Founder Replacement and Startup Performance

Michael Ewens and Matt Marx*

Abstract

We provide causal evidence that venture capitalists (VCs) improve the performance of their portfolio companies by replacing founders. Augmenting a database of U.S.-based, VC-backed startups from 1995–2008 with hand-collected information regarding turnover, we exploit shocks to the supply of outside executives via state-level changes in the enforceability of employee non-compete agreements. Although naive regressions of startup performance on founder replacement would suggest a negative correlation, this may be due to selection as founders are likely to leave or be pushed out of poorly-performing startups. Indeed, instrumented regressions reverse the sign of this effect, suggesting that replacing founders improves the performance of venture-backed entrepreneurial firms. Replacement helps more when founders hold CXO roles and when the incoming replacement has substantial work experience. The evidence points to the replacement of founders as a specific mechanism by which VCs add value.

JEL classification: G24, G34, L2, M12.

Keywords: Executive turnover, venture capital, financial intermediation, entrepreneurship.

*Ewens: California Institute of Technology (mewens@caltech.edu). Marx: Massachusetts Institute of Technology (mmarx@mit.edu). Corresponding author: Ewens, California Institute of Technology D.H.S.S. (MC 228-77),1200 East California Blvd. Pasadena, CA 91125 USA, 619-512-3820. Both authors recognize the support of the Kauffman Junior Faculty Fellowship. We thank David Denis, Matthew Rhodes-Kropf, the participants at the Duke Strategy Conference, Duke Finance department, Georgia Tech strategy department, the Northeastern entrepreneurship department, and the NBER Entrepreneurship Working Group for their comments. The VentureSource data are provided by Correlation Ventures, to which Ewens is an advisor and investor.

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 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 (2016) similarly find that VC-backed firms are more likely to achieve liquidity events once their investors are able to visit via a nonstop 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 where investors spend time, ostensibly in the interest of improving performance. 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 among VC-backed startups 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

[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)

with increased sales. But customer introductions are difficult to observe empirically, so 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 may resist being replaced.

Four studies have examined founder replacement. In a survey of 170 Silicon Valley based startups, Hellmann and Puri (2002) find that VC-backed ventures are more likely than others to replace the original founder-CEO (and earlier), attributing founder replacement to a process of "professionalization" whereby the adolescent venture becomes a more mature company. Consistent with this view, and also using survey data from approximately 200 companies (though nationwide), Wasserman (2003) finds that founder replacement coincides with milestones in the development of a startup such as completing product development or raising a new round of financing. Wasserman also finds that the likelihood of a founder being replaced is increasing in the number of outsiders on the board of directors, suggesting that investors may replace founders proactively. Kaplan, Sensoy and Strömberg (2009) find that 42% of founders among 50 VC-backed startups that completed an IPO were replaced, possibly indicating that such "professionalization" is associated with positive venture outcomes. Although these studies provide a wealth of insight into the phenomenon of founder replacement, including reasons for replacement and the subsequent disposition of the replaced founder, they do not formally test the connection between founder replacement and venture outcomes. Chen and Thompson (2015) attempt to draw a connection between founder re-

placement and performance by merging the Danish register of 4,172 of new businesses with the country's Census data. They note that startups experiencing founder replacement were more likely to fail, although firms that had a replacement and nonetheless survived grew faster. However, they stop short of making causal claims.

Drawing inferences regarding the impact of founder replacement on performance is challenging because founder replacement is endogenous. Founders may decide to leave the firm voluntarily, either because they have given up hope or because they are "serial" entrepreneurs who prefer to be involved only in the early stages and then depart to start another venture. Replacement may instead be involuntary. Control rights afforded investors via contracts as well as voting rights on the board of directors enable investors to force founders to relinquish their role. They may replace a founder either when the business is struggling—as Chen and Thompson (2015)'s data might suggest—but they may also elect to replace when the startup is growing quickly yet the investors doubt the founder's ability to scale up the company as Wasserman (2003) suggests. In addition, the quality of the person hired to replace the founder may be endogenous. It may be harder to attract strong executives to struggling startups. Moreover, the ability to attract top talent may depend on the quality of the investors and their networks (Amornsiripanitch, Gompers and Xuan (2015)).

It might seem straightforward that replacing founders would help firm performance. If investors are rational and add value by monitoring the firm (Bottazzi, Da Rin and Hellmann (2008)); Chemmanur, Krishnan and Nancy (2011); Bernstein, Giroud and Townsend (2016)), then they should not replace founders unless doing so is beneficial. However, if investors think that they are better informed than the founders but are often incorrect, replacement could be detrimental. Investors may underestimate the key influence of a founder. Moreover, if a founder who has attracted many early employees is replaced, loyal-to-the-founder employees may become disenchanted and possibly leave. Even if investors are correct that the startup would be better off with the founder serving in a different role at the company, the founder may thwart this plan by refusing to stay once replaced by an outsider.

To address the issue of the effect of founder replacement on firm outcomes, we construct a novel database of VC-backed founders and their replacements. The data builds on the VentureSource repository of entrepreneurial firms, financings, investors and executives. A multi-pronged data collection and cleaning augmented VentureSource by identifying founders as well as replacements of founder-executives at VC-backed firms founded between 1995-2008. Our final sample includes 11,929 firms and 19,830 founders. Some 15% of firms have at least one founder replacement in our sample period, almost 40% of whom appear to stay at the startup after they are replaced. The first major question that we ask is whether these replacements correlate with startup firm exit outcomes.

Naive regressions show a negative correlation between founder replacement and liquidity events. We analyze founder replacements in U.S. venture-backed firms, augmenting VentureSource records from startups founded 1995-2008 with extensive hand-collection of individual career histories. Similar to Wasserman (2003) we find that founder replacement is more likely to occur following a new round of financing as well as when the board contains more investors. But as argued above, even if VCs play a primary role in replacing founders the negative correlation between replacement and subsequent performance could be explained by selection if investors choose to replace a founder when a startup is in trouble or because a highly-qualified replacement is hard to find (including when founders relinquish their role voluntarily).

We instrument for founder replacement using a plausibly exogenous shock to the supply of executives who might serve as suitable replacements: changes in the enforceability 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); Marx (2011); Garmaise (2011)). Thus the ability of an investor to attract a qualified replacement may depend on the extent to which non-compete agreements are enforceable. The large-scale data on founder replacement we build off of VentureSource enables us to assess the impact of non-compete

enforceability by exploiting reforms in eight different states, some of which tightened enforceability while others loosened enforceability. Founders are less (more) likely to be replaced when non-compete enforceability has been strengthened (weakened). The larger dataset of founder replacements we collected enables us to test the effect of these employment contracts on replacement rates and subsequent venture performance in a new way.

Instrumenting for founder replacement with these policy changes shows that replacement increases the likelihood of achieving a high-quality liquidity event such as an IPO or attractive acquisition. The instrumented finding reverses the correlation found in the naive cross-sectional analysis. Decomposition of the instrumented results reveals which types of replacements have the greatest impact. Replacing founders who hold CXO roles such as CEO and CFO is more consequential than replacing founders in lower roles. The type of the incoming replacement also matters: the startup is more likely to achieve liquidity if the new executive has substantial corporate experience. However, whether the replaced founder leaves the company or stays on in a different role does not appear to matter.

Taken together, these findings seem to point to the role of venture capitalists value-add in professionalizing their portfolio companies by replacing founders with more experienced executives. Replacement is more common following a round of funding and when investors hold more board power, and replacement contributes more to positive venture outcomes when CXO-level founders leave the company to make room for seasoned executives to take their place. In other words, replacement does not seem to involve bringing on mentors to coach the original founders, or to find "better entrepreneurs" to run the company. Insofar as venture capitalists play an important role in both the decision to replace founders and identify their replacements, the positive causal effects we find point directly to a key mechanism by which VCs add value to their portfolio firms.

2. Data

The objective of the data collection discussed here and in more detail in the Appendix is to create a representative sample of VC-backed founders and the incidence of their replacement. To our knowledge, such a database with broad coverage does not exist, so we assembled one using several different sources of information. To start, we collected the set of VC-backed entrepreneurial firms founded from 1995 through 2008 using VentureSource. VentureSource is a database of venture capital transactions, entrepreneurial firms, company executives, investments and outcomes provided by Dow Jones.[2] VentureSource is however less reliable in capturing information about founders as startups are not required to report exhaustive founder data. Rather, VentureSource gathers information on founders from the startups themselves as well as third-party sources. We addressed these limitations with several data collection efforts.

VentureSource has incomplete coverage of founders either because some firms have no founders identified or similarly, some of the executives of the firm are incorrectly labeled as non-founders. We addressed these issues by starting with the data from Ewens and Fons-Rosen (2015) firms along with an extensive search for missing founders using LinkedIn, Crunchbase, company websites, and CapitalIQ. For the 2,159 firms where VentureSource listed no founders, we found 3,516 missing founders. Next, even if a startup has one founder it may be that other executives listed in VentureSource for that firm are missing the founder label. To begin, for all 6,219 firms with just one founder, a research assistant examined all the other executives using the websites mentioned above to determine whether that executive was also a founder. This process resulted in 1,226 additional founders. Several other data collection tasks were completed to try and remedy missing founders (the Appendix details each of these in depth).

Using the steps above we found 5,259 additional founders, which raised the average founding team size from 1.6 in the raw VentureSource data to 2.15 in our final sample. This

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

compares favorably with prior work on founding teams. Kaplan, Sensoy and Strömberg (2009) report 1.9 founders on average in their same of 48 venture-backed companies that completed an IPO. Beckman (2006) extend the dataset used by Hellmann and Puri (2002) to include all founders of the 173 Silicon-Valley-based companies collected by Burton (1995), finding 2.2 founders on average. Wasserman (2003) reports an average of 2.5 founders among a combination of 202 venture-backed and non-venture-backed startups.

With the new founders collected, sample creation begins with the the set of all VC-backed entrepreneurial firms founded between 1995 and 2008. 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 further filter VentureSource data according to its coverage of management teams by requiring that the firm has at least one founder (90% of firms have at least one identified in the data after data cleaning) and raised some capital from a traditional venture capital firm.[3] We also require that the founder 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 19,830 founders of 11,929 entrepreneurial firms. Over 75% of the firms in the sample have exited by the end of the 2014. A summary of most of the variables used in this paper is found in Table 1; summary statistics are found in Table 2.

2.1. 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. But recruiting can also take place for roles already occupied when a replacement is sought. Additive recruiting is unlikely to be controversial, whereas

[3]The second condition excludes firms that raise capital strictly from angel investors, hedge funds or corporations.

our interest is in the dynamics and impact of replacement. Replacement might be uncontroversial if founders are eager to relinquish their role, or it might be difficult if founders and investors differ in their view of the founders' suitability to continue in their current role.

VentureSource includes information on top-level managers, executives and investor board members. For each executive, VentureSource contains the title held at the venture-backed firm(s) 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.

As we are ultimately interested in the dynamics of founder replacement, the join date for each new occupant of a given title is essential.[4] Unfortunately, join dates are missing for approximately 70% of the replacement executives in VentureSource. We undertook 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 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 replacement executives, reducing the missing join dates to 16% of replacement executives. 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 establish the timing of such.[5]

For non-joint titles for which we have join dates for all occupants, we take the join date(s)

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

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

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.

One additional 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 that known online. Such selection will attenuate any negative relationship between replacement and firm performance. 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.

2.1.1. Decomposing the nature of replacement

As characteristics of the replaced founder, the incoming executive, or the replacement more generally may affect subsequent performance, we collect additional data regarding replaced founders and their incoming replacements. The above-described data collection tells us the role held by the replaced founder, and VentureSource contains a "biography string" listing previous positions. The string does not indicate the years of experience the replacement had, whether s/he had previously founded a startup, or anything regarding educational background. We also want to know what happened to the founder following that replacement as well as characteristics of the incoming executive. Using LinkedIn, we

were able to capture career histories for 1,322 of the 1,999 replaced founders as well as the new, incoming managers who replaced them.[6]

In their detailed survey data, Hellmann and Puri (2002) find that 40% of replaced founders continue at a startup in a new role, which they refer to as an "accommodating" replacement as opposed to a "separating" replacement where the replaced founder leaves the company. We classify replacements into these two categories as follows: If VentureSource lists a subsequent job for that founder at the startup then we label the replacement as "accommodating." Even if VentureSource does not show a subsequent role for the replaced founder, we label the replacement as "accommodating" if their LinkedIn profile claims that they were employed by the firm for at least two years after the replacement date in VentureSource. This condition holds unless LinkedIn lists another job within that two-year period (in case the founder moved to an advisory role or such). The Appendix provides additional detail on the data collection process. We find 38% of replacements to have been "accommodating", rather close to the 40% reported in Hellmann and Puri (2002). Replaced founders who stay have less experience than those who leave but are more likely to have previously founded a company. They also appear somewhat more likely to have a Ph.D and somewhat less likely to have an MBA (Table A2 in the Appendix). Our analysis thus builds on Hellmann and Puri (2002) in that we are able to assess the causal effect of different types of replacement.

These data also enable us to describe incoming replacement executives and compare them with the founders they replace. Among the fields captured in this data collection effort were the person's years of work experience, whether the person had previously founded a startup, and education. Regarding education, we noted whether the person had a bachelor's degree, master's degree, MBA, MD, or Ph.D. Table A3 in the data appendix compares replaced founders with the new, incoming replacement managers along all of the fields we collected. The comparison suggests that new, incoming managers tend to have more experience than the replaced founders but are less likely to have previously founded a startup. They are

[6]Section 1.2 in the Appendix has more details on the data collection.

more likely to have an MBA and also somewhat more likely to have completed college.

2.2. Descriptives and correlates of founder replacement

Before proceeding to analysis of venture performance, we characterize founder replacement in our data. Table 3 show the dynamics of founder replacement by financing round. As noted in the second to last row of Table 3, 15.1% of venture-backed firms in our sample experience a founder-replacement event (18.6% if we include those without dates). Founder replacement is less common in the first round but rises in the second round and continues well into the sixth round.

Next, we adopt a hazard specification of a particular founder being replaced as the data are right-censored and the phenomenon is observed on a continuous basis. Although we observe the exact date of a replacement, we create quarterly spells (results are robust to the use of monthly spells). We first account for characteristics of the founder, including whether that founder had a CXO title and whether there were any co-founders. We then track financing, including new rounds of funding, the overall level of funding to date, and whether the startup was profitable as of that round of financing.

Recent financing rounds and the overall level of investment may proxy for the power of investors to replace founders. To further assess the role of investor power in replacing founders, we examine the number of directors who are investors. 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.

Analyzing board investor power requires the number of VCs who are directors in each 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.[7] 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).[8]

Table 4 reports factors associated with the hazard of founder replacement. We find that the hazard of replacement is increasing not only in a new round of funding (whether in that quarter or the previous two quarters) but also in the total amount of funding raised so far and in the number of investors on the board of directors. Consistent with Wasserman (2003), this suggests that investors indeed play a role in founder replacement, perhaps as a condition of a new round of funding or as part of their governance responsibilities as members of the board of directors. However, profitability is not associated with replacement in a statistically-significant fashion. Founders holding a C-level role are more likely to be replaced than others.

We also see some differences among industries. Healthcare in particular differs from other industries in that the level of funding to date does not predict replacement. Also, solo founders are somewhat more likely to be replaced in Consumer and IT startups but not in Healthcare or Consumer.

[7]It may be that an investor remains on the board even after they stop investing in the startup, even if a new investor takes a board seat, so our count of investor-directors may be conservative.

[8]The noise inherent in assigning exits dates to board seats leads to some large boards. In the raw data, fewer than 3% of boards have more than 10 outside board members, however, these boards are comprised of members that are listed as "former" but do not have an exit date. We truncate the outside board size at ten to remove some of this measurement error.

3. Does founder replacement impact performance?

As noted above, Bernstein, Giroud and Townsend (2016) establish a causal connection between investor activism and IPO/acquisition outcomes but do not demonstrate specific mechanisms as to how these benefits are achieved. 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)), the only paper to draw a connection between founder replacement and venture outcomes is Chen and Thompson (2015) which stops short of claiming causality.

To study the impact of founder 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.[9] An unattractive acquisition, failure of the firm, or remaining in the living-dead state all set the dependent variable to zero.[10] While 10% of firms achieve an IPO, some 20% of firms in our sample achieve our exit success.

The empirical model ties the number of replacements to this outcome Y_i:

$$\mathrm{Y}_i = \rho_0 + \rho_1 R_i + \rho_2 X_i + v_i. \tag{1}$$

Here X_i contains entrepreneurial firm characteristics such as firm age, syndicate size, and profitability and total capital raised. These time-varying measures are calculated either at each financing event, each quarter or at the time of the law changes we use in the instrumental variable analysis. It will also capture fixed effects for founding year, stage, state and industry fixed effects. R_i indicates whether a founder was replaced. The unit of observation is an entrepreneurial firm.

[9] Bernstein, Giroud and Townsend (2016) 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.

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

Table 5 estimates this equation on the full sample of firms as described above. As is visible from Column 1, there is a strong negative correlation between founder replacement and favorable outcomes. Column 2 repeats the exercise for CXO replacements only. Columns 3 and 4 restrict the analysis to firms with positive outcomes, instead measuring the size of the liquidity event. That this is also negatively correlated with founder replacement seems inconsistent with the Chen and Thompson (2015) finding that Danish startups that had founders replaced but did not fail experienced greater sales growth, although the two datasets and variables are not directly comparable.

We cannot draw causal conclusions from Table 5 for several reasons. Venture capitalists may be more likely to replace founders when their startups are struggling, and founders may be more likely to relinquish their roles when prospects appear bleak. We propose an instrumental variables approach below.

4. Instrumental variables

The variable R_i is likely correlated with the current and future prospects of the entrepreneurial firm, both omitted from (1). 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 (1) (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 both rate of founder replacement and the quality of replacements recruited.

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. Table A1 in the

appendix shows the most frequent prior employers of the incoming replacement executives in our sample are large, established firms. This is consistent with the "professionalization" notion that new, incoming replacements are not fresh entrepreneurial blood but rather seasoned executives from established firms. Given that these large firms are an attractive source of replacement executives, exogenous changes to interorganizational mobility act as a shock to the supply of replacement executives to VC-backed firms.[11]

In an ideal experiment, the researcher would randomly restrict the ability for the firm or VC to replace a founder of their choice. For example, one could simply impose a no-firing rule or restrict labor mobility in the firm's industry. 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. Our proposed instrument exploits changes in labor laws that in turn may impact the supply of replacement executives. 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.

[11] 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.

4.1. Employee non-compete agreements and executive replacements

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 to 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 are likely candidates to be targeted as replacements for founders (e.g. Table A1 in the data Appendix). Marx, Strumsky and Fleming (2009) provide causal evidence linking the 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 – given fixed demand – alter the VC's opportunity cost of either replacing or retaining the existing founding team. Importantly, 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 occurred well in advance of our sampling period, but several reversals in other states facilitate such an analysis using our data. During the 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.[12] The Idaho law,

[12]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.

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.[13] 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.[14]

Around the same time-frame, 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 successfully 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.[15] 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[16] 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.[17] Over-

[13]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.

[14]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.

[15]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.

[16]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.

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

all, none of these changes in the enforceability of non-compete agreements likely occurred in response to anticipated future change in startup prospects.[18]

Our instrumental variable captures these labor law changes across time and US states in our sample period. Defined in detail below, the variable identifies whether a startup active in a given state experienced a change in non-compete – strengthening or weakening – or had no change in labor laws. For the set of startups in states that weakened non-compete laws, we expect such a change to make it relatively easier to replace founders. Alternatively, those states that strengthened their non-compete rules should exhibit relatively fewer replacements as the supply of possible replacement executives is smaller.

4.2. External validity

Are these states where non-compete laws changed representative of VC-backed firms during our sample period? The eight states with non-compete reforms comprise 18.2% of overall VC investments. Given that nearly 40% of all VC investments are in California, the treated states compose nearly 30% of the remaining startups in the U.S. Moreover, 38.7% of all venture capitalists have portfolio companies in the treated states. Table 6 compares more variables for startups in the treated states to those in all other states in our sample period.

The third column reports the full sample means for each variable. The first observable difference shows up in first capital raised, where firms in treated states raise roughly three quarter million dollars more on average. After all financing rounds, firms in non-treated states have raised roughly 20% more ("Total capital raised (m)"). The second major difference is an under-representation of healthcare companies. Firms in treated states have a lower IPO rate but roughly the same acquisition and failure rates as firms in other states. This difference means that the treated firms in the IV regression start with a lower chance

[18]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. As in other states with policy changes, we do not use startups headquartered in these states after the reversal as controls.

of success as measured by the dependent variable.

A key feature of the data construction for the IV analysis described below is the use of a startup's VC investor's portfolio around the law change. The variable "Portfolio size of VC investor" shows that VCs investing in treated states have 11% smaller portfolios; however, these investors are quite similar in terms of raising from large VCs. The variable "Firm raised from top 10% VC" is one if at least one of a startup's investors are in the top 10% of investing experience by the end of the sample. Treated state startups are five percentage points (6%) less likely to have such an investor. This difference is as expected, but we believe reasonably close. Overall, there do not appear to be large economic differences in observables between firms and investors in the treated states so we believe the IV results generalize to the average VC-backed firm in our sample period.

4.3. Is there home-bias in hiring?

We take as our treatment group startup companies active and VC-backed in the states in the years prior to the passing of these policy changes. For the non-compete reversals above to have affected the ability of startup companies in those states to recruit replacement executives, there must be a material "home bias" in recruitment. In other words, although startups could in theory recruit replacement executives from anywhere in the world, the non-compete reversal should have an effect only if the startups are disproportionately likely to recruit a replacement executive from the same state where they are located.

In order to establish this, it is not sufficient to merely count the proportion of replacement executives that come from the same state, as states have different supplies of potential replacements. We thus proceeded to build a baseline of the percentage of public firms in each state in order to inform the likelihood that a replacement executive would come from the same state as the focal startup. We did this for the IT and Healthcare sectors, for which we could match directly the classification codes from VentureSource to Compustat. IT and Healthcare represent 68% of all VC-backed ventures in our full sample (not just those used

for the IV).

The next step was to compare the same-state replacement rate for IT and Healthcare startups in each state to the percentage of public companies in those states to determine whether there is a "home bias." This required looking up the location of the replacement executives. A research assistant was tasked with finding the prior work location—not simply the company headquarters—of all 1,991 replacement executives. Sources included LinkedIn, ZoomInfo, BusinessWeek, and others. Reliable data was available for 1,373 replacement executives; the remainder were either not locatable or could not be disambiguated between multiple locations. For the replacement executives for whom we could not reliably establish their previous geographical location, the firms that hired them did not differ significantly in terms of year of founding, the amount of capital raised in the first round, or the total rounds of funding.

We find substantial "home bias" in the recruitment of replacement executives. First of all, every state with at least ten replacements (and all but one state with five or more replacements) has evidence of a home bias. Even in California, which as mentioned above is home to 26% of public IT and Healthcare companies, there is a home-bias of more than double as 65% of replacement executives are recruited from within the state. Moreover, we note that each of the states with a non-compete reversal used for our instrument exhibits a home bias from 8.6-15x.[19] Of course, the above does not control for possibly confounding factors, so we next turn to multivariate analysis.

Table 7 analyzes several sources of bias in the selection of replacement executives. For each replacement, we have 50 observations corresponding to U.S. states. The dependent variable is set to one if the replacement for that focal firm's departing founder came from that state. Column (1) formally tests our home-bias hypothesis. The estimate of the coefficient on the startup being in the focal state is positive and statistically significant in all models. The

[19]We repeated this exercise with population instead of the count of public firms, and found even stronger evidence of home-bias (though we think the number of public firms a better proxy for the availability of attractive executives). The above exercise confirms that startups are sensitive to changes in non-compete enforceability when hiring replacement executives.

marginal effect on the probability of the replacement coming from that same state is 7.9%. In column (2) we add additional VC-related controls, including whether the VC is in the same state as the replacement and the (logged) count of the VCs investments in that state. Although both of these are estimated with positive and statistically significant coefficients, their marginal effects are an order of magnitude smaller than home bias.

Column (3) introduces a control for the number of public firms in the same state as the replacement. Doing so requires mapping industry categories from VentureSource to Compustat. The two categories that map directly are Information Technology and Healthcare; hence, the remainder of the table is restricted to these industries. As in the previous column, although there is a positive association between the likelihood of replacement and the presence of public firms from the same industry (at least for IT and Healthcare), the marginal effect is again an order of magnitude smaller than for home bias. Column 4 adds state fixed effects, with consistent results.

4.4. Construction of the treatment and control groups

Within the treated states, we first have to select the set of treated startups. The treatment group for our analysis is the set of all startups headquartered in one of the treated states and that received a round of financing before that state's policy change. It is crucial that we not select firms based on post-law change investments as that could lead to confounds from selection into treatment. The second major condition is that the firm still has all of its founder executives at the firm at the time of the law change (to ensure they have a risk of replacement).[20] 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 include all

[20]The results are robust to removing this condition.

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.

For each treated startup that passed the conditions above, we can identify all their current and past investors. These investors' portfolios help us construct a "control" sample. We take the set of startups (in all other states) invested in *by the same set of VCs who invested in these treated startups* in the years prior to the 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. Simply, any startup that is active and has not had a founder replacement before its state's law change or before the matched state's law change is included in the sample. We thus have an estimator where controls are matched to the closest-in-time treated investment for the same 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. VCs in our sample also typically invest out of a fund or two, of which each selects startups of similar development stage and industry. Controlling for firm founding year, firm state headquarters, law change year, financing stage, total capital raised and round number addresses cross-sectional differences in firm maturity that could impact replacement rates.

Table 8, column (1) presents the results of the equation (1) estimated on the treated and control startups as described above. One difference is that for our IV sample, only replacements after the policy reversal in the relevant state are considered. This table shows a negative and statistically significant association between replacing founders and eventually achieving an attractive liquidity event, consistent with Table 5 above. Thus it does not appear that the IV sample is materially different from the overall sample. As discussed above, this correlation could be downward biased given that investors are more likely to replace founders in struggling companies (and founders are also more likely to resign voluntarily,

necessitating replacement).

4.5. Empirical model

Our instrument for replacement is an indicator for a change in the enforceability of non-compete agreements. If the law changes impact the supply of available executives, then they should predict differential founder replacement after the law change for treated firm compared to control firms (again, taken from the same VC portfolio headquartered in non-treated states but that also took investments prior to the law change in the focal state). Let the variable I_i represent if and how non-compete enforcement changed in the startup's state, taking on values {-1, 0, 1} which correspond to loosening of enforceability, no change, and tightening enforceability (similar to the variable in Garmaise (2011)). 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 reduced form first stage regression that relates replacement to changes in non-compete laws is then:

$$\mathrm{R}_i = \beta_0 + \beta_1 X_i + \gamma_1 I_i + \epsilon_{it}. \tag{2}$$

Again, R_i is whether a founder was replaced after the focal policy reversal. X_i are firm i controls such as capital raised, syndicate size, and profitability. The X_i also include year, round, state, and industry fixed effects. The estimate of γ_1 reveals whether there is a reduced form correlation between changes in non-compete enforceability and founder replacement (R_i). We predict that increased enforcement should lead to relatively fewer replacements ($\gamma_1 < 0$).

The second stage is now:

$$\mathrm{Y}_i = \phi_0 + \phi_1 R_i + \phi_2 X_i + u_{it} \tag{3}$$

where R_i is instrumented from equation (2). The dependent variable Y_i includes the exit

outcome studied in Table 5 and a financing-level variable that measures the speed to a next financing event. The speed to raise a new round of financing strongly correlates with IPO probabilities in the full sample and signals the firm's ability to attract new investors.

Table 8 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 (2) used in the two stage least squares.[21] The estimate of the coefficient on "Increased Enforceability" (i.e. γ_1) is economically and statistically significant, with the predicted negative sign. The weak instruments F-statistics (e.g. Stock and Yogo (2005)) is large at over 40. 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 sign on the IV is also as expected: increased enforcement correlates with a lower probability of replacement.

The second stage estimate in column (3) presents the instrumented coefficient for replacements R_i.[22] 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) and also in Table 5, 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 (4.2% to 17%) implies a 27% 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.[23] The estimates suggest a positive causal effect of founder

[21] 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.

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

[23] 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.

executive replacement in VC-backed firms.

In the remaining columns of Table 8 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 correlates 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 negative but not statistically significant). The first-stage estimates in column (5) shows a positive sign (opposite of our prediction) of the coefficient, while the first stage F-statistic is low. We thus conclude that founder replacement primarily has an impact on exit opportunities of startups.

5. Decomposing the positive impact of replacement on venture outcomes

The results in Table 8 demonstrate a positive causal effect of founder replacement of firm exit outcomes. In this section, we attempt to disentangle how this value is created. Although we saw above a correlation between investor power and replacement (Table 4), we cannot state categorically that all replacements are involuntary. Rather, it may be that many founders relinquish their roles voluntarily but stay on, contributing in a different capacity. The combined human capital of an original founder and "new blood" may represent a net positive for the firm, suggesting that the benefit is more of an augmentation story about bringing in new executives to increase the pool of skills, and with founders making accommodations for those new executives by taking on a new formal role (even if their day-to-day responsibilities change little). Such a story would stand in contrast to the "professionalization" story of Hellmann and Puri (2002). Alternative explanations might also

include that the incoming executives act in large part as "coaches" for the original founders, grooming them on a temporary basis so that they can reassume their former responsibilities. In order to assess the mechanisms at play, we decompose our instrumental-variables analysis along three axes: the replaced founder's role; characteristics of the incoming executive; and whether the replaced founder stays with the firm.

We first exploit variation in the types of founder executives replaced using their titles prior to replacement. Titles include president, CEO, CFO or vice president. As is visible in Table 4, those who hold a CXO role are considerably more likely to be replaced than others. It is plausible that replacing top executives who have more decision-making power at the firm should have relatively bigger benefits to the firm. Columns (1) and (2) of Table 9 consider two alternative indicators for replacement that split the main variable from Table 8: founders with CXO titles and those with titles below this rank.[24] The positive causal effect of replacement is stronger in column (1). Note that although we cannot reject the null that coefficients across columns (1) and (2) are different due to the naturally large standard errors in IV – in fact, they capture different types of replacement – the lack of significance in the non-CXO column is still suggestive of effects residing in top-ranked founder replacement.

Next, we investigate whether the nature of the incoming replacement executive matters. Here we take advantage of data regarding the nature of the replacement executive's prior experience, including whether the replacement had previously founded a startup and also the overall number of years of work experience. We might have hypothesized that prior entrepreneurial experience would be beneficial and generalized experience less so, but in columns (3) and (4) of Table 9 we find that the most successful replacements brought in executives who more years of experience. Incoming executives with more than 15 years of experience—approximately corresponding to the median level of experience among all incoming executives in our sample (not just the IV subsample). Thus it seems that founder replacement improves outcomes by "professionalizing" the young company as suggested by

[24]Our IV estimator uses the first stage predicted probability from a probit following Wooldridge (2010), so we have to run each as a separate regression.

prior work (Hellmann and Puri (2002); Wasserman (2003)).

Third, we compared the effect of replacements where the founder stays as opposed to leaving. To this end, as mentioned above we collected career histories for 1,322 of the 1,999 replaced founders. These new data allow us to conclude whether a replaced founder stayed with the company or, if not, how long after the replacement they departed. As noted above, it could be that that "accommodating" replacements are be more beneficial as the replaced founder's human capital is not lost; rather, the firm's total expertise is augmented by the arrival of the replacement executive. Moreover, following "separating" replacements bitterness may arise among founders if they were forced out, leading them to hire away key employees (who have allegiance to the deposed founder) at a new or existing rival. At the same time, a replaced founder who stays behind may attempt to undermine the authority of the incoming executive. Indeed, the estimates in columns (5) and (6) of Table 9 do not paint a clear picture. Although the estimate of the coefficient on replaced founders who leave is somewhat larger and more precisely estimated, we cannot conclude anything definitive from these analyses. More likely, it may not matter – for the outcome variable that we study – whether the replaced founder stays or leaves.

6. Robustness and identification assumptions

The instrumental variable regression exploits the staggered changes in eight states. One question is how important any one state is to the results presented above. In unreported regressions we repeat the main IV regressions and exclude one of each of the states. These leave-one-out estimates are quantitatively and qualitatively similar. In each specification, the sign flips from the naive to to second stage estimates as in Table 8, while the first stage coefficient estimates are consistent across samples. We are thus confident that one state is not driving the main results. The results in the instrumental variables analysis are also robust to alternative definitions. Modifying the exit outcome dependent variable window

from five years to two, three or four has no statistical impact on results. The relationship between replacement and exit rates is stronger for shorter windows to exit. Similarly, when we allow replacements to occur outside of the two year post-law change window, the results are unchanged in Table 8.

Regarding the exclusion restriction, one might be concerned that shifts in the enforceability of non-compete agreements might affect venture outcomes. The literature on non-competes does not address this possibility empirically, instead focusing on how such contracts affect the *founding* of new firms (see Stuart and Sorenson (2003); Samila and Sorenson (2011); Starr, Balasubramanian and Sakakibara (2015)). Still, it is possible that non-compete agreements might affect venture outcomes either by affecting the market for talent, as described above, or for startups themselves (i.e., the acquisition market).

To the latter point, Younge, Tong and Fleming (2015) show evidence that enforceable non-competes fuel the acquisition market for startups—presumably because it is easier to retain employees post-acquisition. If true, this would work against our finding. To the former point, the effect of non-competes on hiring by startups is ambiguous. If non-competes are unenforceable then startups can more easily hire workers away from established companies. But by the same token, it is easier for established companies to poach talent from startups if they cannot use non-competes.

Absent such evidence, we investigated the relationship between labor market fluidity and the success of venture-backed startup companies. We first check whether changes in non-compete enforceability correlate with overall performance of VC-backed firms, not just those in our IV sample. In Appendix Table A4 we regress state-level liquidity outcomes for venture-backed startups on the law change variable in our specification. Negative binomial analysis of the counts of IPOs and acquisitions given the number of active startups in the previous year, we find that performance correlates negatively with non-compete enforcement, but the coefficients are statistically insignificant and the implied marginal effects are small.

Next we check whether non-competes facilitate venture outcomes by enabling hiring

growth (again, the effect is theoretically ambiguous). We investigate this question by merging the National Establishment Time-Series (NETS) dataset, which contains employment data from Dun & Bradstreet, to firms in the sample (match rate of 62%). Table A5 in the Appendix resembles that of our above IV except that the endogenous variable is not found replacement but an increase in overall employment in the startup. Although we see the expected positive correlation between increased employment and startup success in a naive regression, the first-stage using increase/decrease in the enforceability of non-competes is weak and with a small F-statistic. The weakness of this result is compatible with our claim that the effect of non-competes on venture outcomes is primarily through executive-level replacement as opposed to hiring more broadly. Although we cannot prove the exclusion restriction, we would have expected Table A5 to have more power if this alternative labor-market fluidity channel were responsible for our claimed results.

7. Discussion and Conclusion

This paper draws a causal link between founder replacement and startup performance. Using data on VC-backed startups in the U.S. from 1995–2008, we show that although it may appear that replacing founders hurts startup performance, this is due to selection. We introduce exogenous variation in the ability of investors to find qualified replacements by exploiting changes in the enforcement of employee non-compete agreements in eight states. Non-competes make it more difficult to hire talent, especially among the sort of established-company executives who would be attractive replacements for founders. Instrumenting our regressions reverses the result of the naive regression, indicating that founder replacement boosts the performance of startups.

We find moreover that the most consequential replacements are of founders who hold CXO-level titles. Further, replacement is most helpful when the incoming executive has substantial work experience. We also find that replacement is more common after a round

of financing and when investors have more seats on the board. Taken together, these results paint a picture of activist investors "professionalizing" the nascent firm.

We build most directly on four papers that explore founder replacement, three using detailed survey data from samples of 50-200 firms (Hellmann and Puri (2002); Wasserman (2003); Kaplan, Sensoy and Strömberg (2009)) and one using register data from Denmark (Chen and Thompson (2015)). Our results particularly echo Hellmann and Puri (2002)'s notion that VCs "professionalize" their portfolio companies, adding a causal link.

Our results complement those of Bernstein, Giroud and Townsend (2016) and Chemmanur, Krishnan and Nancy (2011), who have sought causal evidence regarding whether investors provide "more than money" by monitoring the progress of their portfolio firms. Although Gorman and Sahlman (1986) reveal areas in which investors spend time, we do not know which of these activities create real value. This study shows that the replacement of executives by investors is a key mechanism by which investors improve the performance of their portfolio companies.

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 (2015); 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.

Finally, our results speak to the tension between maintaining a founder-friendly reputa-

tion and optimizing for the performance of the current portfolio. Entrepreneurs care about their expected financial return but also about keeping their jobs. Investors' aggressive replacement of founders may optimize the performance of the current portfolio, as our results suggest. But developing a reputation as having little patience with founders could also 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. Future work is required to explore this tension.

References

Amornsiripanitch, Natee, Paul Gompers, and Yuhai Xuan. 2015. "More than Money: Venture Capitalists on Boards." *Working paper*.

Beckman, Christine M. 2006. "The influence of founding team company affiliations on firm behavior." *Academy of Management Journal*, 49(4): 741–758.

Bernstein, Shai, Xavier Giroud, and Robert R. Townsend. 2016. "The Impact of Venture Capital Monitoring." *Journal of Finance*, 71: 1591–1622.

Bottazzi, Lauren, Marco Da Rin, and Thomas Hellmann. 2008. "Who are the active investors? Evidence from venture capital." *Journal of Financial Economics*, 89(2008): 488–512.

Burton, M Diane. 1995. *The emergence and evolution of employment systems in high-technology firms*. Stanford University.

Chemmanur, Thomas, Karthik Krishnan, and Debarshi Nancy. 2011. "How does venture capital financing improve efficiency in private firms? A look beneath the surface." *Review of Financial Studies*, 2011.

Chen, Jing, and Peter Thompson. 2015. "New Firm Performance and the Replacement of FounderCEOs." *Strategic Entrepreneurship Journal*, 9: 243–262.

Ewens, Michael, and Christian Fons-Rosen. 2015. "Innovation and Experimentation in the Entrepreneurial Firm." *Working Paper*.

Ewens, Michael, and Matthew Rhodes-Kropf. 2015. "Is a VC Partnership Greater than the Sum of its Partners?" *The Journal of Finance*, 70(3): 1081–1113.

Garmaise, Mark J. 2011. "Ties that Truly Bind: Noncompetition Agreements, Executive Compensation, and Firm Investment." *Journal of Law, Economics & Organization*, 27(2).

Gompers, Paul, Anna Kovner, Josh Lerner, and David Scharfstein. 2010. "Performance persistence in entrepreneurship." *Journal of Financial Economics*, 96(1): 18–32.

Gorman, M., and W. A. Sahlman. 1986. "What do Venture Capitalists Do?" *Journal of Business Venturing*, 4: 231–248.

Hall, Robert E, and Susan E Woodward. 2010. "The Burden of the Nondiversifiable Risk of Entrepreneurship." *The American Economic Review*, 100(3): 1163–1194.

Hellmann, Thomas, and Manju Puri. 2002. "Venture Capital and the Professionalization of Start-Up Firms: Empirical Evidence." *The Journal of Finance*, 57(1): 169–197.

Hsu, David H. 2006. "Venture capitalists and cooperative start-up commercialization strategy." *Management Science*, 52(2): 204–219.

Kaplan, Steven N, and Per Strömberg. 2003. "Financial contracting theory meets the real world: An empirical analysis of venture capital contracts." *The Review of Economic Studies*, 70(2): 281–315.

Kaplan, Steven N, Berk A Sensoy, and Per Strömberg. 2009. "Should investors bet on the jockey or the horse? Evidence from the evolution of firms from early business plans to public companies." *The Journal of Finance*, 64(1): 75–115.

Korteweg, Arthur, and Morten Sorensen. 2010. "Risk and Return Characteristics of Venture Capital-Backed Entrepreneurial Companies." *Review of Financial Studies*, 23(10): 3738–3772.

Lerner, Josh. 1995. "Venture capitalists and the oversight of private firms." *The Journal of Finance*, 50(1): 301–318.

Marx, Matt. 2011. "The firm strikes back non-compete agreements and the mobility of technical professionals." *American Sociological Review*, 76(5): 695–712.

Marx, Matt, Deborah Strumsky, and Lee Fleming. 2009. "Mobility, skills, and the Michigan non-compete experiment." *Management Science*, 55(6): 875–889.

Puri, Manju, and Rebecca Zarutskie. 2012. "On the Life Cycle Dynamics of Venture-Capital-and Non-Venture-Capital-Financed Firms." *The Journal of Finance*, 67(6): 2247–2293.

Samila, Sampsa, and Olav Sorenson. 2011. "Noncompete covenants: Incentives to innovate or impediments to growth." *Management Science*, 57(3): 425–438.

Sorensen, Morten. 2007. "How smart is smart money? A two-sided matching model of Venture Capital." *The Journal of Finance*, 62(6): 2725–2762.

Starr, Evan P, Natarajan Balasubramanian, and Mariko Sakakibara. 2015. "Screening Spinouts? How Noncompete Enforceability Affects the Creation, Growth, and Survival of New Firms." *US Census Bureau Center for Economic Studies Paper No. CES-WP-14-27.*

Stock, James H, and Motohiro Yogo. 2005. "Testing for weak instruments in linear IV regression." *Identification and inference for econometric models: Essays in honor of Thomas Rothenberg.*

Stuart, Toby E, and Olav Sorenson. 2003. "Liquidity events and the geographic distribution of entrepreneurial activity." *Administrative Science Quarterly*, 48(2): 175–201.

Wasserman, Noam. 2003. "Founder-CEO succession and the paradox of entrepreneurial success." *Organization Science*, 14(2): 149–172.

Weisbach, Michael S. 1988. "Outside directors and CEO turnover." *Journal of Financial Economics*, 20: 431–460.

Wooldridge, Jeffrey M. 2010. *Econometric analysis of cross section and panel data.* MIT press.

Younge, Kenneth A, Tony W Tong, and Lee Fleming. 2015. "How anticipated employee mobility affects acquisition likelihood: Evidence from a natural experiment." *Strategic Management Journal*, 36(5): 686–708.

Tables and figures

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).
IPO / Acq.	A dummy variable equal to one if the entrepreneurial firm exited via an IPO or an acquisition where the latter had a valuation at least 125% of total capital raised.
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.
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 .
CXO	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.
Increased Enforcement	An indicator for whether a startup was active in a state that had no change (0) in non-compete enforceability, a decrease (-1) or an increase (1).
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.

	Firm characteristics							
	mean	sd	min	p25	p50	p75	max	count
Acquired	0.40	0.49	0	0	0	1	1	11929
Went public	0.060	0.24	0	0	0	0	1	11929
Out of Business	0.27	0.44	0	0	0	1	1	11929
Still private	0.28	0.45	0	0	0	1	1	11929
First capital raised	6.47	20.1	0.0100	1.50	3.50	6.79	1500	11929
Year firm founded	2001.0	3.82	1995	1998	2000	2004	2008	11929
Information Technology	0.54	0.50	0	0	1	1	1	11929
Biotech	0.18	0.39	0	0	0	0	1	11929
California HQ	0.41	0.49	0	0	0	1	1	11929
Texas HQ	0.053	0.22	0	0	0	0	1	11929
New York HQ	0.066	0.25	0	0	0	0	1	11929
Total equity financings (all)	3.48	2.35	1	2	3	5	24	11929
Total capital raised (m)	36.8	122.4	0	5.30	15.9	40.1	10328.6	11929
Year first VC	2002.5	4.29	1995	1999	2001	2006	2014	11929
Founder replaced?	0.15	0.36	0	0	0	0	1	11929

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	11,929	10,145	7444	5120	3301	2046
Startups achieving liquidity this round	860	941	785	598	404	499
Startups failing this round	1510	1257	839	531	321	421
Startups with founder replaced this round	429	599	400	229	121	131
Startups with founder replaced so far	429	1008	1379	1586	1697	1811
% Startups with founder replaced this round	3.6%	5.9%	5.3%	4.5%	3.7%	6.4%
% Startups with founder replaced so far	3.6%	8.4%	11.5%	13.2%	14.2%	15.1%

Table 4: Correlates of founder replacement

Notes: The table analyzes the hazard of founder replacement. Observations are firm-founder-quarter triads, with failure indicated by the founder being replaced in that quarter. Variables are as defined in Table 1. Columns 2-4 investigate individual industries. Standard errors reported in parentheses, clustered at the entrepreneurial firm. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Founder replaced?				
	(1)	(2)	(3)	(4)	(5)
New financing round this quarter	0.265***	0.424***	0.316***	0.172**	0.264***
	(0.0519)	(0.126)	(0.114)	(0.0678)	(0.0519)
Profitable at prior financing	0.0604	0.184	-0.404	0.116	0.0872
	(0.109)	(0.182)	(0.342)	(0.156)	(0.109)
Log capital stock at prior financing	0.153***	0.206***	0.104*	0.167***	0.151***
	(0.0254)	(0.0587)	(0.0595)	(0.0337)	(0.0255)
Size of VC board	0.114***	0.169***	0.0567	0.115***	0.112***
	(0.0192)	(0.0467)	(0.0459)	(0.0253)	(0.0194)
Founder held CXO role	0.264***	0.427***	0.355***	0.174***	0.266***
	(0.0534)	(0.130)	(0.131)	(0.0669)	(0.0533)
Solo founder	0.0814	0.0449	-0.0477	0.202***	0.0985*
	(0.0526)	(0.119)	(0.117)	(0.0706)	(0.0535)
Observations	411266	91715	79738	225869	411266
Log likelihood	-17067.4	-2542.6	-2570.7	-9643.8	-17061.3
Number of startups	11817	2941	2164	6334	11817
Industries	All	Consumer	Healthcare	IT	All
Industry FE	N	N	N	N	Y

Table 5: Full sample exit outcomes and founder replacement

Notes: Table reports OLS regressions of firm-level outcomes on indicators for whether the startup had one of two types of founder replacement. The unit of observation is an entrepreneurial firm. "Founder replaced?" is equal to one if at least one of the founding team members was observed replaced before the exit or end of the sample. "Founder-CXO replaced?" is one if one of those replaced had the CXO title (e.g. CIO, CFO or CEO). The dependent variable in columns (1)-(2) is a dummy equal to one if the firm had an IPO or acquisition with a valuation greater than two times capital invested by the end of the sample period (2014). The last two columns report the log of exit valuation – 25% of capital invested if failure – if known. Fixed effects include the state of firm headquarters, founding year, industry, the interaction of industry and founding year and and indicators for the initial size of the founding team. Unreported is a control for the log of first capital invested. Standard errors clustered at the founding year reported in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Quality exit?		Log exit value	
	(1)	(2)	(3)	(4)
Founder replaced?	-0.0501**		-0.265***	
	(0.0187)		(0.0542)	
Founder-CXO replaced?		-0.0591***		-0.286***
		(0.0174)		(0.0814)
Constant	0.306***	0.305***	3.211***	3.202***
	(0.0468)	(0.0466)	(0.447)	(0.443)
Observations	11401	11401	2757	2757
R^2	0.0642	0.0645	0.0772	0.0769
State FE	Y	Y	Y	Y
Founding year FE	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y
IndustryXFounding Year FE	Y	Y	Y	Y
Team size FE	Y	Y	Y	Y

Table 6: Comparison of VC activity in IV vs. non-IV states

Notes: The table reports entrepreneurial firm and their investor observables for two samples. The first column ("Never treated") reports means of each variable for the states that did not have a non-compete law change in our sample period. The second column ("Treated state") reports the same means for the states with such law changes. The last column ("Total") reports the full sample means. The variable "Founding year" is the year of firm founding and "Year first VC" is the year the firm first raised VC. "First capital raised (m)" is the capital raised in this first round, in millions. "Total financings at exit" counts the total equity and debt financings raised by the startup. "Total capital raised (m)" reports the sum of all equity and debt raised. "Information Tech." and "Healthcare" are indicators for startup industry. "Went public," "Acquired," "Still private" and "Failed" are dummy variables for firm exit type. "Portfolio size of VC investor" is the count of number of unique entrepreneurial firm investments made by the startup's investors as of the firm's exit. The variable proxies for experience. The variable "Firm raised from top 10% VC" is one if at least one of the entrepreneurial firm's investors was in the top 10% of this portfolio size variable.

	Never treated	Treated state	Total
Founding year	2001.0	2000.8	2001.0
Year first VC	2002.4	2002.5	2002.4
First capital raised (m)	6.765	7.358	6.863
Total financings at exit	3.993	3.792	3.960
Total capital raised (m)	46.22	40.78	45.32
Information Tech.	0.521	0.507	0.519
Healthcare	0.212	0.130	0.198
Went public	0.0694	0.0473	0.0658
Acquired	0.407	0.393	0.405
Still private	0.287	0.306	0.290
Firm failed	0.235	0.251	0.238
Portfolio size of VC investor	95.95	86.30	94.37
Firm raised from top 10% VC	0.864	0.815	0.856

Table 7: Geographic bias in recruitment of replacements for founders

Notes: The table analyzes possible sources of geographic bias in the recruitment of replacements for founders. Each observation is a replacement-state dyad, 50 observations per founder replacement. The dependent variable is 1 if the replacement for the founder at a given startup was recruited from that state. Columns 1-3 are estimated on the full sample of founder replacements; the sample in column (4) is estimated on founder replacements at IT and Healthcare startups because direct matches to Compustat counts of public firms were available only for those two categories. Robust standard errors reported in parentheses, clustered at the entrepreneurial firm. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Replacement executive from state?			
	(1)	(2)	(3)	(4)
Startup in focal state?	2.436***	1.689***	1.630***	1.578***
	(0.0439)	(0.0547)	(0.0624)	(0.0640)
Any of startup's VCs in focal state?		0.572***	0.365***	0.288***
		(0.0419)	(0.0502)	(0.0511)
Log first capital raised		0.0616***	0.0303**	0.0220*
		(0.0126)	(0.0122)	(0.0118)
Total financings		-0.0151***	-0.00697*	-0.00340
		(0.00428)	(0.00419)	(0.00400)
Log total capital raised		-0.0722***	-0.0627***	-0.0520***
		(0.0124)	(0.0118)	(0.0111)
Log # investments of startup's VCs in focal state		0.147***	0.0545***	0.0190***
		(0.00648)	(0.00625)	(0.00593)
Log # public firms in industry in focal state			0.271***	-0.514***
			(0.0184)	(0.122)
Constant	-2.357***	-2.707***	-3.844***	-4.010***
	(0.0533)	(0.0929)	(0.127)	(0.404)
Observations	74850	73300	57850	43966
Pseudo-R^2	0.338	0.405	0.442	0.432
Number startups	1373	1345	1054	1054
Industries	All	All	IT, Healthcare	IT, Healthcare
Founding year FE	Y	Y	Y	Y
Year 1st fin. FE	Y	Y	Y	Y
Industry FE	Y	Y	Y	Y
Replacement state FE	N	N	N	Y

Table 8: 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 where there still remain active founders on the executive team. The sample of entrepreneurial firms is described in Section 4. 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. "Increased Enforceability" is a dummy variable representing whether the state in which a focal startup is located changed its non-compete laws; values of 1, 0, and -1 correspond to an increase in enforceability, no change, and a decrease in enforceability, respectively. Other controls are as defined in Tables 1 and 4. Column (2) reports the first stage probit estimates where the replacement dummy is instrumented by the interaction term "Increased Enforceability" given the policy change (if any) in that startup's state. "1st. stage F" is the first-stage F-statistic for weak instruments. Column (3) reports the two-stage least squares 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	-0.0447*		0.319**	-0.104		-1.502
	(0.0250)		(0.144)	(0.115)		(1.022)
Increased Enforceability		-3.240***			0.197*	
		(0.485)			(0.103)	
Log capital stock	0.0306***	0.173***	0.0221***	0.135***	0.145***	0.171***
	(0.00693)	(0.0295)	(0.00606)	(0.0419)	(0.0304)	(0.0414)
Syndicate size	0.00910***	0.0210*	0.00680**	-0.0224	0.0287**	-0.0100
	(0.00276)	(0.0125)	(0.00300)	(0.0258)	(0.0145)	(0.0201)
Profitable at financing	0.00619	0.0680	0.00339	0.398***	0.0880	0.420***
	(0.0108)	(0.0615)	(0.0109)	(0.127)	(0.0630)	(0.0738)
Constant	0.0941***	-5.896***	0.135	1.380***	-5.620***	1.253**
	(0.0255)	(0.772)	(0.138)	(0.157)	(0.155)	(0.596)
Observations	4610	4610	4610	3488	3465	3465
R^2	0.0814	0.127	.	0.205	0.0868	.
1st. stage F		44.64			3.660	
Year FE?	Y	Y	Y	Y	Y	Y
Founding year FE?	Y	Y	Y	Y	Y	Y
Round # FE?	Y	Y	Y	Y	Y	Y
Industry FE?	Y	Y	Y	Y	Y	Y
Law change year FE?	Y	Y	Y	Y	Y	Y
State FE?	Y	Y	Y	Y	Y	Y

Table 9: Differences in the effects of replacement

Notes: Table reports several additional specifications of the instrumental variables model in Table 8. In columns (1) and (2) we explore whether it matters that the replaced founder had a CXO title. The main replacement variable "Founder-CXO replaced" variable in column (1) is a dummy if a replacement occurs and the replaced founder had a title of CXO (e.g. CFO or CEO). Column (2) include an indicator for whether there was a replacement and the replaced founder had a title below CXO. Columns (5) and (4) compare replacements where the new executive has more than 15 years of experience. All controls are as defined in Table 8. Columns (5) and (6) similarly compare replacements, here distinguished by whether the replaced founder stays at the firm or leaves. The main variable in column (5) is set to one only for founders who were replaced and stayed with the firm. Robust standard errors in parentheses. significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	(1)	(2)	(3)	(4)	(5)	(6)
	Founder type		Incoming type		Separating vs. Accommodating	
Founder-CXO replaced	0.487**					
	(0.239)					
Non-CXO founder replaced		0.184				
		(0.237)				
Replacer has >15yrs exp.			1.105***			
			(0.394)			
Replacer has <15yrs exp.				-0.100		
				(0.185)		
Founder replaced and left					0.379	
					(0.246)	
Founder replaced and stayed						0.177
						(0.312)
Log capital stock	0.0240***	0.0270***	0.0234***	0.0313***	0.0243***	0.0286***
	(0.00610)	(0.00598)	(0.00690)	(0.00543)	(0.00559)	(0.00586)
Syndicate size	0.00557	0.00865***	0.00476	0.00878***	0.00852***	0.00922***
	(0.00341)	(0.00290)	(0.00410)	(0.00286)	(0.00303)	(0.00300)
Profitable at financing	0.00356	0.00462	0.00183	0.00652	0.00397	0.00539
	(0.0116)	(0.0113)	(0.0138)	(0.0107)	(0.0112)	(0.0113)
Constant	0.130	0.0900	0.141	0.111	0.150	0.127
	(0.145)	(0.134)	(0.170)	(0.133)	(0.139)	(0.136)
Observations	4524	4054	3951	4297	4270	4185