes	No	Yes
Industry-by-year fixed effects	No	Yes	No	Yes
Number of observations	37,325	37,325	37,325	37,325
Number of unique startups	4,576	4,571	4,576	4,576

Table C.8:
Common Ownership (Alternative Definition)

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 VC-equivalent measure of common ownership that uses the methodology outlined in Gilje et al. (2019). In Panel A, the full sample is used. In Panel B, the sample is limited to startups incorporated in Delaware, California, Massachusetts, and New York. In Panel C, the sample is limited to startups that do not first incorporate in Delaware. Below the coefficient estimates are robust standard errors clustered by state of incorporation. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	Common ownership (GGL)	
Panel A: Full sample	(1)	(2)
Treat × Post	0.046***	0.017*
	(0.013)	(0.010)
Treat × Post after five years		0.066***
		(0.014)
Adjusted R^2	59.6%	62.2%
Number of observations	129,448	128,587
Panel B: DE, CA, MA, and NY sample	(1)	(2)
Treat × Post	0.072***	0.003
	(0.012)	(0.011)
Treat × Post after five years		0.093***
		(0.013)
Adjusted R^2	60.7%	63.3%
Number of observations	112,106	111,125
Panel C: Non-DE sample	(1)	(2)
Treat × Post	0.058**	0.016
	(0.026)	(0.027)
Treat × Post after five years		0.074***
		(0.025)
Adjusted R^2	53.6%	56.0%
Number of observations	43,592	43,179
Additional startup controls	Yes	Yes
Year fixed effects	Yes	No
Startup fixed effects	Yes	Yes
Headquarter-state-by-year fixed effects	No	Yes
Industry-by-year fixed effects	No	Yes
Number of observations	43,592	43,126
Number of unique startups	3,699	3,671

Table C.9:
Common Ownership (Alternative Samples)

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

Panel A: Excluding "Spray and pray" investment	Any common owner (1)	Total number of common owners (2)
Treat × Post	0.104***	0.187***
	(0.021)	(0.040)
Adjusted R^2	64.1%	74.5%
Number of observations	103,947	103,947
Panel B: Excluding Dotcom bubble	Any common owner	Total number of common owners
Treat × Post	0.133***	0.222***
	(0.029)	(0.058)
Adjusted R^2	63.2%	74.7%
Number of observations	120,356	120,356
Panel C: Excluding financial crisis	Any common owner	Total number of common owners
Treat × Post	0.111***	0.191***
	(0.022)	(0.041)
Adjusted R^2	63.4%	74.1%
Number of observations	109,731	109,731
All regressions include additional startup controls, startup and year fixed effects.		

Table C.10:
Startup Growth by Strength of Intellectual Property Protection

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

	Any common owners		Total number of common owners		Extensive margin investments	
	Strong	Weak	Strong	Weak	Strong	Weak
Panel A: Common ownership	(1)	(2)	(3)	(4)	(5)	(6)
Treat × Post	0.116***	0.098***	0.251***	0.182***	0.009*	0.025***
	(0.023)	(0.018)	(0.069)	(0.034)	(0.004)	(0.003)
Adjusted R^2	64.0%	65.4%	75.7%	76.7%	3.4%	4.5%
Number of observations	30,519	32,910	30,519	32,910	30,519	32,910
Includes additional startup controls, startup, headquarter-state-by-year, and industry-by-year fixed effects.						

	Deal volume		Late stage		Deal value	
	Strong	Weak	Strong	Weak	Strong	Weak
Panel B: VC deals	(1)	(2)	(3)	(4)	(5)	(6)
Any common owners	0.661***	1.279***	0.352***	0.478***	1.677***	2.263***
	(0.151)	(0.196)	(0.089)	(0.084)	(0.375)	(0.361)
Includes additional startup controls, headquarter-state-by-year, and industry-by-year fixed effects.						

	IPOs		Trade sales		Failures	
	Strong	Weak	Strong	Weak	Strong	Weak
Panel C: Startup exits	(1)	(2)	(3)	(4)	(5)	(6)
Any common owners	0.056***	0.032***	-0.006	0.045*	-0.077**	-0.124***
	(0.020)	(0.009)	(0.025)	(0.024)	(0.039)	(0.039)
First-stage F-statistic	26.8	39.7	26.8	39.7	26.8	39.7
t-statistic on instrument	5.17	6.30	5.17	6.30	5.17	6.30
Number of observations	30,519	32,912	30,519	32,912	30,519	32,912
Includes additional startup controls, headquarter-state-by-year, and industry-by-year fixed effects.						

Table C.11:
Startup Exits and Venture Capital (VC) Bargaining Power

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

	IPOs	Trade sales	Common acq.	Failures
Panel A: Startup exits	(1)	(2)	(3)	(4)
Any common owners	0.027***	0.035***	0.010**	-0.077***
	(0.005)	(0.011)	(0.005)	(0.015)
Additional startup controls	Yes	Yes	Yes	Yes
Headquarter-state-by-year fixed effects	Yes	Yes	Yes	Yes
Industry-by-year fixed effects	Yes	Yes	Yes	Yes
First-stage F-statistic	179.7	179.7	179.7	179.7
t-statistic on instrument	13.40	13.40	13.40	13.40
Number of observations	103,873	103,873	103,873	103,873

Table C.12:
Industrial Specialization by Venture Capital (VC) Investors

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

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

Appendix D. Additional Instrumental Variable Tests

Table D.1:
Instrumental Variable (IV): Entrepreneurial States

This table presents results from instrumental variable (IV) regressions examining the type of directorships held at startups with common owners using an alternative subsample of high entrepreneurial states. The sample is limited to startups incorporated in Delaware, California, Massachusetts, and New York. Industry for common ownership is defined based on Preqin's primary and sub industry classification. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. The first-stage F-statistic is the Kleibergen-Paap Wald statistic. ***, ** and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: VC directorships	VC directors	Additional directorships held	Same-industry directorships held
Any common owners	1.781***	3.313***	0.819***
	(0.382)	(0.641)	(0.224)
First-stage F-statistic	131.8	131.8	131.8
t-statistic on instrument	11.48	11.48	11.48
Number of observations	83,885	83,885	83,885
Includes additional startup controls, startup, industry-by-year and headquarter-state-by-year fixed effects			

Panel B: VC deal outcomes	Deal count	Late round	Deal value
Any common owners	1.197***	0.392***	2.369***
	(0.098)	(0.047)	(0.208)
First-stage F-statistic	157.7	157.7	157.7
t-statistic on instrument	12.56	12.56	12.56
Number of observations	111,253	111,253	111,253
Includes additional startup controls, startup, industry-by-year and headquarter-state-by-year fixed effects			

Panel C: Startup exits	IPO	Trade sale	Common acq.	Failure
Any common owners	0.027***	0.046***	0.010**	-0.092***
	(0.005)	(0.011)	(0.005)	(0.017)
First-stage F-statistic	156.0	156.0	156.0	156.0
t-statistic on instrument	12.49	12.49	12.49	12.49
Number of observations	111,255	111,255	111,255	111,255
Includes additional startup controls, industry-by-year and headquarter-state-by-year fixed effects				

Table D.2:
Instrumental Variable (IV): No Delaware

This table presents robustness tests for the instrumental variable (IV) regressions examining the consequences of common ownership using an alternative subsample that excludes startups originally incorporated in Delaware. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: VC directorships	VC directors	Additional directorships held	Same-industry directorships held
Any common owners	2.641***	3.575***	1.180***
	(0.338)	(0.578)	(0.184)
First-stage F-statistic	168.3	168.3	168.3
t-statistic on instrument	12.97	12.97	12.97
Number of observations	29,314	29,314	29,314
Includes additional startup controls, startup, industry-by-year and headquarter-state-by-year fixed effects			

Panel B: VC deal outcomes	Deal count	Late round	Deal value
Any common owners	1.144***	0.396***	2.376***
	(0.088)	(0.042)	(0.189)
First-stage F-statistic	188.0	188.0	188.0
t-statistic on instrument	13.71	13.71	13.71
Number of observations	43,179	43,179	43,179
Includes additional startup controls, startup, industry-by-year and headquarter-state-by-year fixed effects			

Panel C: Startup exits	IPO	Trade sale	Common acq.	Failure
Any common owners	0.064***	0.048**	0.009	-0.014
	(0.015)	(0.022)	(0.010)	(0.027)
First-stage F-statistic	57.9	57.9	57.9	57.9
t-statistic on instrument	7.61	7.61	7.61	7.61
Number of observations	43,179	43,179	43,179	43,179
Includes additional startup controls, industry-by-year and fixed effects				

Table D.3:
Instrumental Variable (IV): "Spray and Pray" Investment

This table presents robustness tests for the instrumental variable (IV) regressions examining the consequences of common ownership. These regressions exclude startup-year observations associated with the "spray and pray" investment strategy adopted after the advent of cloud computing (i.e., software startups from 2006 to 2018). Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: VC directorships	VC directors	Additional directorships held	Same-industry directorships held
Any common owners	2.263***	2.631***	0.582***
	(0.447)	(0.665)	(0.187)
First-stage F-statistic	90.3	90.3	90.3
t-statistic on instrument	9.50	9.50	9.50
Number of observations	74,420	74,420	74,420
Includes additional startup controls, startup, industry-by-year and headquarter-state-by-year fixed effects			

Panel B: VC deal outcomes	Deal count	Late round	Deal value
Any common owners	1.199***	0.376***	2.378***
	(0.119)	(0.055)	(0.250)
First-stage F-statistic	106.2	106.2	106.2
t-statistic on instrument	10.31	10.31	10.31
Number of observations	103,165	103,165	103,165
Includes additional startup controls, startup, industry-by-year and headquarter-state-by-year fixed effects			

Panel C: Startup exits	IPO	Trade sale	Common acq.	Failure
Any common owners	0.030***	0.044***	0.009*	-0.079***
	(0.006)	(0.012)	(0.005)	(0.018)
First-stage F-statistic	133.0	133.0	133.0	133.0
t-statistic on instrument	11.53	11.53	11.53	11.53
Number of observations	103,167	103,167	103,167	103,167
Includes additional startup controls, industry-by-year and headquarter-state-by-year fixed effects				

Table D.4:
Instrumental Variable (IV): Excluding Dotcom Bubble

This table presents robustness tests for the instrumental variable (IV) regressions examining the consequences of common ownership. These regressions exclude startup-year observations associated with the bursting of the dotcom bubble between 1999 and 2001. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: VC directorships	VC directors	Additional directorships held	Same-industry directorships held
Any common owners	1.711***	2.931***	0.852***
	(0.342)	(0.546)	(0.195)
First-stage F-statistic	157.7	157.7	157.7
t-statistic on instrument	12.56	12.56	12.56
Number of observations	88,407	88,407	88,407
Includes additional startup controls, startup, industry-by-year and headquarter-state-by-year fixed effects			

Panel B: VC deal outcomes	Deal count	Late round	Deal value
Any common owners	1.241***	0.415***	2.484***
	(0.096)	(0.044)	(0.198)
First-stage F-statistic	166.6	166.6	166.6
t-statistic on instrument	12.91	12.91	12.91
Number of observations	119,597	119,597	119,597
Includes additional startup controls, startup, industry-by-year and headquarter-state-by-year fixed effects			

Panel C: Startup exits	IPO	Trade sale	Common acq.	Failure
Any common owners	0.025***	0.045***	0.009**	-0.071***
	(0.004)	(0.010)	(0.005)	(0.014)
First-stage F-statistic	213.4	213.4	213.4	213.4
t-statistic on instrument	14.61	14.61	14.61	14.61
Number of observations	119,597	119,597	119,597	119,597
Includes additional startup controls, industry-by-year and headquarter-state-by-year fixed effects				

Table D.5:
Instrumental Variable (IV): Excluding Financial Crisis

This table presents robustness tests for the instrumental variable (IV) regressions examining the consequences of common ownership. These regressions exclude startup-year observations associated with the financial crisis period between 2007 and 2009. Below the coefficient estimates are robust standard errors clustered by startup and adjusted for small clusters. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: VC directorships	VC directors	Additional directorships held	Same-industry directorships held
Any common owners	1.824***	2.958***	0.893***
	(0.350)	(0.587)	(0.206)
First-stage F-statistic	138.6	138.6	138.6
t-statistic on instrument	11.77	11.77	11.77
Number of director observations	80,062	80,062	80,062

Includes additional startup controls, startup, industry-by-year and headquarter-state-by-year fixed effects

Panel B: VC deal outcomes	Deal volume	Late round	Deal value
Any common owners	1.180***	0.387***	2.360***
	(0.105)	(0.050)	(0.222)
First-stage F-statistic	142.7	142.7	142.7
t-statistic on instrument	11.95	11.95	11.95
Number of observations	108,961	108,961	108,961

Includes additional startup controls, startup, industry-by-year and headquarter-state-by-year fixed effects

Panel C: Startup exits	IPO	Trade sale	Common acq.	Failure
Any common owners	0.034***	0.043***	0.009*	-0.096***
	(0.005)	(0.011)	(0.005)	(0.017)
First-stage F-statistic	202.6	202.6	202.6	202.6
t-statistic on instrument	14.23	14.23	14.23	14.23
Number of observations	108,961	108,961	108,961	108,961

Includes additional startup controls, industry-by-year and headquarter-state-by-year fixed effects

Appendix E. Exclusion Restriction Robustness Tests

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

The first approach we use to detect and investigate sensitivity to violations of the exclusion restriction is to estimate the direct effect of the IV on the outcome in a subsample for which the IV likely does not affect the treatment variable, which following the prior literature we refer to as the "zero-first-stage test" (Angrist et al., 2010). This test provides potential evidence for or against the exclusion restriction. This informal test can never verify the exclusion restriction, but it can build confidence that the exclusion restriction is satisfied. The intuition for the test follows the logic of a placebo test. If the first stage is zero, then the reduced-form effect of the IV on the outcome variable should also be zero if the exclusion restriction is satisfied. In our setting, a zero-first-stage implies that being in a treated state has no effect on common ownership. We consider two candidate zero-first stage groups. The first candidate group consists of startups that have not raised capital within at least three years. Presumably, these startups are past a stage where investment by a common owner would change their trajectory. This suggests our instrument is unlikely to matter for this group. We use this candidate group to evaluate our directorship and VC deal outcomes. Given that startup exits can be linked to timing since the most recent deal, we select a different candidate zero-first-stage group. Specifically, we evaluate utilities, power, insurance, defense, and aerospace startups. The rationale is that these are regulated industries that are often subject to

governmental and public scrutiny, which may require common owners to maintain their loyalty obligations. We note that this group is limited to only 602 observations.

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

Similarly, Panel A of Table E.2 and Panel B of Table E.3 summarize these regression results for VC deal outcomes and startup exits respectively. Again, we see great contrasts between the zero-first-stage group and the remaining sample. In each case, the zero-first-stage group is statistically insignificant while the remaining sample is not. In most cases the point estimate is close to zero, but the smaller sample size does lend itself to noisy estimates. We note that we cannot run these tests for the common acquisitions variable because we have no observations for common acquisitions in the zero-first-stage group.

Next, we explore the importance of the exclusion restriction holding precisely for the IV coefficients on common ownership that we estimate. Specifically, we follow the local-to-zero method outlined in Conley et al. (2012) that incorporates different degrees of exclusion restriction violation to generate a plausibly exogenous IV estimate. Their method involves estimating the following equation:

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

where ζ is a parameter measuring the plausibility of the exclusion restriction and Z_{jnhit} is the IV for being in a treated state after the law change. The difference between the primary IV specification

(i.e., Equation 3 in the main paper) is the presence of the term ζZ_{jnhit}. Typically, ζZ_{jnhit} is removed from the specification, because the exclusion restriction holds and $\zeta = 0$. If the exclusion restriction is not violated, then this method produces the same confidence interval as a traditional IV regression.

If the exclusion restriction is violated, then different degrees of violation can be incorporated into the original estimate. Mathematically, the estimates that allow for exclusion restriction violation are Bayesian. The estimates come from updating a prior without exclusion restriction violation to a posterior with violations. To see this more clearly, let the distribution for β be approximated as follows:

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

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

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

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

Alternatively, Kippersluis and Rietveld (2018) develop a method for estimating a sensible prior distribution to use as an input when generating plausibly exogenous IV estimates. The intuition for their approach to generating an informative prior is to use observable changes in coefficient estimates and standard errors associated with the IV stemming from different subsamples of the data. They suggest using the zero-first-stage test discussed above in conjunction with a formula based on

Imbens and Rubin (2015) for estimating the variance. Specifically, $\Omega_\zeta = \left(0.125\sqrt{S_Z^2 + S_R^2}\right)$ where S_Z is the standard error for the zero-first-stage test and S_R is the standard error from the remaining sample.

Panel B of Table E.1 summarizes the plausibly exogenous IV estimates for directorships. The results in the first row assume exclusion restriction violations are symmetric around zero but introduces uncertainty from the Imbens and Rubin approach. While the size of the standard errors increases, it is not enough to make our results statistically insignificant. The results in the second row allow for a positive violation of the exclusion restriction, where the mean is 0.05 and the variance continues to be that from the Imbens and Rubin approach. To put the size of the selected potential positive violation into context, we can compare it with our point estimate for common ownership. Doing so suggests we are allowing for a meaningful unobservable factor that is between 3% to 7% of the magnitude of common ownership. In each case, the results from the plausibly exogenous IV estimates reveal that common ownership remains positively and significantly associated with directorship outcomes even if the exclusion restriction does not hold precisely.

Panel B of Table E.2 and Table E.3 summarize the plausibly exogenous tests for the VC deals and startup exits. First, we see that adding the Imbens and Rubin uncertainty does not materially change the inferences. Second, we see that even with small positive violations of the exclusion restriction, after we account for these hypothetical violations, we still have statistically significant estimates, albeit of a smaller economic magnitude. Thus, these analyses lead us to conclude that our IV is useful, even if there are potentially minor exclusion restriction violations.

Table E.1:
Assessing the Exclusion Restriction Assumption for Directorships

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

Panel A. The effect of COW on directorships	VC directorships (1)	Additional directorships (2)	Same-industry directorships (3)
Zero-first stage group #1	0.194	0.182	0.032
	(0.126)	(0.134)	(0.056)
Observations	6,115	6,115	6,115
Remaining group	0.231***	0.365***	0.125***
	(0.038)	(0.065)	(0.023)
Observations	105,023	105,023	105,023
Panel B. The effect of common ownership on directorships	VC directorships (1)	Additional directorships (2)	Same-industry directorships (3)
IV	1.382***	1.916***	0.723***
	(0.296)	(0.468)	(0.169)
Plausibly Exogenous IV, $\zeta \sim(0.00, \Omega_\zeta)$	1.382***	1.916***	0.723***
	(0.327)	(0.494)	(0.181)
Plausibly Exogenous IV, $\zeta \sim(0.05, \Omega_\zeta)$	0.959***	1.492***	0.300*
	(0.327)	(0.494)	(0.181)
Additional startup controls	Yes	Yes	Yes
Startup fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	129,448	129,448	129,448

Table E.2:
Assessing the Exclusion Restriction Assumption for Deal Outcomes

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

	Deal count	Late stage	Deal value
Panel A. The effect of COW on deal outcomes	(1)	(2)	(3)
Zero-first stage group #1	-0.028	0.013	-0.022
	(0.037)	(0.023)	(0.092)
Observations	6,115	6,115	6,115
Remaining group	0.106***	0.031***	0.221***
	(0.010)	(0.006)	(0.023)
Observations	105,023	105,023	105,023
Panel B. The effect of common ownership on deal outcomes	(1)	(2)	(3)
IV	1.025***	0.335***	2.072***
	(0.086)	(0.041)	(0.183)
Plausibly Exogenous IV, $\zeta \sim(0.00, \Omega_\zeta)$	1.025****	0.335***	2.072***
	(0.095)	(0.048)	(0.209)
Plausibly Exogenous IV, $\zeta \sim(0.05, \Omega_\zeta)$	0.602***	-0.087	1.650***
	(0.095)	(0.048)	(0.209)
Additional startup controls	Yes	Yes	Yes
Startup fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	129,448	129,448	129,448

Table E.3:
Assessing the Exclusion Restriction Assumption for Startup Exits

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

	IPO	Trade sale	Failure
Panel A. The effect of COW on startup exits	(1)	(2)	(3)
Zero-first stage group #1	0.0052	0.0109	0.0135
	(0.0049)	(0.0112)	(0.0126)
Observations	602	602	602
Remaining group	0.0026***	0.0048***	-0.0078***
	(0.0045)	(0.0011)	(0.0013)
Observations	127,832	127,832	127,832
Panel B. The effect of common ownership on startup exits	(1)	(2)	(3)
IV	0.0250***	0.0453***	-0.0697***
	(0.0044)	(0.0114)	(0.0137)
Plausibly Exogenous IV, $\zeta \sim(0.0000, \Omega_\zeta)$	0.0250***	0.0453***	-0.0697***
	(0.0070)	(0.0162)	(0.0200)
Plausibly Exogenous IV, $\zeta \sim(0.0001, \Omega_\zeta)$	0.0241***	0.0445***	-0.0705***
	(0.0070)	(0.0162)	(0.0200)
Additional startup controls	Yes	Yes	Yes
Headquarter-by-year fixed effects	Yes	Yes	Yes
Industry-by-year fixed effects	Yes	Yes	Yes
Observations	128,587	128,587	128,587

Appendix F. Additional Difference-in-differences Tests

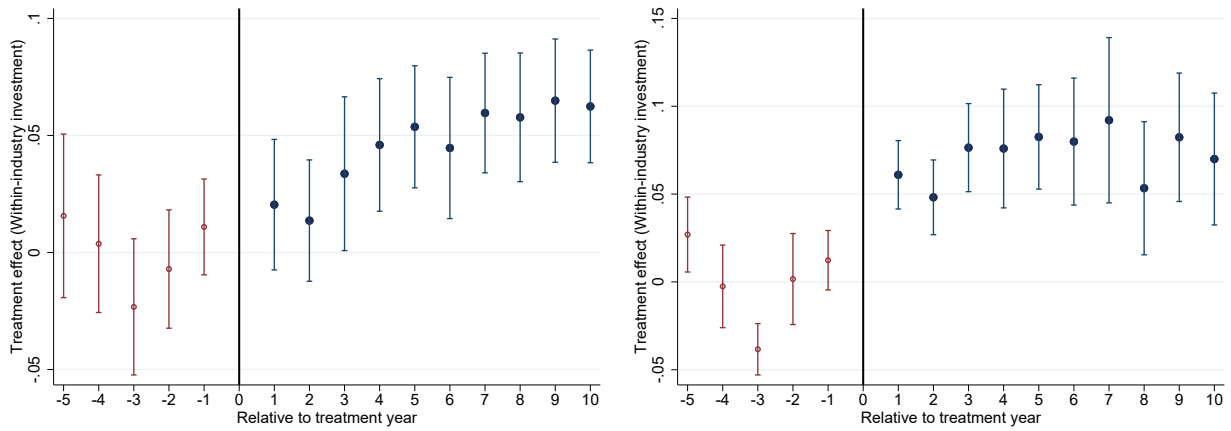

Fig. F.1. Dynamic effects of corporate opportunity waiver (COW) legislation. The figures plot the impact of COW legislation on same-industry investment following the law changes by year relative to the law change. The figures show a window spanning from five years before the law changes to ten years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

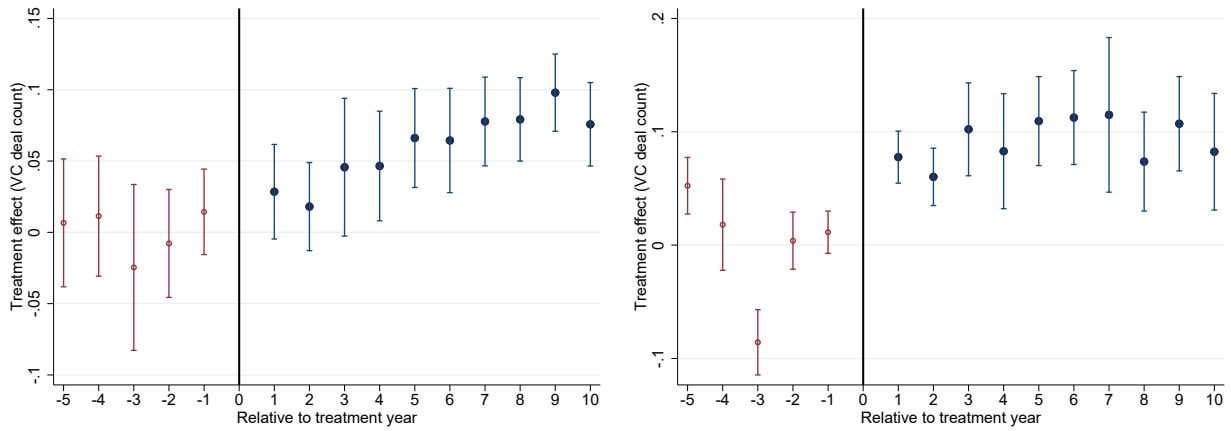

Fig. F.2. Dynamic effects of corporate opportunity waiver (COW) legislation. The figures plot the impact of COW legislation on VC deal volume following the law changes by year relative to the law change. The figures show a window spanning from five years before the law changes to ten years after the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

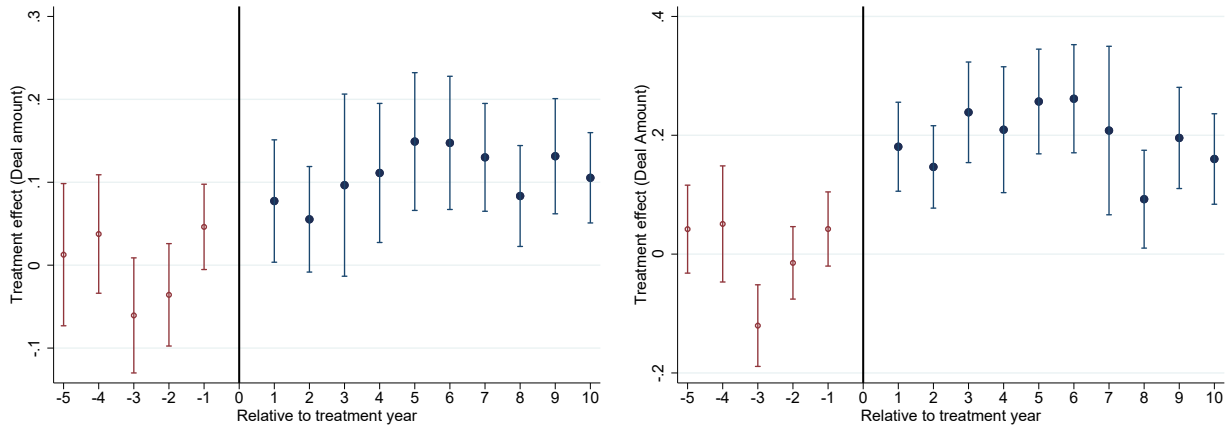

Fig. F.3. Dynamic effects of corporate opportunity waiver (COW) legislation. The figures plot the impact of COW legislation on VC deal amount following the law changes by year relative to the law change. The figures show a window spanning from five years before the law changes to ten years after the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

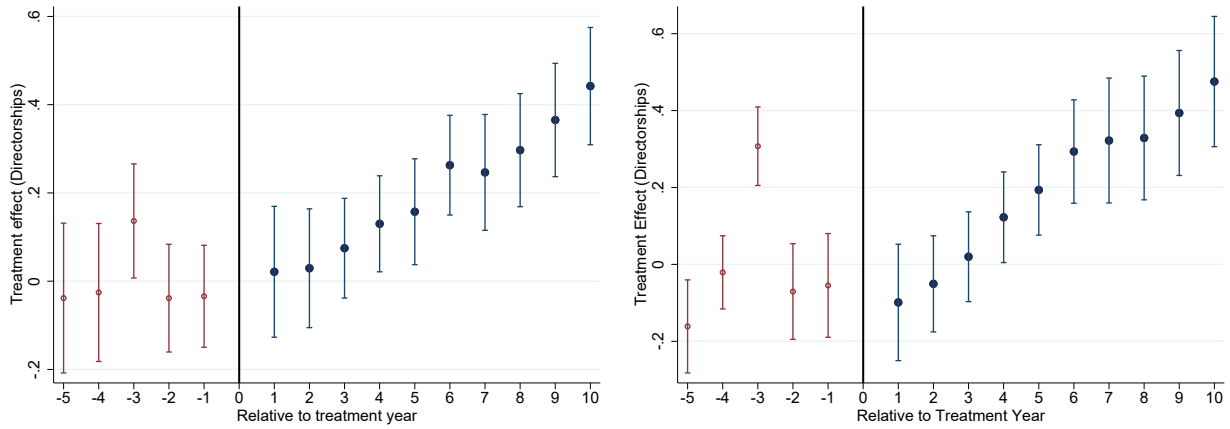

Fig. F.4. Dynamic effects of corporate opportunity waiver (COW) legislation. The figures plot the impact of COW legislation on VC directorships following the law changes by year relative to the law change. The figures show a window spanning from five years before the law changes to ten years after the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

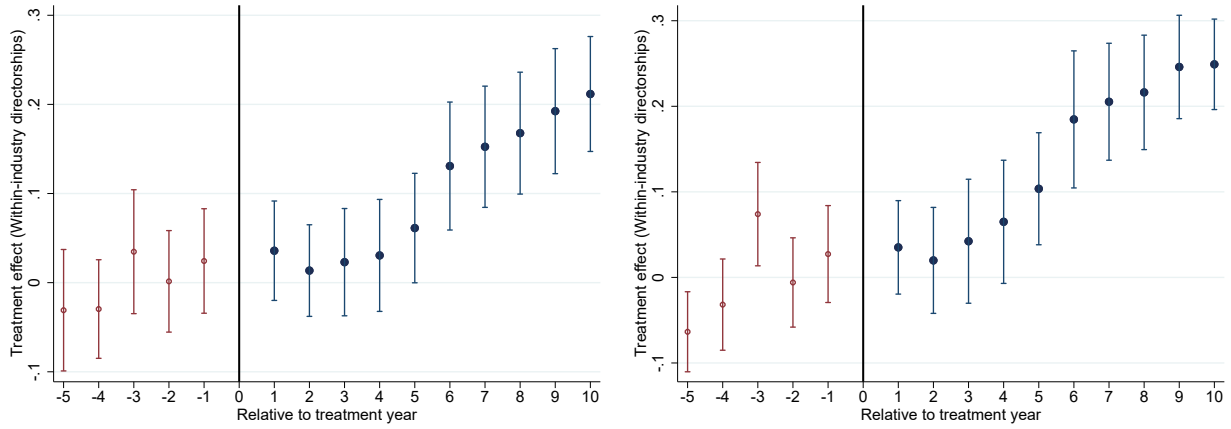

Fig. F.5. Dynamic effects of corporate opportunity waiver (COW) legislation. The figures plot the impact of COW legislation on same-industry VC directorships following the law changes by year relative to the law change. The figures show a window spanning from five years before the law changes to ten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

Table F.1:
Reduced Form: Within-industry Investment by Venture Capital (VC) Investors

This table presents the results from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). In Panel A, the dependent variable is an indicator variable for whether a startup receives financing from a VC with a previous investment in the same industry. In Panel B, the dependent variable is an indicator variable for whether the startup 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 startup receives VC investment in later rounds from the same VC fund that already invested in the startup (intensive margin). For the dependent variables, industry is defined based on Preqin's primary industry classification. For the industry fixed effects, the adjusted primary industry classification, which is coarser than the primary industry classification, is used. Below the coefficient estimates are robust standard errors clustered by state of incorporation. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: Within-industry investment	(1)	(2)	(3)	(4)	(5)
Treat × Post	0.063***	0.086***	0.069***	0.087***	0.083***
	(0.012)	(0.025)	(0.025)	(0.012)	(0.012)
Treat × Post after five years			0.039***	0.033***	0.032***
			(0.011)	(0.006)	(0.006)
Adjusted R^2	3.2%	10.6%	10.7%	10.6%	11.0%
Panel B: Extensive margin					
Treat × Post	0.014***	0.017***	0.017***	0.021***	0.019***
	(0.003)	(0.005)	(0.006)	(0.003)	(0.003)
Treat × Post after five years			0.001	-0.000	0.001
			(0.004)	(0.003)	(0.004)
Adjusted R^2	1.4%	4.5%	4.5%	4.3%	4.4%
Panel C: Intensive margin					
Treat × Post	0.051***	0.071***	0.053***	0.068***	0.066***
	(0.011)	(0.020)	(0.019)	(0.010)	(0.010)
Treat × Post after five years			0.040***	0.035***	0.034***
			(0.007)	(0.004)	(0.004)
Adjusted R^2	2.4%	9.8%	9.8%	9.8%	10.1%
Additional control variables	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	No	No
Startup 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	129,448	129,448	129,448	128,587	128,587
Number of unique startups	13,428	13,428	13,428	13,336	13,336

Table F.2:
Reduced Form: Directorships

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

Panel A: VC directors	(1)	(2)	(3)	(4)	(5)
Treat × Post	0.159**	0.254***	0.102*	0.079	0.090*
	(0.064)	(0.071)	(0.052)	(0.050)	(0.049)
Treat × Post after five years			0.318***	0.399***	0.336***
			(0.068)	(0.071)	(0.047)
Adjusted R^2	36.5%	73.3%	73.3%	73.4%	73.7%
Panel B: Additional directorships held					
Treat × Post	0.374***	0.351***	0.176*	0.232***	0.191***
	(0.086)	(0.123)	(0.107)	(0.045)	(0.035)
Treat × Post after five years			0.364***	0.399***	0.413***
			(0.067)	(0.071)	(0.071)
Adjusted R^2	21.9%	62.9%	62.9%	63.2%	63.8%
Panel C: Within-industry directorships held					
Treat × Post	0.059***	0.129***	0.055***	0.049***	0.028***
	(0.026)	(0.037)	(0.020)	(0.011)	(0.009)
Treat × Post after five years			0.155***	0.189***	0.175***
			(0.026)	(0.025)	(0.027)
Adjusted R^2	11.1%	60.5%	60.6%	60.7%	63.9%
Additional startup controls	Yes	Yes	Yes	Yes	
Year fixed effects	Yes	Yes	Yes	No	No
Startup 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	95,144	95,144	95,144	94,570	94,570
Number of unique startups	9,572	9,572	9,572	9,516	9,516

Table F.3:
Reduced Form: Venture Capital (VC) Deals

This table presents the results from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). In Panel A, the dependent variable is deal volume, which is defined as any VC equity financing deal a firm receives in a given year. In Panel B, the dependent variable is late-round VC deal volume, which is defined as a round of equity VC financing greater than the seed or first round. In Panel C, the dependent variable is deal value, defined as the natural log of one plus the deal value in millions of 2010 dollars. Below the coefficient estimates are robust standard errors clustered by state of incorporation. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: VC deal volume	(1)	(2)	(3)	(4)	(5)
Treat × Post	0.074***	0.121***	0.095***	0.122***	0.117***
	(0.016)	(0.071)	(0.035)	(0.017)	(0.017)
Treat × Post after five years			0.059***	0.051***	0.054***
			(0.016)	(0.009)	(0.009)
Adjusted R^2	4.5%	12.2%	12.3%	12.3%	12.7%
Panel B: Late round deal volume					
Treat × Post	0.036***	0.040***	0.035***	0.048***	0.045***
	(0.006)	(0.011)	(0.014)	(0.007)	(0.007)
Treat × Post after five years			0.009	0.003	0.003
			(0.009)	(0.005)	(0.005)
Adjusted R^2	1.6%	4.4%	4.4%	4.4%	4.7%
Panel C: Deal value					
Treat × Post	0.148***	0.245***	0.209***	0.266***	0.249***
	(0.038)	(0.070)	(0.020)	(0.040)	(0.038)
Treat × Post after five years			0.082***	0.065***	0.175***
			(0.030)	(0.017)	(0.027)
Adjusted R^2	2.1%	6.7%	6.7%	6.8%	7.4%
Year fixed effects	Yes	Yes	Yes	No	No
Startup 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	129,448	129,448	129,448	128,587	128,587
Number of unique startups	13,428	13,428	13,428	13,336	13,336

Table F.4:
Reduced Form: Time Between Venture Capital (VC) Deals

This table presents the results from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). The dependent variable is the time between VC deal rounds, defined in years. Below the coefficient estimates are robust standard errors clustered by state of incorporation. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Dependent variable = Time between deals (years)			
	(1)	(2)	(3)
Treat	0.053***		
	(0.017)		
Treat × Post	-0.117***	0.033	0.034
	(0.018)	(0.062)	(0.055)
Treat × Post after five years			-0.014
			(0.005)
Additional startup controls	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Startup fixed effects	No	Yes	Yes
Headquarter-state-by-year fixed effects	No	No	No
Industry-by-year fixed effects	No	No	No
Adjusted R^2	1.9%	14.8%	14.8%
Number of observations	26,767	22,503	22,503
Number of unique startups	11,826	7,562	7,562

Table F.5:
Reduced Form: High Entrepreneurship States

This table presents the results from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). For these regressions, the sample is limited to startups incorporated in Delaware, California, Massachusetts, and New York. Below the coefficient estimates are robust standard errors clustered by state of incorporation. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: Within-industry investment by VCs	Within-industry	Extensive margin	Intensive margin
Treat × Post	0.132***	0.022***	0.113***
	(0.021)	(0.003)	(0.019)
Adjusted R^2	10.4%	4.5%	9.6%
Number of observations	112,106	112,106	112,106
Number of unique startups	12,056	12,056	12,056
Includes additional startup controls, startup and year fixed effects			

Panel B: VC directorships	VC directors	Other directorships held	Within-industry directorships held
Treat × Post	0.298***	0.624**	0.202**
	(0.078)	(0.113)	(0.022)
Adjusted R^2	73.6%	63.5%	60.9%
Number of director observations	84,429	84,429	84,429
Number of unique startups	8,802	8,802	8,802
Includes additional startup controls, startup and year fixed effects			

Panel C: VC deals	Deal volume	Late round	Deal size
Treat × Post	0.186***	0.063***	0.376***
	(0.029)	(0.007)	(0.054)
Adjusted R^2	12.2%	4.0%	6.3%
Number of observations	112,106	112,106	112,106
Number of unique startups	12,056	12,056	12,056
Includes additional startup controls, startup and year fixed effects			

Panel D: Exits	IPO	Trade Sale	Common Acq.	Failure
Treat × Post	0.0037***	0.0045***	0.0006**	-0.0090***
	(0.0011)	(0.0008)	(0.0001)	(0.0032)
Adjusted R^2	1.1%	1.6%	0.3%	2.2%
Number of observations	112,106	112,106	112,106	112,106
Number of unique startups	12,056	12,056	12,056	12,056
Includes additional startup controls and year fixed effects				

Table F.6:
Reduced Form: Excluding Delaware

This table presents the results from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). For these regressions, the sample is limited to startups not originally incorporated in Delaware. Below the coefficient estimates are robust standard errors clustered by state of incorporation. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: Within-industry investment by VCs	Within-industry	Extensive margin	Intensive margin
Treat × Post	0.102***	0.022***	0.082***
	(0.021)	(0.005)	(0.031)
Adjusted R^2	8.8%	3.2%	8.1%
Number of observations	43,592	43,592	43,592
Number of unique startups	3,699	3,699	3,699
Includes additional startup controls, startup and year fixed effects			

Panel B: VC directorships	VC directors	Other directorships held	Within-industry directorships held
Treat × Post	0.298***	0.567**	0.236***
	(0.078)	(0.183)	(0.059)
Adjusted R^2	73.6%	56.7%	54.1%
Number of director observations	29,698	29,698	29,698
Number of unique startups	2,401	2,401	2,401
Includes additional startup controls, startup and year fixed effects			

Panel C: VC deals	Deal volume	Late round	Deal size
Treat × Post	0.155***	0.054***	0.331***
	(0.045)	(0.016)	(0.091)
Adjusted R^2	10.4%	4.8%	7.1%
Number of observations	43,592	43,592	43,592
Number of unique startups	3,699	3,699	3,699
Includes additional startup controls, startup and year fixed effects			

Panel D: Exits	IPO	Trade Sale	Common Acq.	Failure
Treat × Post	0.0060**	0.0037**	0.0005	0.0061
	(0.0026)	(0.0016)	(0.0005)	(0.0070)
Adjusted R^2	1.1%	1.6%	0.3%	3.1%
Number of observations	43,592	43,592	43,592	43,592
Number of unique startups	3,699	3,699	3,699	3,699
Includes additional startup controls and year fixed effects				

Table F.7:
Reduced Form: "Spray and Pray" Investment

This table presents the results from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). These regressions exclude startup-year observations associated with the "spray and pray" investment strategy adopted after the advent of cloud computing (i.e., software startups from 2006 to 2018). Below the coefficient estimates are robust standard errors clustered by state of incorporation. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: Within-industry investment by VCs	Within-industry	Extensive margin	Intensive margin
Treat × Post	0.071***	0.017***	0.055***
	(0.019)	(0.004)	(0.015)
Adjusted R^2	10.4%	4.6%	9.2%
Number of observations	103,947	103,947	103,947
Number of unique startups	11,169	11,169	11,169
Includes additional startup controls, startup and year fixed effects			

Panel B: VC directorships	VC directors	Other directorships held	Within-industry directorships held
Treat × Post	0.252***	0.259**	0.069**
	(0.072)	(0.120)	(0.031)
Adjusted R^2	73.8%	64.7%	63.3%
Number of director observations	74,933	74,933	74,933
Number of unique startups	7,823	7,823	7,823
Includes additional startup controls, startup and year fixed effects			

Panel C: VC deals	Deal volume	Late round	Deal size
Treat × Post	0.107***	0.033***	0.217***
	(0.028)	(0.008)	(0.058)
Adjusted R^2	12.3%	4.5%	7.0%
Number of observations	103,947	103,947	103,947
Number of unique startups	11,169	11,169	11,169
Includes additional startup controls, startup and year fixed effects			

Panel D: Exits	IPO	Trade Sale	Common Acq.	Failure
Treat × Post	0.0034***	0.0033***	0.0008	-0.0066***
	(0.0010)	(0.0011)	(0.0005)	(0.0021)
Adjusted R^2	1.2%	1.6%	0.4%	2.7%
Number of observations	103,947	103,947	103,947	103,947
Number of unique startups	11,169	11,169	11,169	11,169
Includes additional startup controls and year fixed effects				

Table F.8:
Reduced Form: Excluding Dotcom Bubble

This table presents the results from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). These regressions exclude startup-year observations associated with the bursting of the dotcom bubble between 1999 and 2001. Below the coefficient estimates are robust standard errors clustered by state of incorporation. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: Within-industry investment by VCs	Within-industry	Extensive margin	Intensive margin
Treat × Post	0.104***	0.020***	0.086***
	(0.033)	(0.007)	(0.027)
Adjusted R^2	10.4%	4.3%	9.4%
Number of observations	103,947	103,947	103,947
Number of unique startups	11,169	11,169	11,169
Includes additional startup controls, startup and year fixed effects			

Panel B: VC directorships	VC directors	Other directorships held	Within-industry directorships held
Treat × Post	0.262***	0.415**	0.155**
	(0.077)	(0.171)	(0.048)
Adjusted R^2	74.3%	62.8%	65.4%
Number of director observations	88,917	88,917	88,917
Number of unique startups	9,558	9,558	9,558
Includes additional startup controls, startup and year fixed effects			

Panel C: VC deals	Deal volume	Late round	Deal size
Treat × Post	0.149***	0.049***	0.305***
	(0.048)	(0.014)	(0.091)
Adjusted R^2	12.5%	4.6%	7.1%
Number of observations	120,356	120,356	120,356
Number of unique startups	13,402	13,402	13,402
Includes additional startup controls, startup and year fixed effects			

Panel D: Exits	IPO	Trade Sale	Common Acq.	Failure
Treat × Post	0.0033***	0.0042***	0.0010***	-0.0090***
	(0.0009)	(0.0010)	(0.0003)	(0.0027)
Adjusted R^2	1.1%	1.6%	0.3%	2.4%
Number of observations	120,356	120,356	120,356	120,356
Number of unique startups	13,402	13,402	13,402	13,402
Includes additional startup controls and year fixed effects				

Table F.9:
Reduced Form: Excluding Financial Crisis

This table presents the results from difference-in-differences regressions that exploit the staggered adoption of state legislation permitting corporate opportunity waivers (COWs). These regressions exclude startup-year observations associated with the financial crisis period between 2007 and 2009. Below the coefficient estimates are robust standard errors clustered by state of incorporation. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: Within-industry investment by VCs	Within-industry	Extensive margin	Intensive margin
Treat × Post	0.082***	0.015***	0.070***
	(0.022)	(0.005)	(0.018)
Adjusted R^2	10.4%	4.3%	9.4%
Number of observations	109,731	109,731	109,731
Number of unique startups	13,422	13,422	13,422
Includes additional startup controls, startup, and year fixed effects			

Panel B: VC directorships	VC directors	Other directorships held	Within-industry directorships held
Treat × Post	0.234***	0.346***	0.127***
	(0.072)	(0.099)	(0.037)
Adjusted R^2	75.4%	64.4%	62.2%
Number of director observations	88,917	88,917	88,917
Number of unique startups	9,568	9,568	79,568
Includes additional startup controls, startup, and year fixed effects			

Panel C: VC deals	Deal volume	Late round	Deal size
Treat × Post	0.115***	0.038***	0.236***
	(0.031)	(0.011)	(0.065)
Adjusted R^2	13.3%	5.2%	7.5%
Number of observations	120,356	120,356	120,356
Number of unique startups	13,402	13,402	13,402
Includes additional startup controls, startup, and year fixed effects			

Panel D: Exits	IPO	Trade Sale	Common Acq.	Failure
Treat × Post	0.0041***	0.0042***	0.0008**	-0.0090***
	(0.0008)	(0.0011)	(0.0003)	(0.0027)
Adjusted R^2	1.1%	1.6%	0.3%	2.4%
Number of observations	109,731	109,731	109,731	109,731
Number of unique startups	13,422	13,422	13,422	13,422
Includes additional startup controls and year fixed effects				

Appendix G. Matching Treated and Control Startups

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

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

where $\bar{X}_{ctrl,k}$ is the mean of the characteristics k in the matched control group, and $\bar{X}_{treat,k}$ is the mean of characteristics k in the treated group, and σ_k is the standard deviation of characteristic k among treated startups. To select potential matching algorithms, we use M-to-1 optimal Mahalanobis matching along the characteristics of startup age, capital raised to date, rounds raised to date, and year for various values of M. We also consider probit regressions and the resulting predicted propensity scores as potentially the best matching algorithm. In some cases, we also limit the probit regressions such that the potential matches are within 10 or 7.5 percent caliper of the treated startup.

The results in Table G.1 are in descending order of summary match performance. Based on the composite distance in column (1), the 1-to-1 optimal Mahalanobis algorithm based on the pre-treatment startup characteristics of startup age, capital raised to date, and rounds raised to date as well as exact match for industry and year is the best performer. Among the remaining matching algorithms, the probit regression with the 7.5 percent caliper is next best. The composite distance in characteristics underlying treated and control startups using the second best algorithm is almost twice as large as the distance when using the best algorithm. In terms of statistical inference, in each case the null hypothesis of no change in common ownership is easily rejected

as shown by the t-statistics reported in column (4). The point estimates do fluctuate in terms of economic magnitude depending on the algorithm. For example, the point estimate is 0.050 for the best algorithm and 0.075 for the second best.

Table G.2 shows the results from using our best performing matching algorithm, the 1-to-1 optimal Mahalanobis algorithm, for the various outcomes we evaluate in the paper: common ownership, within-industry investment, VC directorships, and finally, startup growth. In each case, the results remain statistically significant. We note that the economic magnitude is about half that based on the standard difference-in-differences regression. To understand if this variation is attributable to the reduced sample size from non-overlapping matches or bias-adjustments in the matching algorithm, in Table G.3, we repeat our main difference-in-differences regressions on the matched sample. The point estimates are higher in the difference-in-differences specification.

Table G.1:
Comparison of Matching Results

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

	(1)	(2)	(3)	(4)
	Composite	Number of	Common ownership	
Sample limited to DE, CA, MA, and NY	Distance	observations	coefficient	t-statistic
Mahalanobis (1 match), exact for industry and year	0.098	99,359	0.050	9.88
Probit (10 matches), 7.5 percent caliper	0.197	111,633	0.075	16.34
Probit (10 matches), no caliper	0.219	112,106	0.075	16.28
Probit (10 matches), 10 percent caliper	0.231	111,919	0.073	16.11
Mahalanobis (5 matches), exact for industry and year	0.233	96,262	0.049	10.80
Probit (5 matches), no caliper	0.237	112,106	0.076	16.24
Probit (5 matches), 10 percent caliper	0.240	112,067	0.076	16.52
Mahalanobis (10 matches), exact for industry and year	0.268	89,353	0.054	11.54
Probit (1 match), no caliper	0.276	112,106	0.072	14.12

Table G.2:
Mahalanobis Matching

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

Panel A: Common ownership	Any common owner	Total number of common owners	
Treat × Post	0.051***	0.105***	
	(0.005)	(0.011)	
Number of observations	99,359	99,359	
Panel B: Within-industry investment by VCs	Within-industry	Extensive margin	Intensive margin
Treat × Post	0.042***	0.013***	0.031***
	(0.005)	(0.003)	(0.004)
Number of observations	99,359	99,359	99,359
Panel C: VC directorships	VC directors	Other directorships held	Within-industry directorships held
Treat × Post	0.131***	0.129***	0.327***
	(0.024)	(0.017)	(0.044)
Number of director observations	74,169	74,169	74,169
Panel D: VC deals	Deal volume	Late round	Deal size
Treat × Post	0.062***	0.025***	0.128***
	(0.006)	(0.004)	(0.013)
Number of observations	99,359	99,359	99,359

Table G.3:
Robustness Test: Difference-in-Differences with Matched Sample

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

Panel A: Common ownership	Any common owner	Total number of common owners	
Treat × Post	0.240***	0.452***	
	(0.016)	(0.034)	
Adjusted R^2	62.5%	75.3%	
Number of observations	99,293	99,293	
Panel B: Within-industry investment by VCs	Within-industry	Extensive margin	Intensive margin
Treat × Post	0.0237***	0.041***	0.200***
	(0.011)	(0.003)	(0.008)
Adjusted R^2	9.6%	3.9%	8.8%
Number of observations	99,293	99,293	99,293
Panel C: VC directorships	VC directors	Other directorships held	Within-industry directorships held
Treat × Post	0.307**	1.000***	0.345***
	(0.128)	(0.084)	(0.032)
Adjusted R^2	74.1%	63.5%	61.4%
Number of observations	74,106	74,106	74,106
Panel D: VC deals	Deal volume	Late round	Deal size
Treat × Post	0.330***	0.118***	0.672***
	(0.011)	(0.004)	(0.026)
Adjusted R^2	12.0%	3.7%	6.0%
Number of observations	99,293	99,293	99,293
Additional startup controls	Yes	Yes	Yes
Startup fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes