

Eclipsed and confounded identities: when high-status affiliations impede organizational growth

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I propose that an organization might impair its ability to acquire resources for growth if its identity is confounded with the identities of high-status organizations with which it collaborates and competes. The theory is tested using data on U.S. venture capital firm syndication between 1995 and 2009. Stringent estimators replicate results across an array of related independent variables for the status of a firm's co-syndicating partners, related dependent variables for the future investment activity of the firm, and estimators. Controlling for their own status, venture capital firms with higher-status co-syndicators are less likely to raise a new fund and invest less in the future. Further analyses provide evidence consistent with the mechanism claim that this is an identity effect. Highlighting the greater overall importance of status-based market competition, the negative effect of partner status is greater in subsamples in which the positive effect of status is also greater. Moreover, the positive effect of status and the negative effect of partner status are robust to controls for the future performance of the firm, suggesting that the effects are driven by audience perceptions rather than time-varying differences in the quality of the firms that is not otherwise captured in contemporary controls. These findings highlight previously neglected identity costs high-status partners impose on affiliates with whom they collaborate and compete. In status-based market competition organizations benefit from distinguishing their identity claims from the established elite.

Keywords: Identity, distinctiveness, status, networks, resource acquisition, growth, venture capital

How organizations garner favorable responses from important audiences is a central question in broad streams of macro organizational theory. Central to these literatures is the importance of how external audiences view the organization. Among these literatures, the identity and status literatures hold strong notions that a social actor's identity and status as audiences perceive them are defined by, or at least reflected in, his or her social network connections or positions. However, a critical tension exists between these two literatures regarding the benefit of high-status affiliations. Whereas the status literature has theorized and demonstrated the value of high-status affiliations, the identity literature hints their potential drawbacks and limits in theory and anecdote.

The status literature argues that high-status affiliates should benefit firms as they signal to a

firm's audiences that its exchange partners deem the firm worthy of affiliation. Under the assumption that high-status firms are selective in their exchange partner choice (cf. Podolny, 1994; Chung et al., 2000), high-status affiliations reassure third-party audiences that the focal firm is a high-quality organization. Consequently, audiences commit resources more willingly to organizations with high-status affiliates (e.g., Stuart et al., 1999; Khaire, 2010). The early microsociological identity literature, on the other hand, suggests that those strongly associated with high-status affiliates may be perceived as "hirelings" (Weber, 1991), signal inferiority by deferring to those whom they accept to be superior (Homans, 1974), raise doubt about their stand-alone merit (Merton, 1968), and receive less attention and rewards for equal quality contributions than their high-status peers (Merton, 1968). Social actors, including firms, that attempt to establish a high-status identity on the coattails or in the shadow of their affiliates might thus fail to distinguish themselves from the established elite and be discounted by their audiences compared to equally accomplished social actors that did not (have to) rely on such affiliations.

To bring this tension into relief, I propose that the positive effect of high-status affiliations is questionable when social actors with similar capabilities collaborate in their activities but compete for approval from the same audiences. Without overlap in their audience(s), the competition for approval and the associated rewards would be limited. Without collaboration among firms with similar capabilities, a firm's audiences would not face an observability or measurement problem regarding the underlying quality of the firm. Thus if firms with similar capabilities collaborate in what is essentially teamwork with unobservable inputs, then uncertainty about the underlying quality of the firm is not an exogenous market characteristic or a predetermined firm characteristic, but endogenous and endemic to the nature of collaborative effort.

This characteristic of teamwork makes it difficult for a firm to signal its independent merit and should thus heighten the importance to distinguish the firm from those with whom it collaborates. Being distinctive from the high-status firms in an industry should be particularly important as high-status actors attract a disproportional share of attention and resources from their

audiences (e.g., Castellucci and Ertug, 2010; Malter, 2014; Simcoe and Waguespack, 2011; Stuart et al., 1999). The nature of teamwork and the absorptive effect of status give rise to the central proposition of this paper: firms that collaborate too intensely with high-status firms in their industry may fail to signal distinctiveness and see their identities discounted by their audiences. The resulting main hypothesis is that audiences commit resources less willingly to organizations that fail to distinguish their identities from the established elite.

This paper is not the first to stress the role of distinctive identities in markets. On the industry level, identity-based competition has been documented in the organizational ecology literature for the competition among organizational forms (e.g., Carroll and Swaminathan, 2000). On the firm and product levels, extant literature has focused on optimal distinctiveness (Deepphouse, 1999; Gioia et al., 2010; Navis and Glynn, 2011), the value of uniqueness (Bishop Smith, 2011), and on how to craft brand authenticity (Beverland, 2005). Much of this work has focused on the departure of organizations from implicit norms, standards, or average behaviors of their industry, and product differentiation.¹ Notwithstanding the contributions these papers make to our understanding of organizational identities as external audiences perceive them, these approaches could be under-socialized if the competition for identity were most pronounced among those organizations whose identities are intertwined through existing exchange relationships (cf. Bothner et al., 2010).

Based on this consideration, this paper takes a structural approach to identity and distinctiveness. This sociologically grounded approach rests on Goffman's (1986) observation that a social identity as external audiences perceive it is defined by those with whom a social actor is seen in public and White's (1992) insight that identities interpenetrate and constrain each other. Using status as a particularly salient feature of identity (Jensen et al., 2011), I test whether firms whose identities are more strongly associated with high-status firms with whom they collaborate in projects but compete for resources impair their chances to raise new capital and grow.

¹On the flip side, much of the work on organizational categories has investigated the penalties associated with deviating from such norms and standards (for a review, see Hannan, 2010).

By focusing on the status of a firm's exchange partners as a determinant of its identity, the paper aims to not only show that an organization must distinguish itself from other firms in its industry, but also from whom.

The empirical setting is the venture capital industry between 1995 and 2009. The choice of this context is instrumental, as previous literature has emphasized the benefits of high network status, which a firm derives directly from a central position among centrally located others, in this and related financial services industries (e.g., Bothner et al., 2014; Hochberg et al., 2007; Podolny, 1993, 2005).² Demonstrating that high-status affiliations impose identity costs on their affiliates in the same context in which their benefits have been so amply demonstrated would thus be particularly informative about status-based competitive tensions between the identities of collaborating firms.

I analyze how the degree to which a firm's identity is confounded with the identities of high-status co-syndicators affects its likelihood to raise a new fund. Highly similar estimates from fixed effects, linear dynamic panel data, and matching estimators show that, conditional on its own status, a venture capital firm whose identity is more confounded with the identities of high-status co-syndicators is less likely to raise a new venture capital fund. Limited partners commit resources more willingly to organizations that are able to distinguish themselves from the established elite. The results replicate across a number of related independent variables for the status of a firm's exchange partners and dependent variables for the future investment activity of the firm. Further analyses provide evidence consistent with the mechanism claim that this is an identity effect. The negative effect of a confounded identity is stronger on firms that attempt to lay claim to a high-status identity and in subsamples in which the status effect is stronger; the effects are robust to controls for the future performance of the firm, indicating that the effect is driven by audience perceptions and not by time-varying heterogeneity in firm

²Hochberg et al. (2007) showed that among a number of network variables centrality (the commonly used measure for status in the organizational literature) had the largest effect on the investment performance of venture capital firms.

quality that is not otherwise captured in contemporary controls.

Theory

A burgeoning body of organizational literature over the last twenty plus years has studied how firms can better create identities that appeal to their audiences. An important insight from these literatures is that market actors' identities can be defined at the intersection of horizontal and vertical differentiation, that is, by the market domains in which they operate and their position in the social orders within these domains. For horizontal differentiation, the literature has focused, for example, on geographies, industries, or market niches therein. For vertical differentiation, the literature has focused on constructs like quality, reputation, and status.³

Whereas quality and the reputation for quality are *prima facie* important producer or product attributes, status as a social cue is not. Nonetheless, status has been shown to allow extracting greater effort from exchange partners (Castellucci and Ertug, 2010), attaining a lower cost position (Podolny, 1993), charging higher prices (Malter, 2014), receiving more audience attention (Simcoe and Waguespack, 2011), reaching broader or new audiences (Kovács and Sharkey, 2014), and to amplify rewards for quality and reputation for quality (Malter, 2014; Benjamin and Podolny, 1999) as well as benefit resource acquisition and firm survival (Stuart et al., 1999; Bothner et al., 2014). Whereas early status literature was not much interested in the distinction between quality or reputation and status, even accepting that it could be considered “reputation status” (Podolny, 2005), recent literature with strong causality claims has provided evidence for causal symbolic effects of status that cannot be explained by differences in underlying quality (Simcoe and Waguespack, 2011; Azoulay et al., 2013; Kovács and Sharkey, 2014; Malter,

³Within a narrowly defined domain in which the legitimacy and authenticity of producers was not in question, (Malter, 2014) showed among wine producers from Bordeaux that quality, reputation, and status are overwhelmingly important producer attributes that together with year fixed effects explained 85 to 90 percent of the variance in product prices.

2014).⁴ Overall, this body of work shows that status is an important lens through which audiences evaluate, allocate attention and resources to, and determine rewards for market actors' identities.⁵ This suggests that it is imperative to include considerations of status into a treatment of organizational identity.

The notion that social identity more broadly and status as a facet of identity more narrowly are embodied in a social actor's visible affiliations is common to both the identity and status literatures (Goffman, 1986; White, 1992; Podolny, 1993, 2005). Whereas a firm's identity will become visible in the multitude of its affiliations to suppliers, buyers, shareholders, stakeholders, critics or analysts, and so on, status as an important dimension of identity has been argued to be embodied in or reflected by the networks of direct or indirect affiliations of peer organizations with one another. Viewing networks of direct connections as reflecting deference of social actors to one another and networks of indirect connections as reflecting deference from an important audience, greater centrality in exchange networks has come to be interpreted as a position of higher status (Bonacich, 1987; Podolny, 1993). Diverse networks such as networks among investment banks through underwriting securities (Podolny, 1993), venture capital firms through their investments (Hochberg et al., 2007; Bothner et al., 2014), semiconductor firms through patent citations (Podolny et al., 1996), academic institutions and Formula 1 teams through applicants or hires (Burris, 2004; Ertug and Castellucci, 2010), or wineries through affiliations with viticultural areas (Benjamin and Podolny, 1999) have all been interpreted as embodying or reflecting a firm's status among its peers.

The well-established benefits of status, and network status in particular, may serve as the baseline hypothesis for the further development of the theory. For example, Bothner et al. Bothner et al. have shown that network status improves the survival chances of venture capital firms.

⁴However, these effects might be smaller and/or more short-lived than commonly estimated (Azoulay et al., 2013; Malter, 2014).

⁵In a matched sample of prize-winning and non-winning books that were equally likely to win the prize, (Kovács and Sharkey, 2014) also showed that a status shock can affect the composition of the audience with a negative effect of the prize on the popularity of the book among its original audience.

As venture capital funds have a limited lifetime, the survival of the firm is directly linked to its ability to launch a new fund; and the ability to launch a new fund is directly linked to a firm's ability to solicit resource commitments from limited partners (cf. Lerner et al., 2012). We can thus form the following more general baseline hypothesis.

HYPOTHESIS 1.—*The greater the network status of the firm, the more likely it will be able to acquire resources for survival or growth.*

Goffman (1986: p. 47) suggested that audiences might perceive sameness between social actors and the affiliates with whom they appear in public. By that virtue two collaborating peer organizations such as co-investing venture capital firms or co-developing car manufacturers should, for example, be perceived to have more similar identities than firms that do not cooperate. This idea of perceived sameness undergirds the literature on status leakage, transfer, or spillovers from one identity to another, be it in augmenting (Stuart et al., 1999) or detracting ways (Pontikes et al., 2010; Jensen, 2006). From this, the literature has developed the more general understanding that identities that are connected through “networks of intangible flows” interpenetrate (Bothner et al., 2010: p. 944, see also White, 1992). What is less well understood, however, is why social actors might suffer from affiliations with those who are held in high regard.

White asserted that “[d]ispersions are the sources of identities” White (1992: p. 5), suggesting that audiences differentiate even within the sameness of the fairly homogenous identities they might observe. In a way, White has to be tautologically correct. Even an approach geared to identify the relative sameness of identities has to be able to distinguish them from one another because identifying their relative sameness can only be achieved by way of their differences. Status fulfills this requirement by definition because it captures a social actors' positions in a social hierarchy, that is, dispersions in what we have already determined to be an important facet of identity.

All that is needed to make the case for a negative effect of the status of a firm's exchange partners on the focal firm is that there needs to be competition based on identity among them. The identity literature suggests that social actors' positioning in their networks of affiliations embodies an element of identity competition because "identities add through contentions to the contingencies faced by other identities," White (1992: p. 6). "An individual identity is thus a history of positioning acts that is tested in each new interaction," (Czarniawska and Wolff, 1998: pp. 35–36). The very idea of status-based market competition (Podolny, 1993) and the benefits of occupying a high-status position discussed above second this notion.

The last missing piece to this argument is that competition needs to be "local", that is, between direct affiliates. However, this argument is fairly easy to make. Theoretically, if an audience holds a preference or bias for an identity attribute of a social actor such as status, then it ought to hold the same preference or bias for that attribute irrespective of who carries it. To the extent that the resources that the audience can bestow are limited, a focal firm should receive less of them if their affiliates are contenders for the same resources and possess a valued identity trait than when their affiliates do not possess that trait. We know status to have an absorptive effect on limited approval assets such as resources and attention. Hence, the extent to which a focal firm can absorb these based on its status should depend on the extent to which the firm's affiliates absorb them based on their status.

HYPOTHESIS 2.—The more a firm's identity is confounded with the identities of high-status affiliates with whom it collaborates in projects but competes for resources, the less likely it will be able to acquire resources for survival and growth.

Past literature has highlighted that high-status affiliates act as an endorsement for the focal firm (Stuart et al., 1999; Stuart, 2000; Lin et al., 2009). They signal to an audience that the focal firm is a legitimate and high quality firm. We may expect the competitive pressure to distinguish one's identity to increase as a firm rises in status (cf. Phillips and Zuckerman, 2001). On the

flip side, Merton (1968) noted by means of an anecdote that an academic who did all his work with higher-status colleagues might be evaluated harshly in the long run. Analogously, a firm that tries to lay claim to a high-status identity might raise doubts about its independent merit if it has collaborated too intensely with peer high-status firms, because that raises the question to which extent it could have reached its successes on its own or was able to do so only on the coattails of its partners. Hence, I hypothesize that the negative effects of having an eclipsed or confounded identity increases in the status of the firm.

HYPOTHESIS 3A.—*The negative effect of a confounded identity on its ability to acquire resources is stronger on high-status firms.*

We might expect the effects of status and confoundedness hypothesized in hypothesis 1 and 2 to be stronger in some subsamples than in others. I note that mechanism tests whether the status and confoundedness effects are consistent with identity effects do not require *a priori* hypotheses about the specific direction of the effect, although such hypotheses are plausible in some cases. For the effects to be consistent with identity effects it is sufficient that if a firm's outcome is proportional to its audiences' preference or bias for its social status, then it ought to be inversely proportional to the same preference or bias for the status of the firm's exchange partners. Therefore, if the effect of status is amplified (mitigated) by a moderator or in a subsample, then the effect of confoundedness should also be amplified (mitigated) by that moderator or in that subsample.

HYPOTHESIS 3B.—*Moderators that amplify (mitigate) the positive effect of status, amplify (mitigate) the negative effect of confoundedness on a firm's ability to acquire resources.*

Status and confoundedness in venture capital firm identities

Data

To test the hypotheses, I use data on U.S. venture capital firm syndication. The choice of this setting is instrumental, as venture capital syndication and the syndication of similar financial services firms has been used frequently in the status literature (e.g., Bothner et al., 2014; Ozmel et al., 2013; Podolny, 1993). The data were retrieved from VentureXpert and consist of information about all investment rounds recorded for the years 1992 to 2009. The data identify the investing venture capital firms, the target companies, the investment round, the date of an investment, and the amount invested. These data were complemented with data about the location and founding date of the venture capital firms from the same source.

Measures for status and confoundedness are derived from the year-specific co-investment matrices as detailed further below. These data are complemented with data for the dependent and control variables, which were retrieved or derived from the same data and source. All independent and control variables are computed based on the three years prior to the focal year, implying that most of the econometric analysis is performed with data for the years 1995 to 2009. Some of the analyses use future variables either as dependent or independent variables. These variables are computed using three years of forward-looking data. The corresponding analyses are conducted with data for the years 1995 to 2006.

Dependent variable(s). To test the hypotheses, the dependent variable needs to correlate with an external audience's approval of a venture capital firm's identity and reflect its growth. Even though venture capital funds have no set expiration date, their life time is generally limited with an expected lifetime of perhaps ten years. The life cycle of venture capital funds is such that venture capital firms collect money or solicit commitments thereof from limited partners, make initial investments in portfolio firms, and then make follow-on investments from the same fund

into its portfolio companies as desired or needed (Lerner et al., 2012). The bulk of the collection happens before or in the early years of a fund, and thus venture capital firms seek to raise money for new funds on a regular basis to assure the continuing operation and growth of the firm. Or as Bothner et al. (2014) noted: “a focal VC firm fails principally because limited partners—its most consequential audience—lose confidence to such an extent that the focal VC cannot raise new capital and is thus forced to exit.”

A firm’s ability to launch a new fund is thus indicative of limited partners approving of the firm’s identity in a way that assures the continued operation or growth of the firm. I define the variable *fund launched*_{*i,t*} so that it takes 1 in year *t* if firm *i* makes an investment from a fund from which it has never made an investment before and 0 otherwise. I provide evidence for analogous effects on related dependent variables that reflect resource acquisition and growth, such as the future number and sum of investments as well as venture capital firms’ future exits and failures of their portfolio firms. These analyses highlight that the results replicate across related dependent variables and are unlikely to be driven by limited partners’ prescience about the future performance of the venture capital firm.

Independent variable(s). From information about venture capital firms’ participation in investment rounds, it is easy to construct an investment matrix *V* for any given time frame, in which the *v_{if}* capture venture capital firm *i*’s participation in round *f*. The matrix product *VV^T* yields the relational co-investment matrix *R*, in which the *r_{ij}* capture the number of rounds in which venture capital firm *i* has co-invested with venture capital firm *j* over the period of time on which *V* was defined. *V* and *R* are the matrices from which the independent variables and network controls were derived.

Status. I measure status by a venture capital firm’s Bonacich (1987) centrality in the co-investment network, which is the standard metric for status in the organizational network literature (e.g., Podolny, 1993). Higher values of Bonacich centrality capture a more central location among more centrally located others. When based on an adjacency matrix, centrality is in

essence the sum of the discounted number of connections that actors reach in the network. It is computed as:

$$c(\alpha, \beta) = \alpha \sum_{k=0}^{\infty} \beta^k R^{k+1} \quad (1)$$

where R^{k+1} is a relational matrix for actors' connections at the $k + 1$ st remove (actors that a focal actor reaches at a path length of $k + 1$), β^k is the discount factor for connections at the $k + 1$ st remove that determines the direction and degree to which actors benefit (or suffer) from the connections of their connections, the connections of their connections' connections, and so forth, and α is an arbitrary scaling constant, typically to center $c(\alpha, \beta)$ around one (see Bonacich, 1987). The higher the absolute value of β , the lower is the rate of decay with which connections at higher removes are factored into a focal actor's centrality. Consistent with prior literature, I set β to 3/4 times the reciprocal of the largest eigenvalue of R (e.g., Podolny, 1993).

Notwithstanding the well-defined formula for Bonacich centrality, a researcher has some discretion in how exactly V and R should be defined in this setting. First, one has to choose on which level a relationship exists between two venture capital firms. Venture capital firms connect through their investments. One can define the connection on the level of the investment round or the level of the portfolio firm. When defining a connection on the investment round, two firms i and j are connected if and only if they have invested in a portfolio firm in the same investment round. When a connection is defined on the portfolio firm level, two venture capital firms are connected as long as they have invested in the same portfolio firm even if they did not invest in the same round. Second, the relational matrix R can either be defined as the raw co-investment matrix, so that the r_{ij} capture the number of times firms i and j co-invested. Alternatively, R can be defined as the proportional co-investment matrix, where the r_{ij} are the proportions of i 's investments in which j co-invests with i . Whereas the raw co-investment matrix is necessarily symmetric, the proportional co-investment matrix is not.

Even though Bonacich (1987) notes in a footnote that his measure captures centrality when computed on symmetric matrices and prestige status when computed on asymmetric matrices,

this is largely a distinction without a difference in this empirical context. The correlations of the four centrality scores computed based on the two definitions of a connection existing between two firms by the two approaches to defining the r_{ij} exceed 0.927. I carried forward the analyses for the centrality scores computed for the round-based, raw co-investment matrix and the portfolio-company-based, proportional co-investment matrix, for which the correlation was 0.93. I re-computed all other network variables accordingly. Generally, the results reported are for the analyses that use the round-based adjacency matrix, which had slightly more power in explaining the data. I provide evidence for the robustness of the main results to the alternative specification of R when I discuss the results using alternative independent variables.

Confoundedness.

To measure the degree to which venture capital firm i 's identity is confounded with the identities of high-status co-syndicators, I weight the status scores of firm j by the proportion p_{ij} of i 's deals in which it collaborated with j and take the sum over these terms. Formally:

$$\text{confoundedness}_{i,t} = \sum_{j \neq i} p_{ij,t} s_{j,t} \quad (2)$$

Firms with whom i does not collaborate do not contribute to i 's confoundedness score because for these firms $p_{ij} = 0$. A firm that undertakes all investment projects by itself would thus have a confoundedness score of zero. For a firm that undertakes every one of its projects with the same set of partners, so that its identity would be fully confounded with the identities of its co-syndicators. The p_{ij} would be 1, and each of its co-syndicators would contribute its full status score to i 's confoundedness score. The respective others equal, firm i 's confoundedness score thus increases in the number of its co-syndicators, the status of its co-syndicators, and the intensity of its interactions with them. Note that confoundedness is generally not symmetric. Whereas i 's identity may be fully confounded with j 's, j 's identity need not be fully confounded with i 's.⁶

⁶If either all proportions p_{ij} were equal or the status of all of i 's co-investors s_j were equal, then the confound-

I test the robustness of the result to a specification in which I replace the confoundedness variable with the average maximum co-investor status of firm i 's co-syndicators. It is computed as the status score of i 's highest-status exchange partner in a round (alternatively, portfolio firm), averaged over all investment rounds (alternatively, portfolio firms) in which firm i invested in the years $t - 1$ to $t - 3$. The average maximum co-investor status correlates 0.975 with the confoundedness score, indicating that the confoundedness score is largely driven by a venture capital firm's highest-status co-syndicators. The average maximum co-investor status also enables a complementary analysis that might yield additional insights. Because the average maximum co-investor status is measured on the same scale as the status score, it allows testing whether high-status affiliates primarily impose identity costs on the focal actor, if any, when their status exceeds or falls short of the focal actor's status.

Control variables.

The control variables aim to account for alternative explanations for the effects of having a high-status or confounded identity such as the performance or quality of the venture capital firm that might affect its ability to acquire resources and launch a new fund. We should expect status and confoundedness scores to increase in the total number of a firm's exchange partners as well as in the average number of exchange partners per exchange. Therefore, I implement the control variable *degree* for the number of distinct venture capital firms with which a focal venture capital firm has co-syndicated investment rounds in the years $t - 1$ to $t - 3$.⁷ Similarly, I implement the control *average # of investors in round* to account for the typical size of the syndicates with which the focal firm invests.

A venture capital firm's underlying quality should drive its performance and its performance,

edness measure would collapse to the product between the average proportion of investments that i makes with its co-investors and the average status of these co-investors. In this case, it would be equivalent to an interaction term (the product) between these two variables. If neither the p_{ij} nor the s_j are equal, the vector product does not collapse to the product between the two averages. In that case co-syndicators that occupy higher-status positions and with whom i is strongly associated contribute more strongly to a focal actor's confoundedness.

⁷Sauder et al. (2012) noted that it is unknown whether Bonacich centrality (status) and degree centrality have any distinguishable effects and thus discriminant validity. Generally, the two will be fairly highly correlated because degree centrality is the most strongly weighted input into Bonacich centrality.

in turn, its ability to convince limited partners to commit resources to it. The omission of performance controls would lead to an overestimation of the absolute sizes of status and confoundedness effects if high performing venture capital firms also obtained higher-status and more highly confounded network positions. I follow extant literature in implementing performance controls. The control variables include virtually all components of Lee et al.'s venture capital firm reputation index. There is no particular reason to aggregate these controls into a single measure for reputation in this case. As aggregation incurs an inevitable loss of detail, detail that might correlate with a firm's status or confoundedness, the estimated parameters of interest for the status and confoundedness effects stand on firmer ground when the controls are left disaggregated.

I control for successful *exits* and *failures*. Exits are measured by the combined number of IPOs, mergers, acquisitions, and LBOs of a venture capital firm's portfolio companies over the years $t - 1$ to $t - 3$. Analogously, failures are measured by the number of a venture capital firm's portfolio companies that were reported as defunct, Chapter 11, or Chapter 7 over the years $t - 1$ to $t - 3$. The empirical analysis indicated that collapsing exits and failures in this way had greater explanatory power than separating them out. The effects of interest are robust to a specification that separates exits and failures into their components. I implement additional controls that account for firm size and investment behavior. I control for the number of *rounds* in which the focal firm invested in the years $t - 1$ to $t - 3$. I also control for a firm's *sum of investments* in the years $t - 1$ to $t - 3$.⁸ I also control for the number of distinct *funds* from which a venture capital firm has made investments over the past three years. Except for the fund variable, which typically takes a small integer value, I took the logarithm of all variables after adding 1.

In addition, I compute a measure of *diversification* as the Herfindahl index based on the pro-

⁸If undisclosed, the data provider estimates the size of an investment round. To estimate the sum of a venture capital firm's investments, I assumed that all investing venture capital firms contributed equally to an investment round. Estimated round amounts are missing for some rounds. For these rounds I assumed that the information is missing at random and imputed the mean of the available round amounts over that period.

portions of rounds invested across two-digit industry codes. This might be an important control here as it might speak to both whether a firm is a generalist or a specialist and whether a firm is categorically more or less pure. I code the variable *diversification missing* 1 if the diversification variable cannot be calculated because of missing data, and 0 otherwise. Finally, I include year fixed effects to account for the time-varying munificence in the financing available to venture capital firms.

A control for firm age was estimable in linear fixed effect models, but not in the fixed effect logistic regressions, in which it led to a non-concave maximum likelihood function. Logistic fixed-effect models including the logarithm of age were estimable, had a negative but insignificant coefficient for age, and neither qualitatively nor quantitatively affected the economic or statistical significance of the status and confoundedness effects. Fixed effects for the industry (as measured by the two-digit SIC code in which the firm made the greatest number of its investments) and investment stage on which a venture capital firm focused did not change the economic or statistical significance of the coefficients of interest in meaningful ways. They are omitted from the presented analyses for efficiency.

Methods

The empirical analysis has five parts. The first part establishes the main effects and shows the robustness to alternative operationalizations of the independent variables. The second part shows the robustness to using alternative dependent variables for the future investment activity of venture capital firms and that the effect is unlikely to be driven by limited partners' information or prescience about firm quality that manifests itself only in the future performance. The third part provides support for the causality claim of status and confoundedness effects using a dynamic panel data estimator. The fourth part provides evidence consistent with the mechanism claim that the estimated confoundedness effect is an identity effect. The fifth part investigates the discriminant validity of the confoundedness effect from other well-established effects of

structural network position.

All parts of the analysis use fixed-effects estimators. However, not all parts use the same type of estimator (non-linear, linear, dynamic linear) or condition the analysis on the same unit of observation (within firm over time versus pairs of matched firms in fixed time). The within-firm estimators drop all firms that did not raise a new fund during the period of study. This eliminates the concern that time-constant heterogeneity in venture capital firm quality, which might correlate with the network position a firm obtains, affects the results. Conditioning the analysis on firms that were able to raise a new venture capital fund at least once over the period of study also reduces the dataset to an inherently more comparable set of organizations. Whenever possible, I prefer the within firm estimator. However, some tests for the mechanism claim are not plausible in a within-firm framework. For these analyses I employ conditional logistic regression on pairs of firms matched on structural equivalence. The approach will be detailed in the appropriate section below. Whenever there is a change in the estimator (from nonlinear to linear or from within firms to between firms), I first establish that the main effects are qualitatively robust and quantitatively similar across estimators.

Some venture capital firms, typically firms that make very few investments, invest exclusively by themselves or co-syndicate as mutually exclusive partners. In the network, such firms stand as isolates or small isolated clusters detached from the main cluster of the co-investment network. To make sure that the estimated effects are truly network effects rather than effects of being or not being connected to the main cluster, I conditioned the analysis on firms that are part of the main network cluster. In any given year, the largest cluster contains typically more than 90 percent of the firms that have ever raised a new fund during the period of study (low 88.7 % for 1997). The analysis is robust to including the 1776 firm-year observations on firms that are not part of the main cluster.⁹ The main results are further robust to excluding firms with

⁹The status scores are much higher among firms that have ever raised a new fund than among firms that have not, whereas the confoundedness scores among firms that have ever raised a new fund are indistinguishable from the confoundedness scores of the firms that have not. The estimated status and confoundedness effects are qualitatively identical and quantitatively highly similar in economic and statistical significance in both samples.

fewer than five or, alternatively, fewer than ten investments over the trailing three-year period to ascertain that the results are not driven by firms with casual activity in the venture capital space.

Results

Main effect & independent variables

The results of the main analysis are presented in table 1. I show odds ratios (exponentiated coefficients), which are the only statistic from fixed effect logit estimates that is both intuitive and directly informative about a firm's odds to raise a new fund (Karaca-Mandic et al., 2012; Ai and Norton, 2003). Of the other two statistics, the coefficients disallow an intuitive grasp on the effect size, and the marginal effects make a generally unwarranted assumption in the fixed-effects framework. Specifically, the coefficients themselves are the linear effects on the *logged* odds, which are not an intuitive unit, and computing the marginal effects relies on the assumption that all fixed effects are zero, which is logically inconsistent with using fixed effects in the first place.¹⁰

Insert table 1

Model 1 shows the main effects of status and confoundedness. I find a significant positive effect of status and a significant negative effect of confoundedness, in support of hypotheses 1 and 2. Model 2 shows the interaction effect between status and confoundedness and provides evidence that the negative effect of having a confounded identity is stronger for firms that occupy a

¹⁰The latter problem arises because the marginal effects of the logit depend on all covariates, but in the fixed-effects logit the fixed effects, which are a covariate, are not estimated but integrated out of the likelihood (Chamberlain, 1980). If they were estimated in the framework of a regular logit, the estimation would incur the incidental parameters problem, which generally results in biased estimates (Neyman and Scott, 1948).

high-status position, in support of hypothesis 3a. Model 3 operationalizes status based on the proportional relational matrix, where the r_{ij} are the proportions of deals in which j co-invests with i . It shows the effects of status and confoundedness robust to this change in measuring status. Models 4 and 5 show the robustness to using the average maximum co-investor status (*AMCIS*) as the independent variable, which correlates 0.975 with the confoundedness score. The average maximum co-investor status was measured as the average of the within-investment-round maximum of the status scores of firm i 's co-investors, averaged over all investment rounds in which i participated over the trailing three years. Finally, model 6 uses a spline specification for the effect of the average maximum co-investor status. This analysis adds some nuance to the interaction effect estimated in model 5. It finds that a venture capital firm suffers significantly from the status of its co-syndicators only when the status of the focal firm is, on average, eclipsed by the status of its highest-status co-syndicator, but not when its status eclipses theirs on average. Replicating results across related independent variables, this set of analyses has shown a positive effect of a firm's status, but a negative effect of the status of a firm's co-investors on its odds to raise a new fund. I continue the analyses with the confoundedness score based on the adjacency matrix as the independent variable (models 1 and 2), which explained slightly more variation in the data than the alternatives, as indicated by the slightly greater log-likelihood.

To assess the effect sizes of model 2, I split status and confoundedness into their deciles and computed the predicted odds ratio for each observation based on the direct effects and the interaction effect of its status and confoundedness scores. I then averaged the predictions for the observations within each decile combination of status and confoundedness. The advantage of this approach over a more traditional interaction plot is that it does not have to assume or inspect that all combinations of the chosen quantiles exist and can be validly compared. Instead, it allows a comparison only among those quantile combinations of status and confoundedness that are represented in the data. The arbitrarily chosen baseline is observations with status and

confoundedness scores in the first decile of their respective distributions.

Insert table 2 here

The odds ratios increase in status (along the y-axis or row dimension of the table) and decrease in confoundedness (along the x-axis or column dimension of the table), illustrating the main effects. The decrease in the odds ratio associated with a higher decile in confoundedness is larger for firms in higher deciles of status, illustrating the interaction effect. To draw a valid comparison, we can, for example, compare combinations of the third and eighth deciles of both variables (roughly equivalent to comparing the first and third quartile). Increasing confoundedness from the third to the eighth decile is associated with an odds ratio of $0.660/0.956=0.690$ for firms at the third decile of status, but $1.239/2.934=0.422$ for firms at the eighth decile of status. The ratio of these odds ratios is $0.422/0.690=0.612$ and is itself an odds ratio, indicating that higher-status firms are penalized more heavily for a confounded identity.

Causality

Even though the network position a firm obtains is endogenous to it launching a new fund, it is unlikely that the results presented here are entirely driven by reverse causality. I estimate the effect of a firm's network position in the past on its likelihood to raise a new fund. Temporally, the launch of a new fund in time t cannot cause the firm's network position in the past. However, endogeneity between two concurrent variables would carry over to endogeneity between a concurrent and a lagged variable if there were autocorrelation (e.g., Fair, 1970).

Dynamic panel data estimators can address these shortcomings in linear fixed effects-models in which the outcome might depend on its past realizations, regressors are not strictly exogenous,

and autocorrelation or heteroskedasticity among the errors within but not across units of observations (Roodman, 2009; Greene, 2012). I note that if there were autocorrelation, we might expect it to be negative in the short run, because a firm that just raised a new fund might be less likely to raise another fund immediately thereafter. That should lead us to under- rather than overestimate the effect sizes in the plain fixed-effects estimator, if anything. However, the linear dynamic panel data estimator employed in this analysis makes no *a priori* assumption about the temporal structure of autocorrelation. Linear panel data estimators recognize that past (or future) instantiations of suspected endogenous variables are potentially exogenous instruments. Due to the large number of lags that are available as potential instruments, it is often possible to find a set of lagged variables that allows over-identifying the model and deploying a set of statistical tests for the validity of the assumptions to the estimator, specifically for the strength and exogeneity of the instruments and residual autocorrelation.

I estimate an Arellano-Bond, linear probability model in which I allow the launch of a new fund to depend on the launch of a new fund in the periods $t - 1$, $t - 2$, and $t - 3$. I treat the launch of a fund in $t - 1$ as potentially endogenous and the launch of a fund in the periods $t - 2$ or $t - 3$ and all other independent variables as exogenous. I requested lags 4 and 5 of the *new fund launched* variable in addition to the variables considered exogenous. The results are presented in table 4, and the results of the diagnostic tests in the table notes.

Insert table 4 here

In preparation of the Arellano-Bond estimator, I discuss two fixed-effects and a pooled linear probability model first. The first column shows results from the fixed-effects logit on the reduced sample that includes the lagged dependent variables, but is estimated in full analogy to model 2 in 1 otherwise. The results are robust. The second column shows the results of a fixed-effects linear probability model that is otherwise specified identically and run on the

same data as the previous model. The insights remain robust. The coefficient estimates of the lagged dependent variables in models 1 and 2 confirm the suspicion of negative autocorrelation. The third column shows a pooled OLS model, that is, a model without firm fixed effects. Here too, the insights remain robust. However, the point of this model, in conjunction with model 2, is to provide boundaries for the plausible coefficient estimates of the endogenous variable in the Arellano-Bond estimator (Roodman, 2009). Finally, model 4 shows the results of the Arellano-Bond estimator, instrumenting for the possibly endogenous lagged dependent variable. The Arellano-Bond estimator, too, confirms the original insights. The coefficient estimates are greater than in the fixed-effects linear probability estimator for the same subset of the sample. The coefficient estimate of the lagged new fund launched variable in model 4 falls within the range of the estimates in models 2 and 3, as should be the case, and it is not significant. Finally, the diagnostic tests for the Arellano-Bond estimator, provided in the table notes, are not significant, indicating that the assumptions to the estimator are met. This series of tests strengthens the claim that the estimated effects are causal.

Alternative dependent variables

A venture capital firm's ability to launch a new fund is reflective of limited partners' willingness to commit resources to it. Thus it captures an important external audience's approval of the firm's identity. However, a firm might not just be able to attract resource commitments to new, but also to existing funds. I therefore create variables for the future number of funding rounds in which a firm participates and the sum of its future investments. The number of rounds in which a firm participates can be extracted directly from the data. Computing the sum of a firm's investments is less straightforward because the data contain missing values. Assuming that this information is missing at random and that all investors in a round contributed equally, I impute the average size of a firm's investments for the rounds for which this information is missing. This is not possible for a small set of firms for which there is no information about the size of

any of their investments. As an extension of these analyses, I also estimate models for future exits and failures. For each of these variables, I take the forward-looking sum over the years $t + 1$ to $t + 3$ and take the logarithm of these sums after adding 1. I use linear fixed-effects models for the estimation. The results are presented in table 3.

Insert table 3 here

Model 1 shows the linear probability model for the entire sample in analogy to model 2 in table 1, with consistent results. Mirroring the analysis of the likelihood to launch a new fund, the columns for the future number of rounds and sum of future investments (models 2 and 3) show that status is associated with greater and confoundedness with less investment activity in the future, in particular when a high-status firm has a highly confounded identity. In unreported analyses I find the status and confoundedness effects on the future number of rounds and the sum of future investments robust to including the launch of a new fund in time t as an independent variable. This indicates that status and confoundedness do not only affect the firm's ability to solicit resource commitments to new, but also to already existing funds. In the models that estimate the future number of exits and failures (models 4 and 5), the results are similar. In unreported models I standardized the variables for future exits and failures to put them on the same scale. The estimates for the status and confoundedness effects are qualitatively the same as in the reported model and between the two unreported models they are economically and statistically indistinguishable. In a fixed-effects Poisson model for the untransformed number of future exits and failures, I find the relative size of the coefficients virtually identical to the estimates of the reported model.

The question arising from the models for future exits and failures is whether the confoundedness effect might capture differences in the underlying quality of the firm instead of differences in how limited partners *perceive* the identity of the firm. This might be the case if the effect of

exits and failures as measured here were disproportional beyond the ratio of the coefficients. The estimates suggest that the positive effect of one exit on firm performance would have to be roughly equal to the negative effect of two failures, on average, for confoundedness to be uninformative about firm quality as measured by performance. However, the valuation of exits and failures is not in the data, which would force us to base the inference on this very tenuous assumption.

However, there is a workaround for this problem. Firm quality should affect future performance, and we should be able to absorb information about firm quality that (potential) limited partners might have had in t with controls for the number of future exits and failures of a venture capital firm's portfolio firms. I re-estimated models 1-3 accordingly with controls for future exits and failures. In the model for the likelihood to raise a new fund (model 6), the coefficient estimates stay virtually unaffected. In the models for the future number of investment rounds and the sum of future investments (models 7 and 8) the coefficients for the status and confoundedness effects are roughly 50 percent smaller than before, but remain strongly statistically significant. In contrast to model 6, we should expect some reverse causality between the dependent and the independent variables in models 7 and 8 because of their contemporaneity, which might explain the attenuation of the coefficients. Because the future cannot temporally cause the past, model 6 most strongly suggests that there is little information about quality in the status and confoundedness variables that is not already absorbed by the controls or fixed effects in model 1. Notwithstanding the limitations of the data, the fixed effects within estimators, the limited variation of the moving average type variables, and the robustness to controls for future performance suggest that the estimated effects of status and confoundedness are driven by differences in *perceptions* of firm identity rather than true differences in underlying quality.

Mechanism

The theoretical claim of this paper is that the effects of status and confoundedness are identity effects. Two pieces of empirical evidence thus far support this mechanism claim. First, the previous analyses indicated that, conditional on the controls, limited if any information about the underlying quality of the firm can be gleaned from them. Second, the negative effect of confoundedness was stronger among firms that laid claim to a high-status identity, for whom distinctiveness should be a stronger imperative. But if the effects of status and confoundedness were indeed identity effects, then they should become stronger along other margins that might amplify the importance of identity. If status and confoundedness are both identity effects, then moderators that amplify (mitigate) the positive effect of status should also amplify (mitigate) the negative effect of confoundedness. In addition to the interaction inspected earlier between status and confoundedness itself, I inspect six additional moderators here: firm size and age, industry and geographic focus, investment stage focus and average syndicate size.

Whenever plausible I use the more stringent within firm estimator. For the tests for which within firm estimation is not plausible, I turn to an estimation strategy between firms. Specifically, a within firm estimator is not very plausible for firm location and industry focus, because firms rarely change location or focus. For the matching estimator, I match firms on their structural equivalence in the network.¹¹ Two perfectly structurally equivalent actors in a network are structurally identical if they reach the same peers through the same shortest path lengths. If there is more than one structurally most similar peer, I draw one of the matches at random. If two firms are the best match for each other, which is the case for most matched pairs, I remove the duplicate matched pair. If firm *a*'s best match is firm *b*, but firm *b*'s best match is another firm, say *c*, I retain both matched pairs for analysis.

Two structurally identical firms will be status-identical if status is measured using Bonacich

¹¹I thank an anonymous reviewer for this suggestion.

centrality based on the adjacency matrix. As it is unlikely that there is a structurally identical peer for every actor in the network, status differences persist for many pairs of matched actors. Empirically, pairs of structurally most similar firms tend to be status-similar and proximately located in this network. Therefore, if one of the matched firm appears on a limited partner's radar screen for resource commitments, both should, not only because of the similarity in their network position, but also because that similarity implies a similarity in their investment profiles. Two matched firms should thus be competitors for resource commitments from overlapping resource pools. This makes it suitable to analyze the matched data with conditional logit models for the likelihood to raise a new fund, conditioning on (that is, introducing a fixed effect for) each pair of two matched firms. This analysis identifies the coefficients on the switchers, that is, on maximally 5342 of 26411 matched pairs in which one firm raised a new fund in year t whereas the other firm did not.

The conditional logit analysis of the matched sample has a shortcoming compared to the within analysis. In the within estimator the counterfactual is the same firm in a different time period. The same firm in a different time period might be a better counterfactual than a matched firm in the same period, because unobservable time-constant characteristics of organizational structure, culture, communication, and aspects imprinted on the firm are absorbed by a firm fixed effect but not in a matching estimator. It might thus be a stronger assumption that two firms in structurally similar positions in the network arrived in positions of different status and confoundedness exogenously than it is to assume that a firm's variation in status and confoundedness from one year to another is random. Empirically, this might be particularly true here, as all independent variables were modeled as moving averages over the years $t - 1$ to $t - 3$, which only receive an annual update. I therefore estimate the matched sample estimator with more stringent controls by including not only the interactions of interest, but also the interactions of the moderating variable with all independent variables.¹² The abridged results are shown in table 5.

¹²I excluded the *diversification missing* variable, which was not identifiable because the information was available for all observations in the subsamples.

Insert table 5 here

Models 1 and 2 show that the status and confoundedness effects are stronger among older and larger firms, respectively. Model 3 does not find an effect that the status and confoundedness effects are moderate by the average size of the syndicate with which a firm co-invests. This might not be too surprising because such an interaction would suggest that limited partners actively try to triangulate an individual firms's merit out of its co-investment relationships. Model 4 shows that the status and confoundedness effects are roughly squared when two matched firms are headquartered in the same state than when they are headquartered in different states. Although these effects are too noisy to be significant at conventional levels with the weaker matching estimator, the effects are sizable. Similarly, model 5 shows that the status and confoundedness effects are estimated larger when two matched firms focus on the same industry. These estimates are even noisier than the estimates from model 4. This is perhaps not surprising. While focusing on the same industry should increase the competition between the two firms, it might also signal a categorically less pure identity if the two structurally most similar firms in the network focus on different industries. Finally, model 6 shows significant status effects when the matched firms focus on early-stage investments. The status and confoundedness effects are indistinguishable from zero when the two matched firms focus on later stage investments although these effects, too, are too noisy to render the difference statistically significant. However, these quite sizable estimates are consistent with prior literature. Early-stage investments are much riskier and inherently more likely to fail than later-stage investments. Therefore, the signals observed from an early-stage venture capital firm embodies a much greater element of chance or luck, rendering these signal less reliable and increasing the relevance of identity cues such as status and confoundedness.

Overall, these analysis provide additional evidence for the mechanism claim that the effects of

status and confoundedness are identity effects. Where the positive effect of status is stronger, the negative effect of confoundedness is too. Even though the estimates from the weaker matching estimator are too noisy to be statistically significant at conventional levels, they are economically sizable.

Discriminant validity

The development of the social network literature over the last forty five years has brought with it a natural proliferation of measures of an actor's structural position in a network. Any network measure is thus challenged to prove its discriminant validity vis-a-vis potentially related measures. In the part that follows, I attempt to establish the discriminant validity of the confoundedness of an identity from well-established measures of structural position that might be closely related .

Uzzi's (1996) measure of first-order network coupling captures the asymmetry in the strengths of a focal actor's exchange relations. In essence, it is a Herfindahl-index over an actor's tie strengths. In the venture capital context, a low score on this measure indicates a venture capital firm's low propensity to co-syndicate financing rounds with a closed set of other venture capital firms. Bothner et al.'s (2010) measure of fragility extends Uzzi's measure by basing a social actor's fragility on the entire network structure. A focal actor is more fragilely positioned in social structure to the extent that his or her tie strengths are very asymmetric, and he or she is connected to actors whose tie strengths are very asymmetric, and so forth.¹³ As the proposed measure for confoundedness does not only increase in the status of a firm's exchange partners but also in its tendency to jointly engage in exchange with a closed set of exchange partners, confoundedness might be positively correlated with first-order network coupling and fragility. I computed the first-order network coupling and fragility of venture capital firm i in year t based

¹³Fragility is to first-order network coupling like Bonacich centrality is to degree centrality. Just as degree centrality is the most highly weighted input into Bonacich centrality, first-order network coupling is the most highly weighted input into fragility.

on the co-investment matrix for the years $t - 1$ to $t - 3$. Models 1 and 2 in table 6 show the effects of status and confoundedness robust to and nearly unaffected by the inclusion of the embeddedness and fragility variables, which are estimated to have negative effects.

Insert table 6 here

Confoundedness might also correlate with Burt's (1992) measure of network constraint, which captures the degree to which a firm's connections are concentrated in a closed group of interconnected others. High network constraint implies high redundancy in a network and a limited ability to act as a bridge or conduit of resources, information, or opportunity between disconnected others. Model 3 in table 6 shows that higher network constraint is associated with lower odds to launch a new fund, as one might expect, but that it does not affect the statistical or economic strength of the status and confoundedness effects.

Following Podolny's (1993) seminal treatment of organizational status in tombstone advertising in the securities market, Bonacich (1987) centrality has become the standard measure of status in the network literature. In the application of Bonacich centrality as a measure of status, a social actor is high status if he or she is well connected to well connected others. However, both from a perspective of network evolution and the definition of Bonacich centrality, status and power are positively correlated (Malter and Piskorski, 2014; Homans, 1974). In the application of Bonacich centrality as a measure of power, an actor is high power if he or she is well connected to poorly connected others, who are connected to well connected others, and so forth. This is operationalized by parametrizing the computation of Bonacich centrality with a negative instead of a positive value of β . For the same absolute value of β the additive terms for odd removes in Bonacich centrality are the same for the computation of status and power, resulting in a correlation between the two. Status and confoundedness might thus absorb power's effect on

the firm's ability to launch a new fund. Model 4 in table 6 shows that the effects of status and confoundedness are robust to the inclusion of power as a control variable and that the effect of status is estimated substantially larger once power is controlled.

Similarly, Mizruchi et al. (1986) pointed out that a focal actor's Bonacich centrality is additive in reflected and derived centrality. Derived centrality is that part of a focal actor's centrality that he or she truly derives from his or her connections, his or her connections' connections, and so forth. Reflected centrality is that part of a social actor's centrality that the focal actor projects onto his or her connections by virtue of his or her own centrality and that these connections merely reflect back onto the focal actor. The concern is that while status measured as Bonacich centrality might correlate positively with reflected centrality, confoundedness might correlate positively with derived centrality. To assess whether confoundedness adds value above and beyond the distinction between reflected and derived centrality, I substituted a firm's status as measured by its Bonacich centrality by splitting it into its components for reflected and derived centrality (Mizruchi et al., 1986). Model 5 in table 6 shows that the size and statistical strength of the results for confoundedness and its interaction with status are very similar to the original analysis when status is parsed into its reflected and derived components.

Malter (2014) reported that quality, reputation, and status were highly correlated among elite wines from Bordeaux and that their effects all increased at the margin, resulting in an overestimation of status effects if increasing marginal returns to quality and reputation were disregarded. To ascertain that the estimates for status and confoundedness are not nonlinear effects of other venture capital firm qualities or their structural position, I re-estimated model 2 in table 1 including the squared term of every cardinal control variable in separate regressions (not reported).¹⁴ The estimates of the status and confoundedness effects and their interaction was statistically and economically robust in these models.

¹⁴Available from the author.

Discussion

I theorized that high-status firms impose identity costs on their affiliates with whom they collaborate in projects but compete for resources. By separating the status of the focal firm from the status of its affiliates, I showed empirically that, net of the positive effect of its own status, a venture capital firm was less likely to launch a new fund the more it integrated its identity with high-status co-syndicators. This effect was robust across a number of approaches to measuring the status of a venture capital firm's co-syndicating partners, alternative dependent variables for the future investment activity of the firm, and estimators (including a linear panel data estimator with a strong claim to causality). Analyses of moderators provided evidence consistent with the mechanism claim that this effect is related to firm identity. The negative effect of partner status was stronger on high-status firms, for whom distinguish themselves from the established elite should be more imperative. In addition, the negative effect of partner status was generally greater in subsamples in which the positive effect of status was also greater.

The results of this study suggest that a firm can optimize the distinctiveness of its identity by preventing its excessive integration with the identities of high-status exchange partners with whom they collaborate in projects but compete for resources. Identities are intertwined. Being connected through networks implies an influence of our affiliates' identities on our own (e.g., Bothner et al., 2010; Goffman, 1986; White, 1992). Earlier microsociological literature suggested that audiences perceive sameness between an actor and the affiliates with whom the actor is seen in public (Goffman, 1986: 47). The presented evidence shows that audiences critically differentiate within this sameness. Echoing theoretical arguments that social actors compete for resources based on status and the distinctiveness of their identities (Jensen et al., 2011, Gioia et al., 2010, Navis and Glynn, 2010, Podolny, 2005: 25, White, 1992: 5–6), this study showed not only that a firm must distinguish itself, but also from whom. In status-based market competition firms benefit from distinguishing themselves from the established elite.

Any study proposing that a certain firm behavior improves a firm outcome rests on an assertion of disequilibrium behavior, that is, that firms could have done better had they behaved differently. This raises two related questions that have to be answered anew in any empirical study. First, how plausible is it to believe that disequilibrium behavior would exist and persist in the first place? Second, how certain can we be that the observed pattern in the data is disequilibrium behavior and thus that the effects of the independent variables are causal and not merely correlational?

As for the first question, firms overcommitting to high-status co-syndicators is possible in the venture capital context for at least three reasons. First, venture capital firms might simply be uncertain about which relational configuration limited partners regard as best, as the preferences for that might not be explicit and not be well understood even by limited partners themselves. Second, high-status affiliations might become more detrimental over “time”, because high-status partners are less detrimental for low-status firms, low-status firms that seeks to acquire status through affiliations with high-status peers might miss the point in their development at which it would be better to deemphasize these affiliations in their activities or in communications with limited partners. For example, when asked informally whether he mentioned the firm’s high-status co-syndicators in presentations to prospective limited partners, a managing director of an increasingly well-regarded venture capital firm in the Bay area answered “every single time.” Finally, venture capital firms might fail to learn the relational configuration limited partners regard as best even in the long run, not only because it might change over the course of their development, but also because they would have to draw an inference about a counter-intuitive hypothesis from a small sample of noisy data compared to the large sample over an extended period of time studied here.

As for the second question, the concern is that a firm selects into its network position such that the position it obtains is simply optimal at any given point in time given its constraints. Therefore, any departure from the network position would result in a decline in the firm outcome in

question. The empirical approaches taken in this study make it unlikely that selection effects could explain the results. First, the matched sample estimator matched the two most structurally similar peers, which limits the degree to which selection into the network structure at large can explain the result. Second, the estimators with firm fixed effects identified status and confoundedness effects on the narrow margin of variation in variables within firm over time. As these variables were defined as moving averages over the past three years, they only receive an annual update. Even though we might believe that a venture capital firm's initial investments into its portfolio firms is fully endogenous, it would require fairly strong assumptions about the control they exercise over the occurrence and timing of follow-on investment rounds to believe that there is little exogenous variation in a firm's network position from year to year. Third, the linear dynamic panel data estimator allowed testing whether the instruments, which included the status and confoundedness variables, were endogenous. That test maintained the null that the instruments were exogenous. Reassuringly, the coefficient estimates for the independent variables were economically and statistically robust across all estimators, converging on the same insight.

It also seems unlikely that firms overcommit to collaborating with high-status co-syndicators based on the evidence in the extant literature, which has found that both the constraint on and the costs of accessing exchange partners are the highest when these exchange partners occupy high-status positions (Hsu, 2004; Rider, 2009; Castellucci and Ertug, 2010). In light of this evidence it appears implausible that the venture capital firms studied here overcommit to high-status co-syndicators due to a lack or the higher costs of accessing lower-status partners. To the contrary, it should be easier or cheaper to access lower-status partners. This makes it unlikely that identities that are too strongly associated with high-status partners are consistent with an equilibrium approach to relationship formation. Instead, it suggests disequilibrium behavior on part of the firms. The present study shares this notion of biased partner choices with Sorenson and Waguespack (2006) who observed that film producers overcommit resources to exchange

partners with whom they have successfully transacted in the past.

It is a *prima facie* plausible assumption that audiences attempt to identify the independent merit of the social actors to whom they consider allocating resources and/or rewards. If we accepted this to be true, then we should believe that audiences place value on being able to discern whether and to which extent a social actor has reached his or her accomplishments on his or her own or was able to do so only on the coattails of his or her affiliates. This task becomes more difficult when social actors confound their identities with high-status collaborators, who themselves produce high quality or are perceived to do so. In addition, it is harder to stand out as elite in an elite crowd. If the likelihood and/or extent to which audiences allocate resources and/or rewards is proportional to the preference or bias they hold for a focal actor's social status, then it generally ought to be inversely proportional to the preference or bias they hold for the social status of that social actor's affiliates. The present study has given a large-sample existence proof of this principle, which I believe to be a fundamental input into the primitive by which audiences assess the merit of identities that collaborate and compete.

Competition based on and for a distinctive identity might unfold similarly in other social realms. The approach taken in this study lends itself to testing this conjecture and its boundary conditions in other contexts and networks. The approach is not specific to the context studied here and should be adaptable to a wide variety of contexts, for example, continuous flows of deference as embodied in time or resource flows rather than discrete interactions.

Spillovers from one identity to another are not new to the identity or status literature. This paper complements this research stream by highlighting the competitive tensions between or among affiliated identities. The analyses offered compelling evidence that in status-based market competition, high-status affiliates impose identity costs on those with whom they collaborate in projects but compete for resources. Highly regarded identities that absorb limelight and resources do so in part at the expense of their affiliates. When a firm considers engaging in an exchange relationship, it should thus look beyond the immediate benefits and costs of that rela-

tionship and take into account how that relationship might affect outside audiences' perceptions of its identity, merit, or distinctiveness.

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Table 1
ODDS RATIOS FOR LAUNCHING A NEW FUND FROM FIXED-EFFECTS LOGISTIC
REGRESSIONS

	(1)	(2)	(3)	(4)	(5)	(6)
	Adj.	Adj.	Prop.	Adj.	Adj.	Adj.
Status	1.39*** (5.28)	2.52*** (8.03)	1.77*** (6.08)	1.36*** (4.97)	2.05*** (6.32)	1.27*** (3.70)
Confoundedness	0.71*** (-4.95)	0.61*** (-6.70)	0.75*** (-3.76)			
Status × Confoundedness		0.68*** (-6.25)	0.74*** (-5.38)			
AMCIS				0.80*** (-3.80)	0.72*** (-5.02)	
Status × AMCIS					0.75*** (-4.24)	
AMCIS if AMCIS < Status						1.10 (0.62)
AMCIS if AMCIS > Status						0.75*** (-3.58)
Degree	1.01 (0.05)	0.84 (-1.44)	0.70** (-2.39)	0.90 (-1.01)	0.78** (-2.17)	0.93 (-0.72)
Syndicate size	1.03 (1.32)	1.05** (2.40)	1.04** (2.05)	1.01 (0.60)	1.02 (1.18)	1.01 (0.66)
Exits	1.09** (2.03)	1.10** (2.18)	1.13*** (2.89)	1.09** (2.03)	1.09** (2.09)	1.09** (2.05)
Failures	0.88** (-1.97)	0.88** (-2.01)	0.93 (-1.22)	0.89* (-1.87)	0.88* (-1.93)	0.88** (-1.98)
Rounds	0.96 (-0.36)	0.85 (-1.60)	1.17 (1.23)	1.02 (0.22)	0.95 (-0.56)	1.04 (0.41)
Sum invested	1.06 (1.54)	1.09** (2.13)	1.06 (1.49)	1.07 (1.63)	1.08** (2.05)	1.07 (1.61)
Funds	0.69*** (-7.73)	0.67*** (-8.21)	0.68*** (-8.01)	0.70*** (-7.67)	0.69*** (-8.05)	0.69*** (-7.74)
Diversification	0.91 (-0.63)	0.75* (-1.81)	1.00 (-0.02)	0.93 (-0.48)	0.83 (-1.20)	0.93 (-0.44)
Diversification missing	0.69 (-1.23)	0.53** (-2.09)	0.78 (-0.83)	0.73 (-1.07)	0.61 (-1.62)	0.74 (-0.98)
Observations	14713	14713	14713	14713	14713	14713
Firms	1513	1513	1513	1513	1513	1513
log likelihood	-4923	-4888	-4913	-4928	-4915	-4923

NOTE.—*** p<0.01, ** p<0.05, * p<0.1; z-statistics in parentheses. All analyses included year fixed effects.

Table 2
ODDS RATIOS FOR THE JOINT EFFECT OF STATUS AND CONFOUNDEDNESS

Status (decile)	Confoundedness (decile)									
	1st	2nd	3rd	4th	5th	6th	7th	8th	9th	10th
1st	1.000	0.946	0.867	0.802						
2nd	1.039	0.986	0.897	0.817	0.761	0.717	0.680			
3rd	1.124	1.054	0.956	0.871	0.802	0.745	0.703	0.660	0.621	0.553
4th	1.250	1.169	1.050	0.944	0.858	0.798	0.742	0.698	0.643	0.551
5th		1.357	1.211	1.073	0.962	0.881	0.812	0.748	0.687	0.562
6th		1.699	1.484	1.285	1.130	1.014	0.917	0.834	0.753	0.600
7th			1.961	1.667	1.422	1.246	1.100	0.968	0.844	0.658
8th			2.934	2.446	2.005	1.664	1.437	1.239	1.018	0.759
9th				4.451	3.431	2.667	2.197	1.796	1.423	0.952
10th					9.328	8.020	6.235	4.791	3.341	1.590

NOTE.—Average odds ratios of effects predicted into the sample based on model 2 in table 1. Percentile combinations not represented in the data or representing fewer than 30 observations are shown as empty cells.

Table 3
COEFFICIENTS FOR ALTERNATIVE DEPENDENT VARIABLES FROM FIXED-EFFECTS LINEAR
REGRESSIONS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Launch	Rounds	Firm sum	F. Exits	F. Failures	Launch	Rounds	Firm sum
Status	0.15*** (7.62)	0.29*** (6.00)	0.40*** (5.18)	0.22*** (4.33)	0.16*** (4.28)	0.16*** (6.26)	0.16*** (4.50)	0.24*** (3.68)
Confoundedness	-0.07*** (-6.50)	-0.15*** (-4.46)	-0.21*** (-3.83)	-0.13*** (-4.19)	-0.05*** (-3.20)	-0.07*** (-4.45)	-0.08*** (-3.00)	-0.12*** (-2.59)
Status × Confoundedness	-0.07*** (-6.10)	-0.14*** (-5.08)	-0.17*** (-4.18)	-0.13*** (-4.74)	-0.07*** (-3.71)	-0.06*** (-4.29)	-0.07*** (-3.34)	-0.07*** (-2.28)
Degree	-0.04* (-1.90)	-0.02 (-0.43)	-0.08 (-0.99)	0.02 (0.43)	-0.03 (-1.15)	-0.03 (-1.39)	-0.03 (-0.61)	-0.09 (-1.22)
Syndicate size	0.01*** (2.61)	-0.00 (-0.39)	-0.01 (-0.53)	-0.00 (-0.08)	0.00 (0.87)	0.01 (1.26)	-0.00 (-0.55)	-0.01 (-0.75)
Exits	0.01** (2.10)	-0.09*** (-5.41)	-0.11*** (-4.32)	-0.20*** (-12.82)	0.00 (0.01)	0.04*** (3.92)	-0.00 (-0.08)	0.00 (0.00)
Failures	-0.02** (-2.27)	-0.07*** (-3.15)	-0.06** (-1.98)	-0.08*** (-3.53)	-0.29*** (-18.23)	0.01 (0.46)	0.02 (1.19)	0.04 (1.51)
Rounds	-0.03* (-1.72)	-0.11*** (-2.75)	-0.08 (-1.08)	-0.00 (-0.05)	0.01 (0.61)	-0.06*** (-2.73)	-0.11*** (-3.38)	-0.08 (-1.15)
Sum invested	0.01 (1.54)	0.06*** (3.05)	-0.03 (-0.84)	0.04** (2.52)	-0.01 (-1.60)	-0.00 (-0.09)	0.04*** (2.75)	-0.05* (-1.75)
Funds	-0.06*** (-6.00)	0.03*** (3.07)	0.06*** (3.97)	0.02* (1.70)	-0.00 (-0.03)	-0.09*** (-9.53)	0.02*** (2.95)	0.05*** (3.72)
Diversification	-0.04* (-1.88)	-0.10* (-1.65)	-0.22** (-2.08)	-0.10* (-1.74)	-0.02 (-0.66)	-0.06* (-1.80)	-0.05 (-1.06)	-0.15 (-1.61)
Diversification missing	-0.10** (-2.14)	-0.04 (-0.30)	-0.09 (-0.38)	0.11 (0.99)	-0.05 (-0.93)	-0.13* (-1.95)	-0.08 (-0.71)	-0.10 (-0.46)
Future exits						0.06*** (7.88)	0.43*** (31.19)	0.58*** (23.68)
Future failures						0.07*** (5.40)	0.18*** (10.95)	0.19*** (7.08)
Constant	0.63*** (10.40)	3.04*** (16.88)	11.63*** (37.93)	1.00*** (6.02)	0.24*** (2.87)	0.71*** (8.45)	2.56*** (17.96)	10.99*** (41.69)
N	14713	10170	9930	10170	10170	10170	10170	10170
Firms	1513	1438	1429	1438	1438	1438	1438	1438
R-squared	0.090	0.255	0.144	0.145	0.270	0.111	0.452	0.275
log likelihood	-6627	-7179	-12430	-8264	-3714	-4748	-5611	-11602

NOTE.—*** p<0.01, ** p<0.05, * p<0.1; z-statistics in parentheses. All analyses included year fixed effects.

Table 4
LINEAR PANEL DATA ESTIMATOR

	(1) FE logit	(2) FE linear	(3) OLS	(4) LPDE
Status	2.18*** (4.05)	0.12*** (3.65)	0.10*** (6.01)	0.19*** (6.50)
Confoundedness	0.63*** (-3.28)	-0.06*** (-3.05)	-0.04*** (-2.85)	-0.07*** (-3.95)
Status × Confoundedness	0.68*** (-4.00)	-0.06*** (-3.64)	-0.04*** (-3.77)	-0.07*** (-4.81)
Degree	0.93 (-0.37)	-0.02 (-0.66)	-0.04* (-1.90)	-0.06** (-2.35)
Syndicate size	1.03 (0.95)	0.01 (1.30)	0.00 (0.01)	0.01 (1.35)
Exits	1.08 (1.21)	0.01 (1.01)	0.01 (0.64)	0.03** (2.29)
Failures	0.75*** (-3.63)	-0.05*** (-3.84)	-0.05*** (-4.17)	-0.03** (-2.05)
Rounds	0.83 (-1.01)	-0.02 (-0.79)	-0.02 (-1.47)	-0.03 (-1.37)
Sum invested	1.20*** (2.67)	0.02** (2.45)	0.03*** (5.78)	0.03*** (4.21)
Funds	0.83*** (-3.57)	-0.03*** (-3.96)	0.00 (1.22)	-0.05** (-1.97)
Diversification	0.72 (-1.14)	-0.05 (-1.09)	-0.01 (-0.50)	-0.04 (-1.26)
Diversification missing	0.18** (-2.25)	-0.23*** (-2.60)	-0.08 (-1.24)	-0.18*** (-2.97)
Lag 1	0.29*** (-13.22)	-0.21*** (-14.64)	-0.09*** (-7.43)	-0.14 (-1.09)
Lag 2	0.39*** (-11.27)	-0.15*** (-11.95)	-0.04*** (-3.62)	-0.04 (-0.60)
Lag 3	0.60*** (-7.00)	-0.08*** (-6.87)	0.00 (0.42)	0.02 (0.70)
Constant		0.08 (0.71)	0.18*** (2.81)	-0.02 (-0.21)
Observations	7673	7673	7673	7673
Firms	924	924	924	924
(Pseudo) R^2	0.167	0.141	0.083	
Wald χ^2	533.31			775.67

NOTE.—*** p<0.01, ** p<0.05, * p<0.1; z-statistics in parentheses. All analyses included year fixed effects.

Table 5
FURTHER MECHANISM TESTS

	(1)	(2)	(3)	(4)	(5)	(6)
	Age>9	Size>12	Synd. size	Same state	Same ind.	Late stage
Status	1.09 (0.86)	1.00 (-0.02)	1.53*** (3.47)	1.68*** (3.91)	1.72** (2.77)	2.22*** (4.65)
Confoundedness	0.94 (-0.58)	0.87 (-1.44)	0.76* (-1.86)	0.67*** (-3.85)	0.65** (-2.75)	0.62*** (-3.59)
Moderator (column variable)	1.00 (0.03)	1.02 (0.37)	1.03 (0.97)	NA	NA	NA
Status x Moderator	1.28*** (3.12)	1.49*** (5.24)	0.98 (-1.54)	1.59 (1.64)	1.38 (1.21)	0.58 (-1.35)
Confoundedness x Moderator	0.73*** (-2.72)	0.64*** (-3.05)	1.00 (-0.18)	0.67 (-1.52)	0.81 (-0.85)	1.38 (0.88)
Observations	9784	9784	9784	8894	7462	5520
log likelihood	-3414***	-3404***	-3419***	-2757***	-2141***	-1731***

NOTE.—*** p<0.01, ** p<0.05, * p<0.1; z-statistics in parentheses. Within-firm analyses (models 1-3) included year fixed effects and all controls from previous analyses. Matching estimator analyses included all direct effects of the controls and their interactions with the moderator (column variable).

Table 6
ROBUSTNESS ANALYSES

	(1)	(2)	(3)	(4)	(5)
Status	2.81*** (8.43)	2.65*** (7.98)	2.77*** (8.25)	3.74*** (7.36)	
Confoundedness	0.59*** (-7.06)	0.60*** (-6.84)	0.58*** (-7.03)	0.52*** (-7.15)	0.63*** (-6.10)
Status x confoundedness	0.67*** (-6.33)	0.67*** (-6.31)	0.67*** (-6.29)	0.62*** (-6.75)	0.69*** (-5.96)
Embeddedness	0.81*** (-2.73)				
Fragility		0.48 (-1.24)			
Constraint			0.82** (-2.29)		
Power				0.67*** (-3.05)	
Reflected					1.15*** (3.57)
Derived					2.10*** (5.88)
Degree	0.63*** (-2.82)	0.75* (-1.87)	0.66** (-2.55)	1.01 (0.08)	0.90 (-0.86)
Syndicate size	1.05** (2.49)	1.05** (2.48)	1.05*** (2.59)	1.06*** (3.02)	1.04** (2.18)
Exits	1.11** (2.30)	1.10** (2.18)	1.11** (2.29)	1.10** (2.20)	1.11** (2.30)
Failures	0.89* (-1.88)	0.88** (-2.06)	0.89* (-1.89)	0.88* (-1.95)	0.87** (-2.10)
Rounds	0.86 (-1.44)	0.85 (-1.54)	0.85 (-1.59)	0.83* (-1.76)	0.88 (-1.21)
Sum invested	1.09** (2.14)	1.09** (2.15)	1.09** (2.09)	1.08** (1.96)	1.09** (2.14)
Funds	0.67*** (-8.25)	0.67*** (-8.23)	0.67*** (-8.24)	0.67*** (-8.26)	0.67*** (-8.74)
Diversification	0.79 (-1.51)	0.76* (-1.72)	0.79 (-1.50)	0.76* (-1.78)	0.79 (-1.53)
Diversification missing	0.58* (-1.77)	0.55* (-1.93)	0.58* (-1.76)	0.53** (-2.07)	0.57* (-1.82)
Observations	14713	14713	14713	14713	14713
Firms	1513	1513	1513	1513	1513
log likelihood	-4895	-4897	-4896	-4894	-4891

NOTE.—*** p<0.01, ** p<0.05, * p<0.1; z-statistics in parentheses. All analyses included year fixed effects.