Founder Replacement and Startup Performance

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Abstract

We provide causal evidence that venture capitalists (VCs) improve the performance of their portfolio companies by replacing founders. Augmenting a database of VC-backed startups from 1996–2007 with hand-collected executive turnover, we exploit shocks to the supply of outside executives from changes in the enforceability of employee non-compete agreements. VCs are more (less) likely to replace founders when non-competes are harder (easier) to enforce, and instrumented founder replacements predict better investment outcomes. Replacing founders appears to unlock value by undoing managerial entrenchment, suggesting that VCs face a tradeoff between short-term portfolio performance and maintaining a "founder-friendly" reputation.

JEL classification: G24, G34, L2, M12.

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1. Introduction

It is well-accepted that venture capital is a "hits" business. In a sample of over 22,000 VC funded startups founded between 1987 and 2008, 75% had a liquidation value of zero while 0.39% had an exit value of $500 million or greater Hall and Woodward (2010). Research indicates that returns are enhanced by investor skills, which one might group into 1) initial selection of investment targets 2) post-investment intervention.[1] Recently, scholars have recently turned their attention to the question of whether post-investment intervention by "activist" investors truly improves outcomes for portfolio companies (Bottazzi, Da Rin and Hellmann (2008)). Chemmanur, Krishnan and Nancy (2011) use restricted-access Census data to show that startups ineligible for Small Business Adminstration support achieve greater total factor productivity after raising VC, suggesting that investors provide more than just capital. Bernstein, Giroud and Townsend (Forthcoming) similarly find that VC-backed firms are more likely to achieve liquidity events once their investors are able to visit via a non-connecting flight. But neither paper identifies specific actions undertaken by investors, leaving open the question of exactly how activist investors add value.

Gorman and Sahlman (1986) list three non-financial areas in which activist investors might add value. First, VCs assist with strategic and operational planning. In support of the notion that investors influence strategic direction, Hsu (2006) finds that VC-backed ventures are more likely to adopt cooperative commercialization strategies. But whether such assistance improves outcomes remains in question, especially as Kaplan, Sensoy and Strömberg (2009) see little change in business plans in firms that achieve IPOs.

Second, investors may make introductions to customers that facilitate sales and drive revenue growth. The plausibility of this mechanism is underscored by Chemmanur, Krishnan and Nancy (2011), who suggest that TFP gains for VC-backed startups are largely coincident with increased sales. But customer introductions are difficult to observe empirically, so

[1] See Gompers et al. (2010), Hellmann and Puri (2002), Sorensen (2007), Hsu (2006), Bottazzi, Da Rin and Hellmann (2008) and Chemmanur, Krishnan and Nancy (2011)

constructing a clean test of this mechanism is difficult.

The third category—recruiting managers—is easier to observe; indeed, VCs are known to play a role in recruiting (see Amornsiripanitch, Gompers and Xuan (2015)). Nevertheless, it is not straightforward to conclude a direct link between such actions and performance. Recruiting can be purely additive, such as when an investor brings a vice president of marketing to an early-stage company that previously did not have one, or it can involve replacing existing personnel. For example, the survey question in Bottazzi, Da Rin and Hellmann (2008) asks, "Has your firm been involved in recruiting senior management for this company?" which could be of either type. Additive recruiting is unlikely to be controversial whereas founders have several reasons to resist being replaced.

At least three studies have examined founder replacement (Wasserman (2003); Kaplan, Sensoy and Strömberg (2009); Hellmann and Puri (2002)). Because prior work is based on surveys, largely of later-stage startups or IPOs, it is possible that the overall rate of founder replacement differs in the broader population of venture-backed firms. Moreover, prior work does not draw a causal connection between founder replacement and venture outcomes; doing so requires more comprehensive data on a larger set of firms. One might assume that investors would not replace founders if doing so did not improve venture performance, but the impact of replacement is not entirely obvious as founders to some extent represent the "core" of the startup and likely recruited many early employees. Even if founder replacement does no harm, it may be that replacement does not help even though investors think it does—or, more cynically, because they want to be seen as "doing something" even if their actions are not helpful (Fisman et al. (2013)).

Alternatively, VCs may elect not to replace founders, even when doing so would be helpful, for fear that a reputation for replacing founders might discourage future entrepreneurs from working with them. For example, prominent VC Andreesen Horowitz announced its intention not to replace founders, perhaps in hopes of attracting more applicants.[2] Any VC preference

[2]See `www.bhorowitz.com/why_we_prefer_founding_ceos`.

for appearing "founder-friendly" likely comes at a cost of managerial entrenchment. Thus investors face a tradeoff: they may wish to maintain their reputation as patient and failure-tolerant (e.g. Manso (2011)) so that they will continue to attract quality entrepreneurs, but they also seek optimal performance from their portfolio firms (the performance of which will also attract entrepreneurs). If replacing founders does not improve performance, then it is optimal to be founder-friendly.

We analyze founder replacements in venture-backed firms, augmenting VentureSource records from startups founded 1996-2007 with extensive hand-collection of individual career histories. This new database dramatically expands on prior studies that use IPO firms or surveys, allowing us to more confidently generalize our results. Although it may seem intuitive that founder replacement would be involuntary given founders' attachment to their ventures, we nonetheless confirm this by showing that founders are much more likely to be replaced when their startups are doing poorly. Our proxy for poor performance is whether the firm's next round of financing arrives unusually late as compared to others in the same industry and of the same vintage: the we call these "living dead." Startups ever counted among the living-dead are 45% less likely to IPO; have 60% lower valuations at exit; and account for 10% of all financing rounds in our sample but nearly 20% of all founder replacements. These results indicate that a key corrective measure for investors seeking to turn around troubled portfolio companies is replacing founders. Such patterns mirror top manager turnover in public firms (e.g. Denis and Denis (1995)). Moreover, similar to Wasserman (2003) we find that founder replacement is more likely to occur when the board contains more outsiders and thus can exert greater control, suggesting that such replacements are generally involuntary.

Identifying the causal impact of replacing founders is not straightforward given the endogeneity of the decision to replace a founder. Indeed, naive regressions show a negative correlation between founder replacement and liquidity events, but this could be explained by selection because investors choose to intervene when a startup is in trouble. We instrument for founder replacement using a plausibly exogenous shock to the supply of executives who

might serve as suitable replacements: changes in the enforcement of employee non-compete agreements.

Non-competes have frequently been shown to restrict the mobility of workers, especially technologists and executives in the sorts of high-potential industries VCs tend to invest in (Marx, Strumsky and Fleming (2009); Garmaise (2011)). Using a series of staggered adjustments to non-compete enforcement in several states—some favoring the use of such contracts, with other states disfavoring them—we show that a given VC is much less (more) likely to replace founders in its portfolio companies when non-compete enforcement has been strengthened (weakened). Instrumenting for founder replacement with these policy changes shows that replacement both decreases the time until the next round of financing and increases the likelihood of achieving a liquidity event. Further analysis suggests that these benefits obtain primarily for startups where founders are entrenched, as proxied by the size of the founding team as well as first-round capital raised and syndicate size.

This paper is the first to identify a causal mechanism underlying prior findings that intervention by activist investors improves startup performance. Further, the association between future success and replacement of founders suggests that replacement helps to unlock value in core assets of the firm by undoing entrenchment on the part of current management. Hellman and Puri (2000) as well as Ewens and Rhodes-Kropf (2015) find a unique role for both experience and "skill" in the cross-section of VC firm performance; we reveal that VC performance differences stem in part from the ability and willingness to replace founders. Our results also suggest that VCs face a tradeoff between maintaining a founder-friendly reputation in order to attract future entrepreneurs vs. maximizing the performance of their current portfolio by managing it actively and in ways that may be resisted by founders.

2. Data

We rely on VentureSource to track portfolio companies founded from 1996 through 2007. VentureSource is a database of venture capital transactions, entrepreneurial firms, investments and outcomes provided by Dow Jones.[3] A summary of most of the variables used in this paper is found in Table 1.

Sample creation begins with the the set of all VC-backed entrepreneurial firms founded between 1996 and 2007. The lower bound of founding year ensures that we can collect information about replacements via all the sources discussed below, while the upper bound ensures that we have time for exits as the sample ends in 2014. We also require that firms have at least two financings (including an exit by the end of the sample) so that there is time for a potential replacement and received capital from a traditional VC investor.

The second filter concerns the coverage of management teams. We require that the firm has at least one founder (90% of firms have at least one identified in the data) and one non-founder executive (e.g. CEO or CFO) at the first VC financing event for whom we can identify a join date. An executive is designated a founder as described in Ewens and Fons-Rosen (2015) using VentureSource, company websites, Capital IQ and LinkedIn. The founder has to have a title at or above the level of vice president to ensure they have a major operating role at the firm. The final sample has 14,450 founders of 9,184 entrepreneurial firms with 26,910 total financing events. Summary statistics are found in Table 2, with firm-level characteristics in Panel A and financing-level in Panel B. Over 75% of the firms in the sample have exited by the end of the 2014. The average financing occurs in 2003 and the average firm has 3.8 rounds.[4]

[3]The data are graciously provided by Correlation Ventures, a quantitative VC fund.

[4]The minimum number of rounds is one because our sample filter only requires that a firm have at least two total financings.

3. Identifying founder replacement

Recruiting executives is one of the most commonly mentioned value-add activities observed in the literature on VC monitoring (Gorman and Sahlman (1986); Hellmann and Puri (2002); Bottazzi, Da Rin and Hellmann (2008)). Recruiting could be "additive" in that it helps to complete a nascent founding team, e.g., by adding a Vice President of Marketing to a technology-focused startup (i.e professionalization). But recruiting can also take place for roles already occupied when a replacement is sought. Additive recruiting is unlikely to be controversial, whereas our interest is in the dynamics and impact of replacement.

VentureSource includes information on top-level managers, executives and investor board members. For each executive, VentureSource contains each title held at each venture-backed firm where that person worked. Whenever we observe two individuals at a startup with the same title, excepting inherently joint titles such as "Co-CEO", we conclude that a replacement has occurred. We normalize job titles both by level (e.g., "VP" and "Vice President") and by function (e.g., "Software Development" vs. "Software Engineering") while being careful not to lump together titles at the same level and in the same function that are nonetheless distinct (e.g., "VP North American Sales" and "VP International Sales"). Since we aim to identify within-firm replacements, most of the within-firm variation in title naming is due to typography.[5]

As we are ultimately interested in the dynamics of founder replacement, the join date for each new occupant of a given title is essential.[6] Unfortunately, join dates are missing for approximately 70% of observations in VentureSource. We undertake a data collection process using company websites, Capital IQ, Zoominfo and public LinkedIn resumes, which typically include an online biography or resume from which the join date can be extracted

[5]A replacement can involve either a departure or a demotion (as discussed by Hellmann and Puri (2002)). VentureSource does not track the individual's title history, so each founder's full title history within the firm was checked on LinkedIn and CapitalIQ. We are able to determine that at least 14% of founder replacements are demotions. Given that our purpose is to understand the impact of replacement on performance, this distinction is not essential though future work may seek to distinguish the impact of departures vs. demotions.

[6]Founders by definition joined at the start date of the firm.

or inferred. The comparison of titles across all executives identifies a potential replacement. With this list in hand, we have a smaller set of individuals for which to search for join dates. We are able to add the join date for more than 1500 additional executives, reducing the number of missing join dates to 16% of observations. Founders who were replaced but for whom we do not have the join date of the executive who replaced them are dropped from the analysis, as we cannot properly assign their replacement event to a financing round.[7]

For non-joint titles for which we have join dates for all occupants, we take the join date(s) of the non-founder occupant(s) as an indication of a founder replacement. For example, if a startup had both a founder and a non-founder with the job titles "VP Product" and "Vice President of Product Management" with start dates of 1/1/1995 and 6/5/1997, we take 6/5/1997 as the date of the replacement. We then retain the set of these replacements where the first to hold the position was a founder of the company. In models using firm-financing rounds as the unit of observation, replacement occurs on or after the focal financing and prior to any subsequent financing. While it is theoretically possible that a startup might have multiple founders replaced during a single round of financing, in practice this is very rare (0.3% of financing rounds) so we use a dichotomous variable indicating whether any founder was replaced.

3.1. Refining the replacement data

It is possible that VentureSource does not have perfect coverage of all current and former executives. The replacement algorithm requires two individuals with the same title, so missing executives will lead us to miss replacements. To address this, for the over 4,500 non-replaced founders (according to the logic above) we undertook a data collection to find possible replacements not reflected in VentureSource. If we were able to find another person with the same title at the same firm using other sources, we then attempted to locate the

[7]We lose 169 firms and 390 founders based on this rule. The firms and founders exhibit no difference in major observables studied.

date they joined.[8] If that join date was before the exit or failure of the entrepreneurial firm, then the founder's replacement date was set to this join date. Less than 10% of the searches resulted in a new replacement, suggesting first that VentureSource has reasonably good coverage of executives to identify joint titles. More importantly, the additional search for missing executives revealed a replacement rate less than in the full sample (9% vs 16%), so we are confident that our rates of founder replacement are not underestimated. The final sample includes over 95% of all VC-backed firms in VentureSource founded between 1996-2007, with at least one founder, one VC investor and two financing rounds.

One last concern regarding our sample construction is that the firms that are out of the sample either failed or were shut down before VentureSource collected the data on replacement. Similarly, people who were associated with the company may not have made their employment public online. Our research question asks when founder replacement occurs and whether it matters. For the former, any of these sources of selection would attenuate the correlation with low performance and replacement. For the question of whether replacement matters, we may have too many "good" replacements (i.e. those that are worth it and those that end up helping). We researched twenty five random out-of-sample firms to isolate any patterns. Sixteen of the companies appear to have failed and have not raised new VC in many years. Several of the remaining are in non-traditional VC industries such as retail and restaurants where VentureSource may have poor coverage. Overall, the sample of entrepreneurial firms for which we are confident about replacement events is representative of the typical VC-backed firm over the sample period.

3.2. Comparing founder replacement rates with prior studies

Ours is not the only study of founder replacement, though we believe it to be the first not to rely on survey data. Using VentureSource, we are able to observe a much larger array of startups including many young companies that did not raise many rounds of financing

[8]Searches were done on the entrepreneurial firm's website, LinkedIn, CapitalIQ and Google searches.

and did not survive very long, whereas surveys tend to target later-stage firms listed in business registers or IPOs. If there are substantial differences in *cumulative* replacement rates between older and younger firms, we might expect to find lower overall replacement rates in the current study as the data are less censored by failure.

Figure 1 and Table 3 show the dynamics of founder replacement by financing round. As noted in the second to last row of Table 3, 15.7% of venture-backed firms in our sample experience a founder-replacement event (17.2% if we include those without dates).[9] Founder replacement is less common in the first round but rises in the second round and continues well into the sixth round. That replacement occurs over the firm's life confirms our prediction that younger and older firms exhibit substantial differences in overall founder replacement rates. Indeed, all three papers tracking founder replacement with survey data yield higher figures than ours.

Wasserman (2003) conducts a survey of 202 venture-backed startups, reporting that 29.7% of these respondents experience a founder-replacement event. Kaplan, Sensoy and Strömberg (2009) gather data on 50 venture-backed firms that completed an IPO, with founders having been replaced in 42% of these firms. Hellmann and Puri (2002) survey 170 privately-held companies in Silicon Valley, reporting a 53.5% replacement rate and a mean age of 9.5 years. Firms in the Kaplan et al. survey are 6.6 years old on average. (Wasserman controls for but does not report firm age.) By comparison, the 9,184 firms in our sample are much younger: 4.5 years on average. Given the consistently strong positive correlation between firm age and founder replacement—in Hellmann and Puri (2002), Wasserman (2003), and our own Table 5–these sampling differences help to explain why survey-based papers report higher replacement rates.

In the final row of Table 3, we show how focusing on later-stage startups can inflate replacement rates. That row reports the fraction of firms that survived at least N rounds that

[9] As a comparison, Kaplan and Minton (2012) find that the average tenure of replaced public firm CEOs is over seven years while 15% have a turnover. Importantly, forced turnover as defined in Parrino (1997) is much more rare in public firms at 4-5%.

had a replacement by the end of the sample. We do not claim to have a census of venture-backed firms; VentureSource probably does not capture every startup, and as detailed in Section 2 we limit our sample to firms with at least two financings and at least one founder and one non-founder executive. Even with these filters, the mean age of entrepreneurial firms in our sample is less than half of that in the Hellmann and Puri (2002) survey. We can introduce a measure of survivorship bias into our dataset by eliminating firms with only a small number of rounds, which raises both the mean firm age and the founder replacement rate. For instance, if we restrict our sample to firms that obtained at least four rounds of financing, the mean age becomes 6.4 years and the percentage of startups experiencing founder replacement rises to 25.4% (similar to Wasserman's reported rate).[10] We note that by the fourth round of financing, one-quarter of startups have failed (and thus would not be included in a survey). Overall, the observed replacement rates seem reasonable given our broader coverage of young and failed startups, which in turn allow us to address questions of effects of VC intervention.

3.3. Is founder replacement involuntary?

It may seem straightforward that founder replacement is involuntary given attachment to their ventures and large equity stakes, but establishing this is important as it could be that some founders prefer to start companies and then gladly hand over the reins to professional managers. Alternatively, one might be concerned that founders could leave not because they are pushed out but rather because they lose confidence in the company's prospects. We use two measures to establish that founder replacement is largely involuntary: 1) whether the startup is struggling, such that investors might want to replace a founder 2) whether the board is controlled by investors, such that they are able to replace a founder.

Straightforward measures of performance are difficult to obtain for private firms, except perhaps in the restricted-access Census data such as employed by Chemmanur, Krishnan

[10] In order to achieve a sample with average firm age similar to Hellmann and Puri (2002) we need to consider firms with at least eight rounds of financing. Doing so yields a replacement rate of 36.4%.

and Nancy (2011). We construct a novel metric of whether a venture-backed startup is struggling based on the timing of financing rounds, which can be constructed from the more widely available venture data. Few venture capitalists have the goal of funding a company that exists perpetually as a private, independent entity (even if profitable). Rather, they require attractive liquidity events to generate returns for their limited partners. The VC fund's limited life (ten to twelve years) and popular features of the preferred stock contracts held by VCs each reinforce this demand for liquidity. Startups that have not yet achieved a liquidity event are likely to seek additional funding to continue toward that goal. Indeed, a strong measure of success is the rate of capital raised over time and the growth in the capital level (e.g. see Korteweg and Sorensen (2010)). Those that fail to raise a successive round of funding in a timely manner, adjusting for industry and period trends, are sometimes referred to as "zombie" companies or the "living dead."

We formalize this notion as follows: a firm enters the "living dead" state if it does not raise a successive round of capital within the time it took the 90th percentile of peer firms to do so.[11] By "peer" firms we mean those in the same industry, at a similar stage of development (early vs. late) and with a similar stock of capital (e.g., above or below median). A firm emerges from the living-dead state if and when it raises a subsequent round of financing.[12] For example, consider a second round financing in 1998 of a biotechnology firm that has raised a below-median amount of capital. If the 90th percentile of time to next financing is two years, then if the focal firm does not raise its second round by two years and one month after the its first round we label it "living dead" as of that month. Note that if the focal firm obtains its second round of financing arrived three years after its first round, it would be labeled as in the "living dead" state for the 12 months it was "late" in obtaining financing.

Table 4 characterizes the "living dead" variable in terms of both ex-ante and ex-post characteristics. If our living-dead proxy is informative, we would expect such firms to fare

[11] All results are insensitive to using the 75th, 80th, 85th and 95th percentiles.

[12] Firms labeled as profitable do not receive a living-dead classification, as no capital might have been required to keep the firm alive.

worse at exit or end of sample. Indeed, living-dead startups are 41% less likely to IPO, less likely to have attractive acquisitions, and more likely to fail. Furthermore, "living dead" firms raise slightly less capital yet produce exit valuations that are $140m less on average suggesting a meaningful difference in returns.

The performance of the venture indicates whether investors might *want* to replace a founder, but investors must also be *able* to replace the founder. If investors pro-actively replace founders to improve firm performance instead of reactively backfilling those who left voluntarily after losing confidence in the venture, then replacement should increase with investor power. To assess the role of investor power in replacing founders, we examine the number of outside directors. The board is explicitly tasked with hiring and firing the CEO and can exert significant influence over the hiring and firing of other executives. Similar studies of public firm boards, such as Weisbach (1988), show a direct connection between board size and investor power. Furthermore, the VC-backed entrepreneurial firm has a board of directors comprised of three different agents: independent observers, investors and executives (see Kaplan and Strömberg (2003) for details). Independent directors and investors have been shown to play an important role in executive replacement. Lerner (1995) shows that CEO replacement is strongly correlated with an increase in the role of investors on the board of directors. These facts motivate our variable "Large outside board."

Analyzing board investor power requires the number of directors per round. Although VentureSource lists current/former board members, dates of service are often missing. We identify an investor's joining the board by their first investment in which either they are identified as the "lead"—or if they never have a lead position, their first investment in the firm. Identifying their exit from the board is more challenging as most will retain their position, although some early-stage VCs leave a board as the startup approaches an IPO. We date exits by the round where a known investor stops participating in financing events and a new investor takes a board seat. Finally, as the size of boards is discrete and bounded between zero and ten, we create a dichotomous variable set to one if the board size is above

the median outside board size (results are qualitatively similar with the continuous measure).

3.4. Correlates of founder replacement

Having characterized the living-dead variable and board outsiders, we investigate the correlates of founder replacement. If founder replacement were primarily voluntary, we would expect such replacements to be no more common among living-dead financings or where outsiders hold more board seats and hence influence. To establish the relative importance of these two explanations, we estimate the following cross-sectional regression in which the dependent variable R_{it} is an indicator for whether at least one founder in firm i is replaced at financing t:

$$R_{it} = \beta_0 + \beta_1 \text{Living dead}_{it} + \beta_2 \text{Large board}_{it} + \beta_3 X_{it} + \beta_4 Z_i + \gamma_t + \epsilon_{it}. \quad (1)$$

We include time-varying firm controls including the startup's age, total capital raised, syndicate size, and profitability (X_{it}), as well as time-invariant characteristics (Z_i) such as industry and financing year fixed effects (γ_t). Once all the founders are replaced, then the firm and its financings are no longer tracked. Thus the sample only includes financings of firms still at risk of a founder replacement. As expected, Table 5 shows that founder replacement rates increase as firms age and raise more capital. Profitability appears not to affect replacement.

We expect founders to be at a higher risk of replacement in the living-dead state. In addition to living-dead rounds, we also consider "high growth" rounds (if followed by or following a relatively quick financing, namely in the bottom 25% of speed to refinancing, or the first time reporting revenues or profits). The differences in coefficients β_1 and β_2 tell us the relative sensitivity of replacement rates to performance, versus non-living-dead and non-high-growth rounds. Startups struggling to raise new capital are twice as likely to have a founder replaced. Living-dead financings account for 10% of all financing rounds in our

sample but nearly 20% of all founder replacements. This is a large fraction of replacements because the living-dead variable is but one metric of firm under-performance.

Table 5 also explores the role of board outsiders in founder replacement. Columns (4) and (5) indicate that having more outside directors is correlated with founder replacement. The estimates imply a 30% increase in the probability of a replacement for below-median vs above-median board size. Such a change represents on the margin a shift from two outsiders to three. Importantly, these specifications include round number (i.e. stage) fixed effects and capital-stock controls. Thus board size is not simply capturing an increased rate of replacement as firms mature. Such board connections manifest the dynamics of boards and replacement in VC-backed firms as discussed by Lerner (1995) for private firms and by Denis and Sarin (1999) for public firms. Column (6) interacts the board size dummy with the living dead performance variable. The interaction loads positively and shows that much of the outsider control occurs during poor performance. Our results resemble those of Weisbach (1988), who finds that independent boards respond more strongly to (poor) firm performance.

The final column of Table 5 makes the unit of observation founder-financing and introduces dummy variables for the founder's title. Relative to the excluded VP-level founders, C-level founders, and not the CEO, are at the highest risk of replacement. This fact is consistent with CEO founders having the largest equity stakes and power within the firm.

4. Does founder replacement impact performance?

As noted above, Bottazzi, Da Rin and Hellmann (2008), Chemmanur, Krishnan and Nancy (2011), and Bernstein, Giroud and Townsend (Forthcoming) establish a causal connection between investor activism and IPO/acquisition outcomes. And while prior work has shown that investors indeed replace founders of their portfolio companies (Kaplan, Sensoy and Strömberg (2009); Hellmann and Puri (2002); Wasserman (2003)), no connection has

been drawn between founder replacement and subsequent startup performance. Moreover, one of the major "costs" of venture capital for many would-be entrepreneurs is the threat of being replaced. If one were to find a positive causal impact of these events, the underlying risks of replacement would potentially be offset as founders benefit from their equity stakes as the firm's outcomes improve.

Suppose that the researcher randomly allows a VC to replace a founder of their choice. Doing so eliminates any selection issues (e.g., the VC picks the companies that have the best prospects to replace, or those that need replacement are worse firms) and can isolate causal effects. There are three possible outcomes. First, the cause of the firm's struggles could be the "jockey" (i.e. current management) and not the "horse" as suggested by Kaplan, Sensoy and Strömberg (2009) in their analysis of firms that eventually have an IPO. The VC may not have already replaced management, either because they want to be perceived as patient or because the management team is entrenched. Second, replacement might improve firm prospects because the existing match between the founder and the firm limits growth and exit opportunities. Third, replacement could have a negative impact on performance if founders are important assets and the VC incorrectly assesses their value.

To study the impact of replacement on firm outcomes, we measure the ultimate success of the firm. We extend the commonly used outcome variable – an initial public offering – to a more general measure of success. Consider the dependent variable set to one for a portfolio that achieves an IPO or an attractive acquisition that exceeds 125% of total capital raised.[13] An unattractive acquisition, failure of the firm, or remaining in the living-dead state all set the dependent variable to zero.[14] While 10% of firms achieve an IPO, some 30% of firms achieve our exit success. The empirical model ties the number of replacements to this outcome Y_{it+1}, where $t+1$ is the firm's next observed financing or exit event:

[13]Bernstein, Giroud and Townsend (Forthcoming) consider a dependent variable that is one if an IPO or acquisition with at least $25m exit value occurs. Our results are robust to measures of 1X - 3X exit value to total capital raised.

[14]If an acquisition value is unreported, Puri and Zarutskie (2012) suggest that it is small.

$$Y_{it+1} = \rho_0 + \rho_1 R_{it} + \rho_2 X_{it} + \lambda_k + v_{it}. \quad (2)$$

Here X_{it} contains entrepreneurial firm, investor and current investment characteristics measured at the firm's last financing event, which include year and industry fixed effects. The variables λ_k are state fixed effects motivated by our identification strategy detailed below. R_{it} indicates whether a founder was replaced in the financing round or prior to the subsequent financing event. A unit of observation is a VC investor and entrepreneurial firm financing, while the sample is restricted to those firms that satisfy the instrumental variable conditions below. Table 7, column (1) presents the results of the equation (2). It shows a weakly negative and statistically insignificant association between replacing founders and eventually achieving an attractive liquidity event, which might interpreted to mean that replacing founders hurts performance. But this correlation could be downward biased given that, as shown earlier, investors are more likely to replace founders in struggling companies (many of which fail). We now consider an instrumental variables solution to these selection issues.

4.1. Instrumental variables

The variable R_{it} is likely correlated with the current and future prospects of the entrepreneurial firm, both omitted from (2). For example, replacement may coincide with unobserved negative shocks to the firm that would lower future performance. We require a variable Z that predicts the likelihood of replacement but does not belong in equation (2) (i.e. exclusion restriction). Our instrument proxies for changes to the supply of potential replacement executives from other companies using plausibly exogenous changes to the ease of their recruitment. As finding replacement executives is nontrivial, changes to the pool of available executives could affect the rate of founder replacement. Some of the most attractive replacement executives will be those with experience at the sort of company that might

acquire the focal startup, especially for a private firm that may be struggling and for whom an acquisition might seem the most promising exit.

Indeed, Table 6 shows that the most popular last known employer for new executives that replaced founders in our sample are some of the largest established firms. Given that these large firms are an attractive source of replacement executives, exogenous changes to their interorganizational mobility act as a shock to the supply of replacement executives to VC-backed firms. The last column of the table provides the count of acquisitions done by the firms over 1996-2007 as measured from the Securities Data Company (SDC) mergers and acquisitions database. In the majority of cases, the firms have at least as many acquisitions as movement of executives to startups. This suggests another connection between large, established firms and VC-backed startups.[15]

One factor affecting the interorganizational mobility of replacement executives is the enforceability of employee non-compete agreements. Employee non-compete agreements are sections of employment contracts in which an employee covenants neither to join nor found a rival firm within 1-2 years of leaving. A growing body of work shows that non-competes bind employees to their employers, thus making it difficult for small companies to attract workers away from established firms (Stuart and Sorenson (2003)). Garmaise (2011) shows that firms use non-compete agreements with at least 70.2% of their top executives, who we have shown are likely candidates to be targeted as replacements for founders (e.g. Table 6). Marx, Strumsky and Fleming (2009) provide causal evidence linking enforceability of non-compete agreements to worker mobility, leveraging a 1985 reversal of non-compete policy in Michigan. These shocks to the supply of executives should – for fixed demand – alter the VC's opportunity cost of either replacing or retaining the existing founding team. Importantly,

[15]In unreported results, we note a strong reduced-form correlation between founder replacement and the number of acquisitions within the same industry two years prior. The two-year lag stems from a popular contract employed by acquiring firms for the acquired firm's executive teams. These contracts often involve two to four year vesting or bonuses for the executives of acquired firms. Although the stock options of the executives in the target company fully vest on the change of control, incentives are typically added to retain key personnel beyond the acquisition, including large cash-based incentives which are evaluated no later than two years after the acquisition. As two-year lagged acquisitions might correlate with the current exit market (e.g. merger waves), we do not use this as an instrument.

non-competes are more likely to be enforced against top or high-quality management that the established firm most wants to retain. Thus, the changes induced by the law will increase or decrease the supply of higher-quality replacement executives.

The Michigan reversal is well in advance of our sampling period, but several reversals in other states facilitate such an analysis using the VentureSource data. During our sample period, three states strengthened the enforceability of employee non-compete agreements: Florida (1996), Idaho (2008), and Georgia (2010). Importantly, we require that the law changes were not related to the future prospects of the startups in the state. The change in Florida was pushed for (and co-drafted) by the Florida Bar Association, as attorneys in the state had become frustrated with the lack of clarity regarding enforceability of employee non-compete agreements and found it difficult to advise their clients with certainty.[16] The Idaho law, which among other provisions enacted what is commonly called a blue-pencil rule by which a judge facing a lawsuit is allowed to modify the contract to make it more reasonable, was advocated by the Idaho Falls based Melaleuca health products company.[17] Georgia also added a blue-pencil provision, with its change brought about by a 2010 referendum which amended the state constitution. However, the text of the referendum has been criticized as misleading as it did not make direct reference to employee non-compete covenants, so the reversal can reasonably be characterized as non-premeditated.[18]

Around the same timeframe, five states made it more difficult to enforce non-competes against ex-employees: Texas (1995), Louisiana (2001), Oregon (2008), New York (2008), and New Hampshire (2012). The changes in both Texas and Louisiana were enacted by Supreme Court decisions as described in Garmaise (2011), which cannot be reasonably construed as anticipating future startup performance. In 2008, Oregon's Commissioner of Labor success-

[16]For further details, see an account by the Florida Bar Journal at `https://www.floridabar.org/divcom/jn/jnjournal01.nsf/Author/5B76183B1BAEE59585256ADB005D60DA`, accessed 13 Jan 2016.

[17]Source: interview with Nicole Snyder, partner at Holland & Hart of Boise, Idaho. `http://magicvalley.com/business/local/non-compete-bill-passes-house/article_1e38184c-3d97-58a0-be2f-c4d5be4a06f4.html`, accessed 13 Jan 2016.

[18]Interview with David Pardue of Olin, Gleaton, Egan, Jones, & Sweeney. See also `http://tradesecretstoday.blogspot.com/2011/03/failing-to-trust-public-process-of.html`, accessed 13 Jan 2016.

fully lobbied to passed a bill that would invalidate non-compete agreements workers were not told about until after they accepted their offer out of employment.[19] A similar measure was brought about in 2012 by a New Hampshire state representative who had personally been negatively affected by a non-compete; moreover, a review of the legislative history[20] suggests that this reform was undertaken not out of a desire to promote the performance of startups but rather as a workers' rights measure. Finally, New York's 2008 restrictions on non-competes were attributed to successful lobbying by prominent entertainment labor unions, who sent hundreds of letters and emails to state legislators and the mayor.[21] Overall, none of these changes in the enforceability of non-compete agreements likely occurred in response to anticipated future change in startup prospects.

The policy reversals in both Florida and Texas occur near the beginning of our sampling frame, thus few startups headquartered in these states enter as treated in the analysis. While we would consider startups headquartered in these states two years before the policy changes as controls, the fact that these pre-policy-change windows precede our sampling frame means that these states do not supply relevant controls. As in other states with policy changes, we do not use startups headquartered in these states after the reversal as controls. Omitting Michigan, which changed its non-compete policy in 1985, from our analysis does not affect the results.

The next challenge to satisfying the exclusion restriction is the comparison or control sample. We cannot simply track the same entrepreneurial firm over time because once a founder is replaced in a single-founder firm (a large fraction of the sample), the firm can no longer receive treatment. We require some set of firms that were not affected by the law yet were at risk of having their founders replaced. One possibility is to examine startups

[19]The full political economy surrounding the change in Oregon is described in the article "Explaining the outlier: Oregon's new non-compete agreement law and the broadcasting industry" in the University of Pennsylvania Journal of Business Law, available at `https://www.law.upenn.edu/journals/jbl/articles/volume11/issue2/Rassas11U.Pa.J.Bus.L.447(2009).pdf`, accessed 13 Jan 2016.

[20]Interview with attorney Jim Riedy of Sheehan, Phinney, Bass & Green; see also `http://blog.sheehan.com/index.php/business-litigation/non-compete-law`, accessed 13 Jan 2016.

[21]Source: `http://www.nmmlaw.com/pdf/FMP%20Noncompete%20Feb2009.pdf`, accessed 13 Jan 2016.

financed in the same states that had the law changed but which received capital prior to the change. A control group constructed in this way would allow us to address time-invariant unobserved heterogeneity of firms in the state, but this approach suffers from unobservable, confounding trends within each state. Another possibility is thus to include all VC-backed firms not in the treated states, but this option would likely introduce a large set of dissimilar firms, particularly with regard to unobserved trends. Such trends might capture changes in entrepreneurial firm success rather than the proposed treatment effect. Our solution exploits the knowledge about the treated entrepreneurial firm's investors and their portfolios.

The treatment group for our analysis is the set of all startups headquartered in one of the treated states and that received a non-initial round of financing during the two years following that state's policy change. In other words, a VC was actively involved in the treated startup both before and within two years after the relevant policy change. The set of investors in the startups that compose the treatment group is then used to construct the control group, in two steps. First, we consider as controls the set of startups invested in by those same VCs in one of the treated states but which received capital in the two years prior to the law change. These firms represent a within-state control sample. Second, we add to these controls the set of startups (in all other states) invested in by the *same set of VCs* in the four-year window around a law change. A control firm is assigned to a treated state law change that occurred the closest in time within their VC firm's portfolio. We thus have a within-VC-firm difference-in-differences estimator where controls are matched to the closest-in-time treated investment in their investor's portfolio. The tendency of VCs to invest in similar-quality startups within an industry helps to ensure that the treated and control startups exhibit similar unobserved trends.[22]

The following example illustrates the construction of the treatment/control sample. Suppose that VC Ace Partners invested in five startup companies: GA1, GA2, GA3 (all headquartered in Georgia) as well as MD1 and MD2 (both in Maryland). GA1 received a round

[22]Firms in treated states financed more than two years after the law change are not included as controls.

of funding from Ace Partners in 2008 and then again in 2011. Because GA1 was in Ace's portfolio prior to the policy change in Georgia and then received another round of financing from Ace within two years after the policy change, it is a treated observation. GA2 received a round of funding from Ace in 2009. It is a control observation because it was funded during the two years prior to the policy shift, but it is not a treated observation because it did not receive an investment from Ace within two years after the shift. GA3 received one round of funding from Ace in 2006, but it is neither a treated nor control observation because that financing event falls outside the four-year window surrounding the change in Georgia. MD1 received a round of funding from Ace in 2008; it is a control because its investment date falls within two years of the Georgia policy change (where Ace also had an investment near the policy change). MD2 received a round of funding from Ace in 1994, but this does not qualify as a control because it did not occur less than two years before the Georgia reform.[23] Note that as an investor Ace is only considered because it invested in a treated startup within the 4-years of a law change; if it had not invested in GA1 (or any other treated startup), none of the other startups in its portfolio would have served as controls.

4.1.1. Empirical model

The data structure described above that compares investments within the same VC firm across states over time mirrors a difference-in-differences analysis. Motivated by this, the instrument for replacement is the interaction of a law change dummy and an indicator for a post-law time period. If the law changes impacted the supply of available executives, then they should predict differential founder replacement in firms in the treated states after the law change compared to firms at the same time in the same VC portfolio headquartered in other states. Let the variable I_i and D_i be dummy variables for startups who are in states that increased or decreased the non-compete rules, respectively. Let Post_{it} be one if either

[23] If Ace had also invested in a Texas startup less than two years before its 1996 policy change, then MD2 would also qualify as a control because it would be within two years of a policy change that the VC had also invested near.

a treated or control firm was financed in the two years after the change in the law. Recall that an investment in a control state is "matched" to the closest-in-time treated year (thus, state) within their investor's portfolio.

The instrumental variables are the interactions $I_i * \text{Post}_{it}$ and $D_i * \text{Post}_{it}$. The reduced form first stage regression that relates replacement to changes in non-compete laws is then:

$$\text{R}_{it} = \beta_0 + \beta_1 X_{it} + \gamma_1 I_i + \gamma_2 D_i + \gamma_3 \text{Post}_{it} + \delta_1 I_i * \text{Post}_{it} + \delta_2 D_i * \text{Post}_{it} + \lambda_k + \epsilon_{it}. \quad (3)$$

Again, R_{it} is whether a founder was replaced in entrepreneurial firm i's financing t, X_{it} are firm i time-varying controls such as firm age and λ_k are state fixed effects.[24] The estimates of δ_1 and δ_2 reveal whether there is a reduced form correlation between a shift of non-compete enforcement and replacement (R_{it}). If the non-compete law changes act as we predict, then $\hat{\delta}_1 < 0$ and $\hat{\delta}_2 > 0$. The second stage is now:

$$\text{Y}_{it+1} = \phi_0 + \phi_1 R_{it} + \phi_2 \text{I}_i + \phi_3 \text{D}_i + \phi_4 \text{Post}_{it} + \phi_4 X_{it} + \lambda_k + u_{it} \quad (4)$$

where the inclusion of the I_i, D_i and Post_{it} complete the set of diff-in-diff controls, here assumed to belong in the main model and R_{it} is instrumented using (3).

Table 7 contains the results of our instrumental variable regression, first for the liquidity outcome and then for time-to-next-financing. Column (2) presents the first stage estimates of (3) used in the two stage least squares.[25] The coefficient on "Post X Increase" (i.e. δ_1) is economically and statistically significant, with the predicted negative sign. By contrast, the coefficient on "Post X Decrease"(δ_2) is economically and statistically significant, with an expected positive coefficient. The weak instruments F-statistics (e.g. Stock and Yogo (2005)) is 15.29. Here, relative to firms in non-treated states, replacement rates in startups

[24] These are not identified for the treatment states as they are subsumed by D_i and I_i.

[25] As we have a binary endogenous variable, the first stage is a probit estimator following Wooldridge (2010). From this, we gather the predicted probabilities, which we use as the IV. This approach has the advantage or producing first-stage predictions that are inside the unit interval and the first stage standard errors are correct. The results are qualitatively and statistically similar if each stage is a linear model.

that faced weaker non-compete laws increased. The results suggest that founder replacement is indeed sensitive to the supply of available executives in the same state who might take the founder's executive role.

The second stage estimate in column (3) presents the instrumented coefficient for replacements R_{it}.[26] Two results emerge. First, the coefficient is positive and significant, suggesting a positive treatment effect. Second, the sign of the coefficient on founder replacement reverses between the naive regression in column (1), where it is negative, and the 2SLS result in column (3), where it is positive. The economic magnitude of the estimate can be determined by the predicted probability of replacement from the first stage in column (2). A shift in this probability from the bottom to top quartile of predicted replacement (2.5% to 7%) implies a 29% increase in the probability of a liquidity event relative to the mean. The difference in coefficient signs between the naive OLS and 2SLS imply a downward bias, which likely stems from a selection of relatively worse firms requiring VC intervention through founder replacement.[27] As an additional check for weak instruments, we find that the Moreira (2003) conditional likelihood ratio test provides a robust 95% confidence bound on ϕ_1 of $[.44, 2.6]$ that remains positive.

In the remaining columns of Table 7 we examine an alternative dependent variable: time to next financing. With this variable we can study the outcomes of firms who raised a new round of financing rather than exited or failed. The speed of raising capital was shown in Table 4 to correlate with cross-sectional success. Similarly, the study of VC returns by Korteweg and Sorensen (2010) show a strong correlation between the speed of capital raising and returns earned by investors. Here, the naive cross-sectional analysis in column (4) would indicate that founder replacement does not speed the time to the next financing (coefficient is positive and significant), while the instrumented analysis of column (5) reverses the sign of the coefficient. The economic interpretation using the interquartile range of the predicted

[26]The R^2 are not reported for the second stage as they are not a relevant summary statistic in the 2SLS setting.

[27]The Hausman test for whether the 2SLS and OLS differ rejects the null that they are the same. If the IV is indeed valid, this is additional evidence that the replacement dummy is endogenous.

first stage probability of replacement is a 8% faster re-investment relative to the mean. Again, the differences in coefficient estimates suggests the negative bias dominates and VC replacement can be beneficial for entrepreneurial firms.

4.2. Mechanisms underlying IV estimates

The estimates in Table 7 reveal that a positive, causal impact of founder replacement on entrepreneurial firms: allowing the average VC the (random) power to replace a founder would improve the firm's prospects. The specification introduces a randomly larger (or smaller) pool of executives to hire as replacements to a subset of investors and investments. A positive causal impact of such a shock implies that the sample of firms that responds to the instrument had founders with longer than shareholder-maximizing tenure. If VCs optimally replace under-performing managers on average, then our IV approach would find no effect. In this section we explore one explanation for this pattern.

Our results are consistent with venture capitalists correctly identifying management as a cause for under-performance and correctly replacing them. The results could follow from a combination of managerial entrenchment or similarly, weak board power. Scholars have frequently noted that entrenchment or excess autonomy on the part of public-firm managers (e.g., with minimal board supervision) often sacrifice firm value (see Faleye (2007); Berger, Ofek and Yermack (1997); and Wruck (1989)). Such entrenchment could itself stem from strong equity rights held by the entrepreneur, particularly in the early years of a firm's life.

In Table 8 we reestimate equation 4 to explore a possible mechanism by which replacing founders may unlock value: undoing managerial entrenchment. To the extent that entrenchment is driven by the power of insiders vs. outsiders, we might find greater entrenchment in startups that are of higher quality (or at least were perceived to be at founding) or where capital and voting structures favoring insiders may promote entrenchment. Both of these are difficult to observe directly, however. Although the initial quality of a startup may be revealed over time, eventual accumulation of resources as well as liquidity outcomes can be

substantially influenced by activist investors. Likewise, data on founder ownership is limited and often is missing for firms that did not achieve notable outcomes.[28] Given these challenges, we instead proxy for entrenchment using three measures that may indicate greater power on behalf of current management measured at the firm's first financing. Our dependent variable in Table 8 is again whether the next round of funding resulted in an attractive liquidity event.

Our first proxy for entrenchment in columns (1) and (2) is the number of syndicate partners in the first round of financing. Startups perceived to be of higher quality—whether due to the idea, the founders, or other factors—will attract more attention or bidding and may result in a larger number of investors attempting to participate in the round as part of the syndicate. Suggestive of this connection, we show in Figure 2 that syndicate size is increasing in the intensity of venture capital activity (e.g. Gompers and Lerner (2000)). In columns (1) and (2) we perform a split-sample analysis based on the number of syndicate partners within an industry-year and split by the median. As expected, the coefficient on founder replacement is positive and significant for larger syndicates (column (1)) but negative for smaller syndicates (column (2)).

Our second proxy is the relative size of the initial financing round. Following a logic similar to that of syndicate size, ventures that raise more money in their first round compared to others in the same industry that year may be in greater demand and thus be perceived as being of higher quality.[29] Again we see a somewhat stronger association between founder replacement and liquidity in the next round in those startups where more capital was raised in the initial round (column (3)), as reflected by the size of the coefficient and the statistical significance.

The final proxy considers a split-sample of firms based on the founding team size. A

[28] In our data, equity allocation is available for only 46% of startups, a disproportionate fraction of which eventually completed an IPO.

[29] Of course, a company may raise more capital than another due not only to its perceived quality but also the particulars of its business plan; for example, a startup planning to self-commercialize a new technology will need more resources than one that plans to partner with incumbents.

larger founding team may represent both formal and informal influence in the startup and hence greater entrenchment. For example, larger initial founding teams may require more initial equity for management and thus more power for the founders. If our hypothesis is correct, then executive replacement should make a greater contribution in startups with larger founding teams. Column (1) suggests that this may be the case, as the coefficient on replacement is positive and somewhat significant for larger founding teams whereas it is negative for smaller founding teams in column (2).[30]

In sum, our instrumental variables analysis lends support to the notion that replacing founders can unwind managerial entrenchment and unlock value in startup companies. That said, not all VCs replace founders, perhaps stemming from a VC's desire to maintain a reputation as "founder friendly." VCs who want to maintain such a reputation have to be relatively more patient with the management team. In situations where the team underperforms and there are quality thresholds for any replacements, our IV will introduce both an increase in replacement rates and an improvement in firm prospects conditional on the turnover. In this interpretation, the benefits observed from the replacement of founders were costs born by investors for the patience. Thus, we have an indirect view of the "price" investors are willing to pay for maintaining a founder-friendly reputation.

5. Discussion and Conclusion

This is the first paper to draw a causal link between founder replacement and startup performance. The results complement those of Bernstein, Giroud and Townsend (Forthcoming) and Chemmanur, Krishnan and Nancy (2011), both of whom show that VC's involvement with an entrepreneurial firm improves outcomes but do not determine any mechanisms underlying this improvement. More generally, we contribute to a perennial debate in the venture capital literature regarding the value of the VC firm and partner (Ewens and Rhodes-Kropf

[30]Note that the split sample is calculated based on all founding teams, not simply those in our IV subsample; hence there are more rounds to observe in startups with larger founding teams.

(2015); Gorman and Sahlman (1986); Hellmann and Puri (2002)). To date, value added by investors has primarily been found at the point of investment selection or the monitoring of firms as they grow. Given that the majority of entrepreneurial firms fail, establishing that investors can value by replacing founders represents a novel contribution.

Our work is also related to the "horse-vs-jockey" debate in venture capital. Among firms that completed an IPO, Kaplan, Sensoy and Strömberg (2009) found substantial replacement of CEOs. We likewise find a connection between founder replacement and subsequent liquidity events, but in large sample of firms with a range of exit outcomes. Our findings suggest that investors find it productive to replace the "jockey" when they believe the underlying "horse" to be of good stock. Thus a key contribution of investors can be unlocking the value of a firm's core assets by replacing entrenched executives.

Finally, our results speak to the tension between maintaining a founder-friendly reputation and optimizing for the performance of the current portfolio. Entrepreneurs care about their expected financial return but also not being replaced. Replacing founders aggressively may optimize the performance of the current portfolio but could scare off founders—including some of the most highly able founders—who insist on remaining in control of their ventures. Although we do not measure the impact of maintaining a founder-friendly reputation on the ability to attract future entrepreneurs, and suspect that such analysis is not straightforward, our results indicate that not replacing founders is hardly costless.

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Tables and figures

Fig. 1. Hazard of founder replacement by financing round

Note: Figure reports the fraction of founders that are replaced by entrepreneurial firm financing round. A replacement is assigned to a financing if it occurs on or after a financing, but before the next exit or new capital infusion.

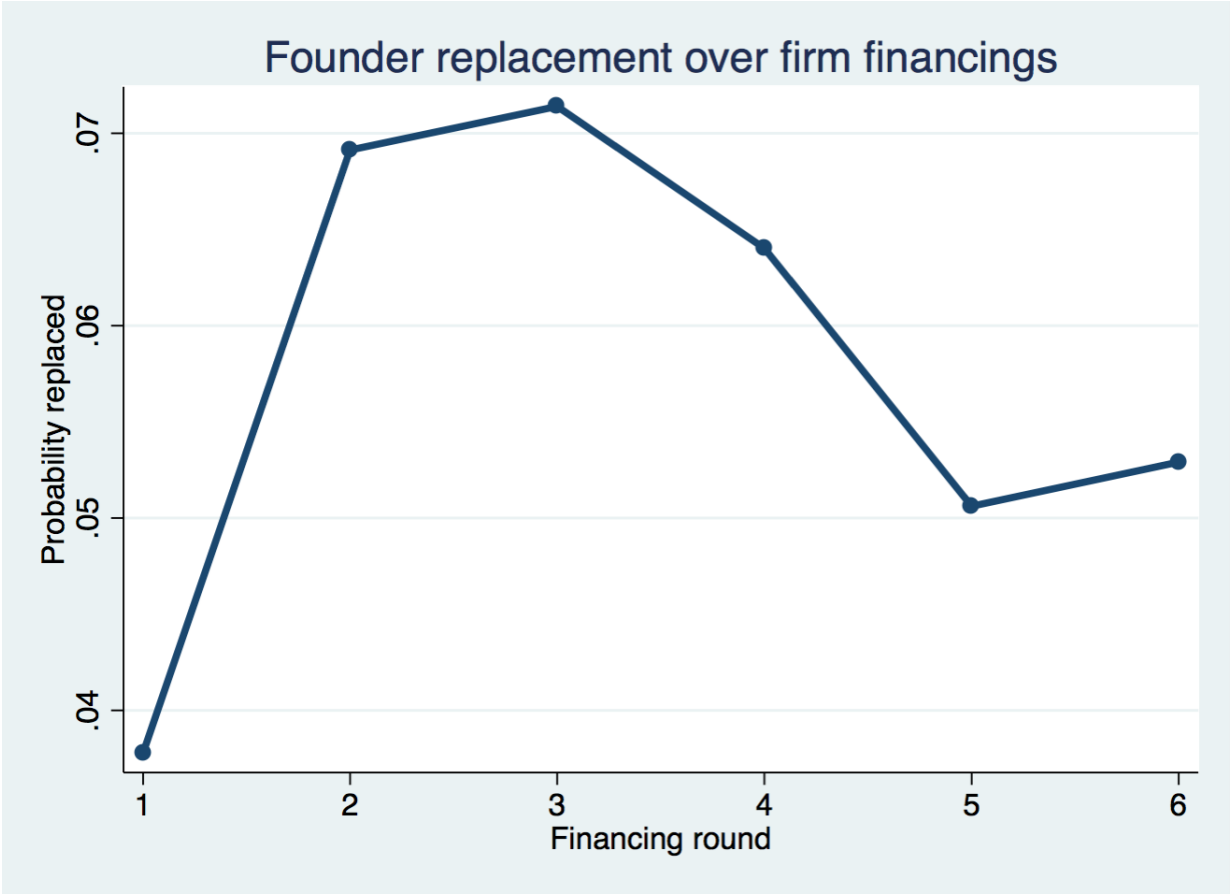

Fig. 2. First round syndicate size and new investments

Note: Figure reports average syndicate size – number of investors in a financing event – by year in first round investments and the total number of new first round VC investments.

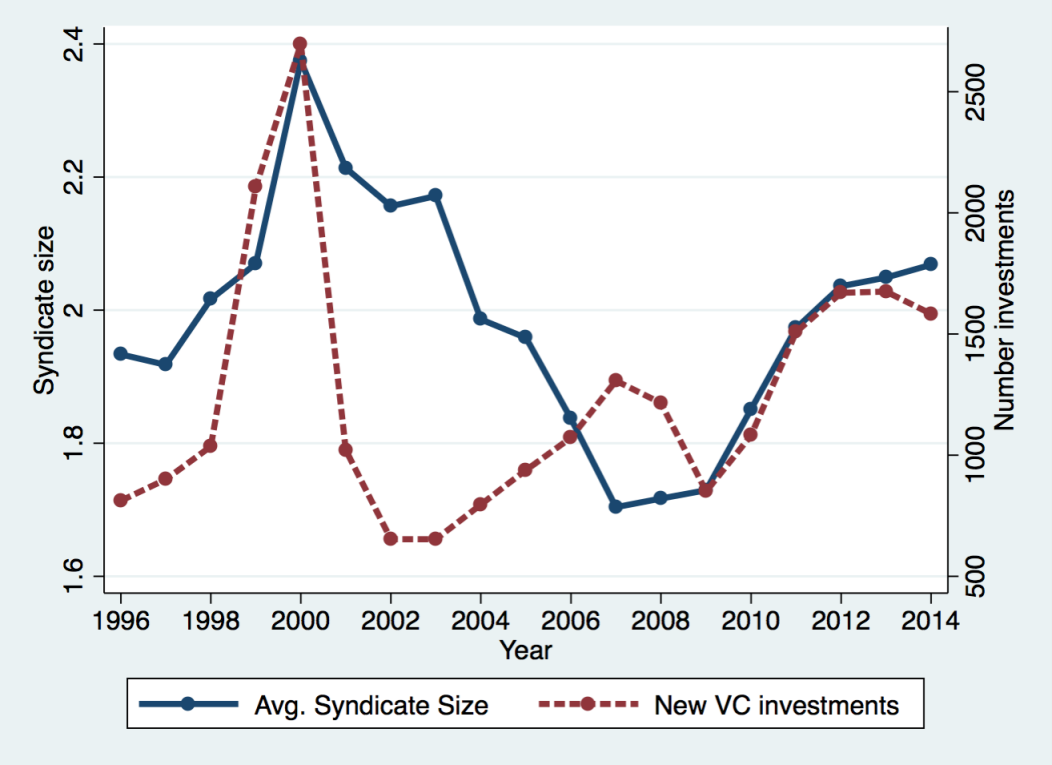

Table 1: Variable description

Note: Description of the variables used in the summary statistics and regression analysis.

IPO	A dummy variable equal to one if the entrepreneurial firm exited via an initial public offering by the end of the sample period (12/2014).
Acq.	A dummy variable equal to one if the entrepreneurial firm exited via an acquisition or merger by the end of the sample period (12/2014).
Private	A dummy variable equal to one if the entrepreneurial firm remains private as of the end of the sample (12/2014).
Failed	A dummy variable equal to one if the entrepreneurial firm failed by the end of the sample (12/2014).
Founding year	The founding year of the entrepreneurial firm, set to the year of first VC financing if unknown.
Biotech	A dummy variable equal to one if the entrepreneurial firm's industry is healthcare or biotechnology.
IT	A dummy variable equal to one if the entrepreneurial firm's industry is information technology.
First capital raised	The total capital raised in the first first VC financing.
Total raised	Total capital raised by an entrepreneurial firm across all its financing events.
Capital stock	Capital raised as of each financing event.
Total rounds	Total financing rounds with VC for the entrepreneurial firm.
Living dead	A dummy variable equal to one if the entrepreneurial firm's financing event took longer than similar financings in the same industry, stage and capital amount over the previous five years (top 10%).
High growth fin.	A dummy variable equal to one if the entrepreneurial firm's financing event was in the top 25% of speed compared to financings in the same industry, stage and capital amount over the previous five years.
Size of VC board	The number of board member investors as of each financing event.
Age of firm	Age of entrepreneurial firm at a financing event in years since firm founding .
CEO?/CXO etc.	Dummy for each of the major titles for executives: CEO, CXO (where "X" can be 'F' or 'I' or 'M') and VP
Syndicate size	The number of investors in the current financing round.
Profitable	Dummy for whether the firm reported profits in the current financing.
Round # FE?	Financing round number fixed effects.
Industry FE	Entrepreneurial firm industry fixed effects. The groups are "Business/Consumer/Retail," "Healthcare," "Information Technology" and "Other."
State FE	Entrepreneurial firm state fixed effects for the headquarters of the firm.
Year FE	Financing year fixed effects.

Table 2: Summary statistics

Notes: Table reports the summary statistics of the firms and financings in our sample.

	Panel A Firm characteristics							
	mean	sd	min	p25	p50	p75	max	count
Acquired	0.45	0.50	0	0	0	1	1	9184
Went public	0.063	0.24	0	0	0	0	1	9184
Out of Business	0.23	0.42	0	0	0	0	1	9184
Still private	0.25	0.43	0	0	0	1	1	9184
First capital raised	6.41	21.8	0	1.40	3.40	6.75	1500	9184
Year firm founded	2000.8	3.33	1996	1998	2000	2004	2007	9184
Information Technology	0.55	0.50	0	0	1	1	1	9184
Biotech	0.19	0.39	0	0	0	0	1	9184
California HQ	0.42	0.49	0	0	0	1	1	9184
Texas HQ	0.050	0.22	0	0	0	0	1	9184
New York HQ	0.059	0.24	0	0	0	0	1	9184
Total equity financings (all)	3.80	2.30	1	2	3	5	24	9184
Total capital raised (m)	40.6	133.8	0	7	19	44.8	10328.6	9184
Year first VC	2002.2	3.77	1996	1999	2001	2006	2012	9184
Founder replaced?	0.16	0.36	0	0	0	0	1	9184
	Panel B Financing characteristics							
	mean	sd	min	p25	p50	p75	max	count
Capital raised in round (m)	11.5	25.0	0	2.50	6.29	13.2	1600	26910
Profitable at financing	0.042	0.20	0	0	0	0	1	26910
Firm age (years)	3.31	2.88	0	1.08	2.55	4.81	17.6	26910
Year of financing	2003.8	4.33	1996	2000	2003	2007	2013	26910
Syndicate size	2.86	2.22	1	1	2	4	24	26910
Round number	2.45	1.59	1	1	2	3	16	26910
Total capital raised as of t (including current)	24.6	53.3	0	3.55	10.7	28	2228.6	26910
Founder replaced in round?	0.056	0.23	0	0	0	0	1	26910

Table 3: Founder replacement patterns

Notes: Sample includes entrepreneurial firms tracked by VentureSource that satisfy the sample conditions in Section 2. The table reports replacement rates across financing round sequence. The number of startups receiving an Nth round of funding is lower than the number who received funding in a prior round, less exits, because some firms continue as private entities without raising subsequent financing.

	Round Number					
	1	2	3	4	5	6+
Startups raising Nth round of funding	9184	7493	4706	2718	1462	756
Startups achieving liquidity this round	957	1053	882	6120	388	395
Startups failing this round	237	869	859	214	99	69
Startups with founder replaced this round	347	503	312	157	66	55
Startups with founder replaced so far	347	850	1162	1319	1385	1440
% Startups with founder replaced this round	3.7%	5.4%	6.6%	5.8%	4.5%	7.3%
% Startups with founder replaced so far	3.7%	9.3%	12.7%	14.4%	15.1%	15.7%
% with replacement and at least N rounds	15.7%	17.5%	21.4%	25.4%	28%	31.2%

Table 4: Living dead and firm characteristics

Notes: Table reports the firm-level predictability of a "living dead" within an entrepreneurial firm's financing. The panel aggregates at the firm-level where a firm is in the first column if it never had a living dead financing in its financing history. Variables defined in Table 1. Significance from a two-sided t-test: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Living dead firms		
	Never living dead	Was living dead	Diff/s.e.
Went public	0.0701	0.0413	0.0288***
			0.00592
Still private	0.215	0.370	-0.155***
			0.0105
Acquired	0.468	0.399	0.0695***
			0.0121
Out of Business	0.246	0.188	0.0578***
			0.0103
First capital raised	6.439	6.407	0.0314
			0.530
Total capital raised (m)	42.01	36.14	5.864
			3.257
Year firm founded	2000.9	2000.7	0.227**
			0.0812
Year first VC	2002.3	2001.9	0.326***
			0.0918
California HQ	0.432	0.391	0.0409***
			0.0120
New York HQ	0.0573	0.0656	-0.00824
			0.00575
Texas HQ	0.0483	0.0544	-0.00607
			0.00530
Total equity financings (all)	3.847	3.641	0.207***
			0.0560
Founder replacement?	0.147	0.186	-0.0388***
			0.00884
Exit value ($m)	248.3	107.7	140.6***
			22.6
Number Firms	6958	2226	9184

Table 5: Correlates of founder replacement

Notes: Table reports OLS regression estimates where the dependent variable is equal to one if any founder of an entrepreneurial firm was replaced at a financing event. Replacements made after a financing, but before any subsequent exit or capital infusion are assigned to the previous financing. Columns (1) - (6) consider any founder replacement from VP-level to CEO. The last column has a dependent variable equal to one only if a founder-CEO was replaced. "Log firm age" is the age of the entrepreneurial firm at the time of the financing and "Log capital stock" is the log of total equity raised as of the financing. "Syndicate size" is a count of the number of investors in the most recent financings and "Profitable" is a dummy variable equal to one if the financing occurred when the firm reported profits. Column (2) introduces the variable "Living dead" which is one if the financing ever entered this state, which we define as being in the 90th percentile of time since last capital raise using historical events for the same industry. "High growth fin." is equal to one if the financing was followed quickly by another round of capital (90th percentile of speed to next round). "large outside board" is a dummy variable equal to one if the board size is greater than the sample median (2). The final column introduces the observation at the person-financing level and the additional controls "CEO" and "CXO." Each are dummies for titles CEO and other C-level executives, where the excluded title are those below (VP). Standard errors clustered at the entrepreneurial firm. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Founder replaced?						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Living dead?		0.0627***			0.0575***	0.0430***	0.0756***
		(0.00674)			(0.00681)	(0.00825)	(0.00901)
High growth fin.			-0.0370***		-0.0312***	-0.0314***	-0.0366***
			(0.00270)		(0.00272)	(0.00272)	(0.00344)
Large outside board				0.0151***	0.0171***	0.0143***	0.0161***
				(0.00351)	(0.00351)	(0.00346)	(0.00444)
Large outside board X Living dead						0.0308**	
						(0.0135)	
CEO							-0.000532
							(0.00236)
CXO							-0.0129***
							(0.00306)
Log firm age (yrs)	0.00424***	0.00319**	0.00377**	0.00461***	0.00329**	0.00332**	0.00410**
	(0.00150)	(0.00150)	(0.00150)	(0.00151)	(0.00150)	(0.00150)	(0.00192)
Log capital stock	0.00837***	0.00872***	0.00871***	0.00671***	0.00712***	0.00710***	0.00767***
	(0.00133)	(0.00133)	(0.00133)	(0.00138)	(0.00138)	(0.00138)	(0.00176)
Syndicate size	0.00312***	0.00342***	0.00273***	0.00224***	0.00208**	0.00207**	0.00298***
	(0.000805)	(0.000802)	(0.000804)	(0.000828)	(0.000823)	(0.000823)	(0.00108)
Profitable at financing	0.0120	0.0170**	0.0106	0.0123	0.0158**	0.0156**	0.0230**
	(0.00772)	(0.00770)	(0.00771)	(0.00771)	(0.00769)	(0.00768)	(0.0101)
Constant	-0.00853	-0.0210	0.00186	-0.00800	-0.0106	-0.0104	0.0117
	(0.0204)	(0.0195)	(0.0206)	(0.0202)	(0.0198)	(0.0194)	(0.0266)
Observations	26910	26910	26910	26910	26910	26910	43743
R^2	0.0141	0.0200	0.0182	0.0148	0.0237	0.0240	0.0316
Firms	9184	9184	9184	9184	9184	9184	9184
Mean dep. var.	0.0561	0.0561	0.0561	0.0561	0.0561	0.0561	0.0663
Year FE?	Y	Y	Y	Y	Y	Y	Y
Round # FE?	Y	Y	Y	Y	Y	Y	Y
Industry FE?	Y	Y	Y	Y	Y	Y	Y
State FE?	Y	Y	Y	Y	Y	Y	Y

Table 6: Top 40 employers of replacing executives joining entrepreneurial firms

Notes: Table tabulates the count of employers for the executives that join the entrepreneurial firms in our sample where we identify a replacement. Employers are from the short biographical string of the executive available in VentureSource (e.g. "CFO, Microsoft"). "Total acquisitions" counts the number of firms acquiring US-based targets over the 1992-2008 sample period.

	Count	Percent	Cumulative %	Total acquisitions
IBM	59	6.81	6.81	80
Oracle	57	6.57	13.38	44
HP	43	4.96	18.34	60
Cisco	42	4.84	23.18	86
Lucent	39	4.50	27.68	26
AT&T	38	4.38	32.06	31
Microsoft	36	4.15	36.22	87
GE	30	3.46	39.68	197
Intel	29	3.34	43.02	42
Nortel	28	3.23	46.25	8
Motorola	26	3.00	49.25	47
Sun Microsystems	24	2.77	52.02	39
EMC	23	2.65	54.67	32
PeopleSoft	21	2.42	57.09	15
Symantec	21	2.42	59.52	34
Ernst & Young	20	2.31	61.82	34
Price Waterhouse	19	2.19	64.01	1
SAP	19	2.19	66.21	1
Deloitte & Touche	18	2.08	68.28	33
Siebel	18	2.08	70.36	15
Lucent	17	1.96	72.32	26
Dell	17	1.96	74.28	9
3Com	16	1.85	76.12	18
Siemens	16	1.85	77.97	23
McKesson	15	1.73	79.70	25
Novell	14	1.61	81.31	17
Cadence Design Systems	13	1.50	82.81	29
EDS	13	1.50	84.31	29
Yahoo	13	1.50	85.81	26
Ariba	12	1.38	87.20	26
Medtronic	12	1.38	88.58	32
i2	12	1.38	89.97	9
AOL	11	1.27	91.23	15
Accenture	11	1.27	92.50	0
Apple	11	1.27	93.77	13
Computer Associates	11	1.27	95.04	2
FOX	11	1.27	96.31	5
Pfizer	11	1.27	97.58	19
Sony	11	1.27	98.85	4
Merck & Co	10	1.15	100.00	7
Total	867	100.00		

Table 7: Founder replacement and firm outcomes: Instrumental variables

Notes: Table reports OLS and 2SLS estimates for founder replacement and entrepreneurial firm outcomes. The unit of observation is a entrepreneurial firm financing where there still remain active founders on the executive team. The sample of entrepreneurial firms is described in Section 4.1. Column (1) regresses a dummy variable for whether a financing is followed by an IPO or attractive acquisition on a set of controls. The control "Founder replaced" is one if a financing had at least one founder replaced on the executive team. "Post law change" is an indicator for whether an entrepreneurial firm financing occurred after an increase in non-compete regulations in either its state or a state of another firm in the entrepreneurial firm's VC's portfolio. "Increase CNC" and "Decrease CNC" are dummy variables for increased and decreased enforcement of non-competes, respectively. Other controls are as defined in Table 5. Column (2) reports the first stage probit estimates where the replacement dummy is instrumented by the interaction term "Post x Increase" or "Post x Decrease" depending on the nature of the policy change. The instrument is the interaction of the post-law change dummy (up to two years after the change) and the dummy for increased or decreased enforcement. "1st. stage F" is the first-stage F-statistic for weak instruments. Column (3) reports the two-stage least squares estimate second stage estimates. Column (4) - (6) consider the dependent variable that is the years to next refinancing event (i.e. non-exit or non-failure). "Year FE" are fixed effects for the financing year and "Round # FE" are fixed effects for the financing round number. "Industry FE" are fixed effects for the seven major industries in VentureSource. Robust standard errors reported in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	IPO/Acq.? OLS (1)	Replaced? First stage (2)	IPO/Acq.? 2SLS (3)	Time to next OLS (4)	Replaced? First stage (5)	Time to next 2SLS (6)
Founder replaced	-0.0219		0.623**	0.213***		-1.491**
	(0.0199)		(0.313)	(0.0554)		(0.671)
Post X Increase		-0.719***			-0.986***	
		(0.275)			(0.316)	
Post X Decrease		0.426***			0.303	
		(0.158)			(0.192)	
Post law change	0.0121	-0.331***	0.0360*	0.0749	-0.352***	-0.00343
	(0.0161)	(0.0959)	(0.0208)	(0.0501)	(0.120)	(0.0619)
Increase CNC	-0.0297	0.466**	-0.0363	0.140	0.659***	0.168**
	(0.0202)	(0.191)	(0.0241)	(0.0899)	(0.224)	(0.0706)
Decrease CNC	0.00316	-0.117	-0.00624	-0.0179	-0.167	-0.0206
	(0.0139)	(0.125)	(0.0154)	(0.0389)	(0.146)	(0.0442)
Log capital stock	0.0280***	0.113***	0.0215***	0.0619***	0.103***	0.0770***
	(0.00484)	(0.0310)	(0.00628)	(0.0158)	(0.0382)	(0.0178)
Profitable at financing	0.0880***	-0.0384	0.0899***	0.362**	-0.0266	0.361***
	(0.0280)	(0.132)	(0.0243)	(0.155)	(0.187)	(0.0828)
Syndicate size	0.00423*	-0.0136	0.00501**	-0.00575	-0.00526	-0.00600
	(0.00244)	(0.0144)	(0.00249)	(0.00809)	(0.0172)	(0.00764)
Constant	-0.0227	-1.972***	-0.0348	0.865***	-2.250***	1.331***
	(0.0217)	(0.190)	(0.0332)	(0.0747)	(0.316)	(0.104)
Observations	6440	6440	6440	4370	4370	4370
R^2	0.0503	0.0444	.	0.105	0.0659	.
1st. stage F		15.29			13.09	
Year FE?	Y	Y	Y	Y	Y	Y
Round # FE?	Y	Y	Y	Y	Y	Y
Industry FE?	Y	Y	Y	Y	Y	Y

Table 8: Founder replacement and firm outcomes: Instrumental variables and subsample

Notes: Table reports OLS and 2SLS estimates for founder replacement and entrepreneurial firm outcomes. Columns (1) and (2) split the sample by the number of investors in the entrepreneurial firm's first financing event, split by the median of the full sample. Columns (3) and (4) split the sample by the relative size of the firm's first financing within the year and industry. Firms that raised about the median capital of firms in the same year and industry are "Large raised." Columns (5) and (6) split the main sample from Table 5 by the size of the founding team (using the median). Controls are as defined in Table 7. "Year FE" are fixed effects for the financing year and "Round # FE" are fixed effects for the financing round number. "Industry FE" are fixed effects for the seven major industries in VentureSource. Robust standard errors reported in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	1st synd. size		1st capital raised		Founding team size	
	Large synd.	Small synd.	Large raised	Small raised	Large team	Small Team
	(1)	(2)	(3)	(4)	(5)	(6)
Founder replaced	2.255**	-0.416	1.705**	0.439	2.679*	-0.0777
	(1.073)	(0.591)	(0.850)	(0.650)	(1.584)	(0.511)
Post law change	0.186**	-0.0491	0.0762	0.0624	0.104	0.0401
	(0.0733)	(0.0316)	(0.0473)	(0.0458)	(0.0804)	(0.0386)
Increase CNC	-0.000923	-0.0770*	0.0353	-0.109***	-0.0118	-0.00382
	(0.0628)	(0.0405)	(0.0552)	(0.0386)	(0.0900)	(0.0330)
Decrease CNC	0.00921	-0.0439*	0.0302	-0.0752***	0.0356	-0.00816
	(0.0395)	(0.0261)	(0.0375)	(0.0255)	(0.0487)	(0.0261)
Log capital stock	0.0157	0.0352***	0.0297*	0.0162	-0.00110	0.0207***
	(0.0151)	(0.0103)	(0.0164)	(0.0107)	(0.0286)	(0.00755)
Profitable at financing	-0.00139	0.0973***	0.0683	0.0703*	0.130*	0.0514
	(0.0674)	(0.0370)	(0.0515)	(0.0369)	(0.0686)	(0.0388)
Syndicate size	0.00590	0.000285	0.00456	-0.00107	0.0143*	-0.00558
	(0.00593)	(0.00412)	(0.00510)	(0.00429)	(0.00774)	(0.00357)
Constant	-0.202**	-0.00139	-0.137*	-0.0922	-0.0898	-0.0689
	(0.0934)	(0.0523)	(0.0803)	(0.0566)	(0.105)	(0.0533)
Observations	2915	2110	2766	2081	2799	2260
Mean dep. var.	0.165	0.140	0.172	0.134	0.173	0.132
Year FE?	Y	Y	Y	Y	Y	Y
Round # FE?	Y	Y	Y	Y	Y	Y
Industry FE?	Y	Y	Y	Y	Y	Y