Educational Networks, Mutual Fund Voting Patterns, and CEO Compensation

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Mutual funds whose managers are in the same educational network as the firm's CEO are more likely to vote against shareholder-initiated proposals to limit executive compensation than out-of-network funds are. This voting propensity is stronger when voting among the funds in a family is not unanimous. Furthermore, CEOs of firms who have relatively high levels of educationally connected mutual fund ownership have higher levels of compensation than their unconnected counterparts. This aspect of executive compensation is related to both the abnormal trading performance of the connected investors in the firm and the perceived quality of firm management by the connected investors. (*JEL* G30, G34)

1. Introduction

Educational connections between parties seem to matter for financial transactions. Cohen, Frazzini, and Malloy (2008) document that the trades made by mutual fund portfolio managers who invest in companies run by people with whom they have an overlap in educational background—that is, those in the same "social" network or, more precisely, educational network—outperform the other trades made by the same portfolio managers in firms with which they have no such connections. In this article, we examine whether educational networks appear to impact portfolio managers' voting on shareholder proposals related to executive compensation and whether top officers at firms that have stock ownership heavily held by connected, in-network mutual funds are compensated differently than their counterparts at less-connected firms.

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We start by examining mutual fund voting on shareholder-initiated executive compensation proposals. Using hand-collected data on the educational status of firm executives and mutual fund managers, we find that, on average, funds in the same educational network as the firm's executives are more likely to vote against shareholder-initiated proposals to limit executive compensation than out-of-network funds. The result is robust and continues to hold under different specifications: with mutual fund family fixed effects, firm fixed effects, or proposal fixed effects. The result also holds with firm-fund pair fixed effects, in which each combination of firm and fund gets a separate dummy variable. This specification allows us to avoid any fund- or firm-level effects by achieving identification through the forming and breaking of connections between a given firm and a given mutual fund because of changes in top-level personnel. The magnitude of the marginal effect on voting propensity implied by our tests ranges from 14% more likely to vote against a proposal (with proposal fixed effects) to 39% for our cleanest test, in which we control for a wide array of confounding factors by including family-proposal dyad fixed effects.

In many instances (in about 19% of the observations in our data), not all funds in a fund family vote the same way on a given compensation proposal. Our fund-level voting data allow us to examine cases where one fund votes differently than other funds in the same family. When there is such a within-family voting disparity, the effect of educational network on voting propensities becomes much stronger. In-network funds are 42% more likely to vote in favor of management. In contrast, when there is unanimity in voting, connected funds are 7% more likely to vote in favor of management.

If in-network ("connected") funds vote differently than out-of-network funds on executive compensation proposals, then perhaps CEO compensation at firms with higher levels of ownership by in-network mutual funds is different than at counterparts with lower levels of in-network ownership. We find that total CEO compensation at firms with higher levels of ownership by in-network mutual funds is significantly higher than at counterparts with lower levels of in-network ownership. Using Execucomp data from 1992–2006 and our hand-collected data on educational ties between firm executives and mutual fund managers, we show that for each percentage point of a firm's ownership that is connected through an educational network, total executive compensation is 2.5% higher, controlling for other determinants of compensation. When computed at the mean compensation in our sample, a one-standard-deviation increase in connected ownership correlates with an increase in total compensation of about $236,000. There are several possible and non–mutually exclusive explanations for this compensation premium in educationally connected firms.

First, these findings may reflect the amount or nature of information flow along the firm's educational network. Consider the Cohen et al. (2008) result that educational connections may generate information flow that enables portfolio managers to make better trading decisions. Information flow could occur if such networks lower the cost to networked investors of gathering information, such as if it is easier for networked investors to gain access to key people in the firm (e.g., it takes "fewer calls," as Cohen et al. 2008 speculate). Moreover, conditional on access, networks may facilitate information being shared more readily with others in the same network (e.g., managers might "be more forthcoming with information," as Cohen et al. 2008 discuss). Or, the information that investors obtain through networks could be more precise and value-relevant. Publicly available data do not provide the detail necessary to distinguish among these channels of information flow. However, regardless of the exact nature of information flow along a network, networked investors may benefit from it, and in equilibrium, corporate officers may receive some consideration for their role in helping connected investors better understand the environment in which the firm operates. Possible channels for such consideration include higher compensation for the officers who make information more accessible to connected investors and voting support in shareholder-initiated proposals regarding executive compensation. We call this the *information hypothesis.*

A second possibility is that the positive relation between a firm's connectedness and compensation level (and voting support) arises because educational commonalities reduce institutional investors' uncertainty about CEO quality. Mutual fund managers may know—or think they know—more about a CEO's type because of an educational link, either through direct knowledge, mutual acquaintances, or a belief that a manager who attended their own school is more qualified. (This phenomenon is essentially statistical discrimination in the sense of Phelps (1972).) Observers may assess CEO quality based on which universities the CEO attended and what degrees he or she earned. We suggest that another proxy for CEO quality is the level of connected ownership in the company. This measure reflects the revealed preferences of networked investors—that is, people who might have directly or indirectly observed other qualities of the CEO through their common educational network. The *quality hypothesis* suggests that this revealed preference measure contains information about CEO quality or perceived quality beyond that of the CEO's educational attainment.

A third possibility is that a CEO with a larger educational network may be valuable per se for corporations. Networks may facilitate information flow to CEOs, and executives with more extensive networks may be able, through their network of contacts, to gather more useful information about matters that affect the firm. Alternatively, the gains to the firm from the CEO's connections could come from sources other than information (Fisman 2001). If some of the value from this network is allocated to the CEOs, then higher executive compensation and voting support will go hand in hand with potential network size. We call this the *network size hypothesis*.[1]

[1] Of course, another possibility is that educational commonalities capture some other unobservable trait that relates to executive compensation. Although it is difficult to completely rule out such a possibility, our vector of control variables and fixed effects (discussed in detail below) should mitigate many such concerns.

The *information hypothesis* suggests that when trades by networked investors are more profitable due to better access to information flow along the network, the more those investors will appreciate executives who facilitate the information flow, and the greater the compensation to those executives. We examine the relation between executive compensation and the correlation of educationally connected fund holdings and subsequent company stock returns. The idea is that a higher correlation between holdings and subsequent returns may indicate more insightful trades, perhaps due to having a better information set on which to trade. Under the *information hypothesis*, better-quality information flow from firm to fund should generate payback in the form of higher compensation. Our empirical findings are consistent with such an effect: a one-standard-deviation increase in the abnormal trading performance of educationally connected funds relates to about 3.4% higher total executive compensation, other things equal.

To test the *quality hypothesis*, we introduce variables that reflect whether the CEO graduated from elite educational institutions and/or holds an MBA. These measures cannot capture CEO quality perfectly, of course, but may relate to the nature of the training the CEO has. In addition to these explicit measures of quality, we use revealed preferences of investors—ownership levels related to educational commonalities—as an indirect measure of the actual or perceived quality of the CEO. Connected ownership levels may be proxying for readily observable measures of CEO quality, or they may contain incremental information about CEO quality. Our evidence is consistent with the *quality hypothesis*. In fact, we find that this soft-information measure of CEO quality, as reflected by connected investors' revealed preferences, is more important in explaining executive compensation than explicit measures based directly on CEO education. Moreover, using a battery of controls, we find that the magnitude of the coefficients on the overt proxies for CEO quality (i.e., graduating from an elite educational institution and/or holding an MBA) are small and are not statistically significant in explaining executive compensation. Of course, our proxies for testing the quality hypothesis do not perfectly reflect CEO quality, and so these findings could be attributed to simply having inadequate measures of the true quality of the CEO.

We test the *network size hypothesis* by including a measure of network size in our baseline regression. Motivated by Engelberg, Gao, and Parsons (2010), we compute an executive's potential network size as the number of other CEOs, fund managers, or both with whom the executive has a network connection through educational overlap in our sample. We find mixed support for the *network size hypothesis*.

Finally, we examine whether network-related compensation impacts the likelihood of shareholder-initiated proposals to reduce executive compensation. If networks like the ones we study produce value for firms (as the *network size hypothesis* might suggest), then shareholders may view the network-related portion of executive compensation as reflecting what the CEO is "worth" through the value of his or her connections. Similarly, if networked ownership reflects investors' perception of CEO quality, the network-related portion of compensation will reflect what the CEO is perceived to be worth due to his or her qualities. But alternatively, if networks were to facilitate favoritism toward connected shareholders (as the *information hypothesis* might suggest), then excluded shareholders may view the network-related portion of compensation as "excessive" and seek to reduce executive compensation. We ask which effect is stronger in the data by examining whether higher levels of network-related compensation lead to fewer or more shareholder-initiated proposals to limit executive compensation for firms with relatively high levels of connected ownership. We find that higher levels of the CEO compensation are associated with a larger likelihood of a shareholder-initiated proposal to limit executive compensation in a year, but only for those firms that have relatively high levels of ownership held by in-network mutual funds. We interpret this finding as being more consistent with the *information hypothesis*.

Our findings that are consistent with the *information hypothesis* are analogous to the "mutual back scratching" documented by Brick et al. (2006) between CEOs and highly compensated directors and the effect of connections on director-CEO relations, as in Hwang and Kim (2009) and Barnea and Guedj (2006). We interpret our results as favoring the idea that corporate executives benefit from network connections with mutual fund managers. We view this finding as the natural complement to the result by Cohen et al. (2008) that mutual fund managers benefit from their educational overlap with corporate executives.

The article proceeds as follows. Section 2 describes our data and variable construction. In Section 3, we examine voting practices of mutual funds as a function of their network status. Section 4 explores which types of firms have higher or lower levels of share ownership by in-network funds. Section 5 presents our results on the relation between executive compensation and the amount of educationally connected ownership of the firm. Section 6 is the Discussion and Conclusion. We list the results of several robustness tests in a separate online appendix.

2. Data and Variable Construction

We use several sources to collect data on mutual fund holdings, votes on shareholders' meeting proposals, individual educational backgrounds, company locations, and firm-specific and fund-specific data. We obtain stock return and accounting data from CRSP/Compustat. We have two sets of main tests. The first set involves the relation between connections and voting behavior by mutual funds in shareholder proposals. The second set of tests involves the relation between executive compensation and educationally connected ownership in the firm.

2.1 Mutual fund voting data

The votes in shareholder meetings in the United States were confidential until 2003. Starting in 2003, the SEC required all mutual funds to disclose their votes in N-PX and N-PX/A filings. We use Riskmetrics' Governance Analytics database to obtain voting records of individual mutual funds collected from these filings for meetings occurring between the beginning of 2004 and the end of 2007. Votes are made by fund managers and are recorded as one decision (for, against, or abstain) per proposal per fund.[2] Our main voting tests involve votes on shareholder-initiated compensation proposals as categorized by RiskMetrics. This sample has 253,903 fund voting decisions made for 610 shareholder resolutions proposed at 257 firms. In this data set, there are 358 mutual fund families and 8,023 individual mutual funds.[3]

2.2 Mutual fund holdings data

To compute connected ownership, we need to know stock ownership levels by institutions and determine whether there are any network connections between the investors and the executives in the firm. We calculate the weight of stock holdings in a given fund using the CDA/Spectrum Mutual Fund Holdings database. This database includes information from all registered mutual funds filing with the Securities and Exchange Commission (SEC). The data include holdings of individual funds that come from fund prospectuses and SEC N30D filings at either quarterly or semiannual frequency. We include only holdings of mutual funds that are Center for Research in Security Prices (CRSP) share codes 10 or 11 (i.e., ordinary common shares). The fund family names, which we use to match funds to voting data, come from the CRSP mutual fund database. Morningstar's biographical data and fund family names are linked to Thomson Mutual Fund Holdings data using the MFLINKS database (see Wermers 2000 for details of merging these two databases).

2.3 Mutual fund manager and company manager education data

To determine whether a firm and an institution have a network connection through a shared alma mater, we need education data for key personnel at both the mutual funds in our sample and for key executives (CEO, CFO, Chairman) at the firms in our sample. Our mutual fund manager education data come from Morningstar's OnDisk and Principia Advanced database.[4]

[2] The decisions of how managers should vote are often made at the fund family level, and fund managers may have input and/or vote their shares on their own. We explore this further below.

[3] Riskmetrics backfills the voting records. (Morgan et al. 2009, p. 15) note that "because parsing routines that would read in a larger number of funds were developed first, smaller fund families are more likely to be omitted [in earlier studies]." Our data are extracted in mid-2009, so we have more observations than earlier studies using the same data source and we are more likely to have voting records of smaller funds.

[4] Morningstar, Inc., used different names for this database throughout our sample period. The three different names are Principia Mutual Funds Plus, Principia Mutual Funds Pro Plus, and Principia Mutual Funds Advanced.

We use the beginning-of-year CDs to collect manager education data, as the January CDs report data as of December 31 of the previous year. We include in our sample all the domestic equity funds with a self-declared investment objective of growth, aggressive growth, growth-income, or equity-income that started their operations after 1991. This restriction has the effect of biasing our sample toward younger funds, though not necessarily toward any particular type of fund managers, as they may move from fund to fund during their career.

Following Cohen et al. (2008), we exclude index funds, balanced funds, and funds of funds, as well as other types of funds that are in some way restricted in their investment decisions. We exclude index funds because their managers have little discretion in which stocks to hold. We exclude balanced funds and funds-of-funds because they generally hold non-equities as a substantial portion of their assets, and non-equities are less information-sensitive and do not generally have voting privileges. Our search yielded 3,116 mutual fund managers for 1,736 funds between 1992 and 2006.[5]

We gather firms' senior officers' (CEO, CFO, Chairman) names from the Execucomp database, supplementing where necessary with board members found in the 2006 Riskmetrics Directorship file. We screen titles of individuals to identify CEO, CFO, and Chairman. We exclude individuals without title identifications. We obtain education information for these people from Bloomberg through its BIO function and from a Web database, Zoominfo.com. In Appendix 1, we outline the data search process. In gathering our education data, we follow Cohen et al. (2008), treating different campuses of a university system as separate universities (e.g., UCLA, UCSD, and UC Berkeley are treated as separate universities). Similarly, if just a university name is given for a university system (e.g., University of Texas for the UT system of schools), we code the entry as belonging to the main campus. If an educational institution's name could apply to two different educational institutions and the biography is not clear about which institution the individual attended, we drop the observation from our sample. For each individual, we collect information for all degrees: bachelor's, master's, doctorate, J.D., M.B.A., M.D., and so forth.

Of the firms in the Execucomp and Riskmetrics Directorship databases (which are primarily the S&P 1500 companies), we were able to collect educational background information for 6,037 senior officers for 1,840 CRSP stocks between the years 1992 and 2006. This reflects about 71% of the Execucomp firm-years, and our data requirements tilt our sample slightly toward firms that are larger and (perhaps because they are larger) have higher

5 We are grateful to Iordanis Karagiannidis for providing mutual fund manager education data between 1992 and 2003. See Karagiannidis (2008) for a detailed description of the data collection procedure. We use CRSP's Mutual Funds database Summary file to identify mutual fund manager names between 2004 and 2006. We find education data for these additional names from Zoominfo.com.

compensation levels than firms with missing educational data.[6] To assess the possibility of sample selection bias in a hand-collected sample such as this, we examine what firm characteristics relate to whether we are able to find educational data. These tests show, not surprisingly, that we are more likely to find educational data for executives at larger firms. Other firm characteristics are not strongly related to the likelihood of our finding information about individuals at these firms.

2.4 Measuring connections

To identify network connections and connected ownership, we create a file in which a record contains a $weight_{ift}$ variable that represents the relative dollar investment in firm i in fund f's total dollar investment at time t, a *broad connection* dummy variable that takes a value of 1 if one of the senior officers/directors of firm i and one of the managers of fund f attended the same school, and a *narrow connection* dummy variable that takes a value of 1 if the one of the senior officers/directors attended at the same time as fund managers. The definitions of the *broad connection* and *narrow connection* variables follow those of Cohen et al. (2008). In our main compensation analysis, we collapse fund-level information to firm level to calculate the firm-level *connected ownership* variable. That is, we use the percentage of the firm's stock that falls under the *broad connection* and *narrow connection* ownership definitions. In our voting and connection analysis, we link the connections between fund managers and firm executives/directors to votes of mutual fund families using the *broad connection* dummy.

2.5 Variables to test the *information*, *quality*, and *network size hypotheses*

We construct a variable, which we name *smart trading correlation*, to measure abnormal trading performance of educationally connected mutual funds trading in a firm's stock. This variable reflects the *abnormal* within-stock time-series correlation between educationally connected funds' holdings in a stock and subsequent returns in the stock. To compute the *smart trading correlation* measure, we start with educationally connected funds, computing a measure, *rho*, as follows. At the beginning of each calendar quarter q, we calculate the aggregate mutual fund holdings of each firm i for connected funds using fund-level shareholdings data. For each firm i and year t, we calculate the correlation between beginning-of-quarter aggregate shareholdings for connected funds and quarterly return. For each fiscal year t, we use quarterly observations obtained from years t, t–1, and t–2 to compute the correlations. To strike a balance between measurement error problems and inclusiveness, we require at least

[6] For comparison, Cohen et al. (2008) have information on 14,122 senior officials for 7,660 CRSP stocks between 1990 and 2006. Because they do not require executive compensation data, their sample includes not only our Execucomp firms but also many smaller firms.

eight quarterly observations (out of twelve) to compute each rho_{it}. Some mutual funds may not report quarterly holdings (e.g., they might report every six months). In such cases, we assume that the holdings at the beginning of the last quarter carry over to the following quarter in which no holding is reported. (Omitting these funds gives very similar results.) We repeat the entire process for unconnected funds to produce a rho' measure. The difference between *rho* and rho' is our measure of *smart trading correlation*, and by using the difference, we are able to interpret the variable as the abnormal trading ability of connected funds over unconnected funds trading in the same stock. The idea behind the *smart trading correlation* measure is that if educationally connected fund managers' trades are premonitory, the fund managers will increase their holdings prior to stock price runups, and will decrease their holdings prior to stock price declines. Such trading behavior would result in a positive *smart trading correlation* measure, and the more insightful the trades, the larger the *smart trading correlation* measure.

We construct a dummy variable, *MBA*, to denote if the CEO has an MBA (1 = MBA, 0 = no MBA). We construct a dummy variable, *Elite Degree*, to denote if a CEO has an educational affiliation with an institution we categorize as elite. The list of elite schools is the intersection of the top-20 ranking lists from *U.S. News & World Report* (2008), *Financial Times* (2006), and *Business Week* (2000). The elite schools are Berkeley, Chicago, Columbia, Dartmouth, Harvard, Michigan, Massachusetts Institute of Technology (MIT), Northwestern, New York University (NYU), Stanford, University of California at Los Angeles (UCLA), University of Pennsylvania, and Yale. Exclusions from our list (e.g., Duke, Cornell, Virginia-Darden) tend to arise because the schools are not on the *Financial Times* list (which has more non-U.S. schools than the other two lists).

We construct a variable to measure the potential network of an individual in our sample using his or her educational background. Specifically, for a given individual, we count the number of potential network connections (i.e., educational commonalities) within the universe of CEO/CFO and Chairman and mutual fund managers in that year in the data.

2.6 Compensation and other data

We obtain CEO compensation data from the Execucomp database. We use total compensation (*TDC1*, logged in our regressions) as our measure of compensation at the end of fiscal year t. This measure includes salary, bonus, total value of restricted stock granted, total value of stock options granted (using Black-Scholes), and long-term incentive payouts.

Our control variables are computed as follows. To measure CEO ability, we use an indicator variable, *Elite Degree*, that takes a value of 1 if the CEO has attended one of the schools we classify as an elite school. To control for firm performance, we include return on assets (*ROA*) of the current fiscal year

(Compustat data item 172/lag of data item 6) and *Sales Growth* (annualized percentage growth of sales in the previous three fiscal years). We also include lagged *ROA* in our specification to capture the prior year's firm performance. To capture effect of firm size on compensation, we include a dummy variable, *Index Member*, that takes a value of 1 if the company's stock is included in the S&P 500 index by the end of the fiscal year, and log of market value *(Market Value* = data item 25 * data item 199) of equity at beginning of the fiscal year. Membership in the S&P 500 index (*index member*) is obtained from the Compustat annual file. To control for future growth prospects, we include the market-to-book ratio, *Market to Book* (the ratio of *Market Value* and book value equity (data item 60) value at the end of the fiscal year), in our specification. To measure the effect of institutional monitoring on compensation (Hartzell and Starks 2003), we include *Institutional Ownership Concentration* (the Herfindahl index of institutional ownership at the end of the fiscal year; using other concentration measures, such as those proposed by Hartzell and Starks 2003, does not alter our findings). Other control variables are *Contemporaneous 12-Month Return* (cumulative stock returns over twelve months before the fiscal year-end), *Lag 12-Month Return* (cumulative stock returns over twelve months before the fiscal year-beginning), *Illiquidity* (average monthly Amihud 2002 illiquidity measure using monthly observations during the fiscal year), *Past Volatility* (the standard deviation of twelve months' returns in the past fiscal year), and *Leverage* (the ratio of book value of liabilities [data item 9] to total assets [data item 6] at the end of the fiscal year).[7]

3. Sample Characteristics

In this section, we present our empirical results. Our tests are partially motivated by the result in Cohen et al. (2008) that information flows along a social network based on educational ties. As a preliminary step in our analysis, we use our data to reproduce one of the results from Cohen et al. (2008). Although we do not tabulate these results, we find, as do Cohen et al. (2008), that mutual fund managers overweight the firms with which they have educational connections. The overweighting of educationally connected firms is statistically significant in all our tests, and is about 3.1 to 4.2 basis points, depending on the stringency of the measure of connectedness. When we do not control for stock characteristics, the magnitude is 21 to 22 basis points. These replication tests control for geographic proximity, "eliteness" of the educational institution of the firm's executives' and fund manager's degree-granting institutions, index membership, earnings surprise, illiquidity, idiosyncratic volatility, market value, book-to-market, last twelve months' return, and whether the quarter is the

[7] Others have shown that executive compensation is related to firm size (Almazan et al. 2005; Baker, Jensen and Murphy 1988; Murphy 1998), firm performance (Smith and Watts 1992), firm growth opportunities (Smith and Watts 1992; Harvey and Shrieves 2001), and firm risk (Aggarwal and Samwick 1999).

Table 1
Descriptive statistics on educational background

CEO/CFO/Chairman		Mutual Fund Manager	
Harvard University	691	University of Pennsylvania	291
Stanford University	345	Harvard University	285
University of Pennsylvania	331	University of Chicago	181
Columbia University	220	Columbia University	175
University of Michigan	170	New York University	165
University of Chicago	164	Stanford	127
UT Austin	160	University of Wisconsin-Madison	126
Northwestern	150	Northwestern	100
New York University	144	Yale University	96
MIT	144	Dartmouth	90
Princeton	132	UCLA	89
Yale University	131	Virginia	84
Cornell	124	University of Michigan	82
Dartmouth	117	UC Berkeley	72
University of Wisconsin- Madison	115	Princeton	71
University of Illinois at Chicago	110	MIT	61
Indiana University	95	University of Illinois at Chicago	57
Virginia	95	UT Austin	54
UCLA	95	Boston College	48

In this table, we list the top twenty most connected academic institutions, ranked by the average number of connected firms or funds over the period 1992 to 2006. A firm (fund) is defined as connected to a fund (firm) if a senior officer and portfolio manager hold a degree from the same institution. We include in the sample of funds/portfolio managers actively managed, domestic equity mutual funds from the merged CDA/Spectrum–Morningstar data with a self-declared investment objective of aggressive growth, growth, or growth-and-income. The sample of firms includes the funds' holdings in common stocks (CRSP share codes 10 or 11).

last of the year. Thus, despite the fact that there are some differences between our data and those of Cohen et al. (2008), we reach the same basic conclusion in a regression of portfolio weights on connectedness measures and other control variables. We surmise that any differences in the data sources and collection procedures are minor and not material to our purposes.

3.1 Representation of educational institutions

In Table 1, we list the most represented universities in our sample. Harvard University is the most represented institution for corporate executives, and the University of Pennsylvania is the most represented for fund managers. Other common institutional affiliations of corporate executives in our sample are Stanford, University of Pennsylvania, Columbia University, and University of Michigan. Common institutional affiliations of mutual fund managers are Harvard University, University of Chicago, Columbia University, and New York University.

3.2 Voting results: The effect of connectedness on voting patterns

We first examine a channel through which equity investors can affect outcomes in the firms in which they invest—voting.[8] Our voting data come from

[8] Rothberg and Lilien (2006) find that mutual funds voted 66% of the time in management's favor on issues of compensation. David and Kim (2007) find that proposals concerning limiting executive pay were often opposed

Riskmetrics and are the votes by funds in shareholder proposals.[9] We use information on mutual fund/fund family links in the CRSP mutual fund database to merge voting data to firm/fund connection relationships. Our main tests use data on shareholder-initiated proposals to reduce executive compensation (e.g., "Limit Executive Compensation").

Panel A of Table 2 presents some descriptive statistics about our voting data. In our sample, on average 29% of mutual fund votes are in favor of shareholder proposals to reduce executive compensation. Mutual funds in a family do not always vote the same way on these proposals. In 19% of the shareholder-initiated proposals to reduce executive compensation we study, not all funds in a family vote the same way for that proposal.

Under the *information* and *quality hypotheses*, connected shareholders are likely to vote against shareholder-initiated proposals to reduce executive compensation. We test this idea formally with a probit model and regress the votes (for = 1) on a characteristic of the voter—that is, whether the vote is coming from an educationally connected shareholder (measured by the *broad connection* dummy variable described in Section 2.4) or a nonconnected shareholder. Theory provides little guidance for control variables, but we have enough observations to use a variety of fixed effects: firm, fund family, proposal, or firm-fund pair, each in turn. The tests include year fixed effects, but excluding year fixed effects does not qualitatively change our results. We compute heteroscedasticity-robust standard errors adjusted for clustering by fund. Panel B of Table 2 presents the results of five probit specifications.

3.3 Voting on shareholder-initiated proposals to reduce executive compensation

The first specification is our baseline, and in it we regress votes (for/against) in shareholder-initiated proposals to reduce executive compensation on whether the voter is educationally connected to the firm in question. We include only year fixed effects in this baseline. We find that educationally connected mutual funds are much less likely—20 percentage points less likely—to vote for shareholder-initiated proposals to reduce executive compensation than an unconnected investor.

Gillan and Starks (2000) present evidence that voting outcomes are related to the proposal sponsor's identity, issue type, prior performance, and time period.

by mutual funds. Ertimur, Ferri, and Muslu (2011) study "vote-no" campaigns and shareholder proposals related to executive compensation and find that shareholder activists target firms with high CEO pay, and, of those, firms with excess CEO pay experience a substantial reduction in total CEO pay on average. In our research design, proposal fixed effects control for proposal-specific characteristics such as being accompanied by vote-no campaigns or not.

[9] We note that in an earlier draft of the article we used voting data aggregated at the fund family level and found very similar results. We are grateful to Andy Eggers for generously sharing these data with us.

Table 2
Voting and connections

Panel A. Descriptive Statistics on Mutual Fund Voting in Shareholders' Compensation Proposals

	Vote in favor? (1=for)	Vote by a connected fund? (1=connected)	Non-unanimous votes
Mean	0.294	0.004	0.189
Std. Dev	0.455	0.064	0.391
N	253,903	253,903	253,903

Panel B. Votes and Connections

	(1)	(2)	(3)	(4)	(5)
Dependent variable = 1 if:	Vote for shareholder's compensation proposal?	Vote for shareholder's compensation proposal?	Vote for shareholder's compensation proposal?	Vote for shareholder's compensation proposal?	Vote for shareholder's compensation proposal?
Connected (broad)	–0.782***	–0.619***	–0.785***	–1.652***	–0.655***
	(0.078)	(0.096)	(0.117)	(0.282)	(0.121)
Firm fixed effects	No	Yes	Subsumed	Subsumed	Subsumed
Proposal fixed effects	No	No	Yes	Subsumed	No
Fund family fixed effects	No	No	No	Subsumed	No
Fund family–Proposal Pair fixed effects	No	No	No	Yes	No
Firm–Fund Pair fixed effects	No	No	No	No	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Intercept	Yes	Yes	Yes	Yes	Yes
Pseudo R^2	0.017	0.173	0.309	0.317	0.143
Number of obs.	253,903	228,137	251,139	48,062	54,683
Marginal effect on *Connected*	–0.200	–0.162	–0.146	–0.394	–0.292

Panel A of this table gives summary statistics for the mutual fund votes database. Panel B reports estimates of the following Probit model: *Vote for proposal* = f(*Connected*, *fixed effects*) + residual. The unit of observation in these Probit tests is fund-proposal votes. *Connected (broad)* is a dummy variable that equals 1 if the mutual fund manager attended the same school as one of the senior officers/directors of the firm (CEO/CFO/Chairman). *Fixed effects* are dummies for each year, fund family, firm, proposal dummies, and/or fund-firm pairs, with the particular fixed effects for each model listed in each regression column (fixed effect coefficients not reported). The standard errors (reported in parentheses) are clustered by fund. ***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively.

Using fixed effects in our analysis allows us to control for variation in firm and proposal characteristics. In specifications (2), (3), and (4) we add to our baseline, in turn, firm, proposal, and fund family-proposal pair fixed effects, respectively. The firm fixed effects specification controls for time-invariant characteristics of firms being voted on—some firms may merit positive or negative votes—and forces identification through variation across funds voting on the firm. As with the baseline result, we find that educationally connected mutual funds are much less likely—sixteen percentage points less likely—to vote for shareholder-initiated proposals to reduce executive compensation than an unconnected investor. This basic finding continues to hold in specification (3), where we use proposal fixed effects. Identification arises from variation across funds of different connectedness status *in a given proposal*. This fixed effect controls for the qualities of the proposal, as well as firm and time effects. The marginal effect of the *connected* variable is fourteen percentage points. Specification (4) is especially conservative because it uses family-proposal dyad fixed effects, which subsume firm, proposal, and family fixed effects. This test eliminates variation in terms of family effects, firm effects, and individual proposal effects, and identifies a difference in voting between hypothetical fund manager John Smith at Family XYZ on Proposal 123 (on Firm ABC) relative to fund manager Jane Brown at Family XYZ on Proposal 123 (on Firm ABC), where one fund manager has a school connection to Firm ABC and the other does not. Because our estimation uses only observations for which there is more than one fund in the same family voting on a given proposal, the cleanness of this test comes at the cost of reduced sample size. Again, we find that educationally connected mutual funds are much less likely to vote for shareholder-initiated proposals to reduce executive compensation than an unconnected investor. The magnitude of the marginal effect is thirty-nine percentage points. In specification (5), we impose a fixed effect of *firm-fund pair*. That is, each *combination* of firm and fund gets a separate fixed effect. Thus, identification in this test comes from changes in a fund's or a firm's educational affiliation due to a change in top-level personnel (e.g., if a mutual fund changes from a "Harvard fund" to a "Yale fund" with a change in portfolio manager), and hence the effect on voting comes from the connection itself, not the characteristics of the firm or the fund. As with our previous tests, we find that educationally connected mutual funds are much less likely to vote for shareholder-initiated proposals to reduce executive compensation than an unconnected investor. Perhaps because this test uses a clean identification strategy, we find the magnitude of the effect of an educational connection to be quite large: educationally connected mutual funds are twenty-nine percentage points less likely to vote for shareholder-initiated proposals to reduce executive compensation than an unconnected investor.

Collectively, these tests suggest that a mutual fund's vote on a shareholder-initiated compensation proposal is associated with the educational connections between the fund manager and the firm's executives.

3.4 Voting propensities: Subsamples and alternative samples

When a fund manager has no discretion in how to vote, there can be no role for educational connections to influence how they vote. Thus, the baseline tests we present above are conservative, in that they pool cases where managers may or may not have voting discretion. We examined proxy voting guidelines for many fund families, and found that most (indeed, all that we saw) families allow some degree of latitude to fund managers to choose how they vote in proposals.[10] We find that, although many times the funds in a family vote in unison on a given proposal, many times they do not (about 40% for all proposals in our data, about 20% for the shareholder-initiated executive compensation proposals in our data).[11]

We find that the educational connections effect on voting propensities is far stronger when funds in a family do not vote in unison. In Table 3, specifications (1) and (2), we explore how educational connections impact voting propensities when not all the funds in the family vote the same way (specification (1)) compared with when there is within-family unanimity in the proposal (specification (2)). The differences are stark. When funds in a family do not vote unanimously, the effect of educational connections on the likelihood of a fund voting against reducing executive compensation is forty-two percentage points. In sharp contrast, when there is unanimity among funds in a family in a proposal, the likelihood of a fund voting against reducing executive compensation is seven percentage points. To the extent that educational connections influence voting behavior of mutual funds, that influence appears to be more acute when a fund deviates from other funds in the same family.

In specifications (3) and (4) of Table 3, we explore how educational connections impact voting propensities for families of different sizes. Specification (3) uses the subsample of votes by funds in relatively large

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10 Fidelity's website reports the following guidelines: "No set of guidelines can anticipate all situations that may arise. In special cases, Fidelity may seek insight from our portfolio managers and analysts on how a particular proxy proposal will impact the financial prospects of a company, and vote accordingly. The Proxy Voting Guidelines are just that—guidelines. They are not hard and fast rules, simply because corporate governance issues are so varied. In conclusion, Fidelity believes that there is a strong correlation between enhancing shareholder value and sound corporate governance. The Fidelity Mutual Funds' Proxy Voting Guidelines are intended to put this belief into action through the exercise of voting rights by the Funds." Putnam has adopted similar language: "The proxy voting guidelines are just that—guidelines. The guidelines are not exhaustive and do not address all potential voting issues. Because the circumstances of individual companies are so varied, there may be instances when the funds do not vote in strict adherence to these guidelines. For example, the proxy voting service is expected to bring to the Proxy Manager's attention proxy questions that are company-specific and of a nonroutine nature and that, even if covered by the guidelines, may be more appropriately handled on a case-by-case basis."

11 In our data, such voting disparities can be found in families of all sizes. Consider one example where funds in the AEGON/Transamerica family differ in how they vote on a May 2005 shareholder-initiated executive compensation proposal to reduce executive compensation for eBay, Inc. There is substantial realized variation in the actual votes. Of the ten funds voting, eight follow the Institutional Shareholder Services recommendation to vote for the proposal, and two funds, including one that we identify as connected, vote against the proposal. In a similar example, funds in the Fidelity family differ in how they vote on an April 2004 shareholder-initiated executive compensation proposal for Novell, Inc. Of twelve Fidelity funds that voted on this proposal, five voted for the proposal, six against, and one abstained. One of the funds is connected and, consistent with our regression results, votes against the proposal.

Table 3
Voting and educational commonalities: Subsamples and alternative samples

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable = 1 if:	Vote for shareholder's compensation proposal?	Vote for shareholder's compensation proposal?	Vote for shareholder's compensation proposal?	Vote for shareholder's compensation proposal?	Vote for shareholder's compensation proposal?	Vote for a *charitable contribution* proposal?	Vote for a *director election* proposal?
	Within family voting disparity subsample	Within family unanimity subsample	Small fund family subsample	Large fund family subsample	Top 5 fund family subsample		
Connected (broad)	–1.827***	–0.403***	–0.390**	–0.857***	–0.223	0.163	0.188***
	(0.268)	(0.101)	(0.161)	(0.143)	(0.321)	(0.121)	(0.039)
Proposal fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Intercept	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R^2	0.054	0.399	0.355	0.283	0.183	0.419	0.095
Number of obs.	48,062	198,004	73,225	158,019	22,624	67,456	8,254,180
Marginal effect on *Connected*	–0.420	–0.072	–0.107	–0.154	–0.149	0.018	0.022

This table reports coefficient estimates and marginal effects of the following Probit model: *Vote for proposal* = f(*Connected*, *fixed effects*) + residual. Column headings denote the type of vote (compensation proposal, charitable contribution proposal, or director election proposal). Column subheadings denote subsamples, if applicable. *Fixed effects* are dummies for each year, fund family, firm, proposal dummies, and/or fund-firm pairs, with the particular fixed effects for each model listed in each regression column (fixed effect coefficients not reported). The standard errors (reported in parentheses) are clustered by fund. ***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively.

fund families (i.e., fund families with many funds, specifically, more than the seventy-fifth percentile that year; other cutoffs give similar results), whereas specification (4) uses the subsample of votes by funds in relatively small fund families (i.e., fund families with few funds, specifically, less than the seventy-fifth percentile that year). We find that in both these subsamples, educational connections have a strong influence on voting propensities (marginal effects of eleven and fifteen percentage points, respectively). This finding that educational connections have a modestly stronger influence on voting propensities in large families than in small families holds for other cuts of the data, and continues to hold for all but the largest families: limiting the sample to the top ten largest families, the result remains (marginal effect of twenty-eight percentage points, significant at the 1% level), but when we limit the test to the top five largest families (specification (5) in the table) the statistical significance of the result dissipates. Thus, there is a nonlinearity in the effect of family size on the relation between educational connections and voting propensities; the result disappears for the very largest of fund families.

Specifications (6) and (7) are placebo tests. Here, the proposals we study relate to charitable contributions (proposed by shareholders) and director elections, respectively. We expect that network connectedness should not relate to voting practices in proposals like charitable contributions to the extent that they are relatively inconsequential to management. This is what we find. For charitable contributions proposals, there is no relation between educational connectedness and voting patterns; the marginal effect is less than two percentage points and is statistically indistinguishable from zero.

For director elections, we expect little impact of educational connectedness on voting practices in proposals like these. Although the outcomes of director elections may be relevant to the firm's management, the direct impact of the outcome of a director election on a CEO's utility is probably less than the impact of the CEO's compensation on his or her utility. Thus, we expect a weak effect of educational connections on voting in director elections. Again, this is what we find. We have a large number of observations for director election votes (more than eight million votes), so it is not surprising that the coefficient is statistically distinguishable from zero. Despite the fact that educational connectedness is statistically significantly related to how mutual funds vote in director election proposals, the effect is very small: a two-percentage-point marginal effect.

From these placebo tests, we conclude that educational connectedness generally does not have an effect on voting patterns in these proposals, and, to the extent that it does, the magnitude is minute compared with compensation proposals.

As an untabulated robustness test, we examine whether these voting results are being driven by educational networks associated with elite institutions. We find that they are not. When we omit CEOs with elite degrees, funds managers with elite degrees, or both, we find that the marginal effect of the connection variable is –13.2% (with a t-statistic of 3.32) with proposal fixed effects.

Table 4
Connected ownership and its determinants

	Connected Ownership (Broad)/Total Shares	*Connected Ownership (Narrow)/Total Shares*	*Connected Ownership/ Mutual Fund Ownership*	*Smart Trading Correlation*	*Market Value*	*Total Comp.*
Mean	0.766	0.394	4.665	0.001	7,090	4,584
Standard Dev.	2.062	1.408	11.046	0.325	24,068	11,652
Q95	4.267	2.360	25.972	0.559	27,589	15,347
Median	0.000	0.000	0.000	0.002	1,400	2,192
Q5	0.000	0.000	0.000	–0.541	148	432
N	13,390	13,390	13,390	5,501	13,390	13,390

This table reports the sample statistics on the ownership amount (in percentages) of connected mutual fund managers using *broad connection* and *narrow connection* definitions of connectedness. We report three measures of *connected ownership*: (1) the ratio of connected shares (ownership of mutual fund managers who attended the same school as one of the senior officers/directors of firm [CEO/CFO/Chairman]) to total shares in the firm; (2) the ratio of connected shares (ownership of mutual fund managers who attended the same school at the same time as one of the senior officers/directors of firm) to total shares; and (3) the ratio of connected shares (ownership of mutual fund managers who attended the same school as one of the senior officers/directors) to shares in the firm that are held by mutual funds in our sample. *Smart trading correlation* is the abnormal within-stock time-series correlation between educationally connected fund holdings in a stock and subsequent returns in the stock. *Market value* is the market value of equity at the beginning of the fiscal year (in millions USD). *Total compensation* (in thousand USD) is the total dollar value of compensation obtained from the Compustat Execucomp database. The sample period is 1992–2006, and the units of observation are firm-year.

We then examine the outcomes of these compensation proposals (untabulated). The connected ownership is significantly larger in shareholder-initiated proposals that are rejected (0.93%) than those that pass (0.63%). The difference is statistically different from zero, with a t-statistic of 5.0, where the standard error is clustered by firm.

These results support the previous findings that how a mutual fund is likely to vote in shareholder-initiated compensation proposals is a function of the educational connections between the fund manager and the firm's executives.

4. Connected Firms: Characteristics and Determinants

In this section, we evaluate what types of firms have higher or lower levels of shares owned by in-network, educationally connected mutual funds. The first step is to compute the percentage of a firm's stock held by educationally connected mutual fund managers. Table 4 presents some descriptive statistics for our sample.

Most sample firms have no connected ownership. The median firm in our sample of Execucomp firms has 0% of its stock held by mutual funds in which the fund manager has an educational connection to the firm's executives, even when we calculate *connected ownership* using the less stringent *broad connection* measure. This statistic understates true connectedness in several ways. We have data only on educational commonalities, but not other ways in which social ties can arise. Moreover, we have data only on mutual fund holdings but not the holdings of hedge funds, pension funds, or individuals. This distinction is important because with our data we cannot identify ownership or connectedness status for those shares *not held by mutual funds*. As a fraction of *all* shares in these firms, the average level of connected ownership held by mutual funds is 0.77%. But, as a fraction of the shares we have ownership data for (that is, those *shares held by mutual funds in our sample*), the average firm has *connected ownership* of 4.7% (Table 4, Column 3). The ninetieth percentile of the ratio of connected ownership to mutual-fund-owned shares in our sample is 15.8%, and the ninety-fifth percentile is 26.0%. If shares held by other investors are connected at a similar rate as for our sample of mutual funds, then the ratio of connected ownership to mutual fund ownership in our sample is more economically relevant to measure connectedness. The measure we use of *connected ownership* as a fraction of total shares in the firm is conservative in that it reflects connectedness as if *none* of the other shares are held by connected investors.

In Table 5, we examine determinants of the percentage of ownership that is educationally connected in a panel regression where each observation is one firm-year. Because the dependent variable, percentage of ownership that is educationally connected, takes only values from zero to 100, we use a Tobit regression with standard errors clustered by firm. Primary determinants of connected ownership (both broad and narrow definitions) are whether the firm's executives attended an elite institution (positive), S&P 500 index membership (positive), firm size (positive), contemporaneous twelve-month return (negative), illiquidity (negative), past volatility (positive), lag twelve-month return (negative), and concentration of stock holdings by institutions (negative).

5. Compensation Results: The Relation Between Network Connectedness and Compensation

In this section, we test whether CEO compensation at firms with higher levels of ownership by in-network mutual funds is higher than at counterparts with lower levels of in-network ownership. Our main results document a positive relation between educationally connected investments and executive compensation. Specifically, we regress the natural logarithm of total CEO compensation on a measure of connected ownership and control variables. In this analysis, our unit of observation is firm-year. The response variable, log of *Total Compensation,* is measured at the end of the fiscal year. We regress the natural logarithm of total CEO compensation on a measure of connected ownership and control variables. *Connected Ownership* is measured using the most recent mutual fund ownership information prior to the end of the fiscal year.

We note that excluding any one of these control variables does not materially alter our results. We also include industry fixed effects (or, as in some specifications, firm fixed effects), year dummies, and an intercept term. For our main tests, we have 13,390 firm-year observations. We compute heteroscedasticity-robust standard errors adjusted for clustering by firm. Table 6 presents the results of several regression specifications.

Table 5
Determinants of connected ownership

	Connected Ownership (broad definition)	*Connected Ownership (narrow definition)*
Elite Degree	2.976***	2.362***
	(0.184)	(0.223)
ROA	0.203	0.120
	(0.306)	(0.344)
Lag (ROA)	−0.187	−0.320
	(0.284)	(0.262)
Sales Growth	0.002	0.001
	(0.002)	(0.002)
Index Member	0.419***	0.259*
	(0.162)	(0.157)
Ln(Market Value)	0.664***	0.765***
	(0.090)	(0.106)
Illiquidity	−2.507***	3.780***
	(0.647)	(0.758)
Market to Book	0.004	0.012
	(0.022)	(0.013)
Past Volatility	0.451***	0.402***
	(0.119)	(0.118)
Contemporaneous 12-Month Return	−0.951***	−0.655***
	(0.121)	(0.116)
Lag 12-Month Return	−0.138*	0.030
	(0.080)	(0.069)
Leverage	−0.414	−0.425
	(0.325)	(0.305)
Institutional Ownership Concentration	−1.209**	−1.132*
	(0.590)	(0.633)
Fixed Effects	Industry	Industry
N	13,457	13,457
Pseudo R^2	0.082	0.090

This table reports the results of a pooled Tobit regression of connected ownership percentage (defined using *Broad Connect* and *Narrow Connect* definitions): *Connected Ownership* = f(*Elite Degree*, *ROA*, *Lag (ROA)*, *Sales Growth*, *Index Member, Ln(Market Value), Illiquidity, Market to Book, Past Volatility*, *Contemporaneous 12-Month Return, Lag 12-Month Return, Leverage, Institutional Ownership Concentration*, *Industry fixed effects*) + residual. *Connected Ownership* is measured using the most recent mutual fund ownership information prior to the end of fiscal year t. *Elite Degree* is a dummy variable that takes a value of 1 if one of the CEO's degrees is from one of the thirteen schools listed in Section 2.5. *ROA* is the return on assets for the current fiscal year. *Sales Growth* is the annualized percentage growth of sales in the previous three fiscal years. *Index Member* is a dummy variable that takes a value of 1 if the company's stock is included in the S&P 500 index by the end of the fiscal year. *Market Value* (in millions USD) is the market value of equity at the beginning of the fiscal year. *Illiquidity* is calculated using the average monthly Amihud (2002) illiquidity measure using the monthly observations during the fiscal year. *Market to Book* is calculated using the ratio of market value and book value equity value at the end of the fiscal year. *Past Volatility* is the standard deviation of twelve months' returns in the past fiscal year. *Contemporaneous 12-Month Return* is the cumulative stock returns over twelve months before the fiscal year-end. *Leverage* is the ratio of book value of liabilities to total assets at the end of the fiscal year. *Institutional Ownership Concentration* is the Herfindahl index of institutional holdings at the end of the fiscal year. The sample period is 1992–2006, and the units of observation are firm-year. Industry fixed effects are based on the Fama-French 48 industry classification. *Ln(Market Value), Illiquidity, Market to Book*, and *Past Volatility* are standardized using sample mean and standard deviation. The standard errors (reported below estimates) are clustered by firm. ***,**, and * represent significance at the 1%, 5%, and 10% levels, respectively.

5.1 Compensation baseline result

Our first specification uses a relatively inclusive definition for connected ownership—whether a mutual fund manager and at least one member of the executive team of the company attended the same educational institution, even if their dates of attendance did not overlap. The coefficient on connected ownership is 0.025 and is statistically significant. This result means that,

Table 6
Compensation determinants

	(1)	(2)	(3)	(4)
Connected Ownership (broad definition)	0.025∗∗∗	0.012∗∗∗	0.042∗∗∗	
	(0.005)	(0.004)	(0.011)	
Connected Ownership (narrow definition)				0.027∗∗∗
				(0.006)
Elite Degree	0.002	−0.040	Subsumed	0.016
	(0.042)	(0.028)		(0.041)
ROA	−0.093	0.099	−0.195	−0.093
	(0.089)	(0.079)	(0.153)	(0.089)
Lag (ROA)	−0.051	0.087	0.062	−0.049
	(0.134)	(0.079)	(0.171)	(0.134)
Sales Growth	0.002∗∗∗	0.003∗∗∗	0.004∗∗∗	0.002∗∗∗
	(0.001)	(0.001)	(0.001)	(0.001)
Index Member	0.049	0.265∗∗∗	0.014	0.051
	(0.049)	(0.097)	(0.064)	(0.049)
Ln(Market Value)	0.658∗∗∗	0.561∗∗∗	0.681∗∗∗	0.658∗∗∗
	(0.025)	(0.029)	(0.032)	(0.025)
Illiquidity	0.032	−0.209	0.185	0.024
	(0.153)	(0.171)	(0.220)	(0.153)
Market to Book	−0.024∗∗	−0.008	−0.025∗∗∗	−0.024∗∗
	(0.011)	(0.013)	(0.007)	(0.011)
Past Volatility	0.129∗∗∗	0.042∗∗	0.119∗∗∗	0.131∗∗∗
	(0.024)	(0.018)	(0.037)	(0.024)
Contemporaneous 12-Month Return	−0.082∗∗	−0.057∗∗∗	−0.05	−0.088∗∗
	(0.040)	(0.019)	(0.049)	(0.040)
Lag 12-Month Return	0.079∗∗∗	0.068∗∗∗	0.072	0.079∗∗∗
	(0.028)	(0.018)	(0.050)	(0.028)
Leverage	−0.709∗∗∗	−0.113	−0.814∗∗∗	−0.711∗∗∗
	(0.084)	(0.079)	(0.118)	(0.084)
Inst. Ownership Concentration	−0.363∗	0.185∗	(0.474)	−0.367∗
	(0.191)	(0.108)	(0.306)	(0.192)
Fixed Effects	Industry	Firm	School, Industry	Industry
Year Dummies, Intercept	Included	Included	Included	Included
N	13,390	13,390	13,390	13,390
R^2	0.42	0.69	0.51	0.42

This table reports the pooled OLS regression: *Compensation* = a + b**Connected Ownership* + c**Controls* + d**Fixed Effects* + residual. *Compensation* is the natural logarithm of *Total Compensation*. *Connected Ownership* is either the broad or narrow definition of connected ownership defined in Table 4. Other control variables are defined in Table 5. *Fixed effects* refer to a series of year, industry (Fama-French 48 industry classification), school, or firm dummies (coefficients not reported). The standard errors (reported in parentheses) are robust to heteroscedasticity and clustered by firm. ***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively.

other things equal, a one-percentage-point increase in connected ownership is associated with a 2.5% increase in total CEO compensation. To put this in perspective, a one-standard-deviation increase in connected ownership translates into an increase in CEO total compensation of about \$236,000 for the mean company in our sample. When we estimate the regression using only the firm-years in which connected ownership is strictly greater than 0%, the coefficient on connected ownership increases to 0.028 (not tabulated). We note that our results are not sensitive to the inclusion of several variables that some researchers use to reflect various aspects of the firm's corporate governance, such as the G-index and insider ownership. We provide details of these and other robustness tests in Appendix 2.

5.2 Firm fixed effects or educational institution fixed effects

Our second specification repeats the first, but replaces industry dummies with firm fixed effects. This specification forces identification through time-series variation of connected ownership within a firm. If some omitted firm-specific, time-invariant factors drive the results in our first specification, adding firm dummies will capture the impact of these factors. The coefficient estimate on *connected ownership* decreases by about 50% to 0.012, but remains statistically significant. Thus, even within-firm time-series variation in *connected ownership* is related to CEO compensation, although the cross-firm variation in *connected ownership* has a stronger effect.

Our third specification repeats the first, but adds dummy variables for each educational institution. If some omitted school-specific, time-invariant factors drive our results (e.g., if Harvard-affiliated executives are systematically better or worse than their counterparts from other schools), then adding school dummies will absorb the impact of these omitted factors. The coefficient estimate on *connected ownership* increases to 0.042, and remains statistically significant.

5.3 Restrictive measure of connected ownership

Our fourth specification repeats the first, but uses a more restrictive definition of *connected ownership*—ownership is connected if a mutual fund manager and at least one member of the executive team of the company attended the same educational institution, and their dates of attendance overlap. The coefficient estimate, 0.027, is statistically significant and slightly larger than that when we use the broader definition of connectedness to compute *connected ownership*. However, the variation in our narrow definition of *connected ownership* is smaller, so a one-standard-deviation increase in this measure of connectedness translates into an increase in CEO total compensation of about $174,000 for the mean company in our sample, ceteris paribus.

5.4 Tests of the *information*, *quality*, and *network size* hypotheses

In Table 7, we introduce to our regression specification proxies for tests of the *information hypothesis*, the *quality hypothesis*, and the *network size hypothesis*. Our regressions in this table are of the same structure as those in Table 6 and use the same vector of control variables, except as noted. We suppress reporting the results for the control variables to conserve space.[12]

[12] Because we are more likely to find educational data for executives at larger firms, we run all the tests in this section for subsets of firms segmented on size (untabulated). Specifically, we run all the tests (except for specification (4), which already incorporates a match on market value) for observations above (and, separately, below) the median market value in a year. All the conclusions from this section are qualitatively unchanged for both size subsamples. With a higher take-up rate for educational data for executives at large firms, we expect for those firms a measure of connected ownership that contains less measurement error than for small firms. Consistent with this expectation, we find that the magnitude of the coefficient on *connected ownership* tends to be much larger for the above-median size sample than for the below-median size sample.

Table 7
Tests of the *information*, *quality*, and *network size* hypotheses

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Connected Ownership	0.029***	0.024***	0.029***	0.034***		0.026***	0.027***	0.012***	0.027***
	(0.006)	(0.005)	(0.006)	(0.008)		(0.006)	(0.006)	(0.004)	(0.006)
Smart Trading Correlation	0.105***		0.119***				0.110***	0.083**	0.111***
	(0.040)		(0.043)				(0.041)	(0.035)	(0.041)
Elite Degree		–0.002	–0.008						–0.015
		(0.043)	(0.050)						(0.050)
MBA CEO		0.029	0.058						0.054
		(0.033)	(0.040)						(0.039)
Network Size					0.364**	0.181	0.222	0.412**	0.175
					(0.147)	(0.145)	(0.185)	(0.165)	(0.181)
Other controls from Table 6	Yes	Yes	Yes	Yes: differenced	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Industry	Industry	Industry	Sample firm's industry	Industry	Industry	Industry	Firm	Industry
Year Dummies, Intercept	Included	Included	Included	Yes	Included	Included	Included	Included	Included
N	5,501	13,390	5,159	2,270	8,219	8,219	5,489	5,489	5,489
R^2	0.41	0.42	0.41	0.40	0.46	0.46	0.41	0.72	0.41

This table reports variations on the pooled OLS regression: *Compensation* = a + b_1**Connected Ownership* + b_2* *Smart Trading Correlation* + b_3* *Elite Degree* + b_4* *MBA CEO* + b_5* *Network Size* + c**Controls* + d**Fixed Effects* + residual. *Compensation* is the natural logarithm of *Total Compensation* at the end of the fiscal year. *Connected Ownership* is the broad definition of connected ownership defined in Table 4. *Smart Trading Correlation* is the abnormal within-stock time-series correlation between educationally connected fund holdings in a stock and subsequent returns in the stock. *Elite Degree* is a dummy variable that takes a value of 1 if one of the CEO's degrees is from one of the thirteen schools we list as elite in Section 2.5. *MBA CEO* is a dummy variable that takes a value of 1 if the CEO has an MBA degree. *Network Size* (x1/1000) is the number of individuals among "active" mutual fund managers (i.e., those who are managing a mutual fund in the observation year) and among other sample firm executives who attended the same school as firm officers or chairman (CEO/CFO/Chairman). Other control variables are the same as those used in Table 6 and are defined in Section 2.6. Specification (4) is a differences regression: we generate a list of potential matches of non-connected firms to sample firms with connected ownership greater than 2.5%, matching year-by-year on the CEOs' undergraduate institution and MBA status. When we cannot match on both undergraduate institution and MBA status, we drop the requirement of a match on MBA status. Among potential matches, we choose as the best match the observation that minimizes the absolute difference in market value. We then regress the difference in log total compensation on the difference in connected ownership and the difference in each control variable and industry fixed effects for the sample firm with connected ownership above our threshold. *Fixed effects* refer to a series of industry (Fama-French 48 industry classification) or firm dummies (coefficients not reported). The standard errors (reported in parentheses) are robust to heteroscedasticity and clustered by firm. ***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively.

5.4.1 *Information hypothesis* tests. Regression specification (1) in Table 7 repeats the baseline specification with industry fixed effects from the previous table, but adds a new variable, *smart trading correlation,* to the regression. Details of how we construct this variable are in the Data section above (Section 2.5). This *smart trading correlation* measure reflects the abnormal within-stock time-series correlation between educationally connected fund holdings in a stock and subsequent returns in the stock. If educationally connected fund managers make better-performing trades than their unconnected counterparts, the connected fund managers will, more than their unconnected counterparts, increase (or choose not to decrease) their holdings prior to stock price runups, and will decrease (or choose not to increase) their holdings prior to stock price declines. Such trading behavior would result in a positive *smart trading correlation* measure, and the more insightful (i.e., better-performing) the trades, the larger the *smart trading correlation* measure. We note that, of course, this variable is not literally a measure of smartness or of trading insight; as well as skill and information, it also captures luck. However, because we are computing the variable as an abnormal trading performance measure, it captures whether for a given stock educationally connected mutual funds' trades are consistently "*more* informed," "smarter," and/or "luckier" than those of unconnected counterparts trading the same stock over the same time period. This variable is unlikely to be related to a firm's network size because it captures within-firm abnormal time-series changes in *connected ownership* differenced from changes in unconnected ownership levels as they each relate to future stock returns in the firm. This variable is more likely to be related to information flow along a firm's educational network than to CEO quality because the variable captures both abnormally "smart" buys and sells by investors, and because information about a firm's future returns is likely to change much faster than information about the CEO's quality.

With this *smart trading correlation* measure in the regression, not only does the connectedness measure remain positive and significant, but also the *smart trading correlation* measure loads positively and significantly. The coefficient on smart trading is 0.105 and is statistically significant. The standard deviation of smart trading is 0.33, so a one-standard-deviation increase in the *smart trading correlation* measure corresponds to 3.4% more total compensation. One interpretation of this result is that the higher the quality of information flow from a firm's managers to educationally connected mutual funds managers, the more the firm's managers are rewarded with higher executive compensation, ceteris paribus.[13] Although we exclude the test from the table, we repeat specification (1) with firm fixed effects. Consistent with the previous specification, the

[13] We note that our findings are qualitatively similar in interpretation if we alter the specification so that instead of using our *smart trading correlation* variable, we include separately the *rho* and *rho′* defined in Section 2.5 above.

coefficient on *smart trading correlation* is 0.090 and is statistically significant, and the coefficient on *connected ownership* remains significant.

5.4.2 *Quality hypothesis* tests. In specifications (2) through (4), we provide some potential tests of the *quality hypothesis*. We use several proxies to test the *quality hypothesis*. The first proxy is *connected ownership,* which reflects the revealed preferences of networked investors. Our other proxies for CEO quality include dummy variables for whether the CEO is affiliated with an elite institution, whether the CEO has an MBA, or both. Although the explicit measures of CEO quality may have a straightforward interpretation, it is also possible that individuals without an MBA or from less prestigious institutions may have to work harder to achieve the position of CEO to overcome any perceptions about their ability. Thus, the revealed preferences measure may be interpreted as a certification by in-network investors about their perception of the CEO's quality.

Specification (2) augments our baseline regression in Table 6 by adding a dummy variable for whether the firm's CEO has an MBA. As in our baseline regression in Table 6, the *Elite Degree* variable does not load, and neither does the *MBA* variable. The coefficient on connected ownership is nearly unchanged at 2.4%. To the extent that our explicit measures of CEO quality are reasonable, this result suggests that the relation between connected ownership and CEO compensation is not solely due to connected institutions gravitating to owning the stocks of companies run by CEOs with MBAs or with degrees from elite institutions. To the extent that connected ownership reflects the revealed preferences of networked investors—that is, people who might have directly or indirectly observed other qualities of the CEO through their common educational network—this result suggests that compensation of CEOs who are of (or are perceived to be of) higher quality is larger.

Exploring this possibility, we find that this non-result is because these characteristics go hand-in-hand with firm size—larger firms hire better CEOs, and better CEOs are attracted to larger firms—and firm size crowds out the relation between CEO quality and compensation.[14]

5.4.3 *Network size hypothesis* tests. In regression specification (5), we add to our baseline regression with industry fixed effects a measure of *network size* (see Section 2.5 for details of how we construct the variable). We omit *connected ownership* in this specification. Although we construct the *network size* variable and our sample somewhat differently from Engelberg, Gao, and Parsons (2010), the spirit is similar. And, consistent with their results, we find that *network size* is positively related to CEO compensation. When we also include *connected*

[14] Indeed, firms with a CEO with an MBA are twice as large as those with a non-MBA CEO (median market value of \$1.90 billion, compared with \$0.97 billion), and firms with an elite-institution executive affiliation are triple the size of firms without (median market value of \$3.02 billion, compared with \$0.93 billion).

ownership in the regression in specification (6), the coefficient on *network size* drops by half and becomes statistically insignificant, whereas the coefficient on *connected ownership* is the same (2.6%) as in our baseline test and remains statistically significant.

In specifications (7) and (8), we include all three variables of *connected ownership*, *smart trading correlation*, and *network size* in models with industry and firm fixed effects, respectively. Here, the *information hypothesis* and *quality hypothesis* continue to receive support. The *network size hypothesis* receives support when we use firm fixed effects, but not when we use industry fixed effects.

5.4.4 Joint hypothesis tests. In specification (9), we include proxies for each of our hypotheses: *smart trading correlation* to proxy for the *information hypothesis*, *elite* and *MBA* to allow us to test the *quality hypothesis*, and *network size* to proxy for the *network size hypothesis*. When we include all of these proxies and our usual control variables, the coefficient on connected ownership is 2.7% and statistically significant at the 1% level. The coefficient on *smart trading correlation* is 0.111 and statistically significant at the 10% level. None of the variables *elite, MBA,* or *network size* is statistically significant.

Thus, we conclude that these tests offer mixed support for the *network size hypothesis* and support for the *information hypothesis* and the *quality hypothesis*. To the extent that our *smart trading correlation* measure is a good proxy for information flow from firm to fund along the educational network, the evidence is consistent with the *information hypothesis*. When we let the *network size hypothesis* and the *information hypothesis* compete for explanatory power, the evidence supports the *information hypothesis* over the *network size hypothesis* with industry fixed effects. With firm fixed effects, the evidence supports both the *information hypothesis* and the *network size hypothesis.* The evidence supports the idea that CEO quality, as measured by educational attainment and affiliation, is rewarded through higher total compensation, but it is difficult to separate this effect from firm size. Moreover, explicit measures of CEO quality do not affect the relation between connected ownership and executive compensation.

5.5 Determinants of shareholder-initiated proposals to limit executive compensation

Our last tests ask what firm characteristics influence the incidence of shareholder-initiated proposals to limit executive compensation. Our hypotheses suggest that these voting events may relate to levels of executive compensation and connections. Both the *quality hypothesis* and the *network size hypothesis* suggest that a higher level of connections between a CEO and the firm's investors is beneficial to investors with and without ties to the CEO. Under the latter hypothesis, connections are valuable per se. Under the former hypothesis, connections are valuable because they indicate higher perception of CEO quality. In contrast, however, the *information hypothesis* suggests that educationally connected investors reward CEOs for information flow along the educational network. If such information flow is a direct benefit mainly to connected investors, then it may reflect a transfer of wealth from investors without network ties to the CEO through higher executive compensation. That is, higher levels of compensation related to connected ownership may be a legitimate reward for skill or resources (under the *quality hypothesis* and the *network size hypothesis*, respectively). Alternatively, higher levels of compensation related to connected ownership may relate to payback for information flow along the network (under the *information hypothesis*).

This difference allows us to construct a test to provide additional evidence to distinguish between the *information hypothesis* and either the *quality hypothesis* or the *network size hypothesis* (or both). We regress the outcome of whether or not there are shareholder-initiated proposals to limit executive compensation for a firm in a year on unexplained CEO compensation and connected ownership. We include our usual vector of control variables to identify unexplained compensation over and above other firm characteristics. Table 8 presents the results of these tests.

We find that higher levels of connected ownership are associated with a smaller likelihood of a shareholder-initiated proposal to limit executive compensation in a year. This finding is consistent with any of the three hypotheses we discuss above: connected ownership insulates CEOs from

Table 8
Predicting shareholder proposals to limit executive compensation

	(1)	(2)
Interaction: Log(Compensation) × *Connected Ownership*	0.017***	
	(0.007)	
Interaction: Log(Compensation) × *Dummy (Connected Ownership > 2.5%)*		0.043***
		(0.010)
Log(Compensation)	0.000	–0.008**
	(0.005)	(0.004)
Connected Ownership	–0.142***	
	(0.051)	
Dummy (Connected Ownership > 2.5%)		–0.346***
		(0.078)
Other controls from Table 6	Yes	Yes
Fixed Effects	Industry	Industry
Year Dummies, Intercept	Yes	Yes
N	3,866	3,866
R^2	0.18	0.18

The first column of this table reports marginal effects of the following linear probability model: *Proposal* = f(*Log(Compensation)* × *Connected Ownership*, *Log(Compensation), Connected Ownership*, Other controls from Table 6, *fixed effects*) + residual. The second column replaces *Connected Ownership* with a dummy variable that takes a value of 1 when *Connected Ownership* > 2.5% at the end of fiscal year t. Response variable, *Proposal,* takes a value of 1 if there is a shareholder-initiated executive compensation proposal in fiscal year t+1. *Fixed effects* are dummies for each year and each industry (fixed effect coefficients not reported). The standard errors (reported in parentheses) are clustered by firm. ***, **, and * represent significance at the 1%, 5%, and 10% levels, respectively.

shareholders' attempts to rein in excess compensation (*information hypothesis*), demonstrates investors' belief that the CEO is of high quality (*quality hypothesis*), and/or reflects the rents paid to the CEO for his or her valuable network (*network size hypothesis*).

We also find that higher levels of CEO compensation are associated with a larger likelihood of a shareholder-initiated proposal to limit executive compensation in a year, but *only* for those firms that have relatively high levels of connected ownership. We interact compensation with connected ownership, and find that compensation has no direct effect on the likelihood of a shareholder-initiated proposal to limit executive compensation, but the marginal effect of compensation on this likelihood is large and positive for firms with relatively high levels of connected ownership. For instance, in specification (2), doubling unexplained compensation reduces the probability of a shareholder-initiated proposal to limit executive compensation by 0.8%, but for firms with relatively higher levels of connected ownership, the larger unexplained compensation increases the probability of a shareholder-initiated proposal to limit executive compensation substantially. This increase is substantial because the unconditional probability of such a proposal is 6.0% per firm-year in the sample we use in Table 8.

We interpret this finding as being inconsistent with the *quality hypothesis* and the *network size hypothesis*: if seemingly unexplained compensation is a legitimate reward for a CEO's quality or his or her valuable network, then higher levels of unexplained compensation should lead to fewer—not more—shareholder-initiated proposals to limit executive compensation for firms with relatively high levels of connected ownership.

6. Discussion and Conclusion

In this article, our strongest and most robust finding is that educationally connected funds are much more likely to vote against shareholder-initiated proposals to reduce executive compensation than their counterpart funds that are not in the firm's educational network. This differential voting propensity is especially strong when some funds vote differently than other funds in the same fund family voting on the same proposal—the effect on voting propensities of being in-network is six times stronger when there is a voting disparity than when there is unanimity among funds in the family. Furthermore, we show that CEOs in companies with high levels of educationally connected ownership have significantly higher compensation than firms without educationally connected ownership.

We discuss three possible hypotheses (*information*, *quality*, and *network size*) that may explain these findings. The *information hypothesis* suggests that higher executive compensation should be positively related to the abnormal trading performance of educationally connected funds. The *quality hypothesis* suggests that individuals with (perceived or actual) higher ability should both command higher compensation and have more connected ownership. Finally, the *network size hypothesis* suggests that CEOs should and do receive higher compensation for being connected to a large valuable network. Our evidence from the determinants of executive compensation is consistent with both the *information* and *quality hypotheses*. Our evidence for the *network size hypothesis* is mixed.

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