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How Does Human Capital Matter? Evidence from Venture Capital

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Abstract

We examine the effect of labor mobility on venture capital (VC) investment. Following the staggered adoption of the inevitable disclosure doctrine that restricts labor mobility, VCs are less likely to invest in affected states. This effect is more pronounced when human capital is more important to startups, when VC investment is more uncertain, and when VCs' monitoring costs are higher. The reduced innovation productivity of employees is a plausible underlying mechanism. To mitigate this adverse effect, VCs stage finance startups more and syndicate more with other VCs. Our paper sheds new light on the real effects of labor market frictions.

I. Introduction

How does the human capital of startups affect the investment propensity and outcomes of venture capital (VC) firms? As emphasized in Zingales (2000), "human capital is emerging as the most crucial asset" in today's world. Human capital is crucial for a firm because employees possess intimate knowledge of the firm's operations, of its trade secrets, and, in particular, of its innovation. This knowledge is lost when employees leave a firm (Hall and Lerner (2010)). Therefore, it is important to understand how the human capital of startups affects VC investment.

VCs have been an essential ingredient of the private market and have contributed significantly to the rapid development of US economic growth, entrepreneurship, and technological innovation in the past several decades.[1] Although numerous studies have explored how a variety of VC investors' characteristics (e.g., industry expertise, reputation, experience,

[1] For example, since 1999 around 60% of IPOs have been backed by VCs.

network connections, etc.) affect their investment in startups, little attention has been given to how the human capital embedded in startups affects VC investment. Studies by Hellmann and Puri (2000) and by Chemmanur, Simonyan, and Tehranian (2016) have explored other aspects of startups' human capital. In their study of 170 high-tech startups in Silicon Valley, Hellmann and Puri (2000) find that VCs help professionalize startup management teams. Chemmanur et al. (2016) show that VC financing is associated with higher-quality startup management teams. These papers, however, do not explore how VC investment is influenced by startup human capital or, in particular, by the mobility of human capital. In this paper, we fill this gap in the existing literature and explore how labor mobility affects VC investment.

A major challenge for our empirical analyses is the likely endogeneity of labor mobility to VC investment. Thus, a correlation between labor mobility and VC investment may tell us little about the causal effect of labor mobility on VC investment. We alleviate endogeneity concerns by exploiting the staggered adoption of the inevitable disclosure doctrine (IDD) by US state courts; this doctrine prevents a firm's employees who have knowledge of the firm's trade secrets from working for another firm, and hence creates plausibly exogenous variations in the mobility of a startup's labor. IDD is a stronger restriction than non-compete or non-disclosure agreements because it can be applied to a broader geographical area and is applicable even in cases where the employee has not signed a non-compete or nondisclosure agreement with the firm. In addition, the variation generated by the IDD represents multiple shocks that affect labor mobility (and hence startups) in different states at different times. We thus avoid a common identification difficulty of studies that use a single shock—namely, the existence of potential omitted variables coinciding with a shock that directly affects VC investment.[2]

We propose two competing hypotheses regarding the effect of labor mobility restrictions on VC investment and outcomes. Our first hypothesis, the talent retention hypothesis, argues that restricting the labor mobility of startups encourages VC investment because the restriction retains talent and thus makes startups more competitive and more attractive to VC investors. Our second hypothesis, the talent distortion hypothesis, predicts that labor mobility restrictions could impede VC investment for two reasons. First, reduced labor mobility makes it more difficult for startups to recruit the necessary talent from outside. Second, labor mobility restrictions reduce existing employees' incentives to work hard (Fulghieri and Sevilir (2011)). Detailed discussions of hypothesis development are in Section III.

We first examine directly whether the adoption of the IDD indeed reduces labor mobility. We focus on the mobility of startups' key employees, i.e., inventors. Inventors are the producers of a startup's innovation output, which is crucial to the startup's survival and growth. Given that a substantial fraction of patentable inventions are not patented and remain trade secrets (Mansfield (1986), Anton and Yao (2004)), inventors may possess important trade secrets of the startups and are thereby restricted from moving because of the IDD. We find that, following IDD adoption, the mobility of inventors is significantly reduced both within a state and across states.

We then undertake our baseline regressions that examine the effect of the IDD on VC investment propensity. Our baseline results show that, following adoption of the IDD, there is a 2% unconditional drop in the likelihood of VC investment; this represents 15% of the average

[2] There is a fair amount of VC investment in states that have adopted the IDD. In our sample period from 1980 through 2012, around 40% of VC investment took place in states that eventually passed the IDD.

investment likelihood.[3] The evidence is consistent with the talent distortion hypothesis. To ensure that the parallel trends assumption of the difference-in-differences approach is satisfied, we follow Bertrand and Mullainathan (2003) and examine the dynamics of VC investments surrounding the adoption and rejection of the IDD. We find no prior trend in VC investment in the pre-IDD era and the results become significant only after IDD adoptions.

To further strengthen the causal link, we next examine how our baseline results vary in the cross section. If labor mobility restrictions reduce the likelihood of VC investment through increased talent distortions, we expect to observe that our baseline results are more pronounced in more innovative industries in which human capital is particularly important, in early-stage VC investment when investment uncertainty is especially high, and in “long-distance” investment in which VC monitoring costs are particularly high. These are indeed what we find. Our cross-sectional tests lend further credence to the negative, causal effect of labor mobility on VC investment likelihood and support the talent distortion hypothesis.

We then explore plausible mechanisms through which the IDD affects the likelihood of VC investment. We conjecture that the reduction in employees’ innovation productivity might be a plausible underlying mechanism. Consistent with our view, we find a reduction in patent counts and in patent citations at both the inventor-year level and the startup-year level following IDD adoptions. To explain the reduction in patenting activities, we explore two plausible reasons. We term these the incentive channel and the knowledge spillover channel. Under the incentive channel, inventors are incentivized to produce innovation output to signal their quality to the external labor market; this incentive is reduced by the passage of the IDD, which restricts

[3] The unconditional mean of the investment probability is 13%. Therefore, the drop in investment probability that is conditional on the mean is 15% (2%/13%=15%).

inventors' mobility to rival companies. If the incentive channel is supported, the productivity of inventors in industries with higher labor mobility should be hurt more by the passage of the IDD; we find evidence consistent with this conjecture. Under the knowledge spillover channel, the IDD reduces patenting activities because it restricts the idea recombination that comes from the moves of inventors. If the knowledge spillover channel is supported, we should observe that the effect of the IDD is more pronounced in industries with higher knowledge spillovers. We, however, do not find consistent evidence that supports the knowledge spillover channel.

Next, we examine whether VCs are able to counter the perceived adverse effects of restricted labor mobility on startups by adjusting their investment strategies.[4] We focus on two important VC investment strategies: staging and syndication. Staging refers to the stepwise capital infusions from VCs to startups and has been well documented as an effective way to mitigate agency problems (Gompers (1995), Tian (2011)). According to the real options theory, VCs stage their financing of startups to reduce investment uncertainty (Sahlman (1988), (1990)); this is an effective tool for mitigating agency problems and for keeping entrepreneurs on a tight leash, which helps overcome talent distortions (Sahlman (1990), Gompers (1995)). Syndication refers to the cooperation among VCs when they invest in startups and is an enduring and distinct feature of the VC industry (Lerner (1994), Tian (2012), Gompers, Mukharlyamov, and Xuan (2016), and Tian, Udell, and Yu (2016)). Syndication allows VCs to seek a second opinion from other VCs about the startups, to peer monitor, and to share the risk—especially that of possible talent distortions associated with startups—with other VCs (e.g., Lerner (1994), Brander, Amit, and Antweiler (2002), and Bayar et al. (2020)). Therefore, if the restriction of labor mobility

[4] Unlike the analysis of investment likelihood, which focuses on the extensive margin, we focus on the intensive margin in this analysis.

leads to talent distortions, VCs could mitigate the adverse effect by intensifying staging and engaging more in syndication. Consistent with our conjecture, we find that VCs increase their staging efforts and are more likely to co-invest with other VCs in startups following the adoption of the IDD.

Given that VCs adjust their staging and syndication strategies in response to the IDD, it is interesting to examine a "bottom line" question, i.e., whether VCs are able to reduce the adverse effects of labor mobility restrictions. We investigate the question by looking into VCs' investment outcomes. Following the literature, we measure VCs' successful investment outcomes by IPOs and by acquisitions of VC-backed startups (see Da Rin, Hellman, and Puri (2013) for a discussion). We find the restriction of labor mobility leads to fewer successful exits, especially in terms of IPOs. This finding suggests that, while VCs increase both stage financing and syndication with other VCs, they still cannot completely eliminate the negative effect of restricted labor mobility. Consistently, we find that VCs tilt their portfolios to states without the IDD.

Finally, it is worth noting that our sample includes only startups that have received funding from at least one VC investor. Our analysis does not include startups that have never received funding from any VCs. Therefore, our study examines whether the IDD affects investment decisions in all "infra-marginal" startups, excluding any effect that the IDD may have on the marginal startups. It is possible that the IDD reduces entrepreneurship. We show that startups are less likely to receive subsequent-round financing from VCs after the passage of the IDD. We further show that VCs shift their investment away from states that passed the IDD; this could lead to a reduction in entrepreneurship. These results are consistent with the talent distortion hypothesis.

The rest of the paper proceeds as follows: Section II reviews the literature. Section III describes the institutional background of the IDD and develops the hypotheses. Section IV reports data and summary statistics. Section V presents our main empirical results. Section VI concludes.

II. Literature Review

Our paper contributes mainly to three strands of the literature. First, it contributes to the literature on VC investment. Prior research has studied how a variety of VC investor characteristics—such as industry expertise, reputation, past experience, and network connections—as well as startup characteristics affect VCs' investment in startups and eventually affect the public market. (See Da Rin et al. (2013) for a survey of the literature.) Regarding the human capital of startups, Hellmann and Puri (2000), in their study of 170 high-tech startups in Silicon Valley, find that VCs help professionalize startup management teams. Chemmanur et al. (2016) find that VC financing is associated with higher-quality startup management teams. Amornsiripanitch, Gompers, and Xuan (2019) find that successful and well-connected venture capitalists play an important role in recruiting managers and outside board members for portfolio companies; this finding is consistent with that of Ewens and Marx (2018) who provide evidence that VCs improve the performance of their portfolio firms by replacing founders. Gompers et al. (2016) show that VCs who share the same ethnic, education, or career background are more likely to syndicate with each other, but this kind of collaboration tends to perform poorly. Our study complements the existing literature by exploring how labor mobility affects VC investment likelihood, investment strategy, and investment outcomes.

Second, this paper is related to the studies of non-compete agreements and labor mobility in general. For example, Garmaise (2011) shows that increased non-compete agreements lead to executive stability and reduction in firm investment per employee. Samila and Sorenson (2011) document the enforcement of non-compete agreements as impeding entrepreneurship and reducing patenting activities. Using Michigan data and the auto-manufacturing-industry setting, Marx, Strumsky, and Fleming (2009) present direct evidence that non-compete agreements lead to a reduction in labor mobility. Jeffers (2019) shows that greater enforceability of non-compete agreements leads to a substantial decline in employee departures, especially in knowledge-intensive occupations, and reduces new firm entry in corresponding sectors. Given that an IDD is applicable regardless of whether an employee signs a non-compete agreement, our finding that there is a lower likelihood for startups to receive VC funding following adoption of the IDD is generally consistent with that of Jeffers (2019). We take a further step by showing the response of VCs to the IDD in adjusting their investment strategy and the outcomes of VC-backed startups in terms of successful exits.

Third, this paper is related to recent studies that explore the relation between labor mobility and economic dynamism. Klasa et al. (2018) show that firms increase their financial leverage following a state's adoption of the IDD. Chen, Gao, and Ma (forthcoming) find that, when labor mobility is restricted, US firms are more likely to be acquired. Contigiani, Hsu, and Barankay (2018) show that strengthening employer-friendly trade secret protection adversely affects innovation. Ouimet and Zarutskie (2016) find that some mergers and acquisitions (M&As) are motivated by acquirers' incentives to acquire and retain the key talents of target firms. Qiu and Wang (2018) show that the adoption of the IDD leads to positive stock market reactions. By using VC investment amounts aggregated at the state level, Castellaneta et al.

(2016) find that the IDD affects VC financing. Our paper contributes to this literature by showing the economic consequences of restricting labor mobility in the VC setting.

It is worth noting that our results are different from those of Castellaneta et al. (2016) because we take startups' demand for VCs into consideration by creating hypothetical VC-startup pairs. While overall VC investment could increase after the passage of the IDD as shown in Castellaneta et al. (2016), the probability of each startup receiving VC funding could decrease if there is an increase in the number of startups demanding VC funding.[5] Our study contributes to the literature by exploring the underlying mechanism of the reduction in the matching rate between VCs and startups. We present evidence of VCs adjusting their investment strategies and of the drop in startups' innovation productivity due to muted incentives for inventors to signal their capability to the external labor market.

III. Institutional Background and Hypothesis Development

A. Institutional Background

The IDD was first recognized by the state of New York in 1919 to protect trade secrets. In the original New York state court ruling, a trade secret is defined as any business information that can generate economic value if disclosed or used by the companies' employees. The court also ruled that a trade secret is subject to reasonable protections by the company as a business secret. The recognition of the IDD by state courts reinforces the protection of trade secrets for firms located in those states. According to the IDD, a firm can file a lawsuit against another firm that has hired a former employee of the first firm if the first firm can provide evidence that (1)

[5] As a matter of fact, the results of Castellaneta et al. (2016) are consistent with the above argument, as they show that the amount of VC investment scaled by VC-backed startups does not significantly increase following the passage of the IDD.

the employee had access to its trade secrets, (2) the employee's duties in the new employment would inevitably require her to disclose or use those trade secrets, and (3) the disclosure or use of the trade secrets would cause irreparable economic harm to the suing firm. Furthermore, the IDD protects the firm's trade secrets even if the employee is hired by a firm that is located in a state that has not adopted the IDD. The IDD maintains that if the new employment would inevitably lead to the disclosure of the trade secret to competitors and would cause irreparable harm to the suing firm, a state court can prevent the employee from moving to the competitor or can limit her responsibility in the new job.[6]

The IDD reduces the risk that an employee will disclose a business secret to a competitor or take advantage of her knowledge of trade secrets to start a new company in a similar industry. Before an employee decides to move to a new company or to start her own company, she must consider whether she will be breaking any regulations related to the IDD. As a result, an employee has less incentive to switch jobs if doing so could lead to a lengthy lawsuit filed by a prior employer operating in a state that has adopted the IDD.

For our analysis, we start with all court rulings on the IDD. If a state court ruled in favor of the IDD, we categorize this state as one that has adopted the IDD as of the time of the court ruling. If a court in such a state ruled against the IDD in a later case, we define this state as one that has rejected the IDD from the date of the subsequent ruling. For example, a Texas court ruled in favor of the IDD on May 28, 1993. However, on Apr. 3, 2003, another Texas court decided against the IDD. Such occurrences are rare, with only three instances so far. Florida, Michigan, and Texas rejected the previously adopted IDD several years after its initial adoption. Table 1 shows the adoption and rejection dates of the IDD in twenty-one US states. The earliest

[6] See, e.g., Klasa et al. (2018) for detailed discussions about the IDD.

adoption year was New York’s in 1919, and the most recent was that of Kansas in 2006. Klasa et al. (2018) provide details about the precedent-setting legal cases in which state courts adopted the IDD or rejected it after adoption.

[Insert Table 1 Here]

Because of its important impact on young startups, the IDD is of particular relevance in the VC setting. Startups, in their early years, have difficulty providing competitive compensation packages that are comparable to those of their established counterparts. Employees who work in startups, however, are usually passionate about the firms’ growth opportunities and hope for a big payout later when the venture succeeds, even though it is well known that the odds are small. Also, with so much uncertainty, startups are more likely to lose key employees to their competitors.

In the states that have adopted the IDD, employees find it more difficult to move, making it easier for the startups to retain talent. While the adoption of the IDD makes it hard for startups’ employees to leave for a more established firm, it also hampers startups’ ability to attract outside talent. Thus, the adoption of the IDD leads to a decline in the mobility of human capital, which is the key to startups’ success. The suboptimal allocation of human capital, which is caused by the decline in mobility, leads to greater concerns among VCs about talent distortions associated with investing in startups. Therefore, the adoptions and rejections of the IDD provide us a good opportunity to examine the important role played by startups’ labor mobility in various aspects of VC financing.

Furthermore, the staggered adoption and rejection of the IDD in different states provide us with an ideal empirical setting from which to draw causal inferences in the spirit of Bertrand and Mullainathan (2003). States become part of the treated group once they adopt the IDD. The states that have not yet passed the IDD, have rejected the IDD, or have never tried IDD cases are in the control group. Hence, our control group is not restricted to states that have never passed the IDD. Our identification strategy implicitly takes as the control group all firms in states that had not yet adopted the IDD, even if they did so later. We are essentially carrying out a difference-in-differences estimation by exploiting the staggered passage of the IDD.

Certain types of employees—including executives, sales staff, and research and development-related technical workers—are required to sign employment contracts containing a nondisclosure agreement (NDA), a covenant not to compete, i.e., non-compete clauses (CNC), or both, which are designed to protect firms' trade secrets. However, the protection offered by these contracts is limited and the IDD adds significantly more protection to firms' trade secrets.[7]

There are several important differences between the IDD and employment contracts that contain an NDA or a CNC-type clause. First, NDAs or CNCs are usually more effective within a specific geographic area, e.g., a state or a part of a state. However, the IDD can be enforced across a much broader geographic area. For example, the IDD can prevent employees from switching to competing firms that operate in another state, including a state that has not adopted the IDD. Second, the IDD allows state courts to grant injunctions even if the job-switching employees did not sign an NDA or a CNC with their former employers. Third, the IDD allows courts to prevent employees from working for competing firms if such employment would inevitably lead to future violations of NDAs or CNCs, thus even before actual violations are detected. This significantly increases the enforceability of NDAs and CNCs. To clearly identify the impact of the passage of the IDD, we control for the effect of CNCs in the empirical analysis.

[7] Klasa et al. (2018) provide a detailed discussion of the differences between the IDD and employment contracts with a nondisclosure agreement and/or a non-compete covenant.

B. Hypothesis Development

We propose two competing hypotheses regarding the effect of labor mobility restrictions on VC investment and outcomes. Our first hypothesis, the talent retention hypothesis, argues that restricting startups' labor mobility encourages VC investment. Hart and Moore (1994) argue that, in an incomplete contract framework, entrepreneurs could not commit to staying with a startup in which their human capital is critical to achieving the venture's full potential after it starts (i.e., a holdup problem); consequently, the entrepreneurs would not be able to raise capital for some profitable ventures. Bolton, Wang, and Yang (2019) further extend the model and show that the inalienability of human capital has important implications for corporate risk and liquidity management. Consistent with these theoretical arguments, Kaplan and Stromberg (2003) use real contracts between VCs and startups to show that it is prevalent for VCs to use CNCs to compel entrepreneurs to stay with a firm; they suggest that CNCs could mitigate the holdup problem. Examining public companies, Qiu and Wang (2018) show that the adoption of the IDD has positive effects on firms' market valuations. Motivated by these studies, we argue that restricting key employees' mobility prevents the leakage of business secrets and could increase a startup's competitiveness by allowing it to fully appropriate the returns from its own human capital development. In addition, restricting key employees (especially those who possess knowledge about core technologies) from moving to rivals allows startups to retain their key talent. As a result, both the startups and the employees are incentivized to invest more in employees' firm-specific human capital: startups are willing to invest in the employees' human capital, because labor mobility frictions lower the possibility of their losing the return from investment in the employees; and employees are willing to build firm-specific human capital because the labor mobility friction restricts their outside options. Overall, this hypothesis predicts a higher success rate for startups with the restriction of human capital mobility. Therefore, VCs are more willing to invest in these startups.

Our second hypothesis, the talent distortion hypothesis, predicts that labor mobility restrictions could impede VC investment. We argue that there are two plausible channels through which labor mobility restrictions discourage VC investment: the incentive channel and the knowledge spillover channel. The argument for the incentive channel is that low labor mobility could reduce existing employees' incentives to work hard (Fulghieri and Sevilir (2011)). As in the signaling game where there is incomplete information on workers' ability, inventors have incentives to generate patents to signal their ability in the labor market (Spence (1973), Gibbons et al. (2005)). However, restrictions on labor mobility make it difficult for skilled workers to move and therefore reduce their incentives to innovate (Contigiani et al. (2018)), which could negatively affect a startup's performance and VCs' incentives to invest. The knowledge spillover channel argues that low labor mobility makes it more difficult for startups to recruit necessary talent from outside and can distort the allocation of human capital across startups (Amornsiripanitch et al. (2019)). As argued by previous studies (Fleming (2001), Hellmann and Perotti (2011)), idea recombination is important for innovation and hence for a startup's growth potential and eventual success. Low labor mobility dampens the circulation of ideas among inventors and thus reduces the possibility of idea recombination and technological innovation. Therefore, VCs may recognize the risk and be less willing to invest in startups (Contigiani et al. (2018)).

IV. Data and Variable Construction

Our main data come from the Thomson Reuters VentureXpert database. We include all ventures located in the United States that received their first-round funding during the period 1980 through 2012. We require the ventures to have complete financing information. We exclude ventures in the utilities (2-digit SIC code 49) and financial services (2-digit SIC code between 60 and 69) industries to avoid potential confounding effects from deregulation in those industries during the same time period. We end up with about 15,000 unique startups and around 1,100 VCs in the sample. Several other data sources are also employed in our analysis. For example, we obtain firms' accounting information from the Compustat Annual File Database and inventor information from the patent inventor database of Harvard Business School.

In the baseline analysis, we study how labor mobility restrictions affect VCs' investment decisions. That is, we investigate the changes in the likelihood of VC investment in a startup after the IDD. To conduct this analysis, we construct a hypothetical sample of potential deals in the spirit of Bottazzi, Da Rin, and Hellman (2016) and Gompers et al. (2016). Specifically, for every deal in our sample, we create hypothetical VC-startup pairs. For each VC, we consider all possible startups in which the VC could have invested. For example, suppose a VC invests in a startup and there are nine other startups in which the VC could have invested but did not. The data structure for this deal has ten rows: one row for the startup in which the VC has invested plus nine rows, one for each potential startup in which the VC could have invested but did not. Using this hypothetical sample, we construct our main dependent variable, *Investment*, which is a dummy variable that equals 1 if the VC-startup deal takes place and 0 otherwise.

We create this hypothetical sample with two restrictions in mind. First, we require that the VCs exist before the startups are founded. Second, we restrict the sample to VCs that have invested, within the next 30 days, in at least one other deal in the same industry as the startups.[8] This restriction allows us to better capture the true investment intention of the VCs. Finally, we end up with around 372,000 potential deals.

Our main independent variable, IDD, is a dummy variable that equals 1 if the state has passed the IDD and 0 otherwise. In our regression analysis, we control for a set of variables that have been identified by prior literature that could affect VCs' investment decisions: VC_REPUTATION is defined as the number of IPOs backed by a VC as a fraction of IPOs backed by all VCs in the market in the previous 3 years; AGE represents the natural logarithm of the number of years since the inception of the venture; EARLY_DUMMY equals 1 if the firm is in the "startup/seed" or "early stage" as indicated by the VentureXpert database and 0 otherwise; DISTANCE is defined as the natural logarithm of the distance (in miles) between the startup and the VC; INDUSTRY_FIT is the percentage of deals made by a VC in the same industry as its portfolio firm; and, to control for the effect from non-compete clauses, we include CNC, which is an enforcement index for the covenant not to compete at the state level following Bird and Knopf (2015) by using the Garmaise (2011) sample from 1974 through 2004 and by extending the sample using Malsberger (2017) for years from 2005 through 2012.

Moreover, we explore how VCs alter their investment strategies in response to labor mobility decline induced by the IDD. To do this, we construct two variables—DURATION and SYNDICATION—which are used extensively by prior literature, to gauge VCs' investment strategy. DURATION is constructed as the natural logarithm of the number of days between the current-round date and the final-round date divided by the number of rounds left.

[8] We find qualitatively similar results when we eliminate the same-industry requirement and extend the window to 90 days in robustness tests

SYNDICATION is a dummy variable that equals 1 if a round is financed by more than one VC and 0 otherwise.

We also investigate the effect of the IDD on VCs' investment outcomes. To perform this analysis, we define three investment outcome variables: SUCCESS, a dummy variable that equals 1 if the firm exits through either IPO or M&A and 0 otherwise; IPO, a dummy variable that equals 1 if the venture goes public and 0 otherwise; and ACQUISITION, a dummy variable that equals 1 if the startup is acquired and 0 otherwise.

[Insert Table 2 Here]

The Appendix provides detailed variable definitions for all variables used in our tests and Table 2 presents summary statistics of our main variables. We report the mean, the standard deviation, the 25th percentile, the median, and the 75th percentile for each variable. As shown in Table 2, the average startup's probability of receiving VC financing is 13.4% within all hypothetical VC-startup pairs; the average startup in the sample is about 4.5 years old; and 69.2% of startups are in the early stage. In the startup-round panel, we find 14.6% of the firms exit either through an IPO (5.7%) or through M&A (13.5%).

V. Empirical Results

In this section, we present our empirical results. We first explore whether the adoption of the IDD restricts labor mobility and then examine how labor mobility restrictions, i.e., the recognition of the IDD, affect VC investment likelihood. Moreover, to strengthen the causal link and explore underlying mechanisms, we perform cross-sectional tests and examine inventors' patent characteristics and startups' innovative output after the IDD. Finally, we provide evidence on how VCs adjust their investment strategy in response to the effect of the IDD and their ultimate investment outcome.

A. Labor Mobility

For our identification strategy—that is, the adoption of the IDD restricts labor mobility—to work, we first need to establish the fact that employees who work for VC-backed startups are indeed less likely to carry out job-hopping activities after the IDD. In this subsection, we examine inventors' mobility surrounding the adoption of the IDD. We focus on inventors for two reasons. First, for our sample of VC-backed startups, inventors—who are equipped with technical trade secrets that are extremely important to the startup development—are the backbones of startup operations. Second, it is difficult to find micro-level datasets that track each employee's employment history. However, the inventor database maintained by Harvard Business School provides us with abundant information about patent inventors. Using records on patent filings—such as filing company, filing date, patent class, and patent inventors—we are able to track each inventor's employment history information.

In terms of the IDD, court rulings often come as a surprise and represent plausibly exogenous shocks to labor mobility. Different states adopt and reject the IDD on different dates. We implement a difference-in-differences approach in which the staggered recognition of the IDD provides us with both the control and treatment groups (as in Bertrand and Mullainathan (2003)). In our analysis, we examine inventors' within-state mobility and out-of-state mobility separately. As long as the state where a worker's employment contract is written has adopted the IDD, the IDD can prevent the employee from working for competing firms that operate in other states that have not adopted the IDD. Thus, we expect that the adoption of the IDD by a state should reduce the mobility of inventors in that state from relocating to competing firms in the same state or in any other state.

To carry out the test, we obtain the Disambiguation and Co-authorship Networks of the US Patent Inventor Database (1975–2010) maintained by Harvard Business School. We restrict our sample period to 1980–2010 to match the VC sample as closely as possible. We identify an inventor as a “mover” (someone who has moved to a new job) if she has two successive patent filings assigned to different firms. We start by matching the patent assignees to the startups in the VentureXpert database. The goal is to restrict our inventor sample to inventors who work for startups initially. We track the job changes of those inventors, who could be changing to other startups or to established firms. Because the IDD is specifically designed to prevent employees from disclosing trade secrets to their subsequent employers, which are more likely to be rival firms, we further refine our sample by focusing on within-industry moves. We eliminate all inventors who filed only one patent in our sample period. We keep only one observation for each inventor if she has multiple patent filings during the same year.[9] Following the literature on labor mobility, we define the year of the move as the midpoint between the years of filing two successive patents that are assigned to different firms.[10] In our final sample, we have around 220,000 inventors. Inventors on average move to another company every 3 years. We then estimate the following equation:

$$(1) \qquad MOVE_{i,t} = \beta \cdot IDD_{s,t} + \gamma' \cdot X_{s,t} + \mu_i + \delta_s + \eta_{j,t} + \epsilon_{i,t},$$

where the dependent variable, $\mathrm{MOVE}_{\mathrm{i,t}}$, is a dummy variable that equals 1 if an inventor *i* moves in year *t* (as indicated by the same inventor filing two successive patents that are assigned to different firms) and 0 otherwise. To study separately the effects for within-state moves and out-

[9] In the extreme case when an investor filed all her patents within 1 year, we drop this observation.

[10] We find qualitatively similar results if we assume that the inventor moves immediately after the first patent filing or immediately before the second patent filing.

of-state moves, we create two other dependent variables: IN_STATE_MOVE, a dummy variable that equals 1 if an inventor moves within the same state and 0 if an inventor does not move; OUT_OF_STATE_MOVE, a dummy variable that equals 1 if an inventor moves to a different state and 0 if an inventor does not move. In other words, the control groups for both in-state moves and out-of-state moves are inventors who do not move. $\mathrm{IDD}_{s,t}$ is a dummy variable that equals 1 if state *s* has passed the IDD by year *t* (and has not subsequently rejected the IDD) and 0 otherwise. $X_{s,t}$ represents a set of state-year-level variables that might affect an inventor's mobility in the analysis: UNEMPLOYMENT is the unemployment rate of each state; GDP_GROWTH is the growth rate of each state's GDP; and POLITICAL_BALANCE is defined as the fraction of a state's representatives in the US House of Representatives that belong to the Democratic Party. To control for the effect of CNC on labor mobility, we include CNC, which is an index of CNC enforcement. The symbols μ_i, δ_s, and $\eta_{j,t}$ represent inventor, state, and industry-year fixed effects, respectively.

[Insert Table 3 Here]

Table 3 presents the results of our linear probability regression related to the effects of IDD adoption on inventor mobility. As shown in column 1, the coefficient estimate on the IDD dummy is negative and statistically significant, which suggests that IDD adoption makes it more difficult for inventors to move. In columns 2 and 3, we test in-state and out-of-state moves, respectively. Our results indicate that after adoption of the IDD, inventors are less likely to move, either in state or out of state, indicating that the IDD restricts overall labor mobility. In terms of economic significance, the decrease in the probability of moving is around 40% compared to the unconditional mean.[11]

Overall, the analysis in this subsection suggests that there is a significant reduction in inventor mobility after the adoption of the IDD. The passage of the IDD distorts human capital allocation across startups by reducing labor mobility. This finding provides further support to our identification strategy and serves as a foundation for our main analysis of the impact of the IDD on VC investment.

B. Baseline Results

In this subsection, we present our baseline findings on how the adoption of the IDD affects the likelihood of VC investment.

1. Investment Likelihood

We construct a hypothetical sample of potential deals in the spirit of Bottazzi et al. (2016) and Gompers et al. (2016). Specifically, for each VC in our sample, we create hypothetical VC-startup pairs by forming a universe of startups that are in the same industry in which the VC has invested within 30 days; this yields approximately 372,000 potential deals. We then estimate the VCs' investment decisions with the following specification:

$$INVESTMENT_p = \beta \cdot IDD_{s,t} + \gamma' \cdot X + \varphi_k + \delta_s + \eta_{j,t} + \epsilon_p, \tag{2}$$

[11] In Supplementary Material Table IA1, we exclude firms that have had M&A activities during the sample period and we find qualitatively similar results. To identify the startups that are eventually acquired by another company we match names of assignees in the patent database with names of targets in the Thomson Reuters SDC Mergers and Acquisitions Database. One caveat here is that name matching may not be exhaustive and some startups that are eventually acquired may not be identified in the process.

where p indexes potential investor-firm pairs. The dependent variable is $\text{INVESTMENT}_\text{p}$, which is a dummy variable that equals 1 if a VC finances the startup and 0 otherwise. IDD is a dummy variable that equals 1 if firm i is in state s that has adopted the IDD by year t and 0 otherwise. If state s subsequently rejected the IDD at year $t+n$, then we assign 0s to IDD for firm i in state s for the years after the rejection. X represents the host of variables to account for observable characteristics. Specifically, X includes VC_REPUTATION, EARLY_DUMMY, AGE, DISTANCE, INDUSTRY_FIT, and CNC. These variables potentially influence the likelihood of VC investment and are frequently examined in the VC literature. Moreover, we include various fixed effects: φ_k, δ_s, and $\eta_{j,t}$ represent lead VC, state, and industry-year fixed effects, respectively. These fixed effects control for unobservable lead VC characteristics, state heterogeneity, and industry-year specific characteristics, respectively. We cluster standard errors by state. β is the coefficient of interest, and it captures the effect of the IDD on VCs' investment decisions.

[Insert Table 4 Here]

Table 4 presents our baseline results. Columns 1, 2, and 3 report the estimation results for three different specifications. All three columns show a negative and statistically significant coefficient estimate on the IDD dummy. Taking column 3 as an example, the regression coefficient on the IDD dummy is –2%, which is statistically significant at the 5% level. This result is also economically sizable. Given that the unconditional probability of VCs' investing in startups is around 13%, our findings represent a 15% drop in the likelihood of VC investment in a startup after the adoption of the IDD. Together our results suggest that, after the adoption of the IDD, the likelihood of VC investment decreases significantly. In other words, startups are less likely to receive VC financing after IDD adoption. This observation is consistent with our hypothesis that VC investors are concerned about talent distortions embedded in startups after the adoption of the IDD and hence are less likely to invest in startups.[12]

2. Dynamic Trend

As shown in Table 1, there are three states that rejected previously adopted IDD rulings several years after the initial adoption. The IDD dummy in Table 4 captures the effect of both adoptions and subsequent rejections (if any) of the IDD by each state. Next, we examine the effects of adoptions and rejections separately.

To carry out the analysis, we slightly modify equation (2) by replacing the IDD dummy with an ADOPTION dummy and a REJECTION dummy. Specifically, we estimate the following model:

$$\text{(3)} \quad \mathrm{INVESTMENT_p} = \beta_1 \cdot \mathrm{ADOPTION} + \beta_2 \cdot \mathrm{REJECTION} + \gamma' \cdot \mathrm{X} + \varphi_k + \delta_s + \eta_{j,t} + \epsilon_p,$$

where ADOPTION is a dummy variable that equals 1 if firm *i* is in state *s* that has adopted the IDD from year *t* and 0 otherwise. REJECTION is a dummy variable that equals 1 if firm *i* is in state *s* that has rejected IDD from year *t* and 0 otherwise. β_1 and β_2 are the coefficients of interest. They separately demonstrate the effects of IDD adoptions and rejections on VC investment likelihood.

[Insert Table 5 Here]

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[12] In Supplementary Material Table IA2, we carry out a robustness test by constructing a panel data at the startup-year level (the beginning year equals the year following the first round of investment for the startup and the ending year equals the last year of investment for the startup). The dependent variable takes the value of 1 if the startup receives round financing in that year, and 0 otherwise. We exclude all first-round investments and those firms with only one round of VC financing, effectively studying the subsequent round investments. We obtain qualitatively similar results.

Columns 1 and 2 in Table 5 present the estimation results. Similar to the baseline results, we observe negative and statistically significant coefficients on the ADOPTION dummies. The regression coefficients are qualitatively similar to those in Table 4. This observation tells us that adopting the IDD leads to a significant decrease in the likelihood of VC investment. Rejecting the IDD should have the opposite effect, and this is exactly what we observe in column 1: a positive and statistically significant coefficient on the IDD rejection dummy. However, this coefficient becomes insignificant in column 2 after more control variables are included. This result is not surprising because the number of states (three) that have rejected the IDD is very small.

We then carry out formal tests to ensure that the parallel trends assumption of the difference-in-differences approach is satisfied. Following Bertrand and Mullainathan (2003), Giroud and Mueller (2010), and Acharya, Baghai, and Subramanian (2013) among others, we replace the IDD dummy in equation (2) with six dummy variables that capture different time points around the adoption of the IDD. Specifically, we estimate the following model:

(4) $\mathrm{INVESTMENT_p} = \beta_1 \cdot \mathrm{ADOPTIONM3\_PLUS} + \beta_2 \cdot \mathrm{ADOPTIONM2} + \beta_3 \cdot \mathrm{ADOPTIONM1} + \beta_4 \cdot \mathrm{ADOPTIONP1} + \beta_5 \cdot \mathrm{ADOPTIONP2} + \beta_6 \cdot \mathrm{ADOPTIONP3\_PLUS} + \beta_7 \cdot \mathrm{REJECTION} + \gamma' \cdot \mathrm{X} + \varphi_k + \delta_s + \eta_{j,t} + \epsilon_p$

We define ADOPTIONM3_PLUS, ADOPTIONM2, ADOPTIONM1, ADOPTIONP1, ADOPTIONP2, and ADOPTIONP3_PLUS as dummy variables that equal 1 if the state adopts the IDD within 3 or more years, 2 years, 1 year, the past year, the past 2 years, and the past 3 or more years respectively in reference to the date of VC investment and 0 otherwise. The REJECTION dummy is defined the same as in equation (3). If we observe statistically significant coefficients on the ADOPTIONM3_PLUS, ADOPTIONM2, or ADOPTIONM1 dummies, it implies that the parallel trends assumption is not satisfied and changes in VC investment precede the changes in law.

Column 3 in Table 5 shows no statistically significant coefficients on the ADOPTIONM3_PLUS, ADOPTIONM2, or ADOPTIONM1 dummies, which suggests that the parallel trends assumption of the difference-in-differences approach is satisfied and changes in VC investment do not precede the adoption of the IDD. The coefficient estimates on ADOPTIONP1, ADOPTIONP2, and ADOPTIONP3_PLUS are consistent with our baseline results in Table 4. Note that the rejection effect is again statistically insignificant.

Taking the investment likelihood analyses together, we are able to test the two competing hypotheses, i.e., the talent retention hypothesis and the talent distortion hypothesis. Our main results show that, following the adoption of the IDD, there is a drop in the likelihood of VC investment with meaningful economic magnitudes, i.e., IDD reduces the likelihood of VC investment by around 15%. The evidence appears to be consistent with the talent distortion hypothesis.

C. Cross-Sectional Tests

Our baseline results suggest that the adoption of the IDD discourages VC investment. If talent distortions created by the adoption of the IDD are indeed the reason, we would expect this negative effect to be stronger in human-capital-intensive industries, for startups that are in greater need of human capital, and among startups whose human capital is difficult to monitor by VCs. Therefore, in this subsection, we strengthen the causal link between talent distortions caused by the IDD and VC investment by carrying out tests with our rich cross-sectional data. More concretely, we examine whether our main results become more pronounced in startups that are in industries with intensive patenting activity, are in the early financing stage when concerns about talent distortions are more significant, or are located farther from VCs and hence are more difficult for VCs to monitor.[13] We expect to observe more negative effects of the IDD among these firms.

We estimate equation (2) by including an interaction term to capture the cross-sectional effects. Table 6 presents our estimation results. We test the investment likelihood effect using the hypothetical sample of VC-startup pairs from Subsection V.B.1. The dependent variable is INVESTMENT, which is a dummy variable that equals 1 if the VC-startup deal takes place and 0 otherwise.

[Insert Table 6 Here]

We start by comparing startups in patenting-intensive industries with firms in industries with low patenting intensity and report the results in column 1 of Table 6. We construct an INDUSTRY_PATENTING_INTENSITY_DUMMY using all firms in the Compustat database by calculating the average number of patents in an industry classified at the 3-digit SIC code level.[14] We define patenting-intensive industries as those with patenting output in the top 50% and low-patenting-intensity industries as those with patenting intensity in the bottom 50%. Because startups in patenting-intensive industries devote more resources to research and development, they are in greater need of talent. The adoption of the IDD should, therefore, have a stronger effect on those startups. We find statistically significant results, which is consistent with our conjecture.

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[13] According to Chemmanur et al. (2014) and Tian and Wang (2014), early-stage startups are subject to a higher risk of failure, and thus are more subject to concerns about talent distortions.

[14] The patent data come from Kogan et al. (2017). We thank them for making the data publicly available.

We next compare startups that receive investments at early stages with those that receive investments at later stages. The results are presented in column 2 of Table 6. Startups that seek VC financing at an early stage need more talented employees to help the development of the venture and thus should be affected more by talent distortions created by the adoption of the IDD. Therefore, we expect our main findings to be more pronounced among startups that receive VC financing at their early stages. We define EARLY_DUMMY as 1 if the firm is in the "startup/seed" or "early stage" as indicated by the VentureXpert database and 0 otherwise. We interact EARLY_DUMMY with the IDD dummy, and the coefficient estimate on the interaction term is the coefficient of interest. Again, the test result is in line with our expectation, as shown by the negative and significant coefficient estimate on the interaction term.

Lastly, we investigate how our main results are altered by VCs' cost of effectively monitoring startups. As shown in Tian (2011), the physical distance between VCs and startups is a good proxy for VCs' monitoring cost. Hence, when talent distortions are high, we postulate that VCs invest in startups that are in closer proximity to themselves for more effective monitoring. If this conjecture is supported, we expect to observe a reduction in VC investment for startups located farther from them. We define DISTANCE_DUMMY as a dummy variable that equals 1 if the firm is located more than 150 miles from the VC (Tian (2011)) and 0 otherwise, and interact it with the IDD dummy. We present the results in column 3 of Table 6. For the interaction term, we find a negative and statistically significant coefficient, suggesting that VCs are less likely to invest in startups that are located farther from them after the adoption of the IDD.

To summarize, the cross-sectional tests show that our main findings are more pronounced in industries with higher patenting intensity, among startups with earlier-stage VC investments, and among firms located farther from VCs. Startups with these characteristics tend to rely more on human capital in their development; hence restrictions on labor mobility lead to greater talent distortions in these firms. These results suggest that reduced labor mobility (induced by the adoption of the IDD) leads to reductions in VC investment.

D. Mechanisms

In this subsection, we explore plausible underlying mechanisms through which the IDD affects VC investment. We suspect that the reduction in employees' innovation productivity is a plausible underlying mechanism. To test this conjecture, we examine patent counts and patent citations at both the inventor-year level and the startup-year level following the IDD adoptions. Next, we propose two potential reasons why reduced labor mobility caused by the IDD affects employees' innovation productivity. First, under normal conditions in a signaling game with incomplete information, inventors are highly incentivized to generate patents in their focal domain; this signals their abilities to innovate to the labor market, which could lead to outside employment options (Spence (1973), Gibbons et al. (2005)). However, after the adoption of the IDD, inventors are less likely to switch jobs due to restrictions on labor mobility, leading to lower incentives to innovate in their focal patents; this could take a toll on the startups' productivity and eventually on the VCs' bottom lines (Fulghieri and Sevilir (2011)). Second, innovation requires idea recombination (Fleming (2001), Hellmann and Perotti (2011)). The adoption of the IDD impedes labor mobility, which makes it more difficult for ideas to circulate, thus resulting in lower innovation output from startups.

We empirically test the above mechanisms and two plausible explanations using data on individual inventors' innovative output. We again use the patent network database from Harvard

Business School to construct inventor-level innovative output measures. We restrict our sample period to 1980–2010 to match the VC sample as closely as possible. We also require our sample to include only inventors who work for startups. First, we directly test whether there is a reduction in firms' innovative output as a response to the IDD shock. We use two dependent variables to gauge a firm's innovative output: NUMBER_OF_PATENTS and NUMBER_OF_CITATIONS. We define Ln(PATENT) as the natural logarithm of 1 plus the total number of patents produced by the startup in a year. We also define Ln(CITATION) as the natural logarithm of the average number of citations per patent for a startup.

[Insert Table 7 Here]

Panel A in Table 7 reports the estimation results at both the inventor-year level and the startup-year level. As shown in columns 1 and 2, the coefficient estimate on the IDD dummy is negative and significant at the 5% and 1% levels, respectively. This result suggests that inventors produce fewer patents and that their patents receive fewer subsequent citations after the adoption of the IDD. The lower output at the inventor-year level is also confirmed by the results at the startup-year level reported in columns 3 and 4, which show that startups experience a drop in the number of patent counts and patent citations following the adoption of the IDD. Overall, our findings in Table 7 Panel A suggest that the adoption of the IDD has a negative effect on startups' patenting activities.[15]

We then examine plausible reasons for our above observations by distinguishing the incentive channel and the knowledge spillover channel in the cross section of innovative output.

[15] We do not necessarily conclude that there is a drop in startups' innovation from the decline in the patenting activities, given that startups might have an incentive to keep innovation out as a trade secret rather than filing patents following the IDD. However, given that trade secrets are difficult for an outside investor to verify, the drop in patenting activities sends the VCs a negative signal about a firm's innovations.

We implement the same empirical design as in Panel A and define two indicator variables, HIGH_MOBILITY_DUMMY and HIGH_SPILLOVER_DUMMY, to capture signaling and idea recombination, respectively. In industries with higher labor mobility, the incentives to signal are higher. As a result, the decrease in innovation output following the adoption of the IDD (restricting labor mobility) would be higher in these industries. We define a HIGH_MOBILITY_DUMMY as 1 if the industry-level number of inventor moves scaled by the total number of inventors is above the median and 0 otherwise. We then interact this variable with the IDD dummy. We would expect a negative coefficient estimate on the interaction term after the adoption of the IDD if our early finding is due to the incentive channel. To test the knowledge spillover channel, we define a HIGH_SPILLOVER_DUMMY as 1 if the industry-level number of within-industry patent citations scaled by total patent citations is above the median and 0 otherwise. We then interact this variable with the IDD dummy. We would expect a negative coefficient estimate on the interaction term after the adoption of the IDD if the idea combination is at play here because our measure captures the industries in which knowledge spillovers are more important.

To test these two plausible channels, we carry out regressions similar to equation (1) and present the results in Panel B of Table 7. In columns 1 to 4, we carry out an inventor-year level analysis. We use Ln(PATENT) and Ln(CITATION) as the dependent variables. In columns 1 and 2, we test the incentive channel. We find a negative and significant relation between the innovation measures and the interaction term, suggesting that inventors further decrease their innovative output in industries with higher labor mobility. In other words, inventors are not actively patenting their innovative work that pertains to their focal areas, which could serve as a potential explanation for the talent distortions that concern VCs. In columns 3 and 4, we test the knowledge spillover channel. The coefficient estimates on the interaction terms are economically small and statistically insignificant, suggesting that the knowledge spillover argument is not supported by the data. In columns 5 to 8, we implement the same analysis, but at the startup level. We find qualitatively similar results.

E. VC Investment Strategy

So far, we have established the adverse effect of the IDD on the likelihood of VC investment. In this subsection, we study the intensive margin: given their investment in startups, whether VCs adjust their investment strategies to mitigate talent distortions caused by the IDD. In particular, we focus on two important aspects of VC investment strategies, i.e., staging and syndication.

Staging, the stepwise disbursement of capital from VCs to startups, is an effective way to mitigate agency problems in VC financing. This is because VCs split funding for startups into multiple rounds of financing instead of making a larger, lump-sum payment upfront (Gompers (1995), Tian (2011)). VCs take such caution to reduce investment uncertainty because it keeps entrepreneurs on a “tight leash” (Sahlman (1990), Gompers (1995)), and hence staging has real option value. Syndication, a striking feature of the VC industry, is co-investment in the same startups by multiple VCs (Lerner (1994)). Similar to syndicated bank loans, syndication allows VCs to share the risk associated with startups. In a VC syndicate, the participating VCs can share opinions about the investment and make joint decisions based on their combined knowledge. Therefore, VCs could mitigate the adverse consequences of talent distortion by intensifying staging and relying more on syndication.

To test the above conjecture, we estimate a regression specification that is similar to equation (2), but we replace the outcome variable with VC investment strategy measures. More specifically, we run OLS regressions in the following model:

(5) $$INVESTMENT\ STRATEGY_l = \beta \cdot IDD_{s,t} + \gamma' \cdot X + \varphi_k + \delta_s + \eta_{j,t} + \epsilon_p.$$

In a startup-round level panel, we use two variables to gauge VC investment strategy in startups: DURATION and SYNDICATION. We define DURATION as the natural logarithm of the number of days between the current round date and final round date divided by the number of rounds left. The variable SYNDICATION is a dummy variable that equals 1 if a round is financed by more than one VC and 0 otherwise. Table 8 presents the estimation results. In column 1, DURATION is the dependent variable.[16] We observe a negative and significant coefficient estimate on the IDD dummy, which suggests that, in response to increased talent distortions, VCs maintain greater control over the startups by splitting financing into a shorter duration between two successive rounds after the adoption of the IDD.

[Insert Table 8 Here]

In column 2, we replace the dependent variable with SYNDICATION. The coefficient estimate on the IDD dummy is positive and significant at the 1% level. The evidence suggests that VCs are likely to co-invest with other VCs in startups after a state adopts the IDD to mitigate the adverse effect of talent distortions. It is, once again, consistent with our conjecture.

F. VC Investment Outcome

The ultimate goal for VCs is to earn high financial returns when they exit from the startups. As we have argued before, the adoption of the IDD could have both positive and

[16] A longer incubation period is typically associated with more rounds of financing. Thus, we include startup incubation period as one of the control variables in the estimation.

negative effects. On the one hand, the IDD deters key talents from leaving for a competing firm. On the other hand, labor mobility restrictions reduce employees' outside options and thus their incentives to work hard to signal their ability in the labor market. Furthermore, the IDD makes it more difficult for startups to recruit outside talent. If the positive effect dominates, startups are more likely to exit successfully. If the negative effect dominates, startups are less likely to exit successfully. Overall, the effect of IDD adoption on startups' exits depends on the dominance of the positive or negative effects of the IDD. Therefore, how the IDD affects VCs' investment outcomes is an empirical question.

To test the effect of IDD adoptions on VC exits, we consider IPOs and M&As as two successful exit pathways (e.g., Brander et al. (2002), Sørensen (2007), Bottazzi et al. (2016)). We conduct the analysis at the startup-round level. Therefore, the SUCCESS dummy equals 1 if the startup exits during the round by IPO or M&A and 0 otherwise. We also distinguish the two successful exit pathways. The IPO dummy equals 1 if the firm exits by going public and 0 otherwise. And similarly, the ACQUISITION dummy equals 1 if the firm exits by M&A and 0 otherwise. Specifically, we estimate the following equation:

$$OUTCOME_{l,t} = \beta \cdot IDD_{s,t} + \gamma' \cdot X + \varphi_k + \delta_s + \eta_{j,t} + \epsilon_p, \quad (6)$$

where $\mathrm{OUTCOME}_{\mathrm{l,t}}$ equals 1 if the startup *i* has a successful exit during round *t* and 0 otherwise. Following the standard approach in the VC literature on VC exit, we require the sample to include VC-backed firms that received first-round funding from 1980 through 2012. Table 9 presents our estimation results.

[Insert Table 9 Here]

In column 1, we use SUCCESS as the dependent variable. We find a negative and significant relation between probabilities of successful exits and the passage of the IDD. This result suggests that the probability of successful VC exits is significantly lower after the adoption of the IDD than it is before the adoption. In column 2, we examine how VC exits through IPOs are affected by the IDD adoption, excluding from the sample those startups that are eventually acquired. We find that, after the adoption of the IDD, VCs are less likely to exit through an IPO. In column 3, we examine how the IDD affects VCs' exits through M&As, excluding from the sample those startups that eventually go public. We also find a negative coefficient estimate on the IDD dummy, suggesting that fewer VC exits are through M&As. The results are both statistically and economically significant.[17]

Overall, if the adoption of the IDD leads to a higher success rate for startups as the positive talent retention effect dominates the negative talent distortion effect, we would expect to observe more successful VC exits. This is because, according to our results on the likelihood of VC investment, VCs tend to reduce the size of their investment portfolios after the adoption of the IDD. A reasonable conjecture is that, if VCs would like to invest in fewer startups, they may select better startups. However, the analysis in this subsection reveals that VCs' investment outcomes become worse after the adoption of the IDD. This finding suggests that the negative talent distortion effect dominates and thus the adoption of the IDD reduces startups' success rate.

One question naturally follows: do VCs tilt their investments toward startups located outside of IDD states to circumvent the negative effects of the IDD? We find evidence that supports this conjecture. Specifically, we show that VCs tilt their portfolios away from states that pass the IDD. We report the results in Table IA4 in the Supplementary Material.

G. Discussions of an Alternative Interpretation

[17] In Supplementary Material Table IA3, we carry out a startup-level robustness test on how the adoption of the IDD affects VC exits.

We have shown that increased restrictions on labor mobility induced by the IDD reduce VC investment. However, labor mobility restrictions also enhance trade secret protections, which could potentially affect VC investment. To investigate how reduced labor mobility and trade secret protections affect VC investment differently, we make use of the Uniform Trade Secret Act (UTSA), which strengthens the protection of trade secrets but does not restrict labor mobility. The UTSA broadens the definition of a trade secret, and merely defines the acquisition of a trade secret as a misappropriation.[18]

[Insert Table 10 Here]

In Table 10, we examine whether the UTSA takes away the effect of the IDD on VC investment likelihood. In columns 1 to 3, the key independent variable is the 2010 UTSA index that we obtained from Png (2017b). Because the index covers years through 2010, we restrict our sample period accordingly. Columns 1 and 2 show that the UTSA does not have a significant effect on VC investment likelihood. Column 3 runs a horse race between the UTSA and IDD, and shows that the UTSA does not absorb the impact of the IDD on the likelihood of VC investment. The coefficient estimate on the IDD also has a similar magnitude to that of the main results (Table 4). In columns 4 to 6, we repeat the analyses reported in columns 1 to 3 using the 1998 UTSA index that we obtained from Png (2017a). In this case, the index covers all the years before 1998. As a result, we end our sample in 1998. Again, the results show that the effect of the IDD on VC investment continues to hold with the UTSA being included in the regressions.

[18] In the United States, trade secrets were historically governed by common law, which originated in England from a seminal case in 1868. Subsequently, the Restatement (First) of Torts defined trade secrets and stipulated the conditions seen as misappropriation of trade secrets. However, the Restatement of the Law is not binding legal authority. In 1979, the National Conference of Commissioners on Uniform State Laws published and recommended UTSA for enactment by states.

Hence, our baseline results are not driven by trade secret protections, but by reduced labor mobility.

VI. Conclusion

In this paper, we investigate how restrictions of labor mobility affect VC investment. To establish causality, we use the plausibly exogenous variation generated by staggered adoption of the IDD, which causes a decline in labor mobility. We find that the adoption of the IDD reduces the likelihood of VC investment. Moreover, this negative effect is concentrated in innovative industries, among firms with earlier-stage VC investment, and in longer-distance investments. Further analysis supports a talent distortion hypothesis. That is, labor mobility restrictions reduce employees' innovation productivity. To mitigate the adverse effect of the IDD, VCs engage in more staged financing of startups and more syndication with other VCs, but these efforts do not completely counter the effect; VCs still experience a drop in successful exit rates. Our paper sheds new light on the real effects of labor market frictions via the lens of VC markets.

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Appendix: Variable Definitions

This appendix presents definitions of all variables used in the analysis. The sample includes VC backed firms from 1980 through 2012. Utility and financial services industries are excluded from the sample.

MOVE: A dummy variable that equals 1 if an inventor moves in a given year (i.e., as indicated by two successive patents filed by the same inventor and assigned to different firms), and 0 otherwise.

IN_STATE_MOVE: A dummy variable that equals 1 if an inventor moves in the same state, and 0 if the inventor does not move.

OUT_OF_STATE_MOVE: A dummy variable that equals 1 if an inventor moves to a different state, and 0 if the inventor does not move.

IDD: A dummy variable that equals 1 if the state in which the startup is located has passed the IDD and 0 otherwise.

UNEMPLOYMENT: The unemployment rate of each state.

GDP_GROWTH: The growth rate of each state's GDP.

POLITICAL_BALANCE: The fraction of a state's House Representatives who are Democrats.

CNC: An index of CNC enforcement in a state following Bird and Knopf (2015) by using the Garmaise (2011) sample from 1974 through 2004 and extending the sample using Malsberger (2017) for years from 2005 through 2012.

INVESTMENT: A dummy variable that equals 1 if the VC-firm deal actually took place and 0 otherwise.

VC_REPUTATION: The number of IPOs backed by a given VC as a fraction of IPOs backed by all VCs in the market in the previous 3 years.

EARLY_DUMMY: A dummy variable that equals 1 if the firm is in the “startup/seed” or “early stage” and 0 otherwise.

AGE: Natural logarithm of the number of years since the venture inception year.

DISTANCE: Natural logarithm of the distance (miles) between firm and VC.

INDUSTRY_FIT: Percentage of deals made by a VC in the same 3-digit SIC industry as its portfolio company.

NUMBER_OF_PATENTS: Total number of patents.

NUMBER_OF_CITATIONS: Total number of citations per patent.

INDUSTRY_PATENTING_INTENSITY_DUMMY: A dummy variable that equals 1 if the firm's patent output is in the top 50% and 0 otherwise.

HIGH_MOBILITY_DUMMY: A dummy variable that equals 1 if the number of inventor moves scaled by total number of inventors at the industry level is above the median and 0 otherwise.

HIGH_SPILLOVER_DUMMY: A dummy variable that equals 1 if the number of within-industry citations scaled by total citations at the industry level is above the median and 0 otherwise.

INCUBATION_PERIOD: Natural logarithm of the number of days between the date of the VC's first investment in a startup and its exit date from the startup.

DURATION: Natural logarithm of the number of days between the current round date and last round date, divided by the number of rounds left.

SYNDICATION: A dummy variable that equals 1 if a round is financed by more than one VC and 0 otherwise.

Ln(AMOUNT): Natural logarithm of the total dollar amount of investment received by each startup.

SUCCESS: A dummy variable that equals 1 if the firm exits through either IPO or M&A and 0 otherwise.

IPO: A dummy variable that equals 1 if the venture goes public and 0 otherwise.

ACQUISITION: A dummy variable that equals 1 if the venture is involved in a merger or acquisition and 0 otherwise.

Table 1

List of the Adoption and Rejection Dates of the IDD

Table 1 presents the adoption and rejection dates of the IDD. Column 1 presents the date on which the IDD was adopted and column 2 presents the date on which the IDD was rejected by each state.

	1	2
State	Adoption Date	Rejection Date
Arkansas	Mar. 18, 1997	
Connecticut	Feb. 28, 1996	
Delaware	May 5, 1964	
Florida	July 11, 1960	May 21, 2001
Georgia	June 29, 1998	
Illinois	Feb. 9, 1989	
Indiana	July 12, 1985	
Iowa	Apr. 1, 1996	
Kansas	Feb. 2, 2006	
Massachusetts	Oct. 13, 1994	
Michigan	Feb. 17, 1966	Apr. 30, 2002
Minnesota	Oct. 10, 1986	
Missouri	Nov. 2, 2000	
New Jersey	Apr. 27, 1987	
New York	Dec. 5, 1919	
North Carolina	June 17, 1976	
Ohio	Sept. 29, 2000	
Pennsylvania	Feb. 19, 1982	
Texas	May 28, 1993	Apr. 3, 2003
Utah	Jan. 30, 1998	
Washington	Dec. 30, 1997	

Table 2

Summary Statistics

Table 2 presents summary statistics of the sample used in the analysis. The sample includes VC-backed firms from 1980 through 2012. Utility and financial services industries are excluded from the sample. Detailed variable definitions are provided in the Appendix. Columns 1 through 6 report the sample size, the sample mean, the sample standard deviation, the sample 25th percentile, the sample 50th percentile, and the sample 75th percentile, respectively.

Table 2 (continued)

Variables	1 N	2 Mean	3 Std. Dev.	4 P25	5 P50	6 P75
Panel A. Human Capital Mobility Test Sample						
MOVE	837,940	0.037	0.189	0	0	0
IN_STATE_MOVE	835,046	0.034	0.181	0	0	0
OUT_OF_STATE_MOVE	809,739	0.004	0.060	0	0	0
IDD	837,940	0.487	0.500	0	0	1
UNEMPLOYMENT	837,940	5.852	1.712	4.758	5.467	6.700
GDP_GROWTH	837,940	0.055	0.033	0.039	0.054	0.073
POLITICAL_BALANCE	837,940	0.559	0.184	0.500	0.569	0.632
CNC	837,940	3.537	2.205	3	4	5
Panel B. Investment Likelihood Sample						
INVESTMENT	371,782	0.134	0.341	0	0	0
VC_REPUTATION	371,782	0.003	0.004	0	0.001	0.004
EARLY_DUMMY	371,782	0.692	0.462	0	1	1
AGE	371,782	4.525	7.156	1	2	5
DISTANCE (000' miles)	371,782	1.305	0.980	0.344	1.231	2.394
INDUSTRY_FIT	371,782	0.368	0.298	0.087	0.333	0.579
Panel C. Patenting Characteristics Sample						
NUMBER_OF_PATENTS (inventor)	1,837,660	1.285	1.798	1	1	1
NUMBER_OF_CITATIONS (inventor)	1,395,542	24.007	43.899	4.198	11.063	25.916
NUMBER_OF_PATENTS (startup)	51,637	6.334	39.839	0	0	0
NUMBER_OF_CITATIONS (startup)	8,561	1,341.987	4,833.736	15.991	66.357	296.452
Panel D. VC Deal Structure Sample						
DURATION	45,108	5.864	0.770	5.371	5.848	6.317
SYNDICATION	45,108	0.668	0.471	0	1	1
INCUBATION_PERIOD	45,108	7.582	0.753	7.185	7.688	8.098
Panel E. Investment Success Sample						
SUCCESS	59,746	0.146	0.353	0	0	0
ACQUISITION	51,057	0.135	0.341	0	0	0
IPO	32,679	0.057	0.232	0	0	0

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Table 3

Labor Mobility

Table 3 presents OLS regression results related to the effect of the IDD on labor mobility. The sample includes inventors who initially worked for a startup and either remained at the startup or left for another startup or mature firm in the same industry during the sample period 1980–2010. In column 1, the dependent variable, MOVE, equals 1 if an inventor moves and 0 otherwise. In columns 2 and 3, we further divide MOVE into IN_STATE_MOVE and OUT_OF_STATE_MOVE. The control group in both columns 2 and 3 is inventors who do not move. IDD is the key independent variable that equals 1 if the state has adopted the IDD and 0 otherwise. Detailed variable definitions are provided in the Appendix. Industry-year fixed effects are defined at the inventor primary patent class-year level. All continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors clustered at the state level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 3 (continued)

Variables	1 MOVE	2 IN_STATE_MOVE	3 OUT_OF_STATE_MOVE
IDD	-0.016***	-0.013***	-0.003***
	(0.004)	(0.003)	(0.001)
UNEMPLOYMENT	0.002	0.002	0.000
	(0.002)	(0.002)	(0.000)
GDP_GROWTH	0.056	0.046	0.013*
	(0.043)	(0.041)	(0.007)
POLITICAL_BALANCE	0.004	0.004	-0.001
	(0.006)	(0.006)	(0.001)
CNC	-0.004	-0.003*	-0.001
	(0.003)	(0.002)	(0.002)
Inventor FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Industry × year FE	Yes	Yes	Yes
No. of obs.	817,776	814,661	785,652
Adjusted R^2	0.121	0.121	0.016

Table 4

Likelihood of VC Investment

Table 4 presents OLS regression results related to the effect of the IDD on the likelihood of VC investment. The sample includes all possible VC-firm pairs from 1980 through 2012. We require the VC to be in existence before the firm and to invest in the same industry as the firm within the next 30 days. Utility and financial services industries are excluded from the sample. INVESTMENT is the dependent variable that equals 1 if the VC-firm deal pair takes place and 0 otherwise. IDD is the key independent variable that equals 1 if the state adopts the IDD and 0 otherwise. Detailed variable definitions are provided in the Appendix. Industry-year fixed effects are defined at the 3-digit SIC industry-year level. All continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors clustered at the state level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 4 (continued)

	INVESTMENT		
Variables	1	2	3
IDD	-0.032**	-0.031**	-0.023**
	(0.014)	(0.015)	(0.009)
VC_REPUTATION			-1.698
			(1.676)
EARLY_DUMMY			0.009***
			(0.003)
AGE			-0.010***
			(0.001)
DISTANCE			-0.018***
			(0.003)
INDUSTRY_FIT			0.013**
			(0.006)
CNC			-0.084*
			(0.044)
Lead VC FE	No	Yes	Yes
State FE	No	Yes	Yes
Industry × year FE	No	Yes	Yes
No. of obs.	371,782	371,161	371,161
Adjusted R^2	0.002	0.306	0.323

Table 5

Dynamic Trend

Table 5 presents OLS regression results related to the dynamic trend of the IDD on the likelihood of VC investment. The sample includes all possible VC-firm pairs from 1980 through 2012. We require the VC to be in existence before the firm and to invest in the same industry as the firm within the next 30 days. Utility and financial services industries are excluded from the sample. INVESTMENT is the dependent variable that equals 1 if the VC-firm deal pair takes place and 0 otherwise. In columns 1 and 2, ADOPTION and REJECTION are the key independent variables that equal 1 if the state adopts or rejects IDD, respectively and 0 otherwise. In column 3, ADOPTIONM3_PLUS, ADOPTIONM2, ADOPTIONM1, ADOPTIONP1, ADOPTIONP2, and ADOPTIONP3_PLUS are dummy variables that equal 1 if the state adopts the IDD within 3 or more years, within 2 years, within 1 year, within the past year, within the past 2 years, and within the past 3 or more years, respectively, in reference to the date of investment, and 0 otherwise. REJECTION is a dummy variable that equals 1 if the state rejects the IDD and 0 otherwise. Detailed variable definitions are provided in the Appendix. Industry-year fixed effects are defined at the 3-digit SIC industry-year level. All continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors clustered at the state level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 5 (continued)	INVESTMENT		
Variables	1	2	3
ADOPTION	-0.034**	-0.038**	
	(0.015)	(0.019)	
ADOPTIONM3_PLUS			0.003
			(0.032)
ADOPTIONM2			-0.006
			(0.029)
ADOPTIONM1			0.011
			(0.024)
ADOPTIONP1			-0.005
			(0.019)
ADOPTIONP2			-0.036**
			(0.016)
ADOPTIONP3_PLUS			-0.040**
			(0.017)
REJECTION	0.026**	-0.002	-0.001
	(0.008)	(0.008)	(0.006)
VC_REPUTATION		-1.748	-1.794
		(1.656)	(1.622)
EARLY_DUMMY		0.009***	0.010***
		(0.003)	(0.003)
AGE		-0.010***	-0.010***
		(0.001)	(0.001)
DISTANCE		-0.018***	-0.018***
		(0.003)	(0.003)
INDUSTRY_FIT		0.013**	0.013**
		(0.006)	(0.006)
CNC		-0.083*	-0.083*
		(0.044)	(0.044)
Lead VC FE	No	Yes	Yes
State FE	No	Yes	Yes
Industry × year FE	No	Yes	Yes
No. of obs.	371,782	371,161	371,161
Adjusted R^2	0.002	0.323	0.323

Table 6

Cross-Sectional Tests

Table 6 presents OLS regression results, obtained by exploring the cross section, that are related to the effect of the IDD on the likelihood of VC investment. The sample includes all possible VC-firm pairs from 1980 through 2012. We require the VC to be in existence before the firm and to invest in the same industry as the firm within the next 30 days. Utility and financial services industries are excluded from the sample. INVESTMENT is the dependent variable that equals 1 if the VC-firm deal actually takes place and 0 otherwise. We calculate industry patenting intensity with all Compustat firms by computing the average number of patents in an industry classified at the 3-digit SIC code level. INDUSTRY_PATENTING_INTENSITY_DUMMY is a dummy variable that equals 1 for industries with patent output in the top 50% and 0 for industries with patenting intensity in the bottom 50%. EARLY_DUMMY is a dummy variable that equals 1 if the firm is in the "startup/seed" or "early stage" as indicated by the VentureXpert database and 0 otherwise. DISTANCE_DUMMY is a dummy variable that equals 1 if the firm is located more than 150 miles from the VC and 0 otherwise. We interact these variables with the IDD dummy that equals 1 if the state adopts the IDD and 0 otherwise. Detailed variable definitions are provided in the Appendix. Industry-year fixed effects are defined at the 3-digit SIC industry-year level. All continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors clustered at the state level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 6 (continued)

Variables	INVESTMENT 1	2	3
IDD x INDUSTRY_PATENTING_INTENSITY_DUMMY	-0.009** (0.004)		
IDD x EARLY_DUMMY		-0.021*** (0.007)	
IDD x DISTANCE_DUMMY			-0.037* (0.020)
DISTANCE_DUMMY			-0.073*** (0.010)
DISTANCE	-1.862 (1.539)	-0.018*** (0.003)	
IDD	0.009*** (0.002)	-0.012 (0.013)	0.006 (0.027)
VC_REPUTATION	-0.010*** (0.001)	-1.880 (1.536)	-1.761 (1.555)
EARLY_DUMMY	-0.018*** (0.003)	0.017*** (0.004)	0.008*** (0.002)
AGE	0.013** (0.006)	-0.011*** (0.001)	-0.010*** (0.001)
INDUSTRY_FIT	0.021 (0.021)	0.013** (0.006)	0.014** (0.006)
CNC	-1.862 (1.539)	0.022 (0.020)	0.021 (0.020)
Lead VC FE	Y	Y	Y
State FE	Y	Y	Y
Industry × year FE	Y	Y	Y
No. of obs.	371,161	371,161	371,161
Adjusted R^2	0.316	0.316	0.315

Table 7

Patenting Characteristics

Table 7 presents the results for the tests of underlying mechanisms. The sample includes inventors of all patents filed from 1980 through 2010. In Panel A, we study innovative output. We focus on both the inventor-year level and the startup-year level. In columns 1 and 3, the dependent variable is Ln(PATENT), which is defined as the natural logarithm of the total number of patents produced by the inventor and by the firm, respectively. In columns 2 and 4, the dependent variable is Ln(CITATION), which is defined as the natural logarithm of the number of citations per patent at the inventor and firm levels, respectively. IDD is the key independent variable that equals 1 if the state adopts the IDD and 0 otherwise. In Panel B, we study the underlying channels. In columns 1, 2, 5, and 6, we interact the IDD dummy with a high mobility dummy, which equals 1 if the number of inventor moves scaled by the total number of inventors at the industry level is above the median and 0 otherwise. In columns 3, 4, 7, and 8, we interact the IDD dummy with a high spillover dummy, which equals 1 if the number of within-industry citations scaled by total citations at the industry level is above the median and 0 otherwise. The interaction terms are our variables of interest. The control variables are UNEMPLOYMENT, GDP_GROWTH, POLITICAL_BALANCE, and CNC. Detailed variable definitions are provided in the Appendix. Industry-year fixed effects are defined at the inventor primary patent class-year level for the inventor-level analysis and the 3-digit SIC industry-year level for the startup-level analysis. All continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors clustered at the state level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 7 (continued)

Panel A. Innovative Output

	1	2	3	4
	Inventor-Year Level		Startup-Year Level	
Variables	Ln(PATENT)	Ln(CITATION)	Ln(PATENT)	Ln(CITATION)
IDD	-0.024**	-0.034***	-0.093**	-0.378***
	(0.009)	(0.012)	(0.041)	(0.099)
Controls	Y	Y	Y	Y
Inventor FE	Y	Y	N	N
Firm FE	N	N	Y	Y
Lead VC FE	N	N	Y	Y
State FE	Y	Y	Y	Y
Industry × year FE	Y	Y	Y	Y
No. of obs.	1,242,763	1,193,851	49,980	7,322
Adjusted R^2	0.208	0.512	0.875	0.794

Table 7 (continued)

Panel B. Underlying Channels

	1	2	3	4	5	6	7	8
	Inventor-Year Level				Startup-Year Level			
Variables	Ln(PATENT)	Ln(CITATION)	Ln(PATENT)	Ln(CITATION)	Ln(PATENT)	Ln(CITATION)	Ln(PATENT)	Ln(CITATION)
IDD × HIGH_MOBILITY_DUMMY	-0.012*	-0.051**			-0.020**	-0.196*		
	(0.006)	(0.021)			(0.009)	(0.101)		
IDD	-0.016*	-0.033***			-0.082**	-0.216		
	(0.010)	(0.012)			(0.039)	(0.131)		
IDD × HIGH_SPILLOVER_DUMMY			-0.004	-0.007			0.073	0.061
			(0.004)	(0.011)			(0.046)	(0.303)
IDD			-0.022**	-0.031**			-0.131**	-0.425
			(0.010)	(0.014)			(0.057)	(0.297)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Inventor FE	Yes	Yes	Yes	Yes	No	No	No	No
Firm FE	No	No	No	No	Yes	Yes	Yes	Yes
Lead VC FE	No	No	No	No	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	1,242,763	1,193,851	1,242,763	1,193,851	49,980	7,322	49,980	7,322
Adjusted R^2	0.208	0.512	0.208	0.512	0.875	0.794	0.875	0.794

Table 8

VC Investment Strategy

Table 8 presents OLS regression results related to the effect of the IDD on VCs' investment strategy at startup-round level. The sample includes VC-backed firms from 1980 through 2012. Utility and financial services industries are excluded from the sample. In column 1, the dependent variable is DURATION, which is defined as the natural logarithm of the number of days between the current round date and the final round date divided by the number of rounds left. In column 2, the dependent variable is SYNDICATION, which is a dummy variable that equals 1 if a round is financed by more than one VC and 0 otherwise. IDD is the key independent variable that equals 1 if the state adopts the IDD and 0 otherwise. Detailed variable definitions are provided in the Appendix. Industry-year fixed effects are defined at the 3-digit SIC industry-year level. All continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors clustered at the state level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 8 (continued)

Variables	1 DURATION	2 SYNDICATION
IDD	-0.063**	0.026***
	(0.027)	(0.008)
INCUBATION_PERIOD	0.470***	2.392
	(0.018)	(3.061)
VC_REPUTATION	6.435	0.024***
	(4.881)	(0.003)
Ln(AMOUNT)	0.063***	0.130***
	(0.003)	(0.003)
EARLY_DUMMY	0.128***	-0.011***
	(0.010)	(0.003)
AGE	-0.061***	0.010
	(0.013)	(0.028)
DISTANCE	-0.002	0.006
	(0.003)	(0.007)
INDUSTRY_FIT	0.167***	-0.000
	(0.042)	(0.001)
CNC	0.004	-0.006
	(0.019)	(0.009)
Lead VC FE	Yes	Yes
State FE	Yes	Yes
Industry × year FE	Yes	Yes
No. of obs.	34,060	34,060
Adjusted R^2	0.375	0.265

Table 9

VC Investment Success: Round Level Tests

Table 9 presents OLS regression results related to the effect of the IDD on VC exit outcomes at the round level. The sample includes VC backed firms from 1980 through 2012. Utility and financial services industries are excluded from the sample. In column 1, the dependent variable SUCCESS is a dummy variable that equals 1 if the firm exits by going public or being acquired by another firm during the current round and 0 otherwise. In column 2, the dependent variable IPO is a dummy variable that equals 1 if the venture goes public during the current round and 0 otherwise. In column 3, the dependent variable ACQUISITION is a dummy variable that equals 1 if the venture is involved in a merger or acquisition during the current round and 0 otherwise. IDD is the key independent variable that equals 1 if the state adopts the IDD and 0 otherwise. Detailed variable definitions are provided in the Appendix. Industry-year fixed effects are defined at the 3-digit SIC industry-year level. All continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors clustered at the state level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 9 (continued)

Variables	1 SUCCESS	2 IPO	3 ACQUISITION
IDD	-0.022***	-0.019***	-0.018*
	(0.006)	(0.006)	(0.009)
INCUBATION_PERIOD	-0.068***	-0.020***	-0.065***
	(0.002)	(0.001)	(0.001)
VC_REPUTATION	1.864	0.147	3.317
	(2.009)	(1.784)	(2.328)
EARLY_DUMMY	-0.074***	-0.046***	-0.061***
	(0.003)	(0.003)	(0.003)
AGE	0.054***	0.018***	0.057***
	(0.004)	(0.004)	(0.004)
DISTANCE	-0.002***	-0.000	-0.003***
	(0.001)	(0.001)	(0.001)
INDUSTRY_FIT	0.025*	-0.005	0.022
	(0.014)	(0.010)	(0.014)
CNC	0.008	0.013	0.006
	(0.008)	(0.008)	(0.004)
Lead VC FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Industry × year FE	Yes	Yes	Yes
No. of obs.	53,765	29,144	45,719
Adjusted R^2	0.164	0.085	0.170

Table 10

Uniform Trade Secret Act (UTSA) Tests

Table 10 presents OLS regression results related to the effect of the Uniform Trade Secret Act (UTSA) on the likelihood of VC investment. The sample includes all possible VC-firm pairs from 1980 through 2010 in columns 1 to 3 and from 1980 through 1998 in columns 4 to 6. We require the VC to be in existence before the firm and to invest in the same industry as the firm within the next 30 days. Utility and financial services industries are excluded from the sample. The dependent variable is INVESTMENT, which is a dummy variable that equals 1 if the VC-firm deal pair takes place and 0 otherwise. UTSA_2010 and UTSA_1998 are the key independent variables constructed following Png (2017a, b). Detailed variable definitions are provided in the Appendix. Industry-year fixed effects are defined at the 3-digit SIC industry-year level. All continuous variables are winsorized at the 1st and 99th percentiles. Robust standard errors clustered at the state level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 10 (continued)

Variables	INVESTMENT 1	2	3	4	5	6
UTSA_2010	0.014	0.035	0.021			
	(0.035)	(0.059)	(0.067)			
UTSA_1998				-0.028	-0.007	-0.050
				(0.033)	(0.068)	(0.091)
IDD			-0.026**			-0.060**
			(0.013)			(0.030)
VC_REPUTATION		-5.571***	-5.670***		-5.745*	-6.251**
		(1.782)	(1.783)		(2.982)	(2.674)
EARLY_DUMMY		-0.010***	-0.010***		-0.017***	-0.017***
		(0.001)	(0.001)		(0.003)	(0.003)
AGE		0.012***	0.013***		0.021***	0.021***
		(0.003)	(0.003)		(0.005)	(0.005)
DISTANCE		-0.018***	-0.018***		-0.019***	-0.019***
		(0.003)	(0.003)		(0.003)	(0.003)
INDUSTRY_FIT		0.019***	0.019***		0.005	0.004
		(0.006)	(0.006)		(0.004)	(0.004)
CNC		-0.081**	-0.080*		-0.090**	-0.088**
		(0.040)	(0.040)		(0.042)	(0.042)
Lead VC FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry × year FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of obs.	342,383	342,383	342,383	104,190	104,190	104,190
Adjusted R^2	0.321	0.338	0.338	0.366	0.392	0.393