© Academy of Management Journal
2019, Vol. 62, No. 3, 641–666.
https://doi.org/10.5465/amj.2016.0796

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

DAVID GOMULYA
Singapore Management University

KYUHO JIN
Gwangju Institute of Science and Technology

PEGGY M. LEE
Arizona State University

TIMOTHY G. POLLOCK
University of Tennessee—Knoxville

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 (VC) 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 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 reputation 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 et al., 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.

We would like to thank our associate editor and the three anonymous *AMJ* reviewers for their many helpful comments and suggestions. We would also like to acknowledge the thoughtful contributions made by Warren Boeker, Glenn Hoetker, Mike Pfarrer, Matthew Semadeni, and reviewers at the Academy of Management annual meetings.

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, Certo, Ireland, and Reutzel (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." They further noted that "The signaling environment on the whole is an under-researched aspect of signaling theory" (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 have only examined the effects of endorsements on the endorsed firm, rather than following the feedback loop and examining 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 initial public offering (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 that arise 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 they 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 firms search for opportunities to provide signals that can reduce these information asymmetries. Spence (1973) argued characteristics that 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 1% 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, Voss, & Chakraborty, 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, & Boal, 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, & Yang, 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 United States, and of those, only about 1,000 businesses receive VC financing. Thus, only about one-sixth of 1% 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% 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 SEC verifies that all material information about the firm has been disclosed, and that the firm meets all regulatory requirements for going public (Husick & Arrington, 1998). Knowledgeable potential investors also attend the firm's "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)[1] 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 delistings occur (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 (Fischer & 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 these entities 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).

[1] 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.

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 subsequent hypotheses examine the moderating effects of different contextual factors (Graffin, Haleblian, & 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 websites, an increased number of 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 shown that high levels of underpricing benefit the VCs who fund the startups because they are credited with being able to spot 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 experiences 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 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 firms' 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 have been backed by VCs. Second, we only included firms whose delistings were 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 the 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 were unavailable) showed that there were no differences in terms of age, revenues, total assets, return on assets (ROA), and market-to-book (MTB) 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 two-digit SIC codes using both Pearson's χ^2 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 and Jin (2011), which is available at 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 this 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, which 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.[2] It also offers the advantage, as a multi-item measure, of more closely approximating 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[3] 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

[2] Earlier studies using the LPJ index (e.g., Lee et al., 2011; Pollock et al., 2015) have not examined how delistings affect VC reputation. As such, this study is different from and complements prior research.

[3] This study used a snapshot of data obtained from the website TheFunded in May 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.

the LPJ index measure.[4] 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, which rules out the possibility that 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).[5] 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 Arellano–Bond (AB) (Arellano & Bond, 1991) estimation for our analyses, as discussed below.

Independent Variables

Delisting. Our key independent variable is whether 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 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.[6]

Our theory does not require an assessment of whether some reasons for delistings are "more negative" than others, and attempting to do so would raise 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 in the first year after the IPO, 4 in the second year, 11 in the third year, 5 in the fourth year, and 7 in the fifth year after the IPO. The relatively small number of delistings 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.[7]

[4] While they also found that unmotivated entrepreneurs' perceptions were less accurate, as they used an absolute value measure that does not differentiate between over- and underestimating a VC's reputation, we cannot assess whether those who were less motivated tended to over- or underestimate 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.

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

[6] The specific reasons are: bankruptcy; issue withdrawn by underwriter; corporate governance violation; delinquent in filing; nonpayment 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.

[7] 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, MTB 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.

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 come from the CRSP database. This measure was transformed into its natural logarithm to reduce the effect of 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 *ROA* for each year following its IPO, and (2) using the newly public firm's *MTB* ratio 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% in each tail, where all data below the 5th percentile were set to the 5th percentile value, and data above the 95th percentile were 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 fewer 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 the 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[8] each year. Since some of the firms in our sample had no revenues in a given year, we added a 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.

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 VentureXpert database. We then manually coded the VC's 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

[8] 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.

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 nonfocal portfolio firms funded by the same VC by including the average values of the following portfolio firm characteristics. 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.[9] We used this five-year window because the effects of delistings by the other portfolio firms can persist for more than one year.[10]

VC characteristics control variables. We also introduced various controls for the VC characteristics that could influence its reputation. First, its future reputation will be 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 firm's *status*. Past studies have indicated that VC firm's status influences both its reputation (Lee et al., 2011; Pollock et al., 2015) and its strategic and investment performance (Dimov, Shepherd, & Sutcliffe, 2007; Hochberg, Ljungqvist, & Lu, 2007). We operationalized VC firm status using Bonacich's (1987) β centrality, calculated from the VC co-investment networks that were constructed using five-year moving periods. β, 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 VCs' 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 1 minus the value of the network constraint. The network constraint for a VC *i* is computed using the following formula (Burt, 2004: 54):

$$\sum_j \Big(P_{ij} \;+\; \sum_q P_{iq} P_{qi} \Big) (for\;\; q \;\neq\; i, j)$$

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 VentureXpert on a VC's first investment ("company stage level 1") in each company it funded. Company stage level 1 is broken down into Startup or Seed, Early Stage, Expansion, and Later Stage. We then computed the number of Startup or Seed and Early Stage investments as a percentage of the VC's total initial investments.

Model Specification and Estimation Technique

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

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

where Y_{it} represents reputation for VC *i* at time *t*, X_{it} represents a vector of covariates for VC *i* at time *t*, Z_{ijt} represents a vector of covariates for firm *j* invested in by VC *i* at time *t*, μ_i represents the fixed effects for VC *i*, η_{ij} represents the fixed effects for firm *j* invested by VC *i*, ε_{ijt} represents the random disturbance, and ρ 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, the 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 has 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.[11] By

[9] We also reran our analyses using the number of delistings for all prior years, and the results were the same.

[10] 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.

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

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 AB estimator (Arellano & Bond, 1991) with robust standard errors clustered at the VC level. The AB estimator effectively controls for within-group fixed effects through either first-differencing or orthogonal deviation[12] (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.[13] 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, 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 (Cohen, Cohen, West, & Aiken, 2003; Neter, Wasserman, & Kutner, 1990). For ease of interpretation, all means and standard deviations are shown in their original metrics, prior to any transformations.

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 overidentifying 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, 2009), one symptom of which is an excessively high *p* value approaching 1. 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, 2009). 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

[12] In our empirical analyses, we used orthogonal deviation rather than first differencing because it creates fewer missing values and does not give rise to serial correlation in the transformed errors (Arellano, 2003).

[13] 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).

TABLE 1
Correlation Table and Descriptive Statistics

			Mean	*SD*	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	−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	−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

Notes: 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 $t+1$

	Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VC-level attributes	Reputation$_t$	0.78***	0.82***	0.83***	0.82***	0.81***	0.83***	0.82***
		(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
	Status$_t$	0.16	0.03	0.06	0.03	0.03	0.01	0.04
		(0.34)	(0.30)	(0.29)	(0.30)	(0.31)	(0.30)	(0.30)
	Structural holes	−1.44	−1.82	−2.82	−1.47	−0.52	−1.80	−1.68
		(5.34)	(3.34)	(3.66)	(3.39)	(3.76)	(3.54)	(4.22)
	Investment preference for early stage	3.16	2.73	3.79†	2.86	2.65	2.48	3.25
		(2.47)	(2.14)	(2.07)	(2.12)	(2.12)	(2.13)	(2.05)
Portfolio-level attributes excl. focal firm attributes	Company age	−0.84	−1.12	−1.15	−1.12	−0.92	−1.05	−0.91
		(0.81)	(0.77)	(0.77)	(0.79)	(0.73)	(0.77)	(0.76)
	Revenue	−0.53	−0.50	−0.49	−0.51	−0.47	−0.49	−0.45
		(0.47)	(0.48)	(0.50)	(0.50)	(0.47)	(0.48)	(0.51)
	Industry-adjusted ROA	−2.73†	−3.74*	−3.79*	−3.80*	−3.40*	−3.33*	−3.08*
		(1.39)	(1.65)	(1.65)	(1.62)	(1.56)	(1.59)	(1.55)
	MTB	0.25	0.39	0.35	0.37	0.27	0.36	0.24
		(0.29)	(0.31)	(0.30)	(0.29)	(0.28)	(0.30)	(0.28)
	VC ownership	−0.19	−0.22†	−0.25†	−0.22†	−0.20†	−0.22†	−0.23†
		(0.12)	(0.12)	(0.13)	(0.12)	(0.12)	(0.12)	(0.13)
	Other delisting events in the portfolio firms	−0.77*	−0.72†	−0.59†	−0.77†	−0.73*	−0.72†	−0.58
		(0.36)	(0.38)	(0.36)	(0.42)	(0.35)	(0.37)	(0.38)
Focal firm attributes	Company age	−5.61†	−3.72	−3.94	−4.00	−5.00*	−4.80*	−5.82*
		(2.90)	(2.54)	(2.54)	(2.49)	(2.39)	(2.39)	(2.65)
	Revenue	1.40*	0.38	0.49	0.41	0.70	0.60	0.93
		(0.69)	(0.55)	(0.62)	(0.58)	(0.61)	(0.57)	(0.69)
	Industry-adjusted ROA	0.32	0.34	0.17	0.44	0.70	0.48	0.62
		(1.05)	(1.03)	(1.12)	(1.02)	(1.02)	(0.99)	(1.06)
	MTB	−1.24†	−0.61	−0.44	−0.65	−1.07†	−0.93	−1.13
		(0.72)	(0.61)	(0.67)	(0.59)	(0.65)	(0.61)	(0.69)
	VC ownership	−0.08†	−0.07†	−0.07	−0.08†	−0.08†	−0.09†	−0.08
		(0.05)	(0.04)	(0.04)	(0.05)	(0.04)	(0.05)	(0.05)
	Surviving years	0.03	0.16	0.27	0.16	0.17	0.20	0.29
		(0.37)	(0.35)	(0.40)	(0.35)	(0.36)	(0.36)	(0.40)
	Delisting		−6.09*	−6.51*	−6.93*	−9.11**	−11.56**	−13.17**
			(2.39)	(2.64)	(3.28)	(3.30)	(4.42)	(4.77)
	Delisting × underpricing			3.99				3.59
				(3.24)				(3.04)
	Delisting × ROA				−2.62			2.19
					(4.39)			(3.83)
	Delisting × MTB					4.25*		4.03†
						(2.06)		(2.20)
	Delisting × surviving years						1.26*	1.04*
							(0.63)	(0.46)
Estimation statistics	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	0.52	0.60	0.67	0.80	0.65	0.65	0.83
	AR(1)	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	AR(2)	0.09	0.22	0.15	0.18	0.12	0.25	0.12
	Wald χ^2 statistic	4840.55***	4773.48***	4415.21***	5444.15***	4918.95***	6012.26***	5603.06***

Notes: Robust standard errors (clustered at the VC level) in parentheses; two-sided tests. Year dummies included, but not reported. AR(1) and AR(2) represent p-values from the Arellano-Bond test for first- and second-order serial correlation in the disturbances.

† $p < 0.10$
* $p < 0.05$
** $p < 0.01$
*** $p < 0.001$

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 30 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% drop in their reputation index value in 1995, and about a 30% 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 13%. 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.[14] 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. 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.

[14] The main effect of underpricing was included in our analyses, but was dropped from the models by Stata because the AB 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 $B1 \times X$, where X = delisting, $B2 \times Z$, where Z = underpricing, and $B3 \times XZ$, the interaction. $B2 \times Z = 0$ because the main effect of underpricing is absorbed by the fixed effect; thus, the interaction effect becomes $Y = (B1 + B3 \times Z) \times X$, where Y is 0 for firms that do not delist ($X = 0$). When there is a delisting ($X = 1$), Y varies by $B1 + B3 \times Z$, 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 ($B3 \times Z$).

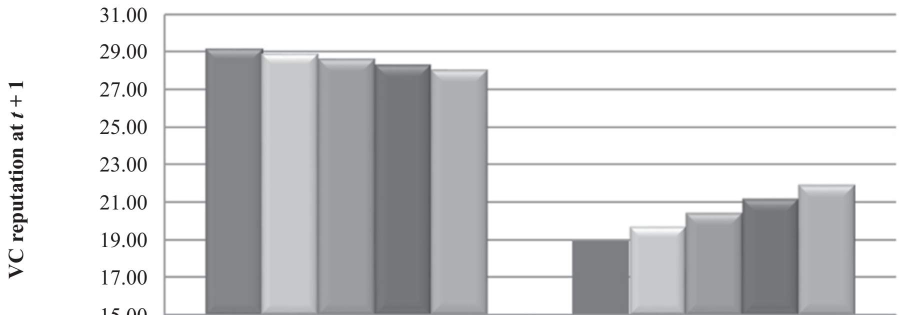
Hypothesis 4 argued that the negative relationship between delisting and the level of VC reputation at $t+1$ will be attenuated when the firm has survived longer after IPO. Models 6 and 7 test this hypothesis. The results in both models show that the years survived after the IPO has a positive moderating 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 35% ((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 22% ((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 38% of the damage caused by the delisting.

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). However, when a portfolio firm delists, the lead VC's reputation score ranges from 17.99 to 23.30 as the post-IPO surviving years increase from one to five. Thus, surviving one year results in a 40.3% (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% (7.96 / 31.26) drop in the lead VC's reputation score. Thus, approximately 37% of the damage to the VC's reputation is attenuated. Overall, while portfolio firm delistings are still damaging to the lead

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



	Not delisted	Delisted
MTB: –1.0 *SD*	29.17	18.92
MTB: –0.5 *SD*	28.88	19.67
MTB: Mean	28.59	20.41
MTB: +0.5 *SD*	28.30	21.15
MTB: +1.0 *SD*	28.01	21.89

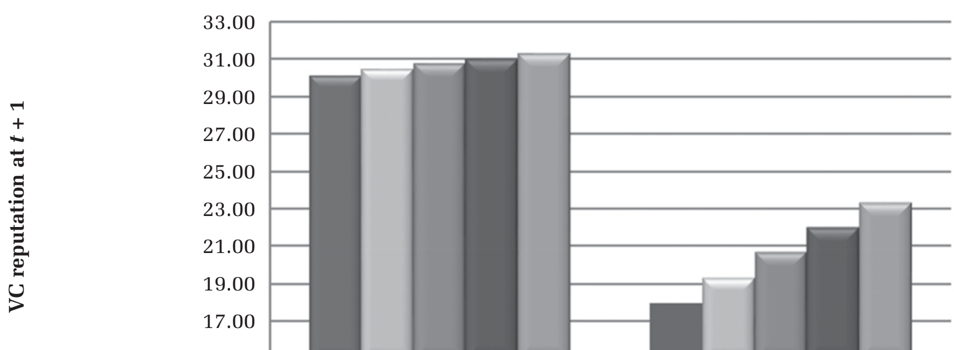
VC's reputation, the damage is greatly reduced the longer the portfolio firm survives following its IPO.

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 are therefore expected to bear more responsibility for a startup's 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

FIGURE 2
Effect of Delisting and Surviving Years



	Not delisted	Delisted
Surviving Years: 1	30.12	17.99
Surviving Years: 2	30.40	19.32
Surviving Years: 3	30.69	20.64
Surviving Years: 4	30.97	21.97
Surviving Years: 5	31.26	23.30

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 tested whether there is any significant negative relationship between delisting and non-lead VCs'

TABLE 3
Arellano–Bond Dynamic Panel GMM Estimates for All VC Reputation at t+1

	Variables	Non-lead VC	Lead VC
VC-level attributes	Reputation$_t$	0.99***	0.82***
		(0.07)	(0.06)
	Status$_t$	0.15	0.03
		(0.34)	(0.30)
	Structural holes	−4.48	−1.82
		(2.96)	(3.34)
	Investment preference for early stage	−1.54	2.73
		(3.08)	(2.14)
Portfolio-level attributes excl. focal firm attributes	Company age	−0.87	−1.12
		(0.55)	(0.77)
	Revenue	−0.31	−0.50
		(0.26)	(0.48)
	Industry-adjusted ROA	0.22	−3.74*
		(1.49)	(1.65)
	MTB	0.50*	0.39
		(0.20)	(0.31)
	VC ownership	0.07	−0.22†
		(0.08)	(0.12)
	Other delisting events in the portfolio firms	−0.38	−0.72†
		(0.23)	(0.38)
Focal firm attributes	Company age	−5.17***	−3.72
		(1.57)	(2.54)
	Revenue	0.04	0.38
		(0.29)	(0.55)
	Industry-adjusted ROA	−0.59	0.34
		(1.73)	(1.03)
	MTB	0.20	−0.61
		(0.58)	(0.61)
	VC ownership	0.05	−0.07†
		(0.12)	(0.04)
	Surviving years	0.52**	0.16
		(0.20)	(0.35)
	Delisting	3.14	−6.09*
		(2.37)	(2.39)
Estimation statistics	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	0.47	0.60
	AR(1)	0.00	0.00
	AR(2)	0.68	0.22
	Wald χ^2 statistic	3517.53***	4773.48***

Notes: Robust standard errors (clustered at the VC level) in parentheses; two-sided tests. Year dummies included, but not reported. AR(1) and AR(2) represent p-values from the Arellano-Bond test for first- and second-order serial correlation in the disturbances.

$^{\dagger}p < 0.10$
$^{*}p < 0.05$
$^{**}p < 0.01$
$^{***}p < 0.001$

subsequent reputations. Table 3 summarizes the results. Our subsample analyses consisting of only non-lead VCs show that this relationship is not significant. The partial adjustment coefficient ρ capturing the influence of the one-year lagged reputation on the current year reputation 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 χ^2 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.

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 would 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 conducted 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 ith observation is excluded.

The jackknife coefficient for delisting is −6.09, which is the same as the original AB coefficient. Further, Hypothesis 1 is supported at $p < 0.05$ in all the 31 replications (Hypothesis 2 remains unsupported). The Jackknife coefficient for the interaction with MTB is 4.09, which is also close to the original 4.03 AB coefficient. Further, Hypothesis 3 is supported at $p < 0.10$ in 27 of the 31 replications. While Hypothesis 3 is not supported in four replications, it is supported at $p < 0.05$ in six 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 1.05, which is again close to the original 1.04 AB coefficient. Not surprisingly, Hypothesis 4 is strongly supported at $p < 0.05$ in 29 out of 31 replications. For the remaining two replications, Hypothesis 4 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% of the firms in our sample went public in 1999 and 2000, which was 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 reran 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 *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 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.[15] 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 continued to be supported ($p = 0.007$).

DISCUSSION

In this study, we considered whether firms that provide endorsements suffer reputational penalties

[15] 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.

when the firms they endorse fail to meet expectations, and the factors that influence 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 attenuate the reputational penalty. As part of our robustness checks, we also found that the penalty only applies 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 scapegoat when an endorser's reputation creates an expectation that is subsequently violated. We also argued and showed that 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%, and in 71% of the delistings the lead VC had no ownership at all. Further, in 78% 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 reputation, 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 is 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, which thus 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 compared to 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 has tended 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 their 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 of 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 a 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 whose 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 VCs 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 United States. 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 our study 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, such as portfolio firm delistings. If reputations are easily damaged, are they also easily fixed? And which repair mechanisms (see Elsbach [2012] and Rhee and 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 contribute 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 that are 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 these 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."

REFERENCES

Ahmad, W., & Jelic, R. 2014. Lockup agreements and survival of U.K. IPOs. ***Journal of Business Finance & Accounting***, 41: 717–742.

Ahrens, F. 2010. Toyota's shares slide as its reputation loses steam. ***Washington Post***. Retrieved from http://www.washingtonpost.com/wp-dyn/content/article/2010/02/03/AR2010020302109.html. Accessed February 4, 2010.

Aldrich, H. E., & Fiol, C. M. 1994. Fools rush in? The institutional context of industry creation. ***Academy of Management Review***, 19: 645–670.

Anderson, T. W., & Hsiao, C. 1982. Formulation and estimation of dynamic-models using panel data. ***Journal of Econometrics***, 18: 47–82.

Arellano, M. 2003. ***Panel data econometrics***. Oxford, U.K.: Oxford University Press.

Arellano, M., & Bond, S. 1991. Some tests of specification for panel data: Monte Carlo evidence and an application to employment equations. ***Review of Economic Studies***, 58: 277–297.

Arthurs, J. D., Hoskisson, R. E., Busenitz, L. W., & Johnson, R. A. 2008. Managerial agents watching other agents: Multiple agency conflicts regarding underpricing in IPO firms. ***Academy of Management Journal***, 51: 277–294.

Baltagi, B. H. 2008. ***Econometric analysis of panel data*** (4th ed.). Chichester, U.K.: John Wiley & Sons.

Barber, B. M., & Lyon, J. D. 1996. Detecting abnormal operating performance: The empirical power and specification of test statistics. ***Journal of Financial Economics***, 41: 359–399.

Boeker, W. 1992. Power and managerial dismissal: Scapegoating at the top. ***Administrative Science Quarterly***, 37: 400–421.

Bonacich, P. 1987. Power and centrality: A family of measures. ***American Journal of Sociology***, 92: 1170–1182.

Bowman, E. H. 1976. Strategy and the weather. ***Sloan Management Review***, 17: 49–62.

Bowman, E. H. 1978. Strategy, annual reports and alchemy. ***California Management Review***, 20: 64–71.

Boyd, B. K., Gove, S., & Hitt, M. A. 2005. Consequences of measurement problems in strategic management research: The case of Amihud and Lev. ***Strategic Management Journal***, 26: 367–375.

Brown, T. A. 2006. ***Confirmatory factor analysis for applied research***. New York, NY: The Guilford press.

Burgoon, J. K. 1978. A communication model of personal space violations: Explication and an initial test. ***Human Communication Research***, 4: 129–142.

Burgoon, J. K., & Le Poire, B. A. 1993. Effects of communication expectancies, actual communication, and expectancy disconfirmation on evaluations of communicators and their communication behavior. ***Human Communication Research***, 20: 67–96.

Burt, R. S. 2004. Structural holes and good ideas. ***American Journal of Sociology***, 110: 349–399.

Cameron, A. C., & Trivedi, P. K. 2005. ***Microeconometrics: Methods and applications***. Cambridge, U.K.: Cambridge University Press.

Carter, R., & Manaster, S. 1990. Initial public offerings and underwriter reputation. ***Journal of Finance***, 45: 1045–1067.

Certo, S. T. 2003. Influencing initial public offering investors with prestige: Signaling with board structures. ***Academy of Management Review***, 28: 432–446.

Chen, G., Hambrick, D. C., & Pollock, T. G. 2008. Puttin' on the ritz: Pre-IPO enlistment of prestigious affiliates as deadline-induced remediation. ***Academy of Management Journal***, 51: 954–975.

Cohen, J., Cohen, P., West, S. G., & Aiken, L. S. 2003. ***Applied multiple regression/correlation analysis for the behavioral sciences*** (3rd ed.). Mahwah, NJ: Lawrence Erlbaum Associates.

Connelly, B. L., Certo, S. T., Ireland, R. D., & Reutzel, C. R. 2011. Signaling theory: A review and assessment. ***Journal of Management***, 37: 39–67.

Daily, C. M., & Dalton, D. R. 2001. Signaling firm value through board structure: An investigation of initial public offerings. ***Entrepreneurship Theory and Practice***, 26: 33–50.

Dechow, P., Ge, W., & Schrand, C. 2010. Understanding earnings quality: A review of the proxies, their determinants and their consequences. ***Journal of Accounting and Economics***, 50: 344–401.

Demers, E., & Lewellen, K. 2003. The marketing role of IPOs: Evidence from internet stocks. ***Journal of Financial Economics***, 68: 413–437.

Deutsch, Y., & Ross, T. W. 2003. You are known by the directors you keep: Reputable directors as a signaling mechanism for young firms. ***Management Science***, 49: 1003–1017.

Dimov, D., Shepherd, D. A., & Sutcliffe, K. M. 2007. Requisite expertise, firm reputation, and status in venture capital investment allocation decisions. ***Journal of Business Venturing***, 22: 481–502.

Elsbach, K. D. 2012. A framework for reputation management over the course of evolving controversies. In M. L. Barnett & T. G. Pollock (Eds.), ***The Oxford***

handbook of corporate reputation: 466–485. Oxford, U.K.: Oxford University Press.

Fama, E. F., & French, K. R. 2004. New lists: Fundamentals and survival rates. ***Journal of Financial Economics***, 73: 229–269.

Field, L. C., & Hanka, G. 2001. The expiration of IPO share lockups. ***Journal of Finance***, 56: 471–500.

Fischer, E., & Reuber, R. 2007. The good, the bad, and the unfamiliar: The challenges of reputation formation facing new firms. ***Entrepreneurship Theory and Practice,*** 31: 53–75.

Fischer, H. M., & Pollock, T. G. 2004. Effects of social capital and power on surviving transformational change: The case of initial public offerings. ***Academy of Management Journal***, 47: 463–481.

Fombrun, C. J. 2001. Corporate reputations as economic assets. In M. A. Hitt, E. Freeman & J. S. Harrison (Eds.), ***The Blackwell handbook of strategic management***: 289–312. Oxford, U.K.: Blackwell Publishers.

Fund, B. R., Pollock, T. G., Baker, T., & Wowak, A. J. 2008. Who's the new kid? The process of developing centrality in venture capitalist deal networks. ***Network strategy***: 563–593.

Gammoh, B. S., Voss, K. E., & Chakraborty, G. 2006. Consumer evaluation of brand alliance signals. ***Psychology and Marketing***, 23: 465–486.

Garg, S. 2013. Venture boards: Distinctive monitoring and implications for firm performance. ***Academy of Management Review***, 38: 90–108.

Gompers, P. A. 1996. Grandstanding in the venture capital industry. ***Journal of Financial Economics***, 42: 133–156.

Gompers, P. A., & Lerner, J. 2004. ***The venture capital cycle***. Chicago, IL: MIT press.

Gomulya, D., & Boeker, W. 2016. Reassessing board member allegiance: CEO replacement following financial misconduct. ***Strategic Management Journal***, 37: 1898–1918.

Gomulya, D., & Mishina, Y. 2017. Signaler credibility, signal susceptibility, and relative reliance on signals: How stakeholders change their evaluative processes after violation of expectations and rehabilitative efforts. ***Academy of Management Journal***, 60: 554–583.

Gorman, M., & Sahlman, W. A. 1989. What do venture capitalists do? ***Journal of Business Venturing***, 4: 231–248.

Graffin, S. D., Haleblian, J., & Kiley, J. T. 2016. Ready, AIM, acquire: Impression offsetting and acquisitions. ***Academy of Management Journal***, 59: 232–252.

Greene, W. H. 2012. ***Econometric analysis*** (7th ed.). Boston, MA: Prentice Hall.

Gulati, R., & Higgins, M. C. 2003. Which ties matter when? The contingent effects of interorganizational partnerships on IPO success. ***Strategic Management Journal***, 24: 127–144.

Guler, I. 2007. Throwing good money after bad? Political and institutional influences on sequential decision making in the venture capital industry. ***Administrative Science Quarterly***, 52: 248–285.

Hallen, B. L., & Pahnke, E. C. 2016. When do entrepreneurs accurately evaluate venture capital firms' track records? A bounded rationality perspective. ***Academy of Management Journal***, 59: 1535–1560.

Hannan, M. T., & Freeman, J. 1977. The population ecology of organizations. ***American Journal of Sociology***, 82: 929–964.

Hansen, L. P. 1982. Large sample properties of generalized-method of moments estimators. ***Econometrica***, 50: 1029–1054.

Hayashi, F. 2000. ***Econometrics***. Princeton, NJ: Princeton University Press.

Heider, F. 1958. ***The psychology of interpersonal relations***. New York, NY: Wiley.

Higgins, M. C., & Gulati, R. 2003. Getting off to a good start: The effects of upper echelon affiliations on underwriter prestige. ***Organization Science***, 14: 244–263.

Higgins, M. C., & Gulati, R. 2006. Stacking the deck: The effects of top management backgrounds on investor decisions. ***Strategic Management Journal***, 27: 1–25.

Hinkin, T. R. 1995. A review of scale development practices in the study of organizations. ***Journal of Management***, 21: 967–988.

Hochberg, Y. V., Ljungqvist, A., & Lu, Y. 2007. Whom you know matters: Venture capital networks and investment performance. ***Journal of Finance***, 62: 251–301.

Hsu, D. H. 2004. What do entrepreneurs pay for venture capital affiliation? ***Journal of Finance***, 59: 1805–1844.

Hubbard, T. D., Pollock, T. G., Pfarrer, M. D., & Rindova, V. P. 2018. Safe bets or hot hands? How status and celebrity influence strategic alliance formations by newly public firms. ***Academy of Management Journal***, 61: 1976–1999.

Husick, G. C., & Arrington, J. M. 1998. ***The initial public offering: A practical guide for executives***. New York, NY: Bowne & Co.

Ibbotson, R. G., & Ritter, J. R. 1995. Initial public offerings. In R. Jarrow, V. Maksimovic & W. T. Ziemba (Eds.), ***Handbooks in operations research and management science***: 993–1016. Amsterdam, Netherlands: Elsevier Science B.V.

Jain, B. A., & Kini, O. 2000. Does the presence of venture capitalists improve the survival profile of IPO firms? ***Journal of Business Venturing***, 27: 1139–1176.

Jones, E. E., & Harris, V. A. 1967. The attribution of attitudes. ***Journal of Experimental Social Psychology***, 3: 1–24.

Kang, E. 2008. Director interlocks and spillover effects of reputational penalties from financial reporting fraud. ***Academy of Management Journal***, 51: 537–555.

Kaplan, S. N., & Lerner, J. 2010. It ain't broke: The past, present, and future of venture capital. ***Journal of Applied Corporate Finance***, 22: 36–47.

Kelley, H. H. 1973. The process of causal attributions. ***American Psychologist***, 28: 107–128.

Kim, S. 2014. The role of prior expectancies and relational satisfaction in crisis. ***Journalism & Mass Communication Quarterly***, 91: 139–158.

Lange, D., Lee, P. M., & Dai, Y. 2011. Organizational reputation: A review. ***Journal of Management***, 37: 153–184.

Lee, P. M., Pollock, T. G., & Jin, K. 2011. The contingent value of venture capitalist reputation. ***Strategic Organization***, 9: 33–69.

Lee, P. M., & Wahal, S. 2004. Grandstanding, certification and the underpricing of venture capital backed IPOs. ***Journal of Financial Economics***, 73: 375–407.

Lester, R. H., Certo, S. T., Dalton, C. M., Dalton, D. R., & Cannella, A. A. 2006. Initial public offering investor valuations: An examination of top management team prestige and environmental uncertainty. ***Journal of Small Business Management***, 44: 1–26.

Loughran, T., & Ritter, J. R. 1995. The new issues puzzle. ***Journal of Finance***, 50: 23–51.

Ma, D., Rhee, M., & Yang, D. 2013. Power source mismatch and the effectiveness of interorganizational relations: The case of venture capital syndication. ***Academy of Management Journal***, 56: 711–734.

Mishina, Y., Block, E. S., & Mannor, M. J. 2012. The path dependence of organizational reputation: How social judgment influences assessments of capability and character. ***Strategic Management Journal***, 33: 459–477.

Mitchell, T. R. 1982. Attributions and actions: A note of caution. ***Journal of Management***, 8: 65–74.

Mouri, N., Sarkar, M. B., & Frye, M. 2012. Alliance portfolios and shareholder value in post-IPO firms: The moderating roles of portfolio structure and firm-level uncertainty. ***Journal of Business Venturing***, 27: 355–371.

Neter, J., Wasserman, W., & Kutner, M. H. 1990. ***Applied linear statistical models: Regression, analysis of variance, and experimental designs***. Homewood, IL: Richard D. Irwin.

Nickell, S. 1981. Biases in dynamic models with fixed effects. ***Econometrica***, 49: 1417–1426.

Pahnke, E. C., McDonald, R., Wang, D., & Hallen, B. 2015. Exposed: Venture capital, competitor ties and entrepreneurial innovation. ***Academy of Management Journal***, 58: 1334–1360.

Park, H. D., & Steensma, H. K. 2013. The selection and nurturing effects of corporate investors on new venture innovativeness. ***Strategic Entrepreneurship Journal***, 7: 311–330.

Petkova, A. 2012. From the ground up: Building young firms' reputations. In T. G. Pollock & M. L. Barnett (Eds.), ***The Oxford handbook of corporate reputation***: 383–401. Oxford, U.K.: Oxford University Press.

Petkova, A. P., Wadhwa, A., Yao, X., & Jain, S. 2014. Reputation and decision making under ambiguity: A study of U.S. venture capital firms' investments in the emerging clean energy sector. ***Academy of Management Journal***, 57: 422–448.

Pfarrer, M. D., Decelles, K. A., Smith, K. G., & Taylor, M. S. 2008. After the fall: Reintegrating the corrupt organization. ***Academy of Management Review***, 33: 730–749.

Pfeffer, J., & Salancik, G. R. 1978. ***The external control of organizations: A resource dependence perspective***. New York, NY: Harper & Row.

Podolny, J. 1993. A status-based model of market competition. ***American Journal of Sociology***, 98: 829–872.

Podolny, J. 2001. Networks as the pipes and prisms of the market. ***American Journal of Sociology***, 107: 33–60.

Pollock, T. G. 2004. The benefits and costs of underwriters' social capital in the U.S. initial public offerings market. ***Strategic Organization***, 2: 357–388.

Pollock, T. G., Chen, G., Jackson, E. M., & Hambrick, D. C. 2010. How much prestige is enough? Assessing the value of multiple types of high-status affiliates for young firms. ***Journal of Business Venturing***, 25: 6–23.

Pollock, T. G., & Gulati, R. 2007. Standing out from the crowd: The visibility-enhancing effects of IPO-related signals on alliance formation by entrepreneurial firms. ***Strategic Organization***, 5: 339–372.

Pollock, T. G., Lee, P. M., Jin, K., & Lashley, K. 2015. (Un) tangled exploring the asymmetric coevolution of new venture capital firms' reputation and status. ***Administrative Science Quarterly***, 60: 482–517.

Pollock, T. G., Rindova, V. P., & Maggitti, P. G. 2008. Market watch: Information and availability cascades among the media and investors in the U.S. IPO market. ***Academy of Management Journal***, 51: 335–358.

Rajan, R., & Servaes, H. 1997. Analyst following of Initial Public Offerings. ***Journal of Finance***, 52: 507–529.

Rao, D. 2013, July 22. Why 99.95% of entrepreneurs should stop wasting time seeking venture capital. ***Forbes***. Retrieved from: https://www.forbes.com/sites/dileeprao/

2013/07/22/why-99-95-of-entrepreneurs-should-stop-wasting-time-seeking-venture-capital/#41ddaddc46eb.

Reuber, A. R., & Fischer, E. 2005. The company you keep: How young firms in different competitive contexts signal reputation through their customers. ***Entrepreneurship Theory and Practice***, 29: 57–78.

Rhee, M., & Haunschild, P. R. 2006. The liability of good reputation: A study of product recalls in the U.S. automobile industry. ***Organization Science***, 17: 101–117.

Rhee, M., & Kim, T. 2012. After the collapse: A behavioral theory of reputation repair. In M. L. Barnett & T. G. Pollock (Eds.), ***The Oxford handbook of corporate reputation:*** 446–465. Oxford, U.K.: Oxford University Press.

Rindova, V. P., Williamson, I. O., Petkova, A. P., & Sever, J. M. 2005. Being good or being known: An empirical examination of the dimensions, antecendents, and consequences of organizational reputation. ***Academy of Management Journal***, 48: 1033–1049.

Roodman, D. 2009. A note on the theme of too many instruments. ***Oxford Bulletin of Economics and Statistics***, 71: 135–158.

Roodman, D. 2009. How to do xtabond2: An introduction to difference and system GMM in Stata. ***Stata Journal***, 9: 86–136.

Salancik, G. R., & Meindl, J. R. 1984. Corporate attributions as strategic illusions of management control. ***Administrative Science Quarterly***, 29: 238–254.

Sanders, W. G., & Boivie, S. 2004. Sorting things out: Valuation of new firms in uncertain markets. ***Strategic Management Journal***, 25: 167–186.

Sapienza, H. J. 1992. When do venture capitalists add value? ***Journal of Business Venturing***, 7: 9–27.

Shymko, Y., & Roulet, T. J. 2017. When does Medici hurt da Vinci? Mitigating the signaling effect of extraneous stakeholder relationships in the field of cultural production. ***Academy of Management Journal***, 60: 1307–1338.

Sine, W. D., Mitsuhashi, H., & Kirsch, D. A. 2006. Revisiting Burns and Stalker: Formal structure and new venture performance in emerging economic sectors. ***Academy of Management Journal***, 49: 121–132.

Spence, M. 1973. Job market signaling. ***Quarterly Journal of Economics***, 87: 355–374.

StataCorp. 2015. ***Stata: Release 14. Statistical software***. College Station, TX: StataCorp LP.

Staw, B., McKechnie, P., & Puffer, S. 1983. The justification of organizational performance. ***Administrative Science Quarterly***, 28: 582–600.

Stinchcombe, A. L. 1965. Social structure and organizations. In J. G. March (Ed.), ***Handbook of organizations***: 142–193. Chicago, IL: Rand McNally.

Stuart, T. E., Hoang, H., & Hybels, R. C. 1999. Interorganizational endorsements and the performance of entrepreneurial ventures. ***Administrative Science Quarterly***, 44: 315–349.

Teoh, S. H., Wong, T. J., & Rao, G. R. 1998. Are accruals during initial public offerings opportunistic? ***Review of Accounting Studies***, 3: 175–208.

Tsang, E. W., & Blevins, D. P. 2015. A critique of the information asymmetry argument in the management and entrepreneurship underpricing literature. ***Strategic Organization***, 13: 247–258.

Weesie, J. 1999. Seemingly unrelated estimation and the cluster-adjusted sandwich estimator. ***Stata Technical Bulletin***, 52: 34–47.

Weiner, B. 1986. ***An attributional theory of motivation and emotion***. New York, NY: Springer-Verlag.

Welbourne, T. M., & Andrews, A. O. 1996. Predicting the performance of initial public offerings: Should human resource management be in the equation? ***Academy of Management Journal***, 39: 891–919.

Worthington, R. L., & Whittaker, T. A. 2006. Scale development research. A content analysis and recommendations for best practices. ***Counseling Psychologist***, 34: 806–838.

Wright, M., & Lockett, A. 2003. The structure and management of alliances: syndication in the venture capital industry. ***Journal of Management Studies***, 40: 2073–2102.

Xia, J., Dawley, D. D., Jiang, H., Ma, R., & Boal, K. B. 2016. Resolving a dilemma of signaling bankrupt firm emergence: A dynamic integrative view. ***Strategic Management Journal***, 37: 1754–1764.

Zavyalova, A., Pfarrer, M. D., Reger, R. K., & Shapiro, D. L. 2012. Managing the message: The effects of firm actions and industry spillovers on media coverage following wrongdoing. ***Academy of Management Journal***, 55: 1079–1101.

David Gomulya (dgomulya@smu.edu.sg) is an assistant professor at the Singapore Management University. His research centers on strategy, entrepreneurship, and organization. He currently examines the advantages and disadvantages of various relationships and associations among organizations and key individuals in influencing the social evaluations and performance of established firms and new ventures. He currently examines how relationships among organizations and key individuals influence the social evaluations and performance of firms. He has published in *Academy of Management Journal*, *Journal of Applied Psychology*, and *Strategic Management Journal*.

Kyuho Jin (kyuhojin@gist.ac.kr) is an assistant professor at Gwangju Institute of Science and Technology. He studied at the W.P. Carey School of Business at Arizona State University and received his PhD from Seoul National University. His research centers on how networks evolve through an individual actor's agency and how they regulate the social construction process of markets regarding reputation and status.

Peggy M. Lee (Peggy.Lee@asu.edu) is an associate professor in the Department of Management and Entrepreneurship at the W.P. Carey School of Business at Arizona State University. She received her PhD in strategic management from the University of North Carolina—Chapel Hill and has previously taught at the Goizueta Business School at Emory University and the McCombs School of Business at the University of Texas at Austin. Her research interests focus on how economic and behavioral aspects of corporate governance effect firm actions and performance.

Timothy G. Pollock (tpollock@utk.edu) is the Haslam Chair in Business and Distinguished Professor of Entrepreneurship in the Haslam College of Business at The University of Tennessee. His research focuses on how reputation, celebrity, social capital, media accounts, and power influence corporate governance and strategic decision making in entrepreneurial firms and the social construction of entrepreneurial markets.