

# **CROSSED WIRES: ENDORSEMENT SIGNALS AND THE EFFECTS OF IPO FIRM DELISTINGS ON VENTURE CAPITALISTS' REPUTATIONS**

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## **CROSSED WIRES: ENDORSEMENT SIGNALS AND THE EFFECTS OF IPO FIRM DELISTINGS ON VENTURE CAPITALISTS' REPUTATIONS**

Signaling theorists have paid a great deal of attention to the costs of acquiring characteristics that can serve as signals, such as endorsements from reputable third parties. However limited attention has been devoted to the penalty costs associated with providing inaccurate signals and the factors that can exacerbate or attenuate the penalties. We examine the effect of negative feedback loops on venture capital firms' reputations that result from the failures (delistings) of the newly-public firms they once endorsed. Drawing on signaling and attribution theories, we argue that endorsements by reputable VC firms create high expectations that when violated cause stakeholders to look for scapegoats, resulting in reputational damage to the endorsing VCs. We find empirical support for this argument, and for the attenuating effect of both post-IPO market performance and survival. Our study contributes to the conversation about endorsements as signals, and empirically tests the implicit assumption that endorsements place the reputation of the endorser at risk.

How do you decide whether to try a new restaurant, see a new doctor, or stay at a new hotel? Odds are that you look for different clues, or signals, that you are likely to have a good experience—chief among them is the organization’s or individual’s reputation. Defined as an intangible asset based on broad public recognition of the quality of a firm’s activities and outputs (Rindova, Williamson, Petkova, & Sever, 2005), a firm’s reputation is critical to its success and forms the basis for observers’ expectations about its ability to create future value (Fischer & Reuber, 2007; Fombrun, 2001; Lange, Lee, & Dai, 2011; Lee, Pollock & Jin, 2011). New organizations, however, often lack the reputations necessary to help them survive, contributing to their “liabilities of newness” (Stinchcombe, 1965).

When information on an organization’s reputation is unavailable, we often turn to the reputations of the firm’s affiliates as signals of its likely quality and capabilities (Lee et al., 2011; Petkova, 2012; Rindova, Williamson, Petkova, & Sever, 2005). For example, when deciding whether to invest in or do business with new firms, affiliations with prominent and reputable third parties such as venture capital (VC) firms, investment banks, and alliance partners are treated as valuable signals (e.g., Lee & Wahal, 2004; Pollock, Chen, Jackson & Hambrick, 2010; Stuart, Hoang, & Hybels, 1999). Signaling theory argues that signals are valuable because they are costly to the signalers (Spence, 1973); in the case of endorsements, the assumed cost is that the endorsers’ reputations will be damaged if the firms they endorse perform poorly. That is, by putting their own reputations at risk, endorsers signal the endorsed firms’ quality and potential.

The endorsing firm’s involvement also suggests that the new firm has access to the endorser’s capital, skills and expertise, networks, and other resources that can enhance its probability of future success (e.g., Gorman & Sahlman, 1989; Jain & Kini, 2000; Pollock & Gulati, 2007). Studies have shown that startups often enjoy favorable market valuations and

superior firm performance after receiving endorsements from prestigious VC firms (Lee et al., 2011; Pollock et al., 2010), underwriters (Gulati & Higgins, 2003; Sanders & Boivie, 2004), top executives (Higgins & Gulati, 2006; Pollock et al., 2010), and directors (Daily & Dalton, 2001; Deutsch & Ross, 2003). As a result, new organizations often pay substantial premiums to garner these endorsements (Chen, Hambrick, & Pollock, 2008; Hsu, 2004).

But what happens when these endorsements prove to be unreliable signals and the new firm fails to meet stakeholders' expectations? If your experience with the new restaurant, doctor or hotel is bad, does that influence the way you view the person who recommended them to you, and whether you will be as willing to accept their endorsement in the future? And what contextual factors influence the extent to which you blame them for your experience? In other words, is the endorser's reputation really "at risk," and damaged if the firm they endorsed performs unsatisfactorily? And what influences the extent of the damage?

Connelly and colleagues (2011: 61) noted that, "the management literature is mainly focused on signal costs, containing less discussion of the role of penalty costs, which are a form of negative feedback from the receiver" (Connelly, Certo, Ireland, & Reutzel, 2011). They further noted that "The signaling environment on the whole is an under-researched aspect of signaling theory" (2011: 62). Indeed, a plethora of studies have shown that new firms benefit from prominent affiliations (e.g., Carter & Manaster, 1990; Petkova, 2012; Pollock et al., 2010; Stuart et al., 1999), and that negative events (Ahrens, 2010; Mishina, Block, & Mannor, 2012; Pfarrer, Decelles, Smith, & Taylor, 2008; Rhee & Haunschild, 2006) or tainted endorsers (Shymko & Roulet, 2017) adversely affect a focal firm's reputation. However, these studies only examine the effects of endorsements on the endorsed firm, rather than following the feedback loop back to examine what happens to the endorsers if the signal turns out to be incorrect.

In this study we draw on signaling theory (e.g. Connelly et al., 2011; Spence, 1973) and attribution theory (Heider, 1958; Kelley, 1973; Weiner, 1986) to test this critical assumption and consider the degree to which endorsers actually put their reputations at risk when endorsing other firms. We also explore the factors that can enhance or attenuate the damage experienced. We specifically examine instances where newly-public firms are delisted from their stock exchange for reasons other than being acquired by another firm (e.g., regulatory violations, poor financial performance, or failing to meet their exchange's minimum listing requirements), and whether the VCs who funded them suffer a reputational penalty. Finally, we consider how the success of the firm's IPO, its post-IPO performance, and the time between the IPO and its delisting affect the magnitude of the penalty.

While successful IPOs are visible, reputation-enhancing events for the VCs who funded them (Lee & Wahal, 2004; Pollock, Lee, Jin & Lashley, 2015), whether the VCs' reputations are actually damaged if the firms are subsequently delisted is unclear. The process of going public involves scrutiny from the Securities Exchange Commission (SEC) as part of the registration process, and from potential investors during "road shows" prior to the IPO (Husick & Arrington, 1998). Thus, firms that go public receive the approval of many stakeholders, not just VCs (Certo, 2003). Furthermore, a public listing is often the start of a new chapter in the firm's life that is decoupled from its VC backers. In fact, the VC's job is largely viewed as complete at the time of the IPO (Pollock et al., 2010), an event that is treated as a successful "exit" or "liquidity event" for VCs (Gorman & Sahlman, 1989; Loughran & Ritter, 1995).

This setting is ideal for exploring the reputational penalties from providing signals later perceived as inaccurate because VCs rely heavily on their reputations to raise investment funds and gain access to promising startups (Lee et al., 2011; Pollock et al., 2015), and the startups

they fund benefit greatly from the signaling value these affiliations provide (Gulati & Higgins, 2003; Lee et al., 2011). VCs tout the successful firms they have funded when marketing themselves to investors and startups (Fund, Pollock, Baker & Wowak, 2008), and studies have shown that younger VCs may even “grandstand” —that is, take a firm public earlier than it should, leaving more money on the table to establish a favorable reputation with investors (Gompers, 1996; Lee & Wahal, 2004). However, few if any studies have examined the consequences for VCs when the firms they take public fail to live up to the expectations their endorsements created.

Our study makes several contributions. First, we contribute to the signaling literature by theorizing about the negative feedback loop from a signal’s receivers to the sender, and testing whether there is a penalty or cost to sending inaccurate signals (Connelly et al., 2011). We advance our understanding of signaling dynamics and the signaling value of endorsements by developing a more nuanced understanding of how this negative feedback loop works, and by examining the factors that can influence the strength of the feedback loop and size of the reputation penalty. These effects occur even when endorsers have little or no direct influence over the other firm’s actions, and vary depending on the post-IPO performance of the endorsed firm. Furthermore, we also find evidence that when a firm has multiple endorsers, not all of them bear the same reputational risk. Finally, we contribute to the entrepreneurship literature by empirically showing that there may be long-term reputational costs for VCs if their actions increase the short-term value of portfolio firms at the expense of their future viability.

## **THEORY AND HYPOTHESES**

All new firms face the fundamental challenge of reducing the information asymmetries that exist between the firm and key stakeholders with whom it wants to engage (Carter &

Manaster, 1990; Higgins & Gulati, 2003; Petkova, 2012; Pollock et al., 2010; Pollock & Gulati, 2007; Stuart et al., 1999). As such, new, young firms search for opportunities to provide signals that can reduce these information asymmetries. Spence (1973) argued that characteristics which are visible and costly to acquire can be used to signal an actor's unobservable quality to others, and thereby reduce information asymmetries. One such signal is affiliations with, or endorsements by reputable actors (Petkova, 2012). Endorsements from established and reputable organizations signal that the new organizations may have the qualities needed to succeed in the future (Carter & Manaster, 1990; Lee et al., 2011; Stuart et al., 1999). These affiliations are visible to others because of the endorser's prominence, and are valuable because they enable new firms to "borrow" (Petkova, 2012: 384) some of their endorser's reputation. They are also costly because reputable actors are presumably putting their reputational capital at risk in a visible way by endorsing the new firm (Pollock, 2004). The new firm's failure to perform should reflect poorly on the endorser and damage its reputation to some degree.

For young ventures whose legitimacy and futures are uncertain (Aldrich & Fiol, 1994; Stinchcombe, 1965), being affiliated with prominent and reputable third parties such as VC firms, investment banks, and alliance partners can make an important difference in their success and life chances (e.g., Lee & Wahal, 2004; Petkova, 2012; Pollock, et al., 2010; Stuart et al., 1999). Indeed, given that less than one percent of new companies receive VC financing in a given year (Rao, 2013), and that only a fraction of those are funded by the highest-reputation VCs (Lee et al., 2011), endorsement by a VC provides a powerful signal to potential stakeholders such as investors (Pollock et al., 2010), customers (Reuber & Fischer, 2005) and alliance partners (Pollock & Gulati, 2007). Such reputation borrowing is thus an important tool new firms can use to build their own reputations (Petkova, 2012).

Research has established the benefits of these reputable affiliates to the firms being endorsed; however, the presence and extent of a negative feedback loop when the signal subsequently proves inaccurate has generally been assumed, rather than empirically assessed (Connelly et al., 2011; Gammoh et al., 2006). The signaling literature has paid limited attention to when and to what extent there is a feedback loop that transfers stakeholders' disappointments back to the endorsers, or whether and when the endorsers may be able to insulate themselves from potential negative consequences. While previous studies have focused on how positive or negative endorsement signals—such as third-party endorsements of bankrupt firms (Xia, Dawley, Jiang, Ma, & Boals, 2016), or theater companies' endorsements by tainted firms (Shymko & Roulet, 2017)—affect the endorsees, scholars have not considered whether endorsers suffer any reputational damage when the actors they endorse fail to perform.

We explore these questions in the context of newly-public firms' delistings and the subsequent changes in the endorsing VCs' reputation. Consistent with prior research (Gompers, 1996; Hochberg, Ljungqvist & Lu, 2007; Lee et al., 2011; Ma, Rhee & Wang, 2013), our hypotheses focus on lead VCs. Lead VCs typically hold the largest investment stake in the company, and take primary responsibility for interfacing with the company's leadership and coordinating the actions of the other VCs (Ma et al., 2013).

### **Delisting of Newly Public Firms**

Endorsements by VCs raise expectations about newly-public firms' potential. As such, when these newly-public firms delist, stakeholders are disappointed. Approximately 600,000 new businesses are started each year in the U.S., and of those, only about 1,000 businesses receive VC financing. Thus, only about one-sixth of one percent of new ventures receives VC funding (Kaplan & Lerner, 2010). However, even this increased level of screening does not

ensure success; of these firms, only about 22.5 percent eventually manage to go public (Gompers & Lerner, 2004). Although this rate is much higher than for all startups generally, there is still great uncertainty about a startup's likelihood of successfully going public, even with VC backing. Accordingly, an IPO is a rare accomplishment (Guler, 2007) that builds positive expectations regarding a newly-public firm's potential.

Further, every newly-public firm undergoes the intense scrutiny of various stakeholders (Pollock et al, 2010). The Securities Exchange Commission (SEC) verifies that all material information about the firm has been disclosed, and that it meets all regulatory requirements for going public (Husick & Arrington, 1998). Knowledgeable potential investors also attend its "road show" (Certo, 2003)—where the startup's management team and underwriters travel all over the country, and increasingly, the world, pitching the company to potential investors—before making their investment decisions. Endorsements by reputable third parties provide additional confirmation of the firm's potential (Lee et al., 2011) and set expectations for a promising future. However, despite this scrutiny (Sanders & Boivie, 2004), delistings still occur for various reasons that are often beyond the control of VCs, and sometimes the firm.

Individual stakeholders are disappointed when their expectations are not met, and the higher their expectations, the greater their disappointment (Burgoon, 1978). Since third-party endorsements raise expectations, they also increase the "negative expectancy violation" (Burgoon & Le Poire, 1993; Kim, 2014)<sup>1</sup> that occurs when those they endorsed fail to perform, because their endorsement makes the disappointment all the more surprising (Burgoon, 1978; Burgoon & Le Poire, 1993). In our context, when startups are backed by reputable VCs, expectations are raised about how they will perform, and so is the disappointment when

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<sup>1</sup> The reverse is also true: if stakeholders have negative or low expectations about the other's behaviors but are pleasantly surprised, then positive expectancy violations occur.

delistings occur (c.f. Gulati & Higgins, 2003; Lee et al., 2011; Pollock et al., 2010). We seek to understand how stakeholders' disappointment about these delistings can create a negative feedback loop that damages the endorsing lead VC's reputation.

### **Attribution Processes**

Human beings have an innate desire to attribute successes and failures to individuals' actions, even when the actual causes are beyond the individuals' control (Heider, 1958; Kelley, 1973; Mitchell, 1982). While individuals are eager to claim responsibility for positive outcomes, they are equally eager to find scapegoats and blame others for negative outcomes (Bowman, 1976, 1978), and they tend to give lesser weight to situational constraints when making attributions than may be warranted (Jones & Harris, 1967). For example, Kang (2008) used attribution theory to explain why innocent firms were punished if they had board interlocks with firms undergoing SEC investigations for potential accounting irregularities. Gomulya and Boeker (2016) showed that while inside directors tend to protect CEOs following poor firm performance that can be attributed to outside causes (Salancik & Meindl, 1984; Staw, McKechnie & Puffer, 1983), they will replace CEOs following earnings restatements where the causal attribution is clearly internal.

In our context, delistings that do not result from a merger are typically regarded as failures (Fisher & Pollock, 2004). As such, these failed companies' stakeholders are likely to look for ways to blame others, rather than accept the blame for making a poor decision themselves. When combined with negative expectancy violations, the desire to find a scapegoat for this failure becomes especially strong. We argue that a primary scapegoat for delistings may be the endorsing VCs that were intimately involved with the delisted firm, and that raised stakeholders' expectations with their endorsements.

## **Negative Feedback Loop from IPO Firm Delisting to the Endorsing VC's Reputation**

While the culprits behind delistings are often beyond any single individual's or firm's control, attribution theory suggests that stakeholders still want a scapegoat to blame (Boeker, 1992; Pfeffer & Salancik, 1978). We argue that they will hold accountable entities that they can identify and that influenced their expectations, even if they had only limited or even no connection with the causes of the delisting. As noted above, VC firms play only a partial role in the certification process (Certo, 2003; Gulati & Higgins, 2003; Lee et al., 2011; Pollock, et al., 2010). However, while it is difficult or impossible for stakeholders to identify and single out road show attendees and SEC examiners, it is easy to identify the lead VCs who endorsed these newly public firms and raised their expectations. Scapegoating VCs for an IPO firm's delisting is thus both plausible and possible.

Further, VCs create expectations about a startup's potential. Like many third parties, VCs reduce perceived uncertainties by providing startups with certification benefits (Gulati & Higgins, 2003; Lee et al., 2011). In contrast to other affiliates such as underwriters, who only provide certification benefits (Pollock et al., 2010), VCs also contribute to a startup's skills and capabilities by providing direct benefits that affect their portfolio firms' operating activities (Garg, 2013; Lee et al., 2011). VC firms contribute financial capital and provide help in formulating and implementing strategy, recruiting key personnel and acquiring needed resources (Garg, 2013; Jain & Kini, 2000; Sapienza, 1992). Because of their financial, operational and reputational investments in their portfolio firms, VCs have a vested interest in seeing startups succeed, and stakeholders therefore rely on VCs' endorsements as signals of the startups' likely success.

Given this, when newly-public firms delist shortly after their IPOs, an attribution process is triggered that feeds back to their endorsing lead VCs, damaging their reputations. Because of the direct role VCs can play in nurturing startups, this negative feedback loop may occur even if the VCs have little involvement with firms after their IPOs (Field & Hanka, 2001). Thus, we argue that the disappointment created by delistings coupled with the need to blame a scapegoat will generate a reputation penalty for the endorsing lead VCs. We therefore hypothesize,

*Hypothesis 1: Delistings by newly-public, VC-backed firms will be negatively related to the lead VC's subsequent reputation.*

This hypothesis captures the baseline relationship between an IPO firm delisting and the endorsing lead VC's reputation. However, signals can be distorted or clarified by environmental factors (Connelly et al., 2011). Thus, the magnitude of the reputational penalty the VC may receive can be enhanced or attenuated by the circumstances in which the relationship is embedded (Lester, Certo, Dalton, Dalton & Cannella, 2006; Sanders & Boivie, 2004). Our following hypotheses examine the moderating effects of different contextual factors (Graffin, Haleblan & Kiley, 2016) on the extent of the reputation damage endorsing VCs bear. Specifically, we consider the effects of the following factors on the magnitude of the lead VC's reputation penalty: market reactions at the time of the IPO (i.e., the firm's underpricing), the firm's post-IPO firm performance, and the time between the firm's IPO and delisting.

### **Moderating Effects of Contextual Factors**

***IPO underpricing.*** Underpricing, defined as the jump in stock price on the day a firm's stock begins trading on a public exchange (Ibbotson & Ritter, 1995; Pollock & Gulati, 2007), creates positive expectations about a newly-public firm's promise (Pollock & Gulati, 2007). Although a variety of reasons have been posited to explain IPO underpricing (Ibbotson & Ritter, 1995; Tsang & Blevins, 2015), research has shown that high levels of underpricing can lead to

positive post-IPO outcomes for newly-public firms, including more traffic to their web sites, more alliance formations, greater analyst coverage, and more positive media coverage (e.g., Demers & Lewellen, 2003; Pollock & Gulati, 2007; Pollock, Rindova & Maggitti, 2008; Rajan & Servaes, 1997). The positive signaling effects of underpricing also persist over time (Pollock & Gulati, 2007). Prior research has also shown that high levels of underpricing benefit the VCs who fund the startups because they are credited with being able to spot and/or develop the most promising firms (Pollock et al., 2015), which is reflected in their ability to raise capital for subsequent funds (Lee & Wahal, 2004). High underpricing might even be treated as validation by investors of the VC firm's endorsement; as such, stakeholders may be even more likely to attribute responsibility to and scapegoat the VCs who backed the firm. Thus, we expect that when a delisted portfolio firm experienced greater underpricing, the subsequent damage to the endorsing VC's reputation will be greater. We therefore hypothesize,

*Hypothesis 2: The negative relationship between delisting and the lead VC's subsequent reputation will be stronger the higher the portfolio firm's underpricing.*

**Post-IPO firm performance.** Just as short-term market reactions at the time of IPO can enhance the negative effect of delistings on VC reputation, how the firm performs after its IPO also plays a significant role. That is, the likelihood that VCs are blamed by stakeholders is often a function of how the startup performs after their "independence" from the VC.

In assessing whether the endorsing lead VC provided an accurate or inaccurate signal at the time of the startup's IPO, it is important to consider what the endorsement is purportedly signaling. As noted earlier, VC firms provide a variety of resources beyond money that can enhance a start-up's success (Garg, 2013; Jain & Kini, 2000; Sapienza, 1992). VCs provide these resources because they help startups overcome their liabilities of newness (Hannan & Freeman, 1977; Stinchcombe, 1965). These liabilities include a lack of organizational structures and

routines for dealing with challenges that more established firms have developed over time, a need to create and fill new roles, a lack of trust based on prior interactions, and a need for relationships that can provide firms with necessary resources (Stinchcombe, 1965).

Once newly-public firms pass a certain performance threshold (Fama & French, 2004; Mouri, Sarkar, & Frye, 2012), stakeholders perceive that they have overcome these liabilities of newness and expect them to continue performing well. Thus, to the extent that stakeholders' expectations are met following an IPO, they are more likely to feel that the IPO firm has lived up to the promise signaled by the VC's endorsement. The independence and performance demonstrated by newly-public firms after their IPOs therefore helps to cognitively decouple portfolio firms from their VCs. In this way, any subsequent failures by IPO firms will be less likely to have negative feedback effects on endorsing VC reputations (Burgoon & Le Poire, 1993; Graffin et al., 2016). Rather, stakeholders will be more likely to attribute a turn in the IPO firm's fortunes to factors that its VCs could not have foreseen or influenced. We therefore hypothesize,

*Hypothesis 3: The negative relationship between delisting and the lead VC's subsequent reputation will be weaker the higher the portfolio firm's performance in the years following its IPO.*

***Post-IPO survival duration.*** Another factor that may decouple portfolio firms from their VCs is their ability to survive (Hannan & Freeman, 1977; Stinchcombe, 1965). For new firms, continued survival suggests that they have overcome the liabilities of newness that VCs are expected to help new firms address. The longer newly-public firms survive, the more likely stakeholders are to believe that VCs have fulfilled their expectations, that the signals from the VCs' endorsements were accurate, and that any eventual setbacks were due to reasons beyond what the VCs should be expected to predict or influence. In other words, like higher post-IPO

firm financial performance, surviving longer post-IPO signals greater independence and cognitively decouples portfolio firms from their VCs (Graffin et al., 2016). Thus, longer post-IPO survival durations before delistings should at least partially attenuate the reputation penalty that the endorsing VCs might receive from delistings. We therefore hypothesize,

*Hypothesis 4: The negative relationship between delisting and the lead VC's subsequent reputation will be weaker the longer the portfolio firm survives following its IPO.*

## METHODS

### Data

Our initial sample comes from a dataset of IPOs provided by Jay Ritter (see <http://site.warrington.ufl.edu/ritter/ipo-data/>) and includes offering dates, offering prices, filing price ranges, closing prices, SIC codes and underwriter prestige rankings. We supplemented the IPO data with data on VC investments from *Securities Data Corporation's (SDC) VentureXpert* database. We obtained data on the number of VC firms with an investment in each IPO at the time of the offering, the round dates, and the dollar value of each investment by each VC firm annually from 1990 to 2010. We distinguished VCs from buyout firms based on investment round. VCs' investments take place in rounds that are classified as Seed, Startup, Startup Financing, Early Stage, First Stage Financing, Expansion, Later Stage, Balanced, or Research and Development. Manual web searches on sample firms in all investment categories identified by *VentureXpert* confirmed that these categories effectively include only VCs in our sample and exclude other types of private equity firms. We then collected market performance data from the *Center on Research in Security Prices (CRSP)*, and firm financial data from *COMPUSTAT*. We also collected data from firm's annual 10(k) and 8(k) filings, when possible, if the necessary firm financial data were missing from *COMPUSTAT*.

Since we are predicting the effect of delistings on the reputations of VCs that supported the firm, we applied several criteria to identify our target firms. First, the IPO firms must be backed by VCs. Second, we only included firms whose delisting was for negative reasons, as coded by *CRSP* and as agreed to by the SEC. Acquisitions were not treated as delistings since they are generally considered a successful outcome for young firms (Guler, 2007) and do not violate stakeholders' expectations. We elaborate on this point when we describe the delisting variable below. Third, we only included firms where VC reputation data were available. Finally, missing data from *COMPUSTAT* and *CRSP* further reduced our sample size to a final sample of 151 unique lead VCs. We then constructed a panel dataset from the year of IPO up to and including the fifth year after the IPO for a total of up to six years of observations per firm, resulting in 1,587 VC-firm-year observations.

T-tests comparing the firms in our sample to IPO firms that were excluded (i.e., that were not VC-backed, were acquired, or for which VC reputation data was unavailable) showed that there were no differences in terms of age, revenues, total assets, ROA, and market-to-book ratio at the time of IPO ( $p=0.532$ ,  $0.618$ ,  $0.208$ ,  $0.579$ , and  $0.641$ , respectively). The excluded and included samples also did not differ in terms of industry membership, which we tested by comparing the distribution of their 2-digit SIC codes using both Pearson's chi-squared test ( $p=0.899$ ) and a two-sample Kolmogorov-Smirnov test for equality of distributions ( $p=0.376$ ). Together, these tests show that sample selection bias is not a concern when drawing inferences from the included sample only.

## **Measures**

***Dependent variable.*** Our dependent variable is the lead VC's reputation following a delisting event. Consistent with prior research, we defined the lead VC as the VC who owned the

greatest percentage of the company's stock at the time of its IPO (Ma et al., 2013; Wright & Lockett, 2003). To measure VC reputation, we used a modified version of the LPJ VC Reputation Index developed by Lee, Pollock & Jin (2011), which is available at [http://www.timothypollock.com/vc\\_reputation.htm](http://www.timothypollock.com/vc_reputation.htm).

Lee and colleagues created an objective, multi-item, time-varying index that increases the reliability of the VC reputation measure and reduces the effects of random error (Boyd, Gove & Hitt, 2005). Their measure captures the theoretical dimensions of visibility and firm quality Rindova and colleagues (2005) identified by averaging the following formative indicators of VC firm reputation: (1) average of the total dollar amount of funds under management over the prior five years (*'Amount of funds'*), (2) average of the number of investment funds under management in the prior five years (*'Number of funds'*), (3) number of startups invested in over the prior five years (*'Number of companies'*), (4) total dollar amount of funds invested in startups over the prior five years (*'Investment amount'*), and (5) number of companies taken public in the prior five years (*'Number of IPOs'*). These measures were standardized and summed, and the total score was then converted to a 100-point scale comparable across years. Although Lee and colleagues also included VC firm age in their index, we excluded VC firm age because the value of VC firm age increases monotonically as long as the VC firm does not fail, and thus cannot be influenced by the dynamics examined in this study. Nonetheless, we also tested our hypotheses using the original LPJ index that includes VC age and found similar results.

The LPJ index has been used in several prior studies (e.g., Hallen & Pahnke, 2016; Lee et al., 2011; Pahnke, McDonald, Wang & Hallen, 2015; Park & Steensma, 2013; Petkova, Wadhwa, Yao & Jain, 2014; Pollock et al., 2015), and this is the only measure of VC reputation

that covers our entire period of study.<sup>2</sup> It also offers the advantage that, as a multi-item measure, it more closely approximates the “true” value of the latent construct, compared to single-item indicators used in prior research that are more subjected to bias and random variation (Brown, 2006; Hinkin, 1995; Worthington & Whittaker, 2006). In our robustness tests, we also examine the relationship between delistings and each individual component of the index.

A potential concern posed by this measure is that it is an objective measure of reputation based on formative behavioral and performance indicators, rather than a perceptual measure of reputation (Hallen & Pahnke, 2016; Pollock et al., 2015). However, recent research by Hallen and Pahnke (2016) using a perceptual measure of VC reputation<sup>3</sup> validated that the LPJ index accurately captures entrepreneurs’ perceptions of VCs’ reputations. They showed that when entrepreneurs were motivated and in a network position to assess a VC’s reputation accurately, their perceptions were consistent with the LPJ index measure.<sup>4</sup> Further, even if stakeholders’ perceptions of delistings are negative and they blame the VCs, this does not necessarily mean that the stakeholders will change their actual behaviors. Our objective measure requires behavioral, as well as perceptual changes by stakeholders, and thus offers a more conservative test of our hypotheses. Finally, the LPJ index does not consider whether a portfolio firm is inactive or delisted in measuring the index’s components, ruling out the possibility that delisting

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<sup>2</sup> Earlier studies using LPJ index (e.g. Lee, Pollock, & Jin, 2011; Pollock, Lee, Jin, & Lashley, 2015) have not examined how delistings affect VC reputation. As such this study is different from and complements prior research.

<sup>3</sup> This study used a snapshot of data obtained from the website TheFunded in May of 2010 to assess the reputations of VCs that made at least one investment between 2009 and 2013. Thus, it covers a different and shorter period than the LPJ index, which provides annual reputation data from 1990 to 2010.

<sup>4</sup> While they also find unmotivated entrepreneurs’ perceptions were less accurate, as they used an absolute value measure that does not differentiate between over- and under-estimating a VC’s reputation, we cannot assess whether those who were less motivated tended to over- or under-estimate VCs’ reputations. Further, in our context, if stakeholders are unmotivated to collect the information necessary to assess a VC’s reputation, it also stands to reason that they will not pay attention to whether the firms they funded delist, and thus are unlikely to punish the VC for the delisting.

events, by construction, decrease VC reputation even in the absence of the theoretical mechanism we propose. We address the limitations of this measure in the discussion section.

For our dependent variable, we used the VC firm's reputation index value in the year after the focal year ( $t+1$ ). However, as this value is likely to be determined at least in part by the VC firm's past reputation (Pollock et al., 2015), we also controlled for the VC firm's reputation index value in the focal year ( $t$ )<sup>5</sup>. When using panel data, inserting a lagged dependent variable as a covariate can introduce biases (Greene, 2012; Nickell, 1981). To address this concern, we used the Arellano-Bond estimation for our analyses, as discussed below.

### ***Independent Variables***

*Delisting.* Our key independent variable is whether or not a firm is delisted within five years following its IPO, which reflects IPO firm failure. We adopted the five-year cutoff because during this period a firm is typically still considered a newly-public firm (Ahmad & Jelic, 2014; Fischer & Pollock, 2004; Loughran & Ritter, 1995; Welbourne & Andrews, 1996). After five years, IPO firms are considered "seasoned" public entities. Empirically, we also examined the Kaplan-Meier survival estimate by examining the survival of the sample firms for up to 25 years after their IPOs. The survival estimate showed that the biggest drop in the survival rate occurred in the fifth year, validating our use of the five-year cut-off.

Consistent with prior research (Fischer & Pollock, 2004), we included delistings by the primary exchange on which a firm is traded with delisting codes between 500 and 587. *CRSP* provides codes that indicate reasons for a delisting. Codes between 500 and 587 are associated

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<sup>5</sup> Failure to control for the past value of the dependent variable in the presence of path dependence leads to biased estimates (Greene, 2012).

with negative events such as firm bankruptcy and the firm's inability to maintain the minimum size, shareholder number, and stock price requirements for continued listing on the exchange.<sup>6</sup>

Our theory does not require assessing whether some reasons for delistings are “more negative” than others, and attempting to do so raises several thorny issues, such as whether all stakeholders would perceive different reasons the same way. Thus, we constructed a time-varying dichotomous *Delisting* measure that is coded 1 in the year a VC is affected by the delisting of an IPO firm and 0 otherwise. Out of 370 firms in the final sample, 31 firms (8.4%) were delisted for the reasons we described above. The delisting occurred with the following pattern: 0 firms delisted in the IPO year, 4 delistings occurred in the first year after the IPO, 4 occurred in the second year, 11 occurred in the third year, 5 occurred in the fourth year, and 7 occurred in the fifth year after the IPO. The relatively small number of delisting makes our tests more conservative, because to yield any significant findings the relationship between delisting and VC reputation must be quite systematic and the effect size large.<sup>7</sup>

*IPO underpricing* was operationalized as the difference between a firm's opening and closing stock prices on its first day of public trading (i.e., closing price minus opening price) divided by its opening stock price, multiplied by 100. The data for this variable comes from the *CRSP* database. This measure was transformed into its natural logarithm to reduce the effect of

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<sup>6</sup> The specific reasons are: bankruptcy, issue withdrawn by underwriter, corporate governance violation, delinquent in filing, non-payment of fees, not meeting exchange's financial guidelines for continued listing, failing to meet exception or equity requirements, insufficient assets, capital, surplus, equity, market makers, number of shareholders, price fell below acceptable level, or for protection of investors and the public interest.

<sup>7</sup> One potential issue with our relatively small number of delistings is that they may somehow be biased relative to delisted firms not included in our sample. We identified 156 delistings that occurred during our study period that we could not include in our sample because we were unable to obtain complete data for all our measures. We conducted t-tests comparing the 31 firms in our sample to these firms on the following dimensions: IPO year, delisted year, founding year, total funds raised, total number of rounds, total number of VCs, age, revenue, ROA, market-to-book ratio, and VC ownership. There were no significant differences between the delisted firms in our sample and the other delisted firms along any of these dimensions, suggesting our delisted firms were representative of delisted firms, more generally.

outliers (we added the positive equivalent of the minimum value plus one to avoid log transformation of negative values) (Pollock & Gulati, 2007).

The newly-public firm's *post-IPO performance* was operationalized in two ways: 1) using the newly-public firm's industry-adjusted return on assets (*ROA*) for each year following its IPO, and 2) using the newly-public firm's market-to-book ratio (*MTB*) for each year following its IPO.

Each firm's annual *ROA* was adjusted for capital expenditures (Barber & Lyon, 1996) and calculated using a firm's operating income before taxes, depreciation and special items, minus its capital expenditures, which was then divided by the firm's total assets. Adjusting for capital expenditures helps offset the use of aggressive accounting practices by young firms who are about to go public. These firms face substantial pressure to make their performance look as good as possible, and are particularly prone to managing their operating performance (Teoh, Wong, & Rao, 1998). We further took industry differences into account by subtracting the IPO firm's *ROA* from the average *ROA* of all publicly listed companies in the firm's two-digit SIC code whose data were available from *COMPUSTAT* for the focal year. Because our data included some outliers, to minimize their effect we followed common practice and Winsorized *ROA* by 5 percent in each tail, where all data below the 5th percentile were set to the 5th percentile value, and data above the 95th percentile set to the 95th percentile value (Dechow, Ge, & Schrand, 2010).

The *MTB* ratio was calculated annually as the ratio of market value (operationalized using the annual number of shares outstanding multiplied by the firm's annual stock price) over total assets. We obtained these data from the *COMPUSTAT* and *CRSP* databases. We transformed this ratio into its natural logarithm to reduce the effect of extreme values. Given that these ratio

variables may fluctuate around the time of delisting, we used a rolling average of the prior three years. For companies that were delisted less than three years after IPO, we used all the available information up to the current year.

Finally, the number of years a newly public firm has survived after IPO, or *surviving years*, was calculated on an annual basis by subtracting the IPO year from the current year.

***Control variables.*** Because the statistical approach we employed already controls for firm and VC fixed effects through orthogonal deviation (one variant of first-differencing; see Arellano, 2003:17), we did not include time-invariant control variables such as the year a firm goes public, or various pre-IPO characteristics of the firm, such as the number of investors and the number of investment rounds, in our models. However, we did include a number of time-varying control variables to rule out alternative explanations.

***Firm characteristics control variables.*** We controlled for the following firm-level variables. First, we controlled for the newly-public firm's age (*'Company age'*), measured as the current year minus the year the firm was founded. Since firms varied significantly in age, we transformed this measure into its natural logarithm. We obtained the firm founding date from the firm prospectus available from the SEC's *EDGAR* website. We also controlled for the newly-public firm's size (*'Revenue'*), which was operationalized as the natural log of the firm's revenues<sup>8</sup> each year. Since some of the firms in our sample had no revenues in a given year, we added one to all values before transforming them. We obtained data for revenues from *COMPUSTAT*. Next, we added *year dummies* to control for year-specific effects, and thus any changes or variations that might exist or occur in the external environment at different years. For brevity however, we did not explicitly list the year dummies in our regression tables.

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<sup>8</sup> Other common proxies for size are total assets and market capitalization, but these measures were used to calculate the MTB ratio, and total assets were also used in calculating ROA.

Additionally, a lead VC's ownership in a firm can vary significantly relative to other VCs' ownership from year to year. To address the possibility that stakeholders may blame a VC more the greater its stock ownership, we controlled for the level of *VC ownership*. We first identified the lead VC's ownership at IPO using data from the *VenturXpert* database. We then manually coded the VCs' ownership each year following the IPO based on the values in the newly-public firms' annual reports. The result is a time-varying measure of the VC's ownership in the firm.

***Portfolio characteristics control variables.*** VCs can invest in multiple firms; thus, just as a focal firm influences its VC's reputation, so can the other firms in the VC's investment portfolio. We therefore controlled for the characteristics of the firms in a VC's investment portfolio by including the following average values of its portfolio firms' characteristics (excluding the focal newly-public firm): *company age*, *revenue*, *ROA*, *MTB* and *VC ownership*.

Further, a delisting by any of the other portfolio firms a VC invested in could also hurt its reputation. Given that our focal independent variable is a delisting that is experienced by a focal portfolio firm, it is crucial to isolate the effect of delistings of non-focal portfolio firms funded by the same VC. We controlled for *cumulative prior delisting events*, which sums the delisting events by all of the VC's portfolio firms except for the focal firm's delisting for a rolling five-year window beginning with the current year<sup>9</sup>. We used this five-year window because the effects of delistings by the other portfolio firms can persist for more than one year.<sup>10</sup>

***VC characteristics control variables.*** We also introduced various controls for the characteristics of the VC that could influence its reputation. First, its future reputation will be

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<sup>9</sup> We also re-ran our analyses using the number of delistings for all prior years, and the results were the same.

<sup>10</sup> In this circumstance, inserting just one-year lagged dependent variables may not be sufficient to capture all the effects of past delistings. We thank our associate editor for pointing this out.

related to the *VC's past reputation*, so we controlled for the VC firm's reputation index value in the focal year ( $Y_t$ ).

Next, we controlled for the effect of VC firms' *status*. Past studies indicate that VC firms' status influences both their reputation (Lee et al., 2011; Pollock et al., 2015) and their strategic and investment performance (Dimov, Shepherd, & Sutcliffe, 2007; Hochberg, Ljungqvist, & Lu, 2007). We operationalized VC firm status using Bonacich's (1987) beta centrality, calculated from the VC co-investment networks that were constructed using five-year moving periods. Beta, the attenuation factor, was set to 75% of the reciprocal of the largest eigenvalue (Podolny, 1993; Pollock et al., 2015). For the sake of readability, we rescaled the resulting scores by multiplying them by 100.

We also controlled for *structural holes* in the VC's investment syndicate network, and whether they had any *investment preference for early stage*. Both of these characteristics have been shown to affect VC reputation and investment performance (Lee et al., 2011; Podolny, 2001; Pollock et al., 2015). Following the prior literature, we operationalized structural holes as one minus network constraint. Network constraint for a VC  $i$  is computed using the following formula (Burt, 1992:54):

$$\sum_j \left( P_{ij} + \sum_q P_{iq} P_{qi} \right) \text{ (for } q \neq i, j \text{)}$$

where  $P_{ij}$  is the proportion of direct ties from  $i$  to  $j$ .

*Investment preference for early stage* measures the propensity of a VC to participate in early-stage investment rounds. We collected information from *VenturXpert* on a VC's first investment ("company stage level 1") in each company it funded. Company stage level 1 is broken down into Startup/Seed, Early Stage, Expansion, and Later Stage. We then computed the number of startup/seed and early stage investments as a percentage of its total initial investments.

## Model Specification and Estimation Technique

We modeled our theoretical process as a dynamic panel linear model as follows:

$$Y_{it+1} = \rho Y_{it} + X_{it}\beta + Z_{ijt}\gamma + \mu_i + \eta_{ij} + \varepsilon_{ijt+1} \dots (1)$$

where  $Y_{it}$  represents reputation for VC  $i$  at time  $t$ ,  $X_{it}$  represents a vector of covariates of VC  $i$  at time  $t$ ,  $Z_{ijt}$  represents a vector of covariates of firm  $j$  invested by VC  $i$  at time  $t$ ,  $\mu_i$  represents the fixed effects for VC  $i$ ,  $\eta_{ij}$  represents the fixed effects for firm  $j$  invested by VC  $i$ ,  $\varepsilon_{ijt}$  represents the random disturbance, and  $\rho$  represents the degree of path dependence for reputation.

It is worth noting that there are several potential statistical concerns to be addressed in this model. First, this model inserts a lagged dependent variable as a control variable to address the potential autocorrelation derived from the path dependent nature of our dependent variable (Arellano, 2003). However, this creates a dynamic panel bias (Anderson & Hsiao, 1982; Baltagi, 2008; Nickell, 1981). Second, reverse causality is also a possibility. If a firm in which a VC invested is expected to delist, the VC may be motivated to adjust its investment behavior in anticipation of the delisting, which in turn could influence its reputation. Third, this model essentially treats the VC-firm-year as the unit of analysis. While this is necessary to test our theory, it also prevents information loss that would occur if firm-level data are aggregated up to the VC level.<sup>11</sup> By using a VC-firm-year data structure we control for both VC fixed effects and firm fixed effects. However, because a single VC may invest in multiple firms at time  $t$ , their disturbances are likely to be correlated, potentially engendering “group-wise heteroscedasticity” (Greene, 2012: 322–323).

We addressed all of these issues by employing the Arellano-Bond (AB) estimator (Arellano & Bond, 1991) with robust standard errors clustered at the VC level. The AB estimator

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<sup>11</sup> Note that if VC-year is chosen as the unit of analysis, all the firm-level data will be aggregated to the VC level.

effectively controls for within-group fixed effects through either first-differencing or orthogonal deviation<sup>12</sup> (Arellano, 2003). It also addresses endogeneity of various kinds by instrumenting endogenous variables with valid instruments that are typically chosen from the lagged values of covariates. The lagged values are predetermined, so they cannot be associated with the disturbances as long as the disturbances are not serially correlated and appropriate lags are used.<sup>13</sup> Further, the AB estimator is capable of incorporating robust estimation of standard errors at the group (i.e., VC) level (Greene, 2012; Roodman, 2009), thus addressing the group-wise heteroskedasticity arising from a VC investing in multiple portfolio firms. The AB estimator uses the generalized method of moments (GMM), which generates consistent and efficient estimates (Hansen, 1982; Hayashi, 2000).

To run the models we employed the *xtabond2* command (Roodman, 2009) with the cluster option specified at the VC level in STATA 14 (StataCorp., 2015).

## RESULTS

Table 1 reports the descriptive statistics for all variables. To test for multicollinearity, we calculated the variance inflation factor (VIF) for all models. The individual VIF for each covariate and the average VIF for the overall models were less than 10, with a maximum of 6.09 and an average of 1.89, indicating that multicollinearity was not an issue (Neter, Wasserman, &

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<sup>12</sup> In our empirical analyses, we used orthogonal deviation rather than first-differencing because it creates fewer missing values and does not create serial correlation in the transformed errors (Arellano, 2003).

<sup>13</sup> Operationally, if a focal variable is strictly exogenous, all its lagged, current, and leading values can be used as valid instruments; if it is predetermined, its one-period or longer lags can be valid instruments; and if it is endogenous, its two-period or longer lags can be valid instruments. Because all the covariates except year dummies are possibly endogenous, two-period and longer lags are good candidates for valid instruments. However, given that our dependent variable is measured at  $t+1$  instead of  $t$ , while covariates are all measured at  $t$ , one-year and longer lags can be used as valid instruments. We used the AR(2) test statistic, Hansen's J statistic, and difference-in-Hansen statistic to fine-tune the lag structure. We ultimately chose as instruments two- and three-year lags for the lagged dependent variable, one- and two-year lags for delisting, and two- to five-year lags with the *collapse* option for the rest of the potentially endogenous variables. The *collapse* option was used to mitigate concerns about too many instruments, which can weaken the reliability of the Hansen test (Roodman, 2008, 2009).

Kutner, 1990; Cohen, Cohen, West, & Aiken, 2003). For ease of interpretation, all means and standard deviations are shown in their original metrics, prior to any transformations.

[Insert Tables 1 and 2 about here]

We report our results in Table 2. Model 1 presents the results for the control variables, Model 2 adds the main effect of delisting, Models 3 to 6 test each of the hypothesized interactions separately, and Model 7 presents the fully-specified model where all the interactions are included. Before discussing our findings, several points are worth noting. First, the test for second-order autocorrelation (i.e., AR(2)) indicated that there is no autocorrelation in the disturbances in differences (Arellano & Bond, 1991; Roodman, 2009). Second, Hansen's J statistics for over-identifying restrictions were not significant, suggesting that the chosen instrument set is valid (i.e., exogenous), and the difference-in-Hansen statistics for each instrument group confirmed that all the instrument groups are also valid. Third, too many instruments can weaken the Hansen test (Roodman, 2008), one symptom of which is an excessively high p-value approaching one. While the suggested rule of thumb is that the number of instruments should not be greater than the number of individual (or cross-sectional) units, even this is considered too generous (Roodman, 2008). With this concern in mind, we conservatively kept the number of instruments lower than half of the number of cross-sectional units. All our models satisfy this condition. Together, these specification tests ensure that the reported parameter estimates are all consistent, and that the endogeneity concerns noted above have been addressed.

Hypothesis 1 argued that the delisting of a newly public firm would be negatively related to the lead VC's reputation score at  $t+1$ . Delisting has a significant and negative relationship in all models ranging from  $p=0.006$  to  $p=0.035$ . Thus, Hypothesis 1 is supported.

To assess the magnitude of this effect, we calculated the effect size of delistings using the coefficient from the full model where all other variables are evaluated at their mean level. Here, the regression indicates that a portfolio firm delisting decreases VC reputation by 9.75, from 30.77 to 21.01. To put this in context, the top thirty highest reputation VCs had LPJ index scores that ranged from 19.51 or better (in 1995) to 32.69 or better (in 2000) across our period of study. Thus, for the thirtieth-ranked VC, a delisting would be related to an approximately 50 percent drop in their reputation index value in 1995, and about a 30 percent drop in 2000. Even for the highest-reputation VCs, who (outside of the highest rated firm, which had a value of 100) had index values of 70-75, the drop in reputation is about thirteen percent. Thus, the practical effects of delistings on VC reputations are consequential.

Hypothesis 2 argued that higher levels of underpricing would exacerbate the negative effects of delisting on the lead VC's reputation. Models 3 and 7 test this hypothesis. The main effect of underpricing is dropped because the AB models treat them as a fixed effect.<sup>14</sup> The results in both models show that underpricing has no significant moderating effect on the relationship between delisting and the level of the lead VC's reputation at t+1. Hypothesis 2 therefore is not supported.

Hypothesis 3 argued that the negative relationship between delisting and the level of VC reputation at t+1 will be attenuated when the portfolio firm's post-IPO performance is higher. We tested this hypothesis using both *ROA* and *MTB* as measures of post-IPO firm performance.

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<sup>14</sup> The main effect of underpricing was included in our analyses, but was dropped from the models by STATA because the Arellano-Bond models expunge fixed effects from estimation and underpricing is time invariant across observations for a given firm. However, its moderating effect is still captured by the interaction term because it is time-varying. To illustrate, assuming no other covariates, we have  $B1 * X$ , where  $X = \text{delisting}$ ,  $B2 * Z$ , where  $Z = \text{underpricing}$ , and  $B3 * XZ$ , the interaction.  $B2 * Z = 0$  because the main effect of underpricing is absorbed by the fixed effect; thus the interaction effect becomes  $Y = (B1 + B3 * Z) * X$ , where  $Y$  is 0 for firms that do not delist ( $X=0$ ). When there is a delisting ( $X=1$ ),  $Y$  varies by  $B1+B3*Z$ , or varies with underpricing ( $Z$ ). Thus, for those that delist, there is a common effect,  $B1$ , and an additional effect that reflects the interaction with underpricing ( $B3*Z$ ).

For ROA, the interaction coefficient is not significant in either Model 4 or 7. However, the interaction using MTB is positive and significant in Models 5 and 7 ( $p=0.039$  and  $p=0.067$ , respectively). Thus, Hypothesis 3 is supported when using market performance but not when using accounting performance.

Hypothesis 4 argued that the negative relationship between delisting and the level of VC reputation at  $t+1$  will be attenuated when the firm has survived longer after IPO. Models 6 and 7 test this hypothesis. The results in both models show that the years survived after IPO has a positive moderating effect on the relationship between delisting and the level of the lead VC's reputation at  $t+1$  ( $p=0.025$  and  $p=0.047$ , respectively). Therefore, Hypothesis 4 is supported.

To explore these relationships further, we plotted the interaction between delisting and MTB in Figure 1, and the interaction between delisting and post-IPO surviving years in Figure 2. We used the coefficients in the full Model for both figures. We employed values ranging from one standard deviation below the mean to one standard deviation above the mean for the MTB, and from one year to five years for the post-IPO surviving years.

As Figure 1 shows, when there is no delisting event a VC's reputation is relatively stable. The change in VC reputation for MTB values from one standard deviation below the mean to one standard deviation above the mean is  $-1.15$  (from 29.17 to 28.01). However, when the portfolio firm is delisted, if the portfolio firm's MTB is one standard deviation below the mean the VC reputation index value is 18.92, while at one standard deviation above the mean it is 21.89. Thus, when portfolio firm MTB is one standard deviation below the mean, delisting reduces the lead VC's reputation by approximately thirty-five percent  $((18.92-29.17)/29.17)$ . When portfolio firm MTB is one standard deviation above the mean, delisting reduces the lead VC's reputation by approximately twenty-two percent  $((21.89-28.01)/28.01)$ . A change in

portfolio firm MTB from one standard deviation below to one standard deviation above the mean therefore attenuates about thirty-eight percent of the damage caused by the delisting.

[Insert Figure 1 about here]

Figure 2 shows that post-IPO survival duration has a significant attenuating effect on the reputation damage from a delisting. When a VC does not experience a portfolio firm delisting, their reputation scores again remain essentially flat (changing from 30.12 to 31.26). But when a portfolio firm delists, the lead VC's reputation score ranges from 17.99 to 23.30 as the post-IPO surviving years increases from one year to five years. Thus, surviving one year results in a 40.3 percent ( $12.13/30.12$ ) decrease in the lead VC's reputation score when a firm is delisted; in contrast, surviving five years results in only an 25.5 percent ( $7.96/31.26$ ) drop in the lead VC's reputation score. Thus, approximately 37 percent of the damage to the VC's reputation is attenuated. Overall, while portfolio firm delistings are still damaging to the lead VC's reputation, the damage is greatly reduced the longer the portfolio firm survives following its IPO.

[Insert Figure 2 about here]

### **Robustness Checks and Additional Analyses**

In addition to our primary analyses, we conducted a variety of robustness tests to rule out alternative explanations and explore additional issues.

*Serving as non-lead VC.* Lead VCs are likely to bear the brunt of any penalty costs because they are perceived to play an active role in developing startups and therefore expected to bear more responsibility for the startups' decline (Gorman & Sahlman, 1989; Wright & Lockett, 2003). However, it is also possible that when there is more than one VC investing in the company, the other "non-lead" VCs in a syndicate may bear some endorsement risks. These VC syndicates help spread the investment risk across multiple firms, allow multiple VCs access to

promising deals, and give the startup access to a greater variety of resources. Syndicates also reduce the risks to the startup by ensuring that no single firm owns too much equity in the company, should their relationship sour (Fund et al., 2008; Gompers & Lerner, 2004).

To test whether non-lead VCs also suffer from penalty costs following an IPO firm's delisting, we test if there is any significant negative relationship between delisting and non-lead VCs' subsequent reputations. Table 3 summarizes the results. Our sub-sample analyses consisting of only non-lead VCs show that this relationship is not significant. The partial adjustment coefficient  $\rho$  capturing the influence of the one-year lagged DV on the current year DV is very close to 1 in the non-lead VCs sample, implying that for non-lead VCs covariates other than the one-year lagged reputation do not explain much of the current reputation. Further, Wald tests showed that the difference between the delisting effects on lead and non-lead VCs is significant. We utilized the following formula where the resulting statistic has a chi-squared distribution with one degree of freedom (Greene, 2012):

$$\frac{(\beta_1 - \beta_2)^2}{var(\beta_1 - \beta_2)} = \frac{(\beta_1 - \beta_2)^2}{var(\beta_1) + var(\beta_2) - 2cov(\beta_1, \beta_2)}$$

Because the covariance of the two-parameter estimates is not automatically computed from the separate samples, we followed the procedure suggested by Weesie (1999) and used the *stack* command in STATA. Estimation from the stacked dataset generates the exact same parameter estimates as those generated from each sample, and provides the covariance between the two-parameter estimates. For the Wald test, we used the *test* command in STATA. The null hypothesis that the coefficients of delistings for the lead and non-lead VC samples are equal was rejected at  $p < 0.01$ , showing that delistings are more damaging for lead VCs than non-lead VCs.

[Insert Table 3 about here]

***Effect of delisting on individual VC reputation index components.*** To enrich our understanding regarding the reactions of different stakeholders, and to ensure that it was not just one indicator driving our findings, we examined the effect of delisting on each of the individual components of the VC reputation index. We found similar support for our main and moderating hypotheses when the dependent variables were: the number of startups invested in, and total dollar amount of funds invested in startups. That is, delisting hurts these individual VC reputation components ( $p=0.098$  and  $p=0.009$ , respectively). Further, MTB ( $p=0.042$  and  $p=0.044$ , respectively) and post-IPO survival duration ( $p=0.053$  and  $p=0.098$ , respectively) partially attenuate the negative effect of delisting. However, we found no support for our moderating hypotheses when the dependent variables were the number of investment funds under management, the total dollar amount of funds under management, and the number of firms taken public—although the main effect of delisting is still supported ( $p=0.099$ ,  $p=0.071$ , and  $p=0.066$ , respectively). We consider the implications of these findings in the Discussion section.

***Reasons for delistings.*** It is also possible that the newly-public firms who delisted might have done so for idiosyncratic reasons that could affect our results. To explore this issue further, we examined newspaper coverage of the firms' delistings. We downloaded newspaper and newswire articles for each firm from one week prior to one week after their delisting. We then coded each article for what they said about the delisting. The results showed that the news articles mostly reported the fact that the company will be delisted, how the delisting might affect certain stakeholders, and what would be done with the firm's assets. Thus there did not appear to be any idiosyncratic events driving the delistings.

***Jackknife analyses.*** Given our qualitative analyses of the reasons behind each delisting, it is very unlikely that our results are driven by outlier delistings. Nonetheless, as another

robustness check, we also ran our analyses using the jackknife estimation to help test the impact of any outliers. We conducted 31 jackknife-type replications of our AB model after excluding one delisting event at a time. We used the following formula (Cameron & Trivedi, 2005: 376):

$$\hat{\beta}_{Jack} = N\hat{\beta} - (N - 1) \frac{1}{N} \sum_{i=1}^N \hat{\beta}_{(-i)}$$

Where  $\hat{\beta}_{(-i)}$  is the parameter estimate computed after the  $i$ th observation is excluded.

The jackknife coefficient for delisting is -6.091, which is very close to the original -6.093 AB coefficient. Further, H1 is supported at  $p < 0.05$  in all the 31 replications. The Jackknife coefficient for the interaction with MTB is 4.086, which is also close to the original 4.028 AB coefficient. Further, H3 is supported at  $p < 0.10$  in 27 of the 31 replications. While H3 is not supported in 4 replications, it is supported at  $p < 0.05$  in 6 out of the 27 supporting replications. Thus, in aggregate, the results from the Jackknife method are similar to the original results. The Jackknife coefficient for the interaction with surviving years is 1.052, which is again close to the original 1.041 AB coefficient. Not surprisingly, H4 is strongly supported at  $p < 0.05$  in 29 out of 31 replications. For the remaining two replications, H4 is supported at  $p < 0.01$  in one and at  $p < 0.10$  in the other. Taken together, the results from the jackknife replications are consistent with the original results. Thus, the possibility of having one extreme outlier driving our overall conclusion is extremely unlikely.

**Sample period.** Another potential explanation for our findings is that our sample period also includes the dot-com boom and subsequent bust (Pollock & Gulati, 2007; Sine, Mitsuhashi, & Kirsch, 2006). If the majority of our delistings occurred during the dot-com bust, this environmental change may have been a driver of both the delistings and VC reputation. However, an examination of our data showed that only 35 percent of the firms in our sample

went public in 1999 and 2000, which is the peak of the dot-com bubble. As such, the dot-com bubble and its bursting are unlikely to be the primary drivers of our results. We also re-ran our analyses excluding this Internet bubble period and found that our results remained similar. ( $p=0.001$ ,  $p=0.028$ , and  $p=0.045$  for delistings, interaction with MTB, and interaction with post-IPO surviving years in the full model, respectively).

***Moderating effects of firm size and age.*** Firm size and age have long been associated with liabilities of newness (Hannan & Freeman, 1977; Stinchcombe, 1965) that could also affect expectations about a firm's odds of survival. Since smaller and younger firms face greater liabilities of newness, their failure may result in smaller expectancy violations. To explore this issue, we tested the moderating effects of firm size and age on the relationship between delisting and VC reputation. We measured firm size using three different proxies: a *newly-public firm's revenue*, *total assets*, and its *market capitalization*. Like revenues, total assets and market capitalization were transformed into their natural logarithms to reduce the effect of extreme values. None of the firm size proxies had a significant moderating effect on delisting. IPO firm age did have a negative and marginally significant ( $p=0.081$ ) moderating effect on delisting, suggesting that older firms are penalized more, but our primary results did not change ( $p=0.008$  for delistings).

***VC age as part of VC reputation.*** In our main analyses we excluded VC age as part of VC reputation because this value increases monotonically as long as the VC firm does not fail, and thus cannot be influenced by the dynamics examined in this study. Past studies have also excluded this variable depending on the study context (Lee et al., 2011; Pollock et al., 2015). However, as a robustness check, we included firm age as part of the VC reputation index and our

results did not change ( $p=0.004$ ,  $p=0.077$ , and  $p=0.022$  for delistings, interaction with MTB, and interaction with post-IPO surviving years in the full model, respectively).

***VC control after IPO.*** We argued that delisting may cause a negative feedback loop even when VCs no longer have substantial control over the newly-public firm. To assess this argument, we tested our hypotheses on a sample restricted to observations where the VCs had no ownership or board ties with the newly-public firms after their IPOs. Our findings for the main effect of delistings and its interaction with MTB were robust ( $p=0.006$  and  $p=0.058$  in the full model, respectively) when using this restricted sample. However, the interaction with post-IPO surviving years was no longer significant. This may be because this restriction drops some observations where VCs may still own shares. This exclusion thus truncates and compromises our measure of post-IPO surviving years.

***VC-year level of analyses.*** In our main analyses we examined our hypotheses with a sample where each observation refers to a VC-firm-year combination. We used this approach to test our theory and hypotheses linking firm characteristics with VC reputation. This also enabled us to better control for the fixed effect for each portfolio firm under each VC. For example, as each newly public firm has a different number of founders, the values of these variables in a VC portfolio will change over time as the VC invests in more firms. As such, aggregating the level of analyses from the VC-firm-year level to the VC-year level does not allow us to control for portfolio firm fixed effects or sources of unobserved heterogeneity such as unobserved relationships between a firm and its VCs, or the quality of founders at the time of IPO. Further, aggregating the level of analyses up to the VC-year level may lead to a loss of information, including but not limited to information loss caused by the fact that the summation of multiplication terms may not be equal to the multiplication of summed terms, a difference that

could be particularly problematic when it comes to examining interaction terms<sup>15</sup>. Nonetheless, in analyses not reported here we aggregated our data to the VC-year level to test the sensitivity of our main effect (i.e. delisting), and our finding continues to be supported ( $p=0.007$ ).

## DISCUSSION

In this study we considered whether firms that provided endorsements suffer reputational penalties when the firms they endorse fail to meet expectations, and the factors that influenced the extent of the penalty. Drawing on signaling theory (Connelly et al., 2011; Spence, 1973) and attribution theory (Bowman, 1976, 1978; Heider, 1958; Kelley, 1973; Mitchell, 1982), we argued that endorsements by reputable lead VC firms increase stakeholders' expectations about the performance of their portfolio firms, and that when these firms subsequently fail to meet these expectations the lead VCs will suffer a reputation penalty. We also argued and found evidence that the newly-public firm's market performance following the IPO, and how long it survived prior to delisting, partially attenuated the reputational penalty. As part of our robustness checks, we also found that the penalty only applied to the lead VC. Our findings have implications for both theory and practice.

### Theoretical Contributions

Signaling theory has often assumed that those who provide inaccurate signals are penalized, but research has not explored whether this occurs or what factors influence the extent of the penalty (Connelly et al, 2011). Our study contributes to signaling theory by demonstrating that a negative feedback loop between a signal's receivers and the signaler exists, and by explicating how the feedback loop functions. We theorized that stakeholders will look for a

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<sup>15</sup> How the information of interaction terms at the VC-firm-year level is lost when aggregated to the VC-year level can be understood by looking at the following inequality:  $\sum_i (X_i * Z_i) \neq \sum_i X_i * \sum_i Z_i$ . As an example, suppose that  $X_1$  and  $X_2$  are 0 and 1 and that  $Z_1$  and  $Z_2$  are -0.3 and 0.3. The left-hand-side representing a VC-firm level interaction equals 0.3 while the right-hand-side representing a VC-level interaction equals 0.

scapegoat when an endorser's reputation creates an expectation that is subsequently violated. We also argued and showed they are likely to focus on and punish those whose endorsement signals they relied on.

Furthermore, our study extends research on the durability of these feedback effects by illustrating that the reputation penalties are not contingent on direct, ongoing linkages between the firms. Overall, VCs owned relatively little stock following the IPO; average VC ownership three years after the IPO was only 1.81 percent, and in 71 percent of the delistings the lead VC had no ownership at all. Further in 78 percent of the delistings the lead VC had no board representation during the year of the delisting. In our analyses we found that portfolio delistings affected lead VC reputations even after controlling for the VC's ownership. However, our results also showed a marginally significant and negative main effect relationship between VC ownership and their reputations, suggesting that continuing to hold stock in a company was negatively associated with the VC's subsequent reputation. Our results also remained unchanged when we used a restricted sample where the VCs held no post-IPO ownership in the firms. In additional analyses not shown here, VC ownership also did not moderate the effect of delisting on VC reputation. Thus, the attribution-based feedback loop does not appear to depend on continuing relationships.

We also argued and found that the strength of the reputation penalty is influenced by the newly-public firm's subsequent performance. Good market performance provides confirmation that the VCs have "done their job" helping young firms overcome their liabilities of newness, thereby decoupling the VCs from their portfolio firms and reducing the blame that VCs receive for the expectancy violation. Consistent with this argument, we also found that the reputational penalty to lead VCs was smaller the longer the firm has survived following its IPO. These

findings are important because they show that firm characteristics and behaviors can influence whether and to what extent the negative feedback loop occurs, but attributions of responsibility are not completely attenuated by situational factors (Jones & Harris, 1967).

However, a firm's accounting performance prior to its delisting did not have a significant moderating effect. A reassessment of our data also showed that only one firm had a positive ROA in the year prior to delisting, and that the average ROA in the year prior to delisting was  $-0.17$ . Thus, it appears that in our context, no firms had good operating performance prior to delisting, some were just "less bad" than others. In contrast, MTB was very positive for delisted firms in the year after their IPOs, but declined in later years. These differences in the performance measures may account for the differences in their influence. Future research should continue to explore how performance affects the attribution processes we consider here.

We did not find any significant support for the moderating effect of IPO underpricing on the relationship between a portfolio firm's delisting and lead VC reputation. We argued that underpricing should increase expectations, making the subsequent delisting more disappointing. However, higher underpricing also means that some stakeholders may have profited from the IPO, offsetting the disappointment that the firms are subsequently delisted. It's also possible that whereas some stakeholders may have treated underpricing as a further validation of the VC's endorsement that increased their expectations, as we argued, other stakeholders may have treated underpricing like good post-IPO performance, and thus it attenuated their attribution processes. Future research should continue to explore this issue, and the complexities associated with interpreting underpricing (Hubbard, Pollock, Pfarrer & Rindova, 2018; Tsang & Blevins, 2015).

Our robustness tests also generated some interesting insights and theoretical implications. First, our analysis for non-lead VCs challenges the assumption that all endorsers will bear

endorsement risks. Rather, we found that the blame for delistings is only attributed to the lead VCs, likely because of their prominence and responsibility in funding and nurturing the startup. This finding is consistent with the findings of Pollock and colleagues (2010), who showed that whereas having one prestigious VC increased the market value of an IPO firm, a second prestigious VC added substantially less value, and a third prestigious VC added no additional value. Future research should continue to explore the signaling dynamics and reputational consequences when multiple possible signalers are involved.

Further, our analyses decomposing the VC reputation index suggested additional nuances in how VC reputations are damaged by delistings. Our results remained robust for models with some dependent variables (the number of portfolio firms a VC invested in and the amount a VC invests in its portfolio firms) but not for others (the number of funds a VC managed, the total dollar amount of funds a VC managed, and the number of portfolio firms taken public). These findings suggest two potential explanations. First, delistings and contingency factors appear to affect VC reputation indicators that are more related to “output” dimensions of VC reputation—that is, dimensions that are related to portfolio firms instead of the investors providing the “inputs” that the VCs invest. The startups seeking and receiving financing may be less willing or interested in accepting funding from VCs whose portfolio firms have failed following their IPOs (Hallen & Pahnke, 2016). It is also possible that the VCs will now have a harder time taking other portfolio firms public.

Second, aspects of VC reputations that are more related to “input” dimensions are less sensitive, at least in the short term, to delistings than aspects related to “output” dimensions. Investors in the VCs’ funds may only pay attention to the funds’ overall returns, and not the particular investments that VCs make. Thus, delistings have little effect on the number of funds

raised or the total amount of money they manage. Taken together, these findings and their potential explanations suggest that future research should continue to explore how different stakeholders, with different interests and time horizons, may react differently to the same event.

More generally, our study also contributes to theory on the dynamics of reputation development and loss, and shows how reputation can be easily damaged by distant events. Previous theorizing and empirical studies have focused on contexts where firm reputation is damaged by crises or proximal events (e.g. Gomulya & Mishina, 2017; Rhee & Kim, 2012; Zavyalova, Pfarrer, Reger, & Shapiro, 2012). We show that a firm's reputation can also be damaged even when the events themselves are not within the full control of the affected firm. The implications of reputation damage extend beyond the same industry or ongoing relationships with the focal firm. Indeed, we show that reputation damage can be caused by negative events, such as delistings, simply because of the enhanced expectations created by reputable endorsements and attributional biases.

Research has also shown that delistings are increasingly prevalent (Fama & French, 2004) and that VC firms are willing to engage in actions at the time of IPO that can enhance their profits, even if they damage their portfolio firms' performance prospects (Arthurs, Hoskisson, Busenitz, & Johnson, 2008; Fischer & Pollock, 2004). The implicit assumption here is that the VCs are unlikely to be affected when the IPO firm's poor performance becomes manifest. Our findings suggest that more ex-post settling up occurs than has been presumed, and that attribution theory can be used to explain the negative feedback loop that exists when the signals provided by an actor's reputation are perceived to be inaccurate.

While reputations evolve based on positive and negative performance, most research tends to focus on the positive factors that help to build firm reputation (e.g., being known, being

known for something, and generalized favorability) (Lange et al., 2011). Our study serves to remind scholars that firm reputation is also formed by negative performance—both the focal actor’s and others’—even if their actions and the associated outcomes are separated in time. Further, because an actors’ actions are difficult to observe (e.g., whether VC firms provide resources and good strategic advice to its portfolio firms), stakeholders will often use subjective assessments of these actions during sensemaking. Firms that take self-serving actions cannot rely on time and opacity to shield their reputations from the consequences of providing signals that are perceived to be inaccurate. Stakeholders will look for culprits to blame, and they will blame those who helped create their expectations in the first place. Thus, our theory suggests that VC firms should prudently manage expectations, and consider the long-term interests of the firms they have endorsed.

Finally, we contribute to the entrepreneurship literature by illustrating the long-term consequences to VCs for prematurely taking firms public in the hopes of quickly “cashing out” on their investments. Our results also suggest that young VC firms who “grandstand” (Gompers, 1996; Lee & Wahal, 2004) by taking firms public too early may do more harm to their reputations than good if these firms ultimately delist at higher rates. VCs likely bear significant settling-up costs if they take actions that increase the short-term value of portfolio firm—and their investment in it—at the expense of its future viability.

### **Managerial Implications**

Our findings also have implications for practitioners – particularly those in professional service firms and other industries where a firm’s actual performance is difficult to observe, and their reputations hinge on the behaviors of other firms they are expected to influence. This includes law and accounting firms, in addition to financial services firms like venture capitalists

and investment banks. Our results suggest that the expectations these firms create about client firms can have long-lasting consequences for their own reputations. Further, they suggest that outcomes that confirm their initial expectations partially attenuate the damage to their reputations. Thus, firms concerned with protecting their reputations should temper the expectations they create, and try to ensure that near-term performance expectations are met.

### **Limitations and Future Research**

All studies have limitations that suggest directions for future research. One limitation of our study is that we observe delistings of VC-backed firms in the US. Institutional and regulatory differences may result in different idiosyncrasies with regard to firm delistings and how they are interpreted in other countries. VC backing may also have differential signaling value in different countries, where their nature and role may vary. As such, future research should explore the extent to which our findings generalize to other national contexts.

Another limitation of our study is that our VC reputation measure is formative and objective. Because it does not directly measure perceptions, we were only able to indirectly measure the perceptions of the stakeholders (e.g., insurance companies, university endowments, wealthy individuals, etc.) who invest in the VCs' funds, or the entrepreneurs who receive their funding (Lee et al., 2011). These limitations primarily serve to make ours a conservative test of our hypotheses. Nonetheless, future research in other contexts should verify and extend our findings by employing longitudinal reputation measures that more directly reflect stakeholders' perceptions.

There are several other interesting opportunities for future research. One question is whether and how VCs can repair their damaged reputations following negative events, like portfolio firm delistings. If reputations are easily damaged, are they also easily fixed? And which

repair mechanisms (see Elsbach [2012] and Rhee & Kim [2012] for reviews of this literature) can be applied in this context? Can VCs convince stakeholders that delistings go beyond the scope of their influence? How long does it take before a tarnished VC firm reputation recovers? To what extent do current successes offset past failures?

Future research should also examine the nature of negative events, and the actions (or lack thereof) that contributes to them. For example, it would be interesting to examine to what extent delistings can be reasonably attributed to the lack of due diligence by VCs or a premature push by VCs to exit an investment, versus to external factors genuinely beyond the control of both the VCs and portfolio firms, and whether these differences matter in the delisting's effects on the VC's reputation. Such an analysis can help us gauge whether stakeholders are reacting reasonably or unreasonably in punishing the VCs.

## **Conclusion**

This study explores the consequences of providing inaccurate signals by examining the relationship between IPO firm delistings and the reputational damage it can cause to the VCs that endorsed them. Studies have shown that new firms receive signaling benefits from prominent affiliations (e.g., Carter & Manaster, 1990; Petkova, 2012; Pollock et al. , 2010; Stuart et al., 1999). Our study shows that the endorser's reputation can be damaged when the firms they endorsed perform poorly, and the signals their reputations provide thus fall short of expectations. It also shows that these effects can be partially mitigated by the endorsed firm's market performance prior to its failure, and the time that has elapsed between the signal and the failure. In so doing, we contribute to signaling theory by showing whether there are consequences to endorsers for getting their "wires crossed."

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**TABLE 1**  
**Correlation Table and Descriptive Statistics<sup>a</sup>**

		Mean	S.D.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17
VC-level attributes	1. Reputation	31.54	22.33																	
	2. Status	8.33	5.13	0.89																
	3. Structural holes	1.01	0.14	0.53	0.62															
	4. Investment preference for early stage	0.58	0.20	0.22	0.25	0.07														
Portfolio-level attributes excl. focal firm attributes	5. Company age	8.13	4.51	0.35	0.36	0.40	0.12													
	6. Revenue	136.85	344.13	0.53	0.50	0.47	0.08	0.78												
	7. ROA (Industry-adjusted net income/assets)	-0.08	0.15	0.16	0.14	0.09	-0.05	-0.08	0.26											
	8. MTB	2.39	1.45	0.40	0.48	0.28	0.21	0.56	0.50	-0.06										
	9. VC ownership	2.20	2.94	0.16	0.07	0.21	0.07	0.21	0.18	-0.12	0.10									
	10. Other delisting events in the portfolio firms	0.60	1.01	0.39	0.34	0.27	0.09	0.19	0.33	0.10	0.19	0.08								
Firm-level attributes	11. Underpricing (%)	43.77	57.60	0.09	0.07	0.12	0.12	0.10	0.20	0.12	0.01	-0.07	0.25							
	12. Firm age	10.41	4.91	-0.09	-0.11	-0.06	-0.17	-0.06	-0.04	0.11	-0.05	-0.04	-0.13	-0.11						
	13. Revenue	183.64	880.72	0.19	0.14	0.15	0.05	0.08	0.22	0.20	-0.03	0.00	0.16	0.22	0.10					
	14. ROA (Industry-adjusted EBIT/assets)	-0.17	0.26	0.00	0.02	0.04	-0.02	0.02	0.13	0.25	-0.03	-0.02	-0.04	0.01	0.23	0.54				
	15. MTB	2.69	1.74	0.13	0.12	-0.03	0.10	0.11	0.11	0.00	0.19	0.00	0.00	0.16	-0.08	0.10	0.11			
	16. VC ownership (%)	2.58	4.93	0.01	-0.08	0.04	-0.08	-0.09	-0.08	-0.02	-0.16	0.21	-0.01	-0.11	-0.08	-0.06	-0.05	-0.13		
	17. Surviving years	3.75	0.62	0.05	0.06	0.03	0.08	0.14	0.11	-0.11	0.08	-0.01	0.09	0.15	0.07	0.03	0.08	0.08	-0.23	
	18. Delisting	0.08	0.28	-0.04	-0.03	0.00	-0.09	-0.06	-0.04	0.01	-0.05	0.02	0.10	-0.01	-0.16	-0.13	-0.34	-0.21	-0.02	-0.16

<sup>a</sup> Data are deleted listwise. Means and standard deviations are reported at firm level in the original metric; correlations whose absolute values are greater than 0.10 are significant at  $p < 0.05$ .

**TABLE 2**  
**Arellano–Bond Dynamic Panel GMM Estimates for Lead VC reputation at t+1<sup>a</sup>**

VARIABLES		(1)	(2)	(3)	(4)	(5)	(6)	(7)
<b>VC-level attributes</b>	Reputation <sub>t</sub>	0.784*** (0.062)	0.824*** (0.061)	0.831*** (0.061)	0.821*** (0.063)	0.809*** (0.063)	0.830*** (0.062)	0.823*** (0.063)
	Status <sub>t</sub>	0.156 (0.344)	0.035 (0.299)	0.056 (0.290)	0.029 (0.301)	0.031 (0.306)	0.014 (0.303)	0.036 (0.299)
	Structural holes	-1.437 (5.342)	-1.817 (3.342)	-2.820 (3.658)	-1.469 (3.394)	-0.516 (3.765)	-1.800 (3.545)	-1.682 (4.223)
	Investment preference for early stage	3.156 (2.473)	2.732 (2.136)	3.788+ (2.074)	2.858 (2.124)	2.645 (2.117)	2.476 (2.127)	3.247 (2.050)
<b>Portfolio-level attributes excl. focal firm attributes</b>	Company age	-0.841 (0.815)	-1.118 (0.772)	-1.149 (0.766)	-1.117 (0.790)	-0.919 (0.730)	-1.052 (0.768)	-0.908 (0.760)
	Revenue	-0.530 (0.474)	-0.495 (0.477)	-0.486 (0.496)	-0.507 (0.497)	-0.468 (0.468)	-0.487 (0.482)	-0.448 (0.505)
	ROA (Industry-adjusted net income/assets)	-2.726+ (1.393)	-3.737* (1.649)	-3.790* (1.655)	-3.798* (1.625)	-3.395* (1.560)	-3.333* (1.589)	-3.084* (1.549)
	MTB	0.250 (0.290)	0.390 (0.310)	0.354 (0.297)	0.368 (0.289)	0.274 (0.276)	0.358 (0.299)	0.240 (0.275)
	VC ownership	-0.192 (0.118)	-0.220+ (0.118)	-0.246+ (0.135)	-0.220+ (0.121)	-0.204+ (0.115)	-0.224+ (0.119)	-0.231+ (0.133)
	Other delisting events in the portfolio firms	-0.772* (0.357)	-0.722+ (0.380)	-0.592+ (0.359)	-0.773+ (0.417)	-0.730* (0.352)	-0.724+ (0.371)	-0.575 (0.378)
<b>Focal firm attributes</b>	Company age	-5.606+ (2.896)	-3.722 (2.535)	-3.940 (2.543)	-3.996 (2.491)	-4.999* (2.387)	-4.797* (2.391)	-5.818* (2.649)
	Revenue	1.397* (0.685)	0.384 (0.553)	0.495 (0.616)	0.405 (0.575)	0.704 (0.613)	0.600 (0.569)	0.929 (0.685)
	ROA (Industry-adjusted net income/assets)	0.318 (1.053)	0.336 (1.027)	0.167 (1.115)	0.436 (1.016)	0.697 (1.018)	0.476 (0.988)	0.624 (1.065)
	MTB	-1.239+ (0.720)	-0.608 (0.614)	-0.438 (0.667)	-0.655 (0.590)	-1.074+ (0.652)	-0.926 (0.614)	-1.128 (0.690)
	VC ownership	-0.085+ (0.048)	-0.074+ (0.043)	-0.066 (0.043)	-0.078+ (0.046)	-0.077+ (0.044)	-0.085+ (0.047)	-0.076 (0.047)
	Surviving years	0.028 (0.369)	0.159 (0.349)	0.268 (0.398)	0.162 (0.355)	0.165 (0.356)	0.204 (0.360)	0.286 (0.399)
	Delisting		-6.093* (2.393)	-6.508* (2.636)	-6.932* (3.282)	-9.106** (3.303)	-11.562** (4.418)	-13.169** (4.769)
	Delisting X Underpricing			3.991 (3.242)				3.594 (3.035)
	Delisting X ROA				-2.618 (4.394)			2.186 (3.833)
	Delisting X MTB					4.251* (2.064)		4.028+ (2.196)
Delisting X Surviving years						1.260* (0.635)	1.041* (0.463)	
<b>Estimation statistics</b>	Observations	1,587	1,587	1,587	1,587	1,587	1,587	1,587
	Number of companies	370	370	370	370	370	370	370
	Number of instruments	130	138	141	141	141	139	141
	Hansen J statistic	89.55	93.97	93.11	88.10	93.84	92.06	83.82
	p-value of Hansen statistic	0.523	0.597	0.674	0.797	0.654	0.650	0.827
	ar(1)	0	0	0	0	0	0	0
	ar(2)	0.0881	0.219	0.145	0.176	0.121	0.251	0.118
	Wald chi-squared statistic	4841	4773	4415	5444	4919	6012	5603

<sup>a</sup>Robust standard errors (clustered at the VC level) in parentheses; two-sided tests. Year dummies included, but not reported.

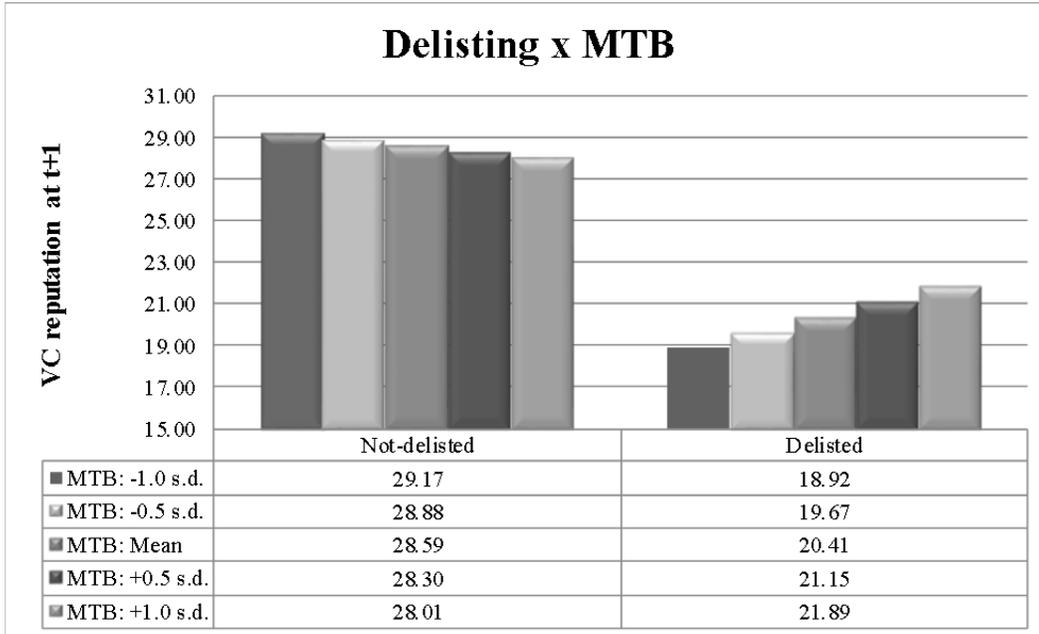
+  $p < 0.10$ ; \*  $p < 0.05$ ; \*\*  $p < 0.01$ ; \*\*\*  $p < 0.001$

**TABLE 3**  
**Arellano–Bond Dynamic Panel GMM Estimates for All VC reputation at t+1<sup>a</sup>**

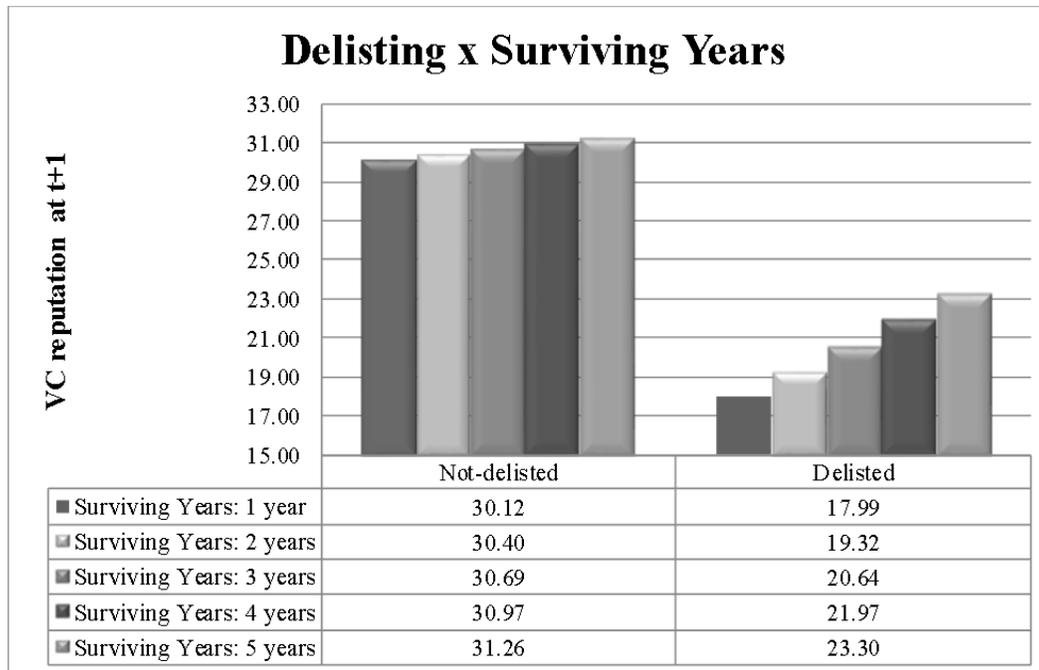
	VARIABLES	Non-lead VC	Lead VC
<b>VC-level attributes</b>	Reputation <sub>t</sub>	0.987*** (0.071)	0.824*** (0.061)
	Status <sub>t</sub>	0.153 (0.343)	0.035 (0.299)
	Structural holes	-4.483 (2.965)	-1.817 (3.342)
	Investment preference for early stage	-1.537 (3.083)	2.732 (2.136)
<b>Portfolio-level attributes excl. focal firm attributes</b>	Company age	-0.868 (0.553)	-1.118 (0.772)
	Revenue	-0.306 (0.259)	-0.495 (0.477)
	ROA (Industry-adjusted net income/assets)	0.225 (1.490)	-3.737* (1.649)
	MTB	0.499* (0.198)	0.390 (0.310)
	VC ownership	0.066 (0.080)	-0.220+ (0.118)
	Other delisting events in the portfolio firms	-0.376 (0.229)	-0.722+ (0.380)
<b>Focal firm attributes</b>	Company age	-5.169*** (1.570)	-3.722 (2.535)
	Revenue	0.037 (0.289)	0.384 (0.553)
	ROA (Industry-adjusted net income/assets)	-0.591 (1.734)	0.336 (1.027)
	MTB	0.199 (0.576)	-0.608 (0.614)
	VC ownership	0.052 (0.116)	-0.074+ (0.043)
	Surviving years	0.521** (0.199)	0.159 (0.349)
	Delisting	3.135 (2.367)	-6.093* (2.393)
<b>Estimation statistics</b>	Observations	3,989	1,587
	Number of companies	943	370
	Number of instruments	105	138
	Hansen J statistic	65.26	93.97
	p-value of Hansen statistic	0.468	0.597
	ar(1)	0	0
	ar(2)	0.681	0.219
	Wald chi-squared statistic	3518	4773

<sup>a</sup>Robust standard errors (clustered at the VC level) in parentheses; two-sided tests. Year dummies included, but not reported; +  $p < 0.10$ ; \*  $p < 0.05$ ; \*\*  $p < 0.01$ ; \*\*\*  $p < 0.001$

**FIGURE 1**  
**Effect of Delisting and Market-to-Book (MTB) Ratio**



**FIGURE 2**  
**Effect of Delisting and Surviving Years**



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