Executive Networks and Firm Policies: Evidence from the Random Assignment of MBA Peers

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Using the historical random assignment of MBA students to sections at Harvard Business School (HBS), I explore how executive peer networks can affect managerial decision making. Within an HBS class, firm outcomes are significantly more similar among graduates from the same section than among graduates from different sections, with the strongest effects in executive compensation and acquisitions strategy. I demonstrate the role of ongoing social interactions by showing that peer effects are more than twice as strong in the year following staggered alumni reunions. Supplementary tests suggest that peer influence can operate in ways that do not contribute to firm productivity. (*JEL* D71, M12, G34)

Recent empirical findings suggest that executives hold significant discretionary powers over a range of firm policies, including the allocation of capital through merger and acquisition (M&A) activities and even the design of their own compensation schemes (Bertrand and Schoar 2003; Frank and Goyal 2007; Bennedsen, Perez-Gonzalez, and Wolfenzon 2010; Graham, Li, and Qiu 2012). If executives can indeed meaningfully affect firm policies, how do they go about making decisions? An emerging strand of literature answers this question by looking at the relationship between corporate outcomes and executive personal characteristics, such as optimism, risk aversion, ability, and resoluteness (e.g., Malmendier and Tate 2008; Graham, Campbell, and Puri 2012; Kaplan, Klebanov, and Sorensen 2012). However, executives are extremely networked and social agents. In addition to being guided by their own

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innate preferences and beliefs, executives are likely to be strongly influenced by their social experiences. Studying executive social networks has the potential to add to our understanding of the determinants of managerial decision making. Meaningful social influence among executives would imply that executives matter for corporate outcomes in systematic and predictable ways that can lead to correlated behavior across firms.

Peer interactions could affect managerial decision making because information and beliefs travel through social networks. These word-of-mouth effects have also been highlighted in theoretical work by Ellison and Fudenberg (1995) and DeMarzo, Vayanos, and Zwiebel (2003) and shown in financial contexts by Davis and Greve (1997), Hong, Kubik, and Stein (2005), and Cohen, Frazzini, and Malloy (2008). Alternatively, peer interactions may induce executives to "keep up with the Joneses" in terms of compensation and acquisitions. For example, Frank (1985), Luttmer (2005), and Card et al. (2012) show in general contexts that individuals value relative earnings, whereas Goel and Thakor (2010) develop a model of envy-motivated mergers.

Estimation of peer effects faces the twin identification challenges of selection and common shocks (Manski 1993). Selection occurs when executives with similar unobserved characteristics select into peer groups. Common shocks occur when group members, by virtue of their association, experience group-level unobserved shocks.

This paper identifies the causal effect of peers on executive decision making using a natural experiment involving randomly assigned peer groups and shocks to those peers over time. Starting with the class of 1949, Harvard Business School (HBS) began randomly assigning all entering MBA students to sections.[1] I refer to students who graduated from the same section in the same class year as section peers and students who graduated from different sections in the same class year as class peers. HBS attempts to foster strong and long-lasting social bonds among section peers—all first-year students take the same nonelective curriculum with their section peers and sections remain organizational focal points during alumni reunions and contribution campaigns. Although many unobserved selective forces may affect the composition of each HBS class, within that class, randomized treatment determines whether any two students are section peers or class peers. To control for any remaining bias from section-level common shocks, I also examine changes in executive behavior following shocks over time to peer outcomes as well as shocks to the strength of peer bonds.

I follow the subset of HBS alumni who become top executives at S&P 1500 companies. HBS provides an ideal empirical setting because it has historically been a major producer of executives, accounting for over 6% of all executives in

[1] HBS attempts to create sections that are balanced in terms of gender, marital status, undergraduate institution, and previous industry experience. Appendix A.1 shows that balanced section assignment does not pose a problem for the empirical strategy, because it creates a small bias against findings of positive peer effects.

the ExecuComp database. In addition, randomized HBS executive peer groups offer an identified laboratory in which to study peer influence among executives more generally, for example, peers connected through common industries, regions, board relationships, and trade associations. This paper's use of the HBS empirical setting builds upon previous work by Lerner and Malmendier (2012), who use HBS sections to identify peer effects in the decision to become an entrepreneur. Aside from focusing on executive decision making rather than entrepreneurship, this paper also uses a longer historical panel of HBS alumni data, which necessitates a different empirical approach from that used in Lerner and Malmendier (2012; discussed in detail in Section 1.1).

I explore the impact of peer interactions on a variety of firm policies, including executive compensation, acquisitions strategy, investment, and financial policy. Whereas existing work has linked executive networks to firm policies (e.g., Bizjak, Lemmon, and Whitby 2005; Fracassi 2012; Hwang and Kim 2009; Barnea and Guedj 2010; Ishii and Xuan 2010; Leary and Roberts 2012; Butler and Gurun 2012; Fracassi and Tate 2012), most of the literature has focused on institutionalized links, such as connections between the CEO and board members, industry bonds, or overlapping board membership, the strength of which may be enhanced through common educational backgrounds. In contrast, I focus on the noninstitutionalized, but potentially powerful, social bonds among executives in firms that generally lack formal linkages. In addition, most of the financial networks literature[2] does not use randomly assigned peer groups. This paper contributes to the literature by using random assignment to establish causality and also by exploring the mechanisms and timing behind peer influence.

Peer influence can occur if individuals react to the mean of group behavior, follow group leaders, or adopt a group norm. Regardless of exactly how peer influence occurs, we expect that section-based peer interactions will lead section peers to become more similar than class peers.[3] I find the strongest evidence of section peer similarities with respect to executive compensation and acquisitions. Relative to class peers, section peers receive significantly more similar compensation and are more likely to pursue similar acquisitions strategies. The variation in compensation and levels of acquisition activity among section peers is around 10% less than the variation among class peers. Under further structural assumptions developed in a linear-in-means model of social interactions, I estimate a lower bound for the elasticity of the individual response to mean section peer characteristics of 10%–20%. Whereas the effect sizes are largest for compensation and acquisitions, I also find evidence of

[2] For example, Hallock (1997), Kirchmaier and Stathopoulos (2008), Hwang and Kim (2009), Barnea and Guedj (2010), and Engelberg, Gao, and Parsons (2013) find that the quality and size of an executive's network are predictive of compensation and firm performance.

[3] Peer influence can also lead to a decrease in group similarity if individuals seek to become outliers. The empirical model and analysis will allow for both types of peer effects.

significant peer effects in investment, leverage, interest coverage, and cash policy.

To investigate underlying mechanisms and rule out potential biases, I begin by differentiating between "contemporaneous" and "past" social interactions. Establishing the timing of peer interactions is important because the analysis focuses on the subset of HBS graduates who become top executives. Given the initial random assignment of students to sections, selection into this executive subsample and/or into similar types of firms can be an important peer effect operating through past interactions. I find that past interactions are indeed important determinants of executive careers—relative to class peers, section peers are around 20% more likely to choose the same industries and geographical locations. In contrast to past interactions that can affect early career trajectories, contemporaneous interactions describe ongoing interactions that occur while executives manage firms and are more informative of the impact of executive networks on firm policies.

I establish the importance of contemporaneous interactions using the natural experiment of HBS alumni reunions, which occur every five years after each executive's graduation year. Staggered reunions introduce exogenously timed shocks to the strength of peer bonds. Because reunions cover the same time period as firm outcome measures, reunions should only affect estimates if contemporaneous interactions drive peer similarities. I find that peer similarities in compensation and acquisitions are more than twice as strong in the year immediately following reunions relative to other years.

Evidence of contemporaneous interactions show that the results are not driven by bias from section-specific common shocks, for example, a professor who indoctrinates her section with a particular management philosophy. Influential professors are unlikely to generate peer similarities that vary according to the staggered reunion schedule. The marginal increase in peer similarities following reunions serves as a lower bound for true peer effects. Evidence of contemporaneous interactions also illustrates the very persistent nature of peer influence; peer groups formed in business school affect executive decision making several decades after graduation.

Using an additional test of "pay for friend's luck" that builds upon the methodology in Bertrand and Mullainathan (2001), I find that executive compensation responds to lucky shocks to the pay of peers. Here, "lucky pay" is defined as the part of each executive's compensation that can be predicted using her mean industry returns (over which she has, arguably, minimal impact). The analysis is restricted to peers working in distant industries to reduce the likelihood that shocks to peers in different industries will have significant direct unobserved effects on executives. I find that individual changes in compensation are significantly more responsive to section peers' lucky pay than to class peers' lucky pay, even after the introduction of numerous controls for own firm and industry performance. Like reunions, lucky industry shocks occur in the same time period as executive outcomes and highlight the importance of contemporaneous interactions. Evidence of pay for friend's luck further offers a check on bias from common shocks (e.g., professors) that are unlikely to generate behavior that varies over time with lucky shocks to peers.

The channels through which peer influence operate can further be divided into two broad categories: reactions to peer fundamentals and reactions to peer outcomes. Reactions to peer fundamentals occur if the fundamental skills, beliefs, or information driving managerial decisions are transferred through networks. For example, compensation may be similar because executives transfer managerial skills to one another, leading to similar levels of productivity that then lead to similar compensation. In contrast, reactions to peer outcomes can occur if executives respond directly to the actions of peers, for example, if executives seek to match or exceed friends' compensation or acquisition levels or if a change in peers' compensation affects executives' outside options. Distinguishing between reactions to fundamentals and outcomes is important for our understanding of policy interventions, for example, industry-level executive pay caps or antitakeover regulations, which affect the actions of peers while leaving their fundamentals unchanged. For these policy interventions, only peer influence operating through reactions to peer outcomes will generate a peer multiplier (Glaeser, Sacerdote, and Scheinkman 2003). Further, reactions to fundamentals can improve firm productivity if information that is useful for the firm is passed through executive networks. In contrast, reactions to peer outcomes may weakly lower firm productivity (e.g., if executives blindly mimic each other's acquisitions activity).

Most likely, both forces are present, and it is not the goal of this paper to claim that peer effects operate only through one channel. Moreover, the empirical tests cannot rule out that reactions to peer fundamentals, for example, information sharing, are important drivers of peer influence. However, a variety of tests suggest that peer influence also operates through reactions to peer outcomes in ways that do not contribute to firm productivity. Evidence of pay for friend's luck suggests that peer similarities in compensation are partly driven by direct reactions to peer compensation rather than by the sharing of underlying fundamentals such managerial skills or insight. This can occur if relative income directly enters into each executive's utility function or if peer's lucky shocks alter the outside options of executives. I also present exploratory evidence showing that peer similarities in acquisitions can operate through channels other than socially optimal information sharing.

Finally, this paper demonstrates the aggregate implications of executive social interactions. Strong social interactions imply a large peer multiplier: The aggregate response to a change in the fundamental determinants of compensation or acquisition activity will be larger than the direct response because of contagion among connected agents (Glaeser, Sacerdote, and Scheinkman 2003). I estimate a substantial peer multiplier of 10%–20%, for example, the aggregate increase in acquisition activity following a fundamental shock may be 10%–20% larger than any individual firm's direct response to the shock. Peer influence can also contribute to clustered financial activity. Differences in fundamentals across groups or over time can be amplified through peer interactions. Along these lines, Demarzo, Kaniel, and Kremer (2007) and Goel and Thakor (2010) present herding models of financial bubbles and merger waves. This paper explicitly shows that peer influence leads to strongly clustered activity among section peers using the natural experiment of HBS sections.

1. Data

1.1 Section assignment at Harvard Business School: 1949–present

Starting with the class of 1949, HBS began assigning all entering MBA students to sections of roughly ninety students. Section assignment continues into the present day. Initially, there were seven sections: A, B, C, D, E, F, and G; sections H, I, and J were added over time. Sections foster close social bonds that last well beyond graduation. Section members take the complete nonelective first-year sequence of courses together in the same classroom. In the second year, students take elective courses, so classrooms contain a mix of students from different sections. However, athletic competitions and student organizations remain organized around sections. After graduation, reunions and alumni contribution campaigns are similarly organized by sections. A detailed summary of the sociological research documenting section-based peer bonds can be found in the Appendix.

Section assignment is random conditional on student characteristics observed by the HBS administration. HBS balances sections on the following observables: race, ethnicity, nationality, industry background, undergraduate institution, geographical origin, and marital status. Balanced section assignment does not pose a problem for the validity of the empirical methodology. In the absence of peer effects, balanced sectioning implies that two randomly selected class peers are actually more likely to have similar characteristics than two section peers. In Appendix A.1, I formalize the intuition that balanced assignment generates a small bias against findings of positive peer effects.

The empirical context of HBS sections used in this paper is most similar to the context used in Lerner and Malmendier (2012), hereafter referred to as LM. LM measure the effect of entrepreneurial peers on subsequent entrepreneurship rates. This paper's methodology differs from LM in three ways. First, this paper uses a longer historical sample (alumni records from 1949 to 2008) whereas LM's sample spans 1997 to 2004. Second, LM identify peer effects using the cross-sectional relationship between peer ex ante entrepreneurship experience and an individual's ex post decision to become an entrepreneur. In contrast, this paper uses data that contains detailed panel coverage of individual ex post outcomes. However, the data contains limited coverage of individual ex ante characteristics (due to the nature of the longer historical sample). Therefore, this paper identifies peer effects using the distribution of ex post outcomes within and across sections for each class year and tests how the distribution of ex post outcomes changes with shocks to the strength of peer bonds over time (e.g., reunions). Third, LM focus on entrepreneurial decisions shortly after graduation, whereas this paper explores how peers affect executive decision making and firm policies several decades after graduation.

1.2 Executive data

The data begins with HBS alumni records from 1949 to 2008, which are matched to the ExecuComp database that covers the compensation of top executives at S&P 1500 firms from 1992 to 2009. Acquisitions data comes from SDC Platinum. Biographical and employment history data comes from ExecuComp and is supplemented with data from BoardEx. Firm measures come from CompuStat and the Center for Research in Security Prices (CRSP), whereas industry returns come from CRSP and the Kenneth French Data Library. Observations are at the executive × year level, where year corresponds to the fiscal year of the executive's firm.

Table 1, Panel (A), summarizes the data coverage of HBS MBA alumni who become top executives in S&P 1500 firms as reported in ExecuComp. There are 596 CEOs/CFOs and 1,051 top executives (inclusive of CEOs and CFOs), resulting in 3,071 CEO/CFO × year and 6,413 top executive × year observations. The median CEO/CFO has 2 section peers (same section and same class year) and 14 class peers (same class year, different sections), excluding herself, whereas the median top executive has 3 section peers and 25 class peers. Note that while the alumni records cover all HBS MBA graduates from 1949 to the present, over 90% of the executive subsample graduated between 1960 and 1990 (this is because ExecuComp covers executives from 1992–2009 with a median age of 58).

Panel (B) summarizes compensation, demographics, and firm policies for both the HBS and complete sample of CEOs/CFOs in ExecuComp. Although non-HBS CEOs/CFOs are not directly relevant for the analysis, their outcomes will be used in some specifications to control for industry and time trends. Executive compensation data takes two forms: Direct compensation is the sum of salary and bonus, and total compensation is the sum of direct and equity-linked compensation (restricted stock grants and the grant-date Black-Scholes value of option grants and long-term incentive plans).[4]

Acquisition policy data comes from the SDC database. I focus on measures of attempted acquisitions because they provide evidence of executives' intentions to acquire even if the acquisitions ultimately fail (e.g., due to takeover defenses or regulatory barriers). However, the results are very similar using completed acquisitions. All acquisitions are assigned to years according to the date of the initial merger announcement. Acquisitions are common occurrences—the

[4] A change in SEC compensation disclosure rules went into effect in 2006. For continuity, I use the ExecuComp measures of equity-related compensation that follow the 1992 reporting format for all years. Table 8 presents evidence that results are robust to the 2006 change in reporting standards.

Table 1
Summary statistics

Panel A: HBS executives

	HBS CEOs/CFOs			HBS all top earners		
	Mean	Median	SD	Mean	Median	SD
Number of executives	596			1,051		
Observations (executive × year)	3,071			6,413		
Number section peers per executive (excl. self)	1.879	2	1.376	3.153	3	1.877
Number class peers per executive (excl. self)	13.755	14	5.530	23.810	25	8.699

Panel B: Executive and firm variables

	HBS CEOs/CFOs			All CEOs/CFOs		
	Mean	Median	SD	Mean	Median	SD
Salary \$K	566	501	301	493	414	287
Bonus \$K	805	424	1,065	577	297	847
Direct compensation \$K	1,168	806	1,104	906	619	901
Total compensation \$K	3,690	1,930	4,796	2,628	1,259	3,882
Annual percent change in direct compensation (%)	14.1	5.8	60.4	12.9	5.9	63.3
Annual percent change in total compensation (%)	44.8	5.4	275.9	42.6	5.8	245.4
Percent female (%)	1.9			0.1		
Age	57.5	58	9.2	58.4	58	9.4
Firm tenure (years)	9.6	6	9.9	12.3	8	11.4
Fiscal year firm return	0.198	0.064	3.503	0.609	0.062	29.942
Fiscal year SIC3 industry return	0.216	0.175	0.398	0.217	0.179	0.423
Sales \$M	6,513	1,462	18,142	4,588	1,107	14,257
Number of attempted acquisitions	1.56	1	3.01	1.28	0	2.59
Number of completed acquisitions	1.24	0	2.38	1.06	0	2.21
Fraction attempted at least one acquisition	0.55			0.48		
Fraction completed at least one acquisition	0.50			0.43		
Value of completed acquisitions \$M	1,005	140	3,359	897	121	3,910
Observations (executive × year)	3,071			55,630		

Panel (A) describes the HBS executive sample of MBA alumni who graduated from 1949 to 2008 and are also covered in ExecuComp, which covers executives in S&P 1500 firms from 1992 to 2008. An executive is included in the CEOs/CFOs subsample for a given year if (1) the ExecuComp ceoann or cfoann markers are flagged, (2) her title indicates that she is a CEO or CFO and her annual total compensation rank is greater than or equal to 5 (to exclude regional or divisional CEOs and CFOs), or (3) her annual total compensation rank is equal to one and there are no other identified CEOs for her firm that year. The All Top Earners sample includes all HBS MBA alumni covered in ExecuComp and includes the CEOs/CFOs sample. All results in later tables refer to the CEOs/CFOs sample unless otherwise noted. All counts of the number of section and class peers exclude the individual herself. Panel (B) describes executive and firm characteristics. Compensation data comes from ExecuComp. *Direct compensation* is the sum of *salary* and *bonus*. *Total compensation* is the sum of *direct compensation*, value of restricted stock grants, and the Black-Scholes value of options granted and long-term incentive plans. *Direct compensation* and *total compensation* are summarized using observations winsorized at the 1% level of both tails. *Percent female*, *age*, and *firm tenure* data come from ExecuComp and are supplemented, if missing, with data from BoardEx. *Firm* and *industry returns* are matched to firm fiscal year month end dates and come from CRSP. Completed acquisitions are documented successful acquisitions in which acquiring firms gained 50% or greater stakes in the acquired entities. *Attempted acquisitions* include any recorded acquisition in the SDC database and is inclusive of completed acquisitions. Acquisitions that are not noted as complete may represent failed acquisition attempts or incomplete reporting in the SDC data.

median HBS CEO/CFO attempts at least one acquisition per year. These acquisitions represent significant changes in firms' allocations of capital, with median and mean values of over \$100 million and \$1 billion, respectively.

2. Empirical Methodology

Peer influence can operate in a variety of ways. Individuals may react to the mean, follow leaders, or develop a common norm. Regardless of the exact form of social interaction, most models of peer effects predict that section peers will have more similar outcomes than class peers.[5] Therefore, I begin by presenting two reduced-form measures of the extent to which section peers are more similar to one another than class peers. Then, to interpret the economic magnitudes of peer influence given that reflection may occur if one peer affects another and vice versa ad infinitum, I develop a model that assumes that individuals react to the mean characteristics of their peer groups. The model builds on existing linear-in-means models developed in Graham (2008) and Glaeser and Scheinkman (2001), with extensions as noted. For brevity, estimation of significance levels is left for Appendix A.3.

2.1 The pairs distance metric

The pairs distance metric measures whether the mean absolute distance in outcomes between two section peers is less than the distance between two class peers. Estimation follows a two-stage procedure similar to that used in Fracassi (2012).[6]

$$\text{1st Stage}: Y_{it} = a_0 + a_1 X_{it} + \widetilde{Y}_{it}, \tag{1a}$$

$$\text{2nd Stage - Levels}: \left|\widetilde{Y}_{it} - \widetilde{Y}_{jt}\right| = \beta_0 + \beta_1 \cdot I_{ij}^{\text{section peers}} + \varepsilon_{ijt}, \tag{1b}$$

$$\text{2nd Stage - Changes}: \left|\left(\widetilde{Y}_{it} - \widetilde{Y}_{i,t-1}\right) - \left(\widetilde{Y}_{jt} - \widetilde{Y}_{j,t-1}\right)\right| = \beta_0 + \beta_1 I_{ij}^{\text{section peers}} + \varepsilon_{ijt}. \tag{1c}$$

Observations in the first stage are at the executive i × firm fiscal year t level. The outcome of interest Y_{it} is regressed on X_{it}, which can consist of individual, firm, industry, and time controls. The residuals $\widetilde{Y}_{it}$ measure the unexplained component of Y_{it} and are used in the second stage. The purpose of the first stage is to allow estimation of "excess" peer influence, for example, peer similarities in compensation beyond what can be explained by observable selection into similar firms and industries (selection into similar firms can be a true peer effect operating through past interactions; tests of "excess" peer influence help to narrow the mechanism through which peer effects operate).

In the second stage, I create all possible pairs of executives who graduated in the same class year from HBS and exist in the same firm fiscal year. Each observation is a pair of executives in a given fiscal year. If we are interested in peer similarities in levels of outcomes, the dependent variable is the absolute pair difference in first-stage residuals $\widetilde{Y}_{it}$. Alternatively, if we are interested in peer similarities in changes in outcomes, the dependent variable is the absolute

[5] Of course, social interactions can also lead to dissimilarity among group members if, for example, individuals choose to be mavericks. Both the reduced-form estimates and model will also allow for this type of peer effect.

[6] The pairs distance metric used in this paper differs from Fracassi (2012) because it uses a random assignment setting to regress pairs distance on a dummy for whether peers are in the same peer group and adopts a linear-in-means framework to estimate an implied peer elasticity.

pair difference in changes in the first-stage residual $\left(\widetilde{Y}_{it}-\widetilde{Y}_{i,t-1}\right)$. The pair absolute distance is then regressed on $I_{ij}^{\text{section peers}}$, a dummy for whether i and j are section peers (same section in the same class year).

The identifying assumption is straightforward: Whether a pair of executives graduating in the same class year are section peers or class peers is exogenously determined by random section assignment. An informative reduced-form statistic is the distance ratio δ^{PDM} equal to the fractional difference in the expected distance between a pair of section peers and a pair of class peers:

$$\text{Distance Ratio } \delta^{PDM} \equiv 1-\frac{E\left[\left|Y_{isct}-Y_{jsct}\right|\right]}{E\left[\left|Y_{isct}-Y_{js'ct}\right|\right]}, \tag{2}$$

$$\widehat{\delta}^{PDM} \equiv -\frac{\beta_1}{\beta_0}. \tag{3}$$

A $\widehat{\delta}^{PDM}$ significantly greater than zero is evidence of positive peer effects. For example, $\widehat{\delta}^{PDM}$ equal to 0.10 implies that section peers are 10% more similar than class peers.

2.2 The excess variance metric

Peer influence will tend to reduce the variance of outcomes within peer groups relative to the variance across groups. The excess variance metric offers a reduced-form measure of the extent to which the across-section variance exceeds the between-section variance in each class year. Estimation follows an ANOVA framework. The variance decomposition can be applied to raw outcomes Y_{isct} or residual outcomes $\widetilde{Y}_{isct}$, as described in Equation (1a). The scaled within- and between-section sum of squares are defined as follows, where m_{sct} is the number of observations in a section $\times$ fiscal firm year:

$$SS_{sct}^{W} \equiv \frac{1}{m_{sct}(m_{sct}-1)}\sum_{i=1}^{m_{sct}}\left(Y_{isct}-\overline{Y}_{sct}\right)^2, \tag{4}$$

$$SS_{sct}^{B} \equiv \left(\overline{Y}_{sct}-\overline{Y}_{ct}\right)^2. \tag{5}$$

An informative reduced-form statistic is the excess variance ratio, defined as the ratio of the between-section sum of squares to the within-section sum of squares:

$$\text{Excess Variance Ratio } \delta^{EVM} \equiv \frac{E\left[SS_{sct}^{B}\right]}{E\left[SS_{sct}^{W}\right]}-1. \tag{6}$$

To limit the bias from outliers, I impose the restriction that peer effects are equal across class and firm years and estimate $E\left[SS_{sct}^{B}\right]$ and $E\left[SS_{sct}^{W}\right]$ using the full sample before forming the ratio. Under the null hypothesis that section divisions do not matter (random assignment and no peer effects), the expected excess variance ratio δ^{EVM} is zero. Therefore, a δ^{EVM} equal to 0.3 implies that the ratio of between- and within-section variances is 30% greater than expected under the null.

2.3 A linear-in-means model of peer influence

This section presents an empirical model in which individuals react to the mean characteristics of their peers. I then combine the model with the two reduced-form metrics, discussed above, to estimate model parameters. Consider individual i in section s in class year c. For simplicity, restrict attention to a single class year c and single firm year. Assume, conservatively, that across-section peer interactions are zero. Individual outcomes Y_{isc} are represented by the following linear function:

$$Y_{isc} = \theta \overline{Y}_{sc} + \phi \overline{v}_{sc} + \alpha_{sc} + \rho v_{isc}. \tag{7}$$

Exogenous student-level fundamentals (e.g., ex ante managerial skills or private information) that affect outcomes are represented by v_{isc}. Following Graham (2008), I allow for two types of peer effects: Responses θ to mean group outcomes $\overline{Y}_{sc}$ and responses ϕ to mean group fundamentals $\overline{v}_{sc}$ (labeled as endogenous/reflective and contextual/exogenous peer effects, respectively, by Manski 1993). Responses $\theta \in (-1,1)$ to mean group outcomes occur when individuals react directly to $\overline{Y}_{sc}$, that is, if peers' compensation or acquisitions outcomes directly impact individual compensation or acquisitions. Responses ϕ to mean group fundamentals occur when individuals react to $\overline{v}_{sc}$: Responses to fundamentals might represent transfers of fundamental skills or attitudes among peers (Ahern, Duchin, and Shumway 2012). Both responses to peer outcomes and fundamentals are true peer effects. However, distinguishing between the two becomes important when evaluating policy interventions that limit outcomes $\overline{Y}_{sc}$, while leaving fundamentals $\overline{v}_{sc}$ unchanged. For such shocks, only responses to peer outcomes will generate a peer multiplier.

α_{sc} (scaled to have mean zero, without loss of generality) represents section-specific common shocks, such as a professor who affects the outcomes of all students in her section. Following the intuition in herding cascade models (e.g., Banerjee 1992; Bikhchandani, Hirshleifer, and Welch 1992), $\rho \in (0,1]$ represents the extent to which peer interactions lead individuals to underweight their own fundamentals. Note that, in the absence of both common shocks ($\alpha_{sc}=0$) and peer influence ($\rho=1, \theta=\phi=0$), individual outcomes are determined by individual fundamentals: $Y_{isc}=v_{isc}$.

Averaging over individuals in the same group yields the average of group outcomes:

$$\overline{Y}_{sc} = \frac{\alpha_{sc}}{1-\theta} + \frac{\phi+\rho}{1-\theta}\overline{v}_{sc}. \tag{8}$$

For now, assume that $\alpha_{sc}=0$. In later sections, I present evidence against common shocks bias.

Using Equations (7) and (8), the individual optimal outcome can be expressed as a simple linear-in-means function of own and group fundamentals:

$$Y_{isc} = \tau \overline{v}_{sc} + \rho v_{isc}, \quad \tau \equiv \frac{\phi+\theta\rho}{1-\theta}. \tag{9}$$

τ represents the effect of changes in mean group fundamentals $\overline{v}_{sc}$ on individual outcomes Y_{isc}. Note that a change in $\overline{v}_{sc}$ can affect Y_{isc} in two ways: (1)

if individual outcomes directly respond to peer fundamentals through the ϕ channel or (2) if individual outcomes respond through the θ channel to mean peer outcomes, which in turn respond to changes in own fundamentals through the ρ channel.

Because θ, ϕ, and ρ all represent peer effects, I take an approach that is common in the literature and estimate a joint peer influence parameter:

$$\text{Peer Elasticity} \quad \gamma \equiv \frac{\tau}{\rho}. \tag{10}$$

γ represents the elasticity of the individual response to a unit change in mean group fundamentals $\overline{v}_{sc}$, scaled by the elasticity of the individual response to a unit change in own fundamentals. For example, $\gamma = 0.20$ implies that the response to a change in mean group fundamentals will be 20% as large as the response to a similarly sized change in own fundamentals. $\gamma > 0$ implies positive peer effects, but peer effects are not restricted to be positive. Although γ is a joint measure of peer influence, later tests will distinguish between responses to outcomes θ and responses to fundamentals ϕ.

2.3.1 Multipliers in levels and variance. A useful and important implication of the linear-in-means model is that social interactions will lead to a multiplier in the aggregate levels of outcomes. Using Equation (9), we find that $(1+\gamma)$ is the multiplier, equal to the ratio of the full effect to direct effect of a change in the mean fundamental determinants of behavior $\overline{v}_{sc}$. If peer effects are positive ($\gamma > 0$), the aggregate response to a change in mean fundamentals will be larger than the direct effect by γ, the peer elasticity.

A second implication is that social interactions will increase the variation across groups relative to the variation within groups. Equation (9) implies that the ratio of the variance of mean outcomes across groups to the variance of outcomes within groups (scaled by group size m) is increasing in γ.

$$\text{Between Section Variance} = Var\left(\overline{Y}_{sc}\right) = (\tau+\rho)^2 Var(\overline{v}_{sc}), \tag{11a}$$

$$\text{Within Section Variance} = Var(Y_{isc}|s) = \rho^2 Var(v_{isc}|s), \tag{11b}$$

$$\text{Variance Ratio} = \frac{m \cdot Var\left(\overline{Y}_{sc}\right)}{Var(Y_{isc}|s)} = (1+\gamma)^2 \frac{m \cdot Var(\overline{v}_{sc})}{Var(v_{isc}|s)}. \tag{11c}$$

Under random assignment to peer groups, the ratio of the scaled between- to within-section variance of fundamentals is equal to unity ($m \cdot Var(\overline{v}_{sc})/Var(v_{isc}|s) = 1$), so the variance ratio reduces to $(1+\gamma)^2$. It is now clear how peer interactions contribute to clustered activity. Outcomes are clustered when the variation across groups is large relative to the variation within groups. Under random assignment, peer influence increases the variance ratio from unity to $(1+\gamma)^2$. If groups are not randomly assigned, peer influence will amplify existing differences across groups by $(1+\gamma)^2$.

2.3.2 Recovering model parameters from reduced form estimates. We cannot estimate the model directly because individual fundamentals v_{isc} are unobserved. However, we can recover model parameters using Equation (11c) and the pairs distance and excess variance metrics. Under random assignment to sections, it is sufficient to measure the extent to which outcomes Y_{isc} are more similar within sections than across sections among graduates of the same class year.

Starting with the distance ratio $\widehat{\delta}^{PDM}$, we know that random assignment implies that fundamentals v_{isc} are distributed *iid* with variance σ_v^2 within a class year. Let m equal the peer group size. To derive analytic solutions, I impose an additional normality assumption that $v_{isc} \sim N\left(\mu, \sigma_v^2\right)$. This assumption will be relaxed in the next section. Using Equation (9), the expected absolute distance between two section peers and two class peers is[7]

$$E\left[\left|Y_{isct}-Y_{jsct}\right|\right]=\rho\sigma_v\frac{2}{\sqrt{\pi}}, \quad i\neq j, \tag{12a}$$

$$E\left[\left|Y_{isct}-Y_{js'ct}\right|\right]=\left\{\frac{1}{m}\left((\gamma+1)^2-1\right)+1\right\}^{1/2}\rho\sigma_v\frac{2}{\sqrt{\pi}}, \quad i\neq j \text{ and } s\neq s'. \tag{12b}$$

By rearranging Equations (12a) and (12b), γ can be expressed as a function of the distance ratio:

$$\gamma=\left\{m\left[\left(\frac{1}{1-\delta^{PDM}}\right)^2-1\right]+1\right\}^{1/2}-1. \tag{13}$$

Note that γ is strictly increasing in m, equal to the number of individuals in the section peer group that each individual responds to (m does not necessarily equal the number of executives in the data). For empirical estimates, I assume a conservative value of $m=2$. This assumption reflects the fact that, on average, only three students per section become CEOs or CFOs who appear in ExecuComp.[8]

Adopting the linear-in-means model, the excess variance ratio δ^{EVM} also offers an estimate of the peer elasticity γ. Using the fact that $E\left[SS_{sct}^B\right]=Var\left(\overline{Y}_{sc}\right)$

[7] The solution uses the following result for the folded normal distribution: If $X\sim N\left(\mu,\sigma^2\right)$, then $E[|X|]=\sigma\sqrt{\frac{2}{\pi}}\exp\left(-\frac{\mu^2}{2\sigma^2}\right)+\mu\left[1-2\Phi\left(-\frac{\mu}{\sigma}\right)\right]$.

[8] Assuming that group size $m=2$ offers a conservative estimate of the true peer elasticity that does not rely on assumptions for the sampling rate. The true magnitude of the peer elasticity may be larger if there are other HBS graduates in executive roles not covered by ExecuComp. However, recent work, such as Carrell, Fullerton, and West (2009) and Carrell, Sacerdote, and West (2012), show that the linear-in-means framework may not scale well to larger groups in which peer responses become nonlinear and individuals begin to react to subgroups. Therefore, the lower bounds estimated for γ are unlikely to substantially underestimate the true magnitude of peer influence within HBS executive networks.

and $E\left[SS^{W}_{sct}\right]=Var(Y_{isc}|s)/m_{isc}$, Equations (11a) and (11b) imply

$$\gamma=\left(1+\delta^{EVM}\right)^{\frac{1}{2}}-1. \tag{14}$$

There are several trade-offs relevant to the pairs distance and excess variance metrics. The excess variance metric's estimation of γ does not require the assumption of the normality of individual fundamentals v_{isc} as in the case of the pairs distance metric. However, the pairs distance metric is more robust to outliers bias because it relies on absolute distance rather than squared terms. It is also more flexible for subsample tests because it uses pairs instead of the full distribution of outcomes by sections within each class year. Therefore, both metrics are presented in the baseline results and the pairs distance metric is used for extensions that require additional flexibility.

2.3.3 Selection into the executive subsample. Although the model described above applies to peer groups generally, the empirical analysis will focus on the subsample of HBS graduates who appear as top executives at S&P 1500 firms during the period covered by ExecuComp—roughly 4%–6%, respectively, of all alumni. I focus on top executives because their outcomes are well documented and because their actions have large consequences. Differential section-level selection into the executive data can be an important peer effect. Given random section assignment and assuming no common shocks, baseline measures of peer similarities will capture the joint effect of past interactions (peers selecting into the executive data and entering similar firms) and contemporaneous interactions (ongoing interactions among executives). Model extensions that account for selection into the ExecuComp subsample are presented in Appendix A.2 and additional tests in Sections 3.3 and 3.4 will isolate contemporaneous interactions.

3. Results

3.1 Verifying balanced conditionally random section assignment

The empirical model assumes that section assignment is a random lottery. In practice, HBS follows balanced, conditionally random, section assignment. As shown in Appendix A.1, balanced section assignment leads to a small bias against findings of positive peer effects. In this section, I present empirical evidence that HBS practices balanced section assignment. Under balanced section assignment, section peers should not be more similar in terms of ex ante characteristics than class peers. Panel (A.1) of Table 2 tests this assumption for the sample of all HBS MBAs by comparing section and class peer commonalities in terms of citizenship, undergraduate institution, and gender—characteristics that are determined prior to matriculation at HBS. "Section commonalities" and "class commonalities" measure the fraction of section and class peers that share each individual's characteristics. The commonalities

Table 2
Peer group commonalities

Commonality rates	Section	Class	Ratio: Section/class	p-Value t-test	Obs	All HBS executives	All ExecuComp
Panel A.1: Pre-HBS student characteristics: All HBS graduates							
Citizenship	0.7181	0.7192	0.998	0.0000	30,385		
Undergraduate institution	0.0197	0.0205	0.959	0.0000	35,155		
Gender	0.7380	0.7396	0.998	0.0000	42,975		
Panel A.2: Pre-HBS student characteristics: ExecuComp top earners							
Citizenship	0.9520	0.9555	0.996	0.4032	750		
Undergraduate institution	0.0211	0.0220	0.959	0.8022	829		
Gender	0.9224	0.9190	1.004	0.3878	964		
Panel B: Post-HBS executive outcomes: ExecuComp top earners							
Director overlap	0.0586	0.0472	1.233	0.0228	960	0.0447	
Industry SIC3	0.0377	0.0296	1.273	0.0545	960	0.0274	0.0196
Industry Fama-French 49	0.0652	0.0522	1.250	0.0134	960	0.0484	0.0405
State of headquarters	0.0926	0.0843	1.098	0.1563	960	0.0790	0.0653
City of headquarters	0.0269	0.0165	1.627	0.0024	960	0.0151	0.0092
Panel C: Selection into ExecuComp top earners							
ExecuComp dummy	0.9221	0.9220	1.000	0.9705	22,066		

Panels (A.1) and (A.2) test for peer similarities in student characteristics that are determined prior to matriculation at HBS. Panel (A.1) includes all HBS MBA graduates from 1949 to 2008, whereas Panel (A.2) includes all HBS ExecuComp top earners as described in Table 1. Section commonalities and class commonalities measure the fraction of section peers (same section and same class year) and class peers (same class year, different sections), respectively, that have at least one characteristic (e.g., undergraduate institution) in common with the student. The commonalities ratio represents the ratio of section commonalities to class commonalities—ratios significantly less than unity show that section peers are not more similar than class peers in terms of ex ante characteristics and support the argument in Appendix A that balanced section assignment leads to a small bias against findings of positive peer influence. The next column reports the p-value from a paired t-test of the hypothesis that section commonalities is equal to class commonalities. Panel (B) performs similar tests for peer similarities in categorical executive labor market outcomes that occur after graduation from HBS. The sample includes all HBS ExecuComp top earners and observations are at the executive level. Each executive is allowed to have multiple values for each characteristic (e.g. multiple firm affiliations) to allow for changes in employment over time. Individuals without at least one section peer and one class peer are not included in the estimation. Commonalities ratios greater than unity imply that section peers have more similar ex post outcomes than class peers. The two right-most columns present commonalities among all HBS ExecuComp top earners (regardless of class year and section boundaries) and all ExecuComp top executives (firm and director overlap are not measured because employment history is only matched for HBS executives), respectively. Although these latter two columns are useful as a reference for the expected base rates of commonalities, the extent to which class commonalities exceeds the commonalities in the two right-most columns can reflect both peer influence and selection into each HBS class year—for that reason, the analysis focuses on the difference between section commonalities and class commonalities. Panel (C) examines section and class commonalities in terms of selection into the ExecuComp subsample. The sample is restricted to HBS graduates from class years in which at least twenty alumni per class year appear in ExecuComp. Results are similar using the full sample of HBS graduates.

ratio is the ratio of section to class commonalities. I find that all commonalities ratios are slightly but significantly less than one. This empirically supports the argument in Appendix A.1 that balanced section assignment presents a small bias against findings of positive peer effects.

Panel (A.2) presents the same ex ante commonalities measures for the executive subsample covering HBS graduates who appear as top earners in ExecuComp. The commonalities ratios are again very close to one, although a commonalities ratio greater than one within the executive subsample would not be evidence against balanced section assignment; it could represent true peer effects that led similar types of MBA graduates to select into the executive subsample (see Appendix A.2).

3.2 Baseline measures of peer influence

Panel (B) of Table 2 estimates peer commonalities in executive labor market outcomes *after* graduation. Ex post measures include director overlap, industry choice, and the location of firm headquarters. The sample includes all HBS top executives who appear in ExecuComp. Commonalities are measured over the known employment history of each executive and are not restricted to concurrent overlap. These ex post commonalities tell a sharply different story relative to the ex ante measures.

The overall base rates of commonalities are low, implying that HBS executives are spread across a variety of firms, industries, and regions. However, commonalities are sharply more likely to occur among section peers than among class peers. Section peers are 23% more likely to be employed in firms with overlapping directors (defined as a director employed by firms in the employment history of both peers).[9] Section peers are also about 25% more likely to overlap in industry affiliation and 10% and 60% more likely to overlap in firm headquarter state and city locations, respectively.

The section and class commonalities in Panel (B) can further be compared to base rates of commonalities among all HBS executive graduates (regardless of class or section divisions) and all ExecuComp executives, presented in the right-most two columns. Class commonalities are around 20% greater than the base rates presented in these last two columns, suggesting that substantial peer effects may also operate among class peers. However, the extent to which class commonalities exceed base rate commonalities may also be due to selection into each HBS class year and changes in curriculum over time. Therefore, this paper identifies a lower bound for peer effects using the marginal increase in commonalities among section peers relative to class peers.

Panel (C) tests for peer influence in terms of selection into the ExecuComp subsample, for example, if peers help each other attain high-level management positions. I find that section peers are not more similar in terms of appearing in ExecuComp relative to class peers. However, the outcome measure is an indicator for whether an HBS graduate appears in ExecuComp, which may be a noisy approximation for what it means to be an "executive" in the more general sense. When outcomes are measured with error, estimates of peer effects using the distribution of outcomes within and across peer groups will biased toward zero (Graham 2008). Therefore, data limitations prevent strong conclusions about peer effects in the selection into becoming executives.

9 Excluding observations corresponding to firms with overlapping board members does not significantly change estimated peer effects in executive compensation or acquisitions behavior, implying that these peer similarities are not driven by peer effects leading to director overlaps.

However, conditional on appearing in ExecuComp, firm outcomes are well measured and allow for the tests presented in later tables.

These results demonstrate that peers can significantly affect career trajectories. Because firm, industry, and location choices occur before executives begin making firm policy decisions, these results also illustrate the importance of "past" social interactions in determining executive career paths.

Next, I explore peer influence with respect to the following firm policies: executive compensation, acquisitions strategy, investment, financial policy (leverage, dividends, interest coverage, and cash holdings), and firm size. Table 3 presents baseline estimates of peer effects in executive compensation and acquisitions activity. In this and future tables, I restrict the sample to CEOs/CFOs, because CEOs/CFOs have greater control over firm outcomes than do other top executives. Results for the full set of top earners in the ExecuComp database are discussed in the Appendix.

Executive compensation can take many forms. For completeness, results are presented for the logs of direct compensation (sum of salary and bonus) and total compensation (sum of direct- and equity-linked compensation). Further, it is not obvious whether peers will influence compensation levels or growth, so both are presented in the baseline results. Extensions will focus on annual changes, because changes in compensation are more useful for identifying responses to shocks over time. Finally, all estimates use compensation measured in the same firm fiscal year. The estimates are also consistent with peers reacting to one another with a lag, as discussed in Section 3.4.

Column (1) presents measures of peer influence in direct compensation. The pairs distance metric δ^{PDM} shows that section peers are 7.4% more similar than class peers for annual levels of direct compensation and 11% more similar for annual changes in direct compensation. The excess variance metric, δ^{EVM}, tells a similar story. For levels and changes in direct compensation, the ratio of between- to within-section variance is roughly 50% greater than expected under the null.

Under the additional assumptions of the linear-in-means model, both metrics imply a lower bound for the peer elasticity γ of around 20%: The individual response to a unit change in mean peer group fundamentals is 20% as strong as the individual response to a unit change in one's own fundamentals. For example, we expect an executive to receive an extra \$200K in direct compensation if a change to the fundamental determinants of compensation leads to a \$1 million increase in mean section peer compensation (equal to one standard deviation in direct compensation).

The peer elasticity directly translates into a peer multiplier. A peer elasticity of 20% implies that the aggregate response to a change in the fundamental determinants of direct compensation will be 20% larger than the direct response of any individual firm to the change in fundamentals, due to contagion across peers. Similarly, the scaled variance of mean outcomes across different peer groups will be up to 50% larger than what we would expect given fundamental

Table 3
Peer influence in compensation and acquisitions

		(1)	(2)	(3)	(4)	(5)	(6)
		Log direct comp (salary + bonus)		Log total comp		Acquisition attempt dummy	
Levels	Panel A.1: Pairs distance metric						
	Distance ratio	0.074*	0.089**	0.029	0.059	0.112***	0.039*
		(0.040)	(0.044)	(0.037)	(0.040)	(0.032)	(0.024)
	γ	0.0154*	0.0187**	0.060	0.123	0.239***	0.079*
		(0.080)	(0.089)	(0.075)	(0.082)	(0.066)	(0.049)
	Obs (pair × year)	10,003	6,963	10,003	6,963	10,155	9,812
	Panel A.2: Excess variance metric						
	Excess variance ratio	0.423**	0.673*	0.207	0.256	0.362***	0.324***
		(0.199)	(0.350)	(0.150)	(0.350)	(0.073)	(0.073)
	γ	0.193**	0.294*	0.099	0.121	0.167***	0.151***
		(0.089)	(0.153)	(0.067)	(0.153)	(0.033)	(0.033)
	Obs (executive × year)	3,042	3,042	3,042	3,042	3,071	3,071
Changes (1st differences)	Panel B.1: Pairs distance metric						
	Distance ratio	0.101**	0.113**	0.008	0.118**		
		(0.046)	(0.051)	(0.041)	(0.048)		
	γ	0.234**	0.241**	0.016	0.254**		
		(0.093)	(0.104)	(0.085)	(0.100)		
	Obs (pair × year)	6,658	4,524	6,658	4,524		
	Panel B.2: Excess variance metric						
	Excess variance ratio	0.584***	0.450**	0.117	0.104		
		(0.189)	(0.196)	(0.155)	(0.072)		
	γ	0.259***	0.204**	0.057	0.051		
		(0.083)	(0.085)	(0.068)	(0.032)		
	Obs (executive × year)	2,320	2,320	2,320	2,320		
Demographic controls and year FE		Y	Y	Y	Y	Y	Y
Empl. controls, excl. firm transitions		N	Y	N	Y	N	Y
1st stage uses ExecuComp sample		N	Y	N	Y	N	Y
Firm and SIC3 returns, size		N	Y	N	Y	N	Y
Industry FF49 × year FE		N	Y	N	Y	N	Y
Exclude pairs with industry overlap[a]		N	Y	N	Y	N	Y

This table follows the methodology described in Section 2. All specifications use residual outcomes that are estimated from a first-stage regression of the raw outcomes on the controls listed in the bottom panel. First-stage results (available upon request) are not reported in this and future tables. Demographic controls include age and gender. Employment controls include dummies for executive type (CEO vs. CFO) and quadratics in firm tenure and CEO or CFO tenure. Exclusion of firm transitions excludes observations reflecting a transition to a different firm and only applies to Panels (A.1) and (B.1). In even-numbered columns, the first-stage estimation uses all observations for CEOs and CFOs in ExecuComp to improve the fit of industry trends over time. Firm and industry SIC3 returns include current and lagged annual returns matched to the fiscal year end month of each firm and are winsorized at the 1 percent level. Significance levels are estimated using the permutation tests described in Appendix A.3. *, **, and *** are significant at 10%, 5%, and 1% levels, respectively. [a]Exclusion of executive pairs in the same Fama-French 49 industry or financials (SIC codes 6000–6999) only applies to Panels (A.1) and (B.1).

differences across groups, that is, outcomes will exhibit excess clustering across peer groups.

Column (3) of Table 3 repeats the baseline analysis for total compensation. In general, peer effects for total compensation are not significant. Point estimates can be economically large—the peer elasticity is 10% for levels of total compensation—but tend to be smaller than those for direct compensation. Total compensation is discussed in detail in Section 4, where I find significant and large peer effects in total compensation using Forbes data that covers an earlier time period. Section 4 also describes institutional structures that confound analysis of total compensation in the more recent sample period. For now, I focus on estimates using direct compensation.

The baseline compensation results in Columns (1) and (3) of Table 3 purposely use the minimum set of controls in the first stage: demographics controls and year fixed effects. Results represent the overall effect of peers on compensation. Specifically, compensation could be relatively more similar among section peers because past social interactions led section peers to differentially select into the ExecuComp subsample and to enter similar firms, industries, and geographical regions as documented in Table 2. Columns (2) and (4) test for "excess" peer influence, that is, peer similarities in compensation beyond what can be explained by observable similarities in firm and industry trends. These tests include controls for firm and industry SIC3 current and lagged fiscal year returns, firm size as measured by the log of fiscal year sales, and Fama-French 49 industry fixed effects interacted with firm fiscal year fixed effects. To estimate the coefficients for the controls as efficiently as possible, the full ExecuComp sample is used in the first stage, although second-stage results only use compensation residuals from the first stage that pertain to the HBS sample. Estimates in Panel (B) also exclude observations representing executive transitions to different firms. All estimates using the pairs distance metric also exclude pairs of executives belonging to the same Fama-French 49 industry and pairs in which at least one executive is employed in the financial sector. Excluding these pairs further reduces the likelihood that compensation similarities are driven by industry interdependencies among section peers. Overall, estimates of peer influence remain stable and significant as more controls and sample restrictions are introduced. The peer elasticities for direct compensation consistently exceed 18% and estimates for total compensation remain stable or increase in magnitude.[10]

Columns (5) and (6) of Table 3 presents evidence of peer influence in acquisitions strategies. For brevity, I measure acquisitions activity using a dummy for whether the executive attempted at least one acquisition in the year. Attempted acquisitions are informative of the intentions of executives even if the acquisitions ultimately fail. Results are similar using alternative measures, such as completed acquisitions, number of acquisitions, or value of acquisitions (see Table 8). I find that executives are more likely to acquire when their section peers acquire than when their class peers acquire. The distance

[10] The addition of controls in the first stage may overcontrol for shared firm characteristics that are chosen by executives as the result of contemporaneous peer interactions. For completeness, I present results with and without additional controls. In some specifications, peer effects are insignificantly stronger with the inclusion of controls for firm and industry trends. This could reflect either improvements in the precision of outcome measures or the case in which executives respond more to the industry-adjusted outcomes of their peers rather than the raw outcomes.

ratio in Column (5) shows that section peers are 11% more similar in terms of acquisition levels. The excess variance ratio shows that the ratio of between- to within-section variance is more than 35% greater than expected under the null. The two ratios imply significant peer elasticities in the range of 15%–25%.

As with compensation, the peer elasticity translates into a peer multiplier for the levels and variance of aggregate acquisitions outcomes. If there is a shock to the fundamental determinants of acquisition activity, the aggregate response to the shock will be 15%–25% larger than the direct response of any individual firm due to contagion among peers. Similarly, acquisition activity across peer groups boundaries will appear 35% more clustered than what we would expect given fundamental differences in the determinants of acquisition activity across peer groups.

In Column (6), additional controls and sample restrictions are introduced to explore excess peer influence in acquisitions activity. For example, the industry x year fixed effects control for industry-level waves in acquisition activity. Relative to the baseline results, the estimates drop slightly and imply peer elasticities ranging from 8%–15%. The drop in magnitudes suggests that a portion of the peer similarities in acquisition policy could be due to past peer interactions leading to selection into similar firms and industries. However, peer effect magnitudes remain sizable and later tests will offer more concrete evidence of contemporaneous peer influence.

Finally, Table 4 estimates peer influence in other firm policies, including investment, firm size, and the set of proxies for financial policy used in Bertrand and Schoar (2003): leverage, dividends, interest coverage, and cash holdings. Columns (1), (3), and (5) use the minimum set of controls to capture overall peer influence. I find large and significant peer elasticities ranging from 15%–25% for investment and firm size, which is consistent with the strong peer effects in acquisitions—acquisitions represent a large discrete form of investment and contribute substantially to firm size. I also find economically large but marginally significant peer elasticities of 10%–20% for leverage and interest coverage. Peer elasticities for dividend policy and cash holdings are zero or slightly negative. However, the standard errors are large, so these are not precisely estimated zero effects.

Columns (2), (4), and (6) estimate "excess" peer influence, that is, peer similarities beyond that which can be explained by observable peer similarities in firm and industry trends. Whereas the magnitude of the peer elasticities for investment, interest coverage, and firm size remain economically meaningful at 10%–20%, point estimates generally decline and become less significant. This suggests that a portion of the peer similarities in these other firm policies was driven by past peer interactions leading to selection into similar types of firms and industries.

Note that insignificant estimates do not definitively imply that excess peer effects are weaker for these other firm policies. As shown in Graham (2008), estimates of peer effects that use the distribution of outcomes will be biased

Table 4
Peer influence in other firm policies

	(1)	(2)	(3)	(4)	(5)	(6)
	Investment		Leverage		Dividends	
Panel A.1: Pairs distance metric						
Distance ratio	0.105*	0.038	0.125*	0.046	−0.054	−0.008
	(0.051)	(0.045)	(0.073)	(0.070)	(0.077)	(0.071)
γ	0.223**	0.078	0.270*	0.094	−0.105	−0.016
	(0.104)	(0.092)	(0.150)	(0.144)	(0.160)	(0.149)
Obs (pair × year)	8,757	8,461	10,070	9,728	10,020	9,681
Panel A.2: Excess variance metric						
Excess variance ratio	0.321**	0.227	0.188	0.012	−0.050	−0.020
	(0.161)	(0.153)	(0.219)	(0.229)	(0.968)	(1.016)
γ	0.149**	0.108	0.090	0.006	−0.025	−0.010
	(0.073)	(0.068)	(0.094)	(0.100)	(0.323)	(0.336)
Obs (executive × year)	2,827	2,827	3,057	3,057	3,049	3,049
	Interest coverage		Cash holdings		Firm size	
Panel B.1: Pairs distance metric						
Distance ratio	0.086	0.082	−0.053	−0.037	0.112**	0.071
	(0.087)	(0.069)	(0.051)	(0.047)	(0.050)	(0.048)
γ	0.182	0.173	−0.104	−0.073	0.239**	0.147
	(0.193)	(0.148)	(0.104)	(0.096)	(0.103)	(0.099)
Obs (pair × year)	7,420	7,167	9,362	9,035	9,298	8,992
Panel B.2: Excess variance metric						
Excess variance ratio	0.417	0.441	0.117	0.057	0.308**	0.291
	(0.303)	(0.273)	(0.170)	(0.189)	(0.154)	(0.181)
γ	0.190	0.200*	0.057	0.028	0.144**	0.136*
	(0.122)	(0.112)	(0.075)	(0.084)	(0.070)	(0.080)
Obs (executive × year)	2,581	2,581	2,920	2,920	2,942	2,942
Demographic controls and year FE	Y	Y	Y	Y	Y	Y
Empl. controls, excl. firm transitions	N	Y	N	Y	N	Y
1st stage uses ExecuComp sample	N	Y	N	Y	N	Y
Firm and SIC3 returns, size	N	Y	N	Y	N	Y
Industry FF49 × year FE	N	Y	N	Y	N	Y
Exclude pairs with industry overlap[a]	N	Y	N	Y	N	Y

This table tests for peer influence in the annual levels of six other firm policies using the methodology described in Section 2. Firm policies in the first-stage estimation are measured as follows: Investment = log(capx), Leverage = lt/ceq (debt to equity ratio, bottom capped at zero, top 1% winsorized), Firm Size = log(ppent), Cash Holdings = (ib+dpc+che)/at (cash reserves to assets ratio, top and bottom 1% winsorized), Dividend Policy = dvpsx_f/prcc_f (dividend to price ratio, bottom and top capped between 0 and 1), and Interest Coverage = oibdp/xint (ratio of operating income before depreciation and amortization to interest expenses, top and bottom 2% winsorized). Specifications and other variable definitions are as described in Table 3. [a]Exclusion of executive pairs in the same Fama-French 49 industry only applies to the pairs distance metric. Standard errors in parentheses are estimated using the permutation test described in Appendix A.3. *, **, and *** are significant at 10%, 5%, and 1% levels, respectively.

toward zero if outcomes are measured with error. Firm policies, such as dividend distributions, tend to be slow moving and may be very noisy proxies for the true policy targeted by the manager (e.g., some executives may target the dividend level, while others target the dividend-to-earnings ratio). Therefore, I leave more in-depth analysis of these other firm policies to future work. In the remainder of the analysis, I focus on exploring the mechanisms behind the strong and significant estimates of peer effects in compensation and acquisitions.

Tests of "excess" peer influence in Table 3 show that peer effects in compensation and acquisitions are not driven only by past peer interactions leading to selection into observably similar firms, but rather are suggestive of contemporaneous interactions. However, the addition of firm controls cannot rule out entry into unobservably similar firms. In the next set of results, I explicitly test for the underlying mechanisms behind peer influence in compensation and acquisitions. Further analysis also serves the purpose of showing that it is not by chance that compensation and acquisitions generate large estimates relative to the other firm policies tested in Table 4. Only true peer effects should to lead to reunion effects or reactions to lucky shocks to peers as shown in the next sections.

3.3 Alumni reunions: The timing of social interactions

I begin by exploring the timing of social interactions. Past interactions describe peers who leave business school holding similar management philosophies or select into similar types of firms (this includes selection into the ExecuComp subsample). Meanwhile, contemporaneous interactions describe interactions that occur while executives manage firms and are more informative about how peers directly affect firm policies.

To determine the timing of social interactions, I use the natural experiment of alumni reunions, which occur every five years after each executive's specific graduation year. Because reunions occur in the same time period as firm policy measures, reunions act as exogenously timed shocks to the strength of contemporaneous peer bonds. Reunions at HBS are extravagant four-day celebrations consisting of formal galas and group discussions, as well as section-based tents and parties. Reunions are well attended—for example, more than 40% of the class of 1985 registered to attend the fall 2010 reunion celebrations. However, reunion year effects do not only operate through attendance. Reunions also intensify accompanying activities and communication. For example, during reunion contribution campaigns, each graduate is contacted by volunteers from her section with requests for donations, with wealthy executives receiving extra attention. HBS administrators collect information on the financial well-being of its graduates through direct research and through surveys of its alumni. This information is then disseminated to section-based reunion contribution campaign volunteers and provides another avenue through which graduates can become more aware of their peers' activities. Individual donation amounts and section-based giving records are then published in a brochure that is mailed to all graduates. In addition, graduates are encouraged to update their personal information and accomplishments in the "Class Notes," a directory for alumni news. These formal updates may be supplemented by informal activities that are coordinated around the reunion schedule.

Importantly, some of the information that is disseminated in the reunion year is likely to be public because of disclosure requirements regarding compensation and M&A. However, the reunion year can affect behavior if reunions increase the salience of peer outcomes. In addition, section peers may increase contact following reunions and jointly plan actions that are not public information.

Reunion-cycle variation in peer effects also offers a check on potential bias from section-specific common shocks, such as a professor who teaches students in her section to be aggressive in compensation negotiations and acquisitions. Section-specific common shocks are unlikely to generate peer similarities that vary with exogenously timed reunion shocks.

The analysis does not use measures of reunion attendance because data on attendance is unavailable going back to the 1980s. Moreover, reunion attendance is an endogenous choice that may be correlated with unobserved individual characteristics, so estimates that compare those who do and do not attend reunions may be biased. Instead, I compare average peer similarities for all executive graduates in the year following reunions with peer similarities in other years. In other words, I measure the intent-to-treat effect of the reunion year, under the assumption that the reunion year schedule is exogenously set. To control for possible direct effects of reunions on executive outcomes, I focus only on the extent to which section peers become more similar than class peers following reunions.[11]

Figures 1 and 2 show that the ratio of within- to across-section similarities in executive compensation and acquisitions increase sharply following alumni reunions. Figure 1 plots the distance ratio for annual changes in log direct compensation for each year in the five-year reunion cycle. Figure 2 repeats the exercise for acquisition policy, as measured by the acquisition attempt dummy. Both figures show that reunions lead to a sharp increase in section peer similarity (relative to class peer similarity) in the year immediately following reunions, that is, reunions affect peer similarity with a one-year lag. This is not surprising given that reunions occur in the summer or fall of each year, so effects may not manifest in terms of firm outcomes until the following fiscal year.

Table 5 presents more detailed evidence that peer influence becomes stronger following reunions. For both direct compensation and the acquisitions attempt dummy, the distance ratio and peer elasticity in the year following reunions is more than two times larger than in the other years of the reunion cycle. p-values test whether the distance ratio in the year following reunions is equal to the distance ratio in other years. Equality can be rejected at the 12% level or lower in all cases.

The above analysis focuses on annual changes in compensation. In unreported results (omitted for brevity) for annual levels of compensation, I also find that peer similarities are higher after reunions, with distance ratios

11 In theory, reunions may also increase similarities among class peers or across class years (e.g., the classes of 1960 and 1965 have their reunions at the same time). In unreported results, I do not find significant increases in similarities across sections or across class years.

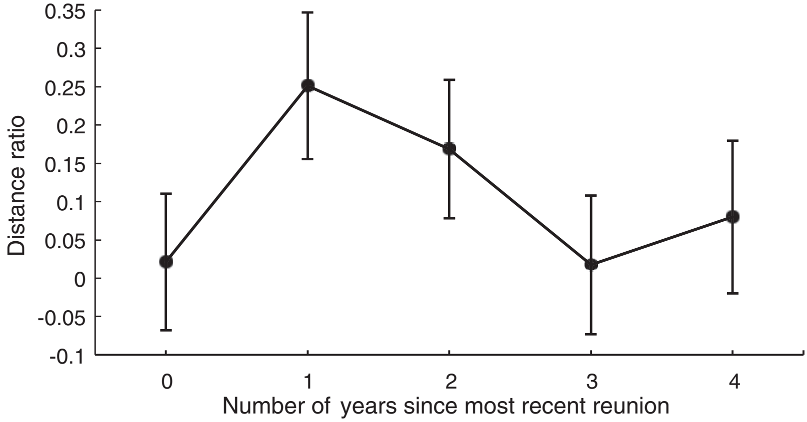

Figure 1
The reunion cycle and peer influence in compensation
Figure 1 plots peer similarities in annual changes in log direct compensation for each year in the five-year reunion cycle. Each point represents a distance ratio, which measures the extent to which section peers are more similar than class peers in terms of pairs absolute distance. Controls and sample restrictions are as described for Table 5, Column (2). Error bars represent standard errors, estimated using the permutation test described in Table 3.

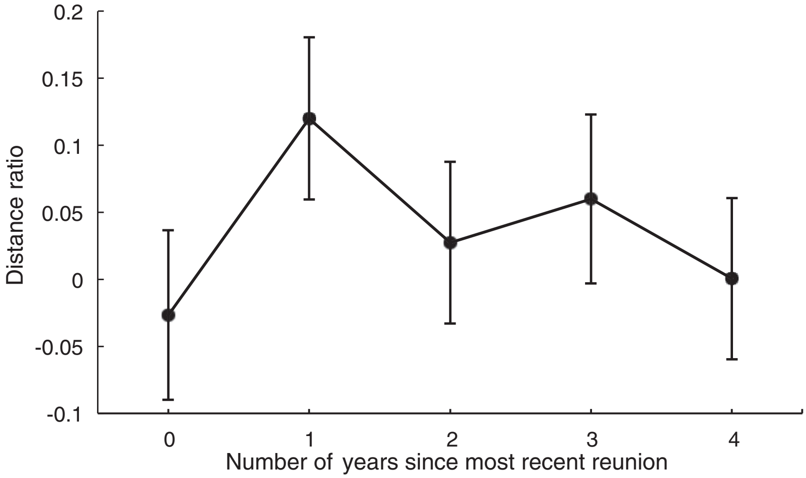

Figure 2
The reunion cycle and peer influence in acquisitions
Figure 2 plots peer similarities, as measured by the distance ratio, in annual levels of the *acquisition attempt dummy* for each year in the five-year reunion cycle. All figure properties are as described for Figure 1.

of around 9% following reunions relative to 6% in other years. However, the difference in effect sizes is not significant. This is likely due to the fact that levels of compensation are more strongly serially correlated than changes in compensation, that is, compensation levels cannot become more similar in reunion year +1 and then sharply less similar in reunion year +2.

Table 5
Shocks to peer influence following alumni reunions

	(1)	(2)	(3)	(4)
	Direct compensation		Acquisition attempt dummy	
Distance ratio: Reunion year + 1	0.178**	0.251***	0.190***	0.114**
	(0.084)	(0.096)	(0.061)	(0.049)
γ : Reunion year + 1	0.399**	0.603***	0.430***	0.244**
	(0.177)	(0.204)	(0.126)	(0.100)
Distance ratio: All other years	0.075*	0.063	0.090**	0.022
	(0.048)	(0.052)	(0.035)	(0.028)
γ : All other years	0.192**	0.155	0.189***	0.044
	(0.097)	(0.105)	(0.071)	(0.056)
p-Value: Distance ratio equality	0.028	0.005	0.120	0.077
Obs (pair $\times$ year)	6,658	4,524	10,155	9,812
Demographic controls	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y
Employment controls, excl. firm transitions	N	Y	N	Y
First stage uses full ExecuComp sample	N	Y	N	Y
Firm and SIC3 industry returns, size	N	Y	N	Y
Industry FF49 $\times$ year fixed effects	N	Y	N	Y
Exclude pairs with industry overlap[a]	N	Y	N	Y

This table tests whether peer influence is stronger following alumni reunions, which occur every five years after each executive's specific graduation year. Columns (1) and (2) examine peer influence in annual changes in log direct compensation whereas Columns (3) and (4) examine annual levels in the acquisition attempt dummy. Variables are as described in Table 3. In the second-stage estimation, the absolute difference of pair residual outcomes are regressed on the same section dummy, a dummy for the year immediately following reunions, and the interaction of the former two dummies. Distance ratio: reunion year +1 is the fractional difference in mean distance between two section peers relative to the mean distance between two class peers in the year following reunions (measured by the negative ratio of the sum of the coefficients on same section and same section $\times$ reunion year +1 to the sum of the coefficients on reunion year +1 and the constant term). Distance ratio: all other years is the fractional difference in distance for section peers relative to class peers in all other years (measured as the negative ratio of the coefficient on same section to the constant term). γ: reunion year +1 and γ: all other years represent, under the assumptions of the linear-in-means model, the elasticity of individual outcomes to mean group fundamentals (scaled by the elasticity of individual outcomes to own fundamentals) in the year following reunions and all other years, respectively. p-values are reported for the test of whether distance ratio: reunion year +1 is equal to distance ratio: all other years. Standard errors in parentheses are estimated using the permutation test described in Appendix A.3. *, **, and *** are significant at 10%, 5%, and 1% levels, respectively.
[a]Columns (2) and (4) exclude pairs of executives in the same Fama-French 49 industry. Column (2) also excludes financials (SIC codes 6000–6999).

Note also that excess peer elasticities in nonreunion years range from 15% for direct compensation to less than 5% for acquisitions. This does not necessarily imply that contemporaneous peer effects are near zero in nonreunion years. In the Appendix, I show that peer effects can be large in nonreunion years if we focus on the subset of graduates who donate to HBS, that is, those who are likely to have the closest bonds with their alumni peers.

Under the assumption that only the marginal increase in peer similarities following reunions reflects contemporaneous interactions, contemporaneous interactions can lead to substantial peer elasticities of over 20%. These numbers may be overly conservative in the likely event that a steady level of contemporaneous interactions occur in all years, which are then magnified during reunion years. These calculations are described in Appendix A.2.

3.3.1 Implications and external validity of reunions analysis. The external validity of the reunion analysis does not hinge upon the existence of randomized section assignment and alumni reunions. Reunions are merely a powerful way to capture contemporaneous interactions among HBS executives. Every executive, including non-HBS graduates, is likely to have a close and influential set of executive peers. This set of close peers may include other executives in the same industry, trading partners, or friends from past employment experiences. The difficulty lies in identifying who, among the numerous people associated with each executive, belongs in this set of very close peers. The benefit of HBS sections is that it allows the econometrician to identify a subset of individuals (randomly assigned section peers) who are likely to belong to each HBS executive's peer group and when that subset of peers is likely to be the most influential (following alumni reunions). Thus, the magnitude of the peer effects among HBS section peers following reunions offers a guide to how strong contemporaneous peer effects can be within other similarly close executive peer groups.

The marginal increase in peer similarity following reunions offers a lower bound for true peer effects because most common shocks, such as influential teachers, should affect behavior in ways that are orthogonal to the reunion schedule. However, it is conceivable, although unlikely, that section members receive section-specific common shocks at reunions. The next section provides an additional check against bias from section-specific common shocks using industry shocks to peers.

3.4 Pay for friend's luck: Reactions to peer outcomes or fundamentals?

This section presents exploratory tests of whether peer influence reflects reactions to peer fundamentals or peer outcomes. As described in Section 2.3, a reaction ϕ to fundamentals can represent the social transmission of any fundamental determinant of outcomes (e.g., managerial skill or insight or compensation negotiation skills). A reaction θ to peer outcomes can represent the effects of relative earnings on compensation (e.g., "catching up with the Joneses" preferences or a change in the executive's outside options). Although both ϕ and θ are peer effects, only θ will generate a social multiplier effect with respect to policies or shocks that affect peer outcomes, while leaving peer fundamentals unchanged (e.g., takeover regulations or compensation caps).

It is possible to isolate reactions to peer outcomes using shocks to peer outcomes that leave peer fundamentals unchanged. The "pay for friend's luck" tests in Table 6 explore one such shock: lucky industry returns as shocks to the compensation of peers in different industries. I adopt a modified form of the second stage of the pairs distance metric:

$$\left|\widetilde{\triangle Y}_{it} - \widehat{\triangle Y}_{jt}\right| = \beta_0 + \beta_1 I_{ijt}^{\text{section peer}} + \varepsilon_{ijt}.$$

Here, $\widetilde{\triangle Y}$ is the residual from the first-stage regression of change in compensation on the controls for firm and industry trends listed in the bottom panel. $\widehat{\triangle Y}$ is the peer's predicted "lucky" change in compensation from a regression, estimated using the full ExecuComp sample, of the change in

Table 6
Pay for friend's luck

	(1)		(2)	
	Direct compensation		Total compensation	
Panel A: First stage: Change in compensation				
SIC3 return	0.079***	(0.009)	0.134***	(0.018)
SIC3 lagged return	0.045***	(0.009)	0.102***	(0.017)
R^2	0.057		0.023	
Obs	41,212		41,212	
Obs (HBS only)	2,320		2,320	
Panel B: Pairs distance metric				
Distance ratio	0.142***	(0.037)	0.078	(0.050)
Obs (pair × year)	6,486		6,486	
Panel C: Pairs distance metric: Lagged				
Distance ratio	0.166***	(0.042)	0.091*	(0.055)
Obs (pair × year)	5,076		5,076	
Demographic controls	Y		Y	
Employment controls, excl. firm transitions	Y		Y	
First stage uses ExecuComp sample	Y		Y	
Firm and SIC3 industry returns, size	Y		Y	
Industry FF49 × year fixed effects	Y		Y	
Excl. pairs in linked industries	Y		Y	

Panel (A) shows the first-stage estimation, in which lucky changes in compensation are predicted using industry returns. The change in annual log compensation is regressed on the firm's SIC3 industry current and lagged fiscal year returns (calculated excluding the firm's own returns) as well as year fixed effects. Observations are at the executive × year level, and the full sample of ExecuComp CEOs and CFOs is included to improve precision. The results are similar and significant if the sample is instead restricted to HBS CEOs and CFOs. Panel (B) examines the relationship between an executive's residual change in compensation and her peer's lucky change in compensation. Specifications follow the modified pairs distance metric described in Section 3. The dependent variable is $(\Delta\tilde{\mathbf{Y}}_{\mathrm{isct}} - \Delta\hat{\mathbf{Y}}_{\mathrm{jsc,t}})$. $\Delta\tilde{\mathbf{Y}}$ is the residual from the standard first-stage regression of the change in log compensation on the set of controls listed in the bottom panel. $\Delta\hat{\mathbf{Y}}$ is the predicted lucky change in log compensation using the first-stage estimation presented in Panel (A). Consider a pair of executives A and B in a given year. This pair will account for two observations. The dependent variable in the first observation is the absolute difference between A's residual change in compensation and B's predicted change in compensation. The dependent variable in the second observation is the absolute difference between A's predicated change in compensation and B's residual change in compensation. Specifications in Panel (C) are identical, except for the use of lagged predicted changes in peer compensation as part of the dependent variable: $(\Delta\tilde{\mathbf{Y}}_{\mathrm{isct}} - \Delta\hat{\mathbf{Y}}_{\mathrm{jsc,t-1}})$. All other variables are as described in Table 3. To address the concern that lucky shocks in an industry may have a greater direct impact across industries linked by section peers than across industries linked by class peers, all tests exclude the financial sector (SIC codes 6000–6999) as well as pairs of peers in linked industries. (Following Ahern and Harford (2010) and using the BEA input-output tables, industries are considered linked if a customer industry buys at least 2% of a supplier industry's total output or if a supplying industry supplies at least 2% of the total inputs of a customer industry.) Standard errors in parentheses are estimated using the permutation test described in Appendix A.3. *, **, and *** are significant at 10%, 5%, and 1% levels, respectively.

compensation on the peer's current and lagged fiscal-year industry returns (calculated excluding the peer's firm returns). The distance ratio δ^{PDM} is again defined as $-\beta_1/\beta_0$. A δ^{PDM} greater than zero implies executives react more to section peer's lucky pay than to class peer's lucky pay.

This specification extends research on "pay for luck" in Bertrand and Mullainathan (2001), who examine how an executive's compensation responds to lucky shocks in her own industry. In contrast, the analysis in this paper does not require the assumption in Bertrand and Mullainathan that pay for luck reflects governance problems. Pay that responds to own industry-level returns can be efficient, as argued in Gopalan, Milbourn, and Song (2010). It is sufficient to assume that industry returns do not alter the underlying managerial skills and other fundamentals of peer j and to test whether i's compensation responds to j's predicted changes in pay.

A potential concern is that j's industry returns may have a direct impact on i's compensation if i and j work in related firms. This is not a problem for the analysis per se, because the distance ratio measures the relative similarity of section peers to class peers. However, if section peers belong to more related firms than class peers, the problem returns. This concern is mitigated through the use of i's residual compensation after controlling for i's own firm and industry current and lagged fiscal year returns. I also limit bias by excluding all executives working in the financial sector and all pairs of executives in linked industries. Using the BEA input-output tables and following Ahern and Harford (2012), industries are considered linked if a customer industry buys at least 2% of a supplier industry's total output or vice versa. Results remain similar if the analysis further excludes all pairs of executives in the same broad Fama-French 5 industry. In unreported tests, I also test whether section peers work in industries with more similar annual industry returns than class peers, and estimate an insignificant and economically small distance ratio of 4%. Nevertheless, these results should be viewed as exploratory because they rest upon the assumptions described above.

Panel (A) of Table 6 shows the first-stage estimation: Industry returns are indeed highly and significantly predictive of compensation growth. Using the fitted values of lucky pay from this first-stage estimation, Panel (B) tests whether agents react more to section peers' lucky pay than class peers' lucky pay. I find that section peers are 14% more similar than class peers, even when peers' compensation is due to lucky shocks and detailed controls are included for own firm and industry performance. Column (2) shows that section peers are also 8% more similar than class peers in terms of total compensation, although the effect is only marginally significant.

3.4.1 Implications of pay for friend's luck. Evidence of pay for friend's luck shows that peer influence can lead to movements in compensation that do not reflect changes to firm productivity. The results are supportive of evidence in Bertrand and Mullainathan (2001), showing that executives are rewarded for more than their effort or skill.

Pay for friend's luck is consistent with two models in which relative compensation directly affects individual compensation. In the first model, relative earnings and status enter directly into an executive's utility function (e.g., Luttmer 2005). Consider a simple Nash bargaining game between the executive and the board in which the executive's utility is a function of wages and job satisfaction. If peer wages increase, job satisfaction decreases for any given level of own wages (Card et al. 2012). Holding bargaining power fixed, Nash bargaining implies that an increase in peer wages will lead to an increase in own wages.[12] In the second model, an increase in peer compensation improves an executive's outside options if the executive can credibly leave to work in his friend's firm or industry. Given that executive transitions are costly, a change in outside options will again lead to higher pay. In general, it is difficult to distinguish between these two models. However, regardless of the exact model, the results imply that an increase in the strength of peer effects can lead to changes in compensation that do not correspond to changes in managerial productivity.

Pay for friend's luck has two other implications similar to those from the analysis of reunions. First, peer effects are unlikely to be driven by section-specific common shocks, because common shocks should not affect behavior that varies over time with shocks to peers in different industries. Second, pay for friend's luck is evidence of contemporaneous interactions—past interactions leading to selection into similar firms should not cause compensation to vary over time with shocks to peers.

3.4.2 Lagged responses. In general, peer similarities could be due to contemporaneous or lagged influences (Abel 1990). So far, I have presented estimates of contemporaneous peer similarities without taking a stand on whether the true effects are contemporaneous or lagged. Panel (C) of Table 6 tests the relationship between one's change in residual compensation and one's peer's lagged change in predicted compensation. The estimates of lagged peer effects are very similar to those of contemporaneous peer effects.

However, lagged effects should be viewed as exploratory because both outcomes and industry shocks tend to be serially correlated. In unreported results, I estimate the baseline results for both compensation and acquisitions policy allowing for a one-year lag in peer responses using the pairs distance metric. The results yield significant peer elasticities of up to 20% and are consistent with the presence of leaders and followers. However, serial correlation implies that we cannot reject the alternative hypothesis that peers talk and jointly plan future actions without a time lag.

3.5 Does peer influence lead to more efficient acquisitions?

Is peer influence in acquisitions driven by information sharing about valuable investment opportunities? If so, peer interactions can lead to more efficient acquisitions. An ideal test would compare peer-motivated acquisitions with non-peer-motivated acquisitions. In practice, even the gold standard of randomly assigned peer groups only provides exogenous variation in the

[12] In this model, the executive never leaves money on the table. Suppose that the executive's utility of working in the firm is the sum of wages and amenities, that is, job satisfaction: $U = w + a$. The executive has a threat point of $\underline{U}$. Define $\underline{w} \equiv \underline{U} - a$ as the minimum wage such that the executive is willing to work. Suppose the executive generates $X > \underline{w}$ in surplus for the firm. With fixed bargaining power $b \in [0,1]$, he will earn $w^* = b(X - \underline{w}) + \underline{w}$. If amenities decrease because of lower job satisfaction, then the threat point increases. Wages must increase, holding bargaining power fixed.

composition of a subset of an executive's peer group (his section peers) and when that subset of peers is likely to be the most salient (after reunions). Most natural experiments cannot provide variation in whether an executive has peers. Executives never live in a social vacuum, so the counterfactual of a pure, non-peer-motivated acquisition does not exist. With these limitations in mind, I use the identifying assumption that acquisitions following reunions are more likely to be peer motivated than acquisitions in other years. To the extent that acquisitions in nonreunion years may also be motivated by peers (including non-HBS peers), this test may be underpowered.

If the market predicts that an acquisition will destroy acquirer value, acquirer returns around the acquisition announcement should be negative. In addition, both diversifying and equity-financed acquisitions are associated with inefficient outcomes in the M&A literature. A large body of literature going back to Jensen (1986) argues that diversifying acquisitions are symptomatic of managerial agency problems such as empire building. Similarly, Shleifer and Vishny (2003) and others argue that equity financed acquisitions are likely to be driven by the desire to off-load overvalued equity rather than by real merger synergies. Column (1) of Table 7 shows that, relative to acquisitions in other years, acquisitions following reunions are 5.4 percentage points more likely to be diversified. Acquisitions are considered diversifying if the acquirer and target belong to different Fama-French 49 industries (results are very similar using SIC2 industry codes). Relative to the base rate (43% of acquisitions are diversifying in general), the reunion year leads to a substantial and significant 13% increase in the diversification rate. Column (2) shows that acquisitions following reunions are six percentage points more likely to be financed by equity rather than by cash. Relative to the base rate (25% of acquisitions are financed with equity), the reunion year again leads to a substantial and significant 25% increase in the equity-financing rate. However, in Column (3), I also test whether acquisitions following reunions exhibit lower ex post acquirer abnormal returns but find noisy zero estimates.

Next, I test whether executives react asymmetrically to positive and negative (or absent) peer acquisition activity. Consider two peers A and B. Suppose B first chooses whether to conduct an acquisition based upon his private information. If A and B share information about how to conduct "optimal" acquisitions strategy, we would generally expect A to be less likely to acquire if B did not acquire in the previous period and vice versa. This prediction holds regardless of whether we define "optimal" from the point of view of the social planner (e.g., it's optimal to share information about the availability of good targets) or firm shareholders (e.g., it's optimal to share information about how to time equity markets). I find sharp asymmetries in the individual response to lagged peer acquisition activity. Following reunions, distance ratios are close to 20% if a section peer acquired in the previous year but are close to zero and significantly different if the section peer did not acquire in the previous year. In other words, an individual is more likely to acquire if his section peer acquired in the previous

Table 7
Efficiency of peer influence in acquisitions

	(1)	(2)	(3)	(4)
	Diversify FF49	Equity financed	Acquisition attempt	Completed acquisition
Reunion yr+1	0.054**	0.061*		
	(0.022)	(0.082)		
Distance ratio 1: Reunion yr+1, $acq_{j,t-1}=0$			−0.006	0.035
			(0.070)	(0.070)
Distance ratio 2: Reunion yr+1, $acq_{j,t-1}=1$			0.175**	0.188**
			(0.077)	(0.076)
Distance ratio 3: All other yrs, $acq_{j,t-1}=0$			0.079	0.060
			(0.044)	(0.045)
Distance ratio 4: All other yrs, $acq_{j,t-1}=1$			−0.011	−0.001
			(0.052)	(0.047)
p-Value: Distance ratios 1 and 2 are equal			0.065	0.391
Obs (executive × year)	26,615	13,727		
Obs (executive × year − HBS only)	1,674	826		
Obs (pair × year)			13,528	13,528
Demographic controls	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y
Employment controls	Y	Y	N	N
First stage uses full ExecuComp sample	Y	Y	N	N
Firm and SIC3 industry returns, size	Y	Y	N	N
Industry FF49 × year fixed effects	Y	Y	N	N

Columns (1) and (2) regress a dummy for whether the acquisition is diversified (acquirer and target are in different Fama-French 49 industries) or equity financed on a dummy for whether the observation corresponds to the year following each executive's specific reunion year and the set of controls in the bottom panel. The sample includes all acquisition attempts matched to ExecuComp CEOs and CFOs (non-HBS observations are included to control for industry trends over time). Columns (3) and (4) test whether an individual's current acquisition activity is more similar to her section peer's lagged activity than to her class peer's lagged activity and how that lagged relationship varies depending on the reunion cycle and whether the peer did or did not attempt an acquisition in the previous year. The specifications are modifications of the pairs distance metric described in Section 2 and use only demographic and year controls in the first stage to capture the full effect of peer influence (results are similar controlling for firm and industry trends). In the second stage, the absolute difference between i's current acquisition activity and j's lagged acquisition activity, $\left|\tilde{\mathbf{Y}}_{isct}-\tilde{\mathbf{Y}}_{jsc,t-1}\right|$, is regressed on the same section dummy, the reunion year +1 dummy, the positive lagged peer outcome dummy (equal to one if $\tilde{\mathbf{Y}}_{jsc,t-1}>0$), and all interaction terms. Distance ratio 1 through 4 describe the distance ratios for the response to nonpositive lagged peer outcomes during the year following reunions, the response to positive lagged peer outcomes during the year following reunions, the response to nonpositive lagged peer outcomes during all other years, and the response to positive lagged peer outcomes during all other years, respectively. All other variables are as defined in Table 5. *p*-values test for equality between the distance ratios for responses to positive and nonpositive lagged peer outcomes in the year following reunions. Standard errors in parentheses are estimated using the permutation test described in Appendix A.3. *, **, and *** are significant at 10%, 5%, and 1% levels, respectively.

period, but he is not less likely to acquire if his section peer did not acquire. This suggests that part of the information contained in the absence of acquisition activity is ignored.

Overall, the results suggest that peer effects do not lead to more efficient acquisitions.[13] Peers tend to ignore information that could lead to more efficient acquisitions. Further, acquisitions following reunions exhibit characteristics commonly associated with inefficient acquisitions. These results are consistent

[13] Fracassi and Tate (2012) find related evidence that firms with more CEO-director connections make more frequent acquisitions, which destroy shareholder value on average.

Table 8
Robustness to alternative outcome measures and samples

	Distance ratio		Obs
Panel A: Annual changes in log compensation			
(1) Forbes sample: Years 1970–1991: Total comp	0.188*	(0.110)	927
(2) Double cluster standard errors	0.110**	(0.043)	6,658
(3) Winsorized compensation top and bottom 1%	0.095**	(0.042)	6,651
(4) Exclude observations after 2006	0.088*	(0.045)	5,601
Panel B: Annual levels in acquisitions			
(5) Double cluster standard errors: Attempted acquisitions	0.112***	(0.032)	10,155
(6) Completed acquisition dummy	0.118***	(0.032)	10,155
(7) Acquisition value to assets ratio	0.148**	(0.071)	8,090
(8) Number of acquisitions completed	0.163**	(0.071)	10,155
(9) Completed acquisitions, known value >$1M	0.102***	(0.032)	10,155

This table supports the robustness of the baseline results for peer influence in log compensation and acquisitions as presented in Table 3. Each row is a variation upon the baseline pairs distance metric specification described in Section 2. Controls in the first stage are limited to year fixed effects and demographics controls as defined in Table 3 to capture the full effect of peer influence. Row (1) uses the measure of total compensation from the Forbes sample of HBS executives (927 panel observations covering 149 CEOs). Because the Forbes data covers an earlier period from 1970 to 1991, direct compensation represents 84% (mean) and 93% (median) of total compensation. All other rows in Panel (A) use the direct compensation measure in ExecuComp. Rows (2) and (5) estimate standard errors and significance levels using the double-clustering procedure described in Appendix A.3. Row (3) uses log direct compensation winsorized at the top and bottom 1% levels as the dependent variable in the first-stage estimation. Row (4) excludes observations with fiscal years ending after December 2006 to ensure that results are robust to a change in SEC compensation disclosure rules. Row (6) uses a dummy for whether the executive completed at least one acquisition with 50% or greater stakes in the acquired entity. Row (7) uses the ratio of acquisitions value to lagged assets (CompuStat data items aqc/at_{t-1}, limited to 0 at the lower bound and winsorized at the 1% upper tail of the CompuStat sample). Row (8) uses the number of known completed acquisitions in which the firm acquired a 50% or greater stake in the acquired entity. Row (9) uses a dummy for one or more completed mergers (with 50% or greater stakes in the acquired entities) in which the transaction value is known and exceeds $1M US. All standard errors, unless otherwise noted, are estimated using the permutation test described in Appendix A.3. *, **, and *** are significant at 10%, 5%, and 1% levels, respectively.

with an alternative hypothesis that the peer effects in acquisitions are driven by social status concerns. However, these results are exploratory due to the empirical caveats described earlier.[14]

4. Robustness

Row (1) of Table 8 addresses the issue that peer effect estimates are large for direct compensation but are often insignificant for total compensation, defined as the sum of direct and equity-linked compensation. Equity-linked compensation has grown in recent decades (Frydman and Saks 2010) and accounts for roughly 50% of total compensation in the sample. However, institutional features suggest that finding measurable peer similarities is unlikely. Hall (1999) and Shue and Townsend (2013) show that stock options (which comprise the bulk of equity-linked compensation) are distributed according to multiyear plans that dictate the value or number of options granted

[14] In unreported results (available upon request), I also test whether section peers are relatively more likely to employ a common investment bank advisor. I find no evidence that this is the case.

in any particular year in advance at the start of the multiyear cycle. Because firms differ in the timing of their cycles, it may be hard for executives to negotiate for stock options to match that of their peers in any year and the amount that peers earn in any particular year may be less meaningful. In unreported results (available upon request), I test for peer effects in moving averages of total compensation. Although I find peer elasticities of more than 10%, *p*-values remain at 20% or larger, suggesting a lack of power given the averaging and limited sample size.

However, peer effects in total compensation can be large during the period prior to the rise of equity-linked compensation. To examine this earlier time period, I match HBS alumni records to the Forbes database, which covers executive compensation from the years 1970 to 1991 for approximately 800 companies each year. Total compensation during this earlier time period is dominated by cash payments; the mean and median direct compensation as a fraction of total compensation is 84% and 93%, respectively. The remainder consists of stock grants and options (which are valued at the time of exercise) and payouts from long-term pay programs.[15] Importantly, the Forbes database also offers an out-of-sample test of the baseline estimates of peer effects in compensation. The Forbes and ExecuComp samples cover nonoverlapping time periods from 1970–1991 and 1992–2009, respectively.

Using the Forbes sample, I find that annual changes in total compensation are 19% more similar among section peers than among class peers and the distance ratio is significant at the 10% level (higher standard errors likely reflect the smaller sample size). This suggests that peer effects for total compensation were large in the period prior to the rise of option grants. However, I cannot conclusively test for peer effects in total compensation or the composition of pay (e.g., percentage of total pay in the form of options) using the more recent ExecuComp sample because of the limited sample size and the noisiness of measures of option grants.

The remaining rows in Table 8 support the robustness of the baseline results for peer effects in annual changes in direct compensation and annual levels of acquisitions. Rows (2) and (5) present alternative estimates of significance levels using the double-clustering procedure described in Appendix A.3. Row (3) uses direct compensation winsorized at the top and bottom 1% levels. Row (4) excludes observations after fiscal year 2006 to ensure that results are not driven by a change in SEC compensation disclosure rules. Rows (6) through (9) test for peer effects in acquisitions strategy using alternative measures of acquisitions activity: completed acquisitions, number of acquisitions, and

15 Forbes data values options as of the exercise date instead of the grant date. Therefore, measures using Forbes data may include peer influence that leads to common timing of option exercise among peers. Total pay prior to 1978 excludes option gains (although option grants are rare in this very early period). In previous tables using ExecuComp, options are valued as of the grant date. In unreported results, I test for peer effects in the timing of options exercise using ExecuComp data and find noisy insignificant estimates.

Table 9
Robustness to outliers and serial correlation

	(1)	(2)	(3)	(4)	(5)	(6)
	Direct comp (salary+bonus)			Aqc / lagged assets		
Panel A: Absolute distance between executive pairs						
	Section Peers	Class peers	p-Value	Section Peers	Class peers	p-Value
Levels						
25th percentile	0.289	0.322	0.015	0.002	0.011	0.000
50th percentile	0.597	0.674	0.005	0.014	0.023	0.000
75th percentile	1.096	1.182	0.010	0.069	0.075	0.229
Changes						
25th percentile	0.123	0.129	0.455			
50th percentile	0.299	0.305	0.629			
75th percentile	0.556	0.624	0.000			
Panel B: Paired tests: Fraction of section peers more similar than class peers						
	≤ Median	≤ Mean		≤ Median	≤ Mean	
Levels						
All years	0.541	0.589		0.654	0.718	
Reunion year+1	0.575	0.617		0.680	0.717	
Reunion year+0	0.530	0.572		0.640	0.698	
Changes						
All Years	0.518	0.602				
Reunion year+1	0.524	0.606				
Reunion year+0	0.482	0.545				
Panel C: Pairs distance metric: One observation per pair						
	Mean	Median		Mean	Median	
Levels						
Distance ratio	0.085*	0.092*		0.078**	0.107**	
	(0.048)	(0.049)		(0.035)	(0.047)	
Obs (pair)	2,992	2,992		3,035	3,035	
Changes						
Distance ratio	0.092*	0.093				
	(0.053)	(0.060)				
Obs (pair)	2,068	2,068				

This table explores robustness to outliers and serial correlation. Acquisitions strategy in Panels (A) and (B) is measured using the ratio of acquisitions value to lagged assets rather than indicator variables (median tests using binary variables are difficult to interpret). Panel (A) compares the absolute distance in outcomes between section peers and class peers for the 25th, 50th (median), and 75th percentiles. Outcomes represent residual compensation and acquisitions after controlling for year fixed effects, age, and gender. Observations are at the executive pair × year level. p-values test the null hypothesis that the absolute distances between section and class peers are equal and are estimated using quantile regressions. Panel (B) shows the fraction of pairs of section peers that are less than the median or mean distance away from the individual, where mean and median distance are calculated as the median and mean absolute distance between pairs of class peers in each firm year. Observations are at the executive pair × year level. The results are reported for all years as well as years zero and one in the five-year reunion cycle. If peer effects are positive, we expect estimates to exceed 0.5. Panel (C) explores whether the results are driven by serial correlation. The sample is reduced to a single observation for each pair of executives. In Columns (1) and (3) the dependent variable is the mean of the absolute distance between each pair of executives over all years in the data. In Columns (2) and (4), the dependent variable is the median distance. Significance levels are estimated using the permutation tests described in Appendix A.3. *, **, and *** are significant at 10%, 5%, and 1% levels, respectively.

scaled acquisitions value. In all cases, estimates of peer effects remain stable and significant.

Finally, Table 9 tests whether estimates of peer similarities in compensation and acquisitions activity may be biased by outliers or serial correlation. Panel (A) tests whether the 25th, 50th (median), and 75th percentile of absolute distance among section peers is less than the corresponding distance among class peers. Because indicator variables are ill-suited for quantile and median tests, acquisitions activity is measured using scaled acquisitions value rather than a dummy for acquisition attempts. As expected, I find that section peers are more similar than class peers across all quantiles. However, it is interesting to note that peer similarities in annual changes in direct compensation are driven by the 75th percentile rather than the 25th or 50th percentiles. In other words, section peers are less likely to have larger-than-median differences in compensation growth when compared to class peers. Panel (B) presents paired tests looking at the fraction of section pairs that are less than the median or mean distance away from one another. Here, median (mean) distance is measured as the median (mean) distance between pairs of class peers in each firm year. As expected, the estimates show that more than 50% of section peers are less than the median and mean distance away from one another, especially in the year following reunions. Finally, Panel (C) imposes more conservative serial correlation corrections by limiting the sample to one observation per pair of executives. I estimate the distance ratio using the mean or median distance between each pair of executives over all years in which the pair exists. Estimates of distance ratios remain similar at 8%–10%.

5. Conclusion

I explore how executive social interactions can affect managerial decision making and firm policies using the historical random assignment of MBA students to sections at Harvard Business School. Under the identifying assumption that social bonds are stronger within randomly assigned sections than across sections in the same class year, I test whether executive and firm outcomes are more similar among section peers than among class peers. I find evidence of significant peer effects in firm investment, leverage, interest coverage, and firm size, with the strongest effects in executive compensation and acquisition activity. Section peers are 10% more similar than class peers in terms of compensation and acquisitions. Under the additional structural assumptions of the linear-in-means model, I estimate a substantial lower bound for the elasticity of individual outcomes to mean section peer characteristics of 10%–20%.

I show that peers are also important determinants of executive career outcomes, such as choice of industry, firm, and geographical locale. However, past peer interactions leading to selection into executive roles and into similar types of firms do not drive peer effects in compensation and acquisitions. Rather, the underlying mechanism is due to contemporaneous social interactions: peer similarities in compensation and acquisitions are more than twice as large in the year following staggered alumni reunions, which act as shocks to contemporaneous interactions. Tests of "pay for friend's luck" further narrow the mechanism driving peer effects in compensation. I find that compensation responds to lucky industry-level shocks to peers in distant industries after controlling for own firm and industry performance. Because these industry shocks alter peer outcomes while leaving peer fundamentals unchanged, peer effects in compensation are not driven only by the sharing of productive managerial skills within peer networks. Rather, relative compensation directly affects individual compensation.

Executive peer effects imply that executives matter for firm policies in a systematic way that can affect aggregate patterns. Multipliers arising from social interactions imply that the aggregate effect of a change in the fundamental determinants of compensation or acquisition activity will be 20% larger than the direct effect because of contagion among connected agents. Positive peer effects also lead to reduced within-group variation and increased across-group variation, that is, clustered financial outcomes. In the context of HBS sections, I find that the ratio of between- to within-section variance is 20%–40% greater than expected under the null hypothesis of no peer effects. A similar peer interaction mechanism can potentially amplify fundamental differences across other types of executive peer groups operating at the industry or geographic level.

Whereas executive peer effects have clear implications for our understanding of managerial decision making, consequences for social welfare are less obvious. Peer effects in acquisitions can be efficient if private information about how to conduct value-enhancing mergers is transmitted through social networks. However, I find evidence that peer influence in acquisitions does not operate through fully efficient information sharing and may lead to weakly less efficient acquisitions. Similarly, peer influence in compensation can lead to movements in compensation that do not reflect changes in firm or managerial productivity. Empirical investigation of the efficiency implications of peer influence among executives is a promising direction for future research.

Appendix

A.1 Balanced section assignment

HBS assigns students to equally sized sections. Assignment is random *conditional* on ex ante student characteristics, such as gender, ethnicity, and previous industry experience, that the Registrar observes. As the econometrician, I do not observe all conditioning variables. However, under the conservative assumption that the Registrar seeks to create mean-balanced sections, the following proof shows that balanced sectioning generates a bias *against* findings of positive peer effects.

The intuition is straightforward. Suppose there are no true peer effects. As described in Section 2.2, peer effects are measured using the ratio of the between- to within-section sum of squares of the outcome of interest Y. Under lotteried sectioning, the expected ratio of the sum of squares of the ex ante student characteristics X is equal to one. Under balanced sectioning, the ratio of the sum of squares of X should be weakly less than one, because the Registrar attempts to equalize the means of X across sections. Assuming that the measured outcome Y is a monotonic differentiable function of X, the expected ratio of the sum of squares of Y will also be weakly less than one, implying weakly negative measures of peer effects in expectation.

Formally, consider a single class c of HBS MBAs. The total number of students equals n, and the Registrar assigns students to k sections each of size m. Sections are indexed by $s=1,\dots,k$. X_{is} is the ex ante student characteristic for student i in section s. For brevity, I omit the time subscript although panel observations will only be compared with others in the same firm fiscal year t. Let $\overline{X}$ be the class mean of X_{is}, and $\overline{X}_s$ be the mean of X_{is} in section s. Let Y_{is} be the ex post student outcome that is used to measure peer effects. Let the between sum of squares, BSS, equal the sum of squared deviations of the section mean from the class mean: $\sum_{s=1}^{k}\left(\overline{X}_s-\overline{X}\right)^2$.

Assumption 1. The between sum of squares under balanced sectioning does not exceed the expected between sum of squares under lotteried sectioning:

$$BSS^{balanced}\leq E\left[BSS^{lottery}\right].$$

Assumption 2. Absent peer effects, $Y_{is}=f(X_{is})+\varepsilon_{is}$ where $f(\cdot)$ is monotonic differentiable, and ε_{is} is an *iid* error term with variance σ_{ε}^{2}.

Assumption 1 is a "do no harm" assumption—in actively trying to balance the mean of ex ante student characteristics across sections, the Registrar does not do worse than they would have if they had randomly lotteried students to sections. Assumption 2 guarantees that, in the absence of peer effects, the ex post outcomes are not backward-bending or otherwise perverse functions of ex ante student characteristics.

Proposition 1. In the absence of peer effects, balanced sectioning implies that the expected excess variance ratio δ^{EVM} and peer elasticity γ are weakly negative:

$$\delta^{EVM\_balanced}=\frac{m\cdot E\left[Var\left(\overline{Y}_s^{balanced}\right)\right]}{E\left[Var\left(Y_{is}^{balanced}|s\right)\right]}-1\leq 0,$$

$$\gamma^{balanced}=\left(\frac{m\cdot E\left[Var\left(\overline{Y}_s^{balanced}\right)\right]}{E\left[Var\left(Y_{is}^{balanced}|s\right)\right]}\right)^{\frac{1}{2}}-1\leq 0.$$

Proof. For any section assignment scheme, standard ANOVA results show that the total sum of squares can be exactly decomposed into the between and within sum of squares:

$$TSS=BSS+WSS,$$

$$\sum_{s=1}^{k}\sum_{i=1}^{m}\left(X_{is}-\overline{X}\right)^2=m\sum_{s=1}^{k}\left(\overline{X}_s-\overline{X}\right)^2+\sum_{s=1}^{k}\sum_{i=1}^{m}\left(X_{is}-\overline{X}_s\right)^2.$$

Note that $E\left[\frac{1}{n}BSS\right]=E\left[Var\left(\overline{X}_s\right)\right]$ and $E\left[\frac{1}{n}WSS\right]=E\left[Var(X_{is}|s)\right]$, so $E\left[Var\left(\overline{X}_s\right)\right]$ and $E\left[Var(X_{is}|s)\right]$ are inversely related under any assignment section assignment scheme because TSS remains fixed. First, consider lotteried sectioning. Independence of X_{is} implies that the variance ratio of ex ante student characteristics equals unity:

$$\frac{m\cdot E\left[Var\left(\overline{X}_s^{lottery}\right)\right]}{E\left[Var\left(X_{is}^{lottery}|s\right)\right]}=1.$$

Letting $\mu\equiv E[X_{is}]$, Assumption 2 and the delta method imply that variance ratio for outcomes Y also equals unity:

$$\frac{m\cdot E\left[Var\left(\overline{Y}_s^{lottery}\right)\right]}{E\left[Var\left(Y_{is}^{lottery}|s\right)\right]}=\frac{f'(\mu)^2 m\cdot E\left[Var\left(\overline{X}_s^{lottery}\right)\right]+\sigma_{\varepsilon}^{2}}{f'(\mu)^2 E\left[Var\left(X_{is}^{lottery}|s\right)\right]+\sigma_{\varepsilon}^{2}}=1.$$

Therefore, measures of peer effects under lotteried sectioning equal zero in expectation: $E\left[\delta^{EVM\_lottery}\right]=E\left[\gamma^{lottery}\right]=0$. Now consider balanced sectioning. Assumption 1 implies that

$E\left[\frac{1}{n}BSS^{balanced}\right] \leq E\left[\frac{1}{n}BSS^{lottery}\right]$, that is, $E\left[Var\left(\overline{X}_s^{balanced}\right)\right] \leq E\left[Var\left(\overline{X}_s^{lottery}\right)\right]$. The sum of squares decomposition implies that $E\left[Var\left(\overline{X}_s^{balanced}\right)\right]$ and $E\left[Var\left(X_{is}^{balanced}|s\right)\right]$ are inversely related. Therefore,

$$\frac{m \cdot E\left[Var\left(\overline{X}_s^{balanced}\right)\right]}{E\left[Var\left(X_{is}^{balanced}|s\right)\right]} \leq 1.$$

Let $\Sigma_s^{balanced}$ be the $m \times m$ covariance matrix for X_{is} within section s under balanced sectioning. The above result, along with Assumption 2 and the delta method, imply that the variance ratio for ex post outcomes under balanced sectioning is weakly less than unity:

$$\frac{m \cdot E\left[Var\left(\overline{Y}_s^{balanced}\right)\right]}{E\left[Var\left(Y_{is}^{balanced}|s\right)\right]} = \frac{f'(\mu)^2 \cdot m E\left[Var\left(\overline{X}_s^{balanced}\right)\right] + \sigma_\varepsilon^2}{f'(\mu)^2 \cdot E\left[Var\left(X_{is}^{balanced}|s\right)\right] + \sigma_\varepsilon^2} \leq 1.$$

Therefore, under balanced sectioning, the expected measures of peer effects are weakly negative in the absence of true peer effects: $E\left[\delta^{EVM\_balanced}\right] \leq 0$ and $E\left[\gamma^{balanced}\right] \leq 0$. ■

A.2 Selection into the executive subsample

Only a subset of HBS MBA graduates become top executives who appear in the sample of S&P 1500 firms covered in the ExecuComp data. Given that HBS students are randomly assigned to sections, selection of students into the executive subsample can be a true peer effect, operating through "past" social interactions.

Baseline results in Section 3.2 cannot separately identify peer similarities that are the result of

1. contemporaneous interactions, that is, interactions occurring while executives manage firms,
2. past interactions, that is, similar people within each section select into the executive subsample, and/or section peers are more likely to enter into similar types of firms, industries, etc., and
3. section-specific common shocks, for example, a professor shock.

Both contemporaneous and past interactions represent true peer effects but only contemporaneous interactions represent the causal impact of executives on firm policies. Tests involving reunions and lucky shocks to peers presented in Sections 3.3–3.4 use variation in peer similarities over time along with exogenous shocks to isolate peer effects due to contemporaneous interactions.

A.2.1 Stage 1: Random assignment to HBS sections. Consider the full class of students. Let Y_{isc} be the ultimate outcome of interest for person i in section s in class year c. For brevity, I omit the time subscript for firm fiscal year t. Let υ_{isc} be the latent fundamental determinant of Y_{isc} (e.g., if Y_{isc} is executive compensation, then υ_{isc} is the exogenous managerial skill of person i that would determine compensation if person i becomes an executive). Random sectioning guarantees:

$$\upsilon_{isc} \sim iid \text{ (within each class year } c\text{)}.$$

A.2.2 Stage 2: Selection into executive roles at firms. A subset of HBS graduates become top executives in S&P 1500 firms covered by the ExecuComp database according to the following selection rule:

$$f\left(\upsilon_{isc}, \overrightarrow{\mathbf{v}}_{-i,sc}\right) \geq \underline{\upsilon} \quad iff \text{ student } i \text{ becomes an executive.}$$

Let ω_{isc} represent the intermediate fundamental determinants of outcomes Y_{isc} as of Stage 2. ω_{isc} is a function $g(\cdot)$ of individual fundamentals υ_{isc} (conditional on i becoming an executive) as well as firm fundamentals ε_{isc}, for example, the firm's q and industry characteristics:

$$\omega_{isc} = g\left(\upsilon_{isc} | f\left(\upsilon_{isc}, \overrightarrow{\mathbf{v}}_{-i,sc}\right) \geq \underline{\upsilon}, \varepsilon_{isc}\right).$$

Unlike the base fundamentals, υ_{isc}, intermediate fundamentals, ω_{isc}, need not be distributed *iid* within each class year. To the extent that ω_{isc} are not distributed *iid*, this could be due to past peer interactions or common shocks leading (1) students with similar fundamentals υ_{isc} within each section to select into becoming executives and/or (2) students within each section to enter similar types of firms such that ε_{isc} is more similar within sections than across sections.

A.2.3 Stage 3: Executives choose firm policies. Now consider only the ExecuComp subsample, consisting of all students i for which $f\left(\upsilon_{isc}, \overrightarrow{\mathbf{v}}_{-i,sc}\right) \geq \underline{\upsilon}$. As in Section 2.3, I approximate optimal outcomes Y_{isc} as a linear function of mean section outcomes $\overline{Y}_{sc}$, mean section intermediate fundamentals $\overline{\omega}_{sc}$, and own intermediate fundamentals ω_{isc}.

$$Y_{isc} = \theta \overline{Y}_{sc} + \phi \overline{\omega}_{sc} + \rho \omega_{isc}, \quad Y_{isc} = \tau \overline{\omega}_{sc} + \rho \omega_{isc}, \quad \tau \equiv \frac{\phi + \theta\rho}{1-\theta}.$$

Applying variance restrictions as described in Section 2.3 yields the following:

$$\text{Peer Elasticity } \gamma = \left(1+\gamma^{contemp}\right)\left(1+\gamma^{past}\right) - 1,$$

$$\gamma^{contemp} \equiv \frac{\tau}{\rho}, \quad \gamma^{past} \equiv \left(\frac{m \cdot Var(\overline{\omega}_{sc})}{Var(\omega_{isc}|s)}\right)^{1/2} - 1.$$

Baseline measures of the peer elasticity γ will capture the joint effects of $\gamma^{contemp}$ and γ^{past}. $\gamma^{contemp}$ captures contemporaneous interactions. γ^{past} captures the extent to which intermediate fundamentals ω_{isc} are more similar within sections than across sections (γ^{past} can represent both past interactions and common shocks).

A.2.4 Isolating contemporaneous peer effects using reunions. I isolate $\gamma^{contemp}$ under the assumption that γ^{past} does not vary with the reunion schedule. Assume that reunions increase the base level of contemporaneous interactions $\gamma^{contemp}_{base}$ by the amount $\gamma^{reunion\_shock}$:

$$\gamma^{contemp}_{postreunion} = \gamma^{contemp}_{base} + \gamma^{reunion\_shock}.$$

Comparisons of measured peer elasticities in the year immediately following reunions with the peer elasticities in other years establish a lower bound for contemporaneous peer effects assuming that contemporaneous peer effects in nonreunion years are nonnegative

$$\frac{1+\gamma^{reunion}}{1+\gamma^{nonreunion}} = \frac{\left(1+\gamma^{contemp}_{postreunion}\right)\left(1+\gamma^{past}\right)}{\left(1+\gamma^{contemp}_{base}\right)\left(1+\gamma^{past}\right)} \leq 1+\gamma^{contemp}_{postreunion}.$$

The above ratio offers a lower bound because it assumes that $\gamma^{contemp}_{base} = 0$ even though positive contemporaneous interactions likely occur in nonreunion years.

A.3 Standard errors and significance levels

Estimation of standard errors and significance levels is complicated because observations in the pairs distance metric represent pairs of executives, so each executive can appear in multiple paired observations. In addition, estimates from the excess variance metric come from an ANOVA decomposition which does not generate an implied standard error. Finally, executive outcomes come from panel data which may exhibit serial correlation.

Estimation of standard errors and significance levels use a nonparametric permutation test in the style of Fisher (1922) and Rosenbaum (1996). Intuitively, the permutation test constructs a confidence interval of placebo estimates around the null hypothesis that section relationships don't matter and offers an estimate of how unlikely we are to observe the true point estimates by chance. I begin by estimating the vector for the parameters of interest $\hat{\beta}$ using the real data. Next, I conduct a Monte Carlo simulation of placebo effects. In each placebo test, students within each class year are randomly shuffled into placebo sections. The test is nonparametric in that the number of students assigned to each section follows the distribution of sections in the real data (e.g., if there are four students in section A and three students in section B in a given class year in the real data, this structure is maintained in each placebo test). Further, student assignment to sections remains the same across all panel observations in each placebo simulation; this accounts for serial correlation. In each placebo simulation, I re-estimate the vector of parameters $\hat{\beta}^{placebo}$. I simulate 10,000 placebo estimates to generate a standard error around the null hypothesis and $G(\cdot)$ as the empirical cumulative distribution function of the placebo effects. Applying $G(\cdot)$ to the original point estimates offers a *p*-value estimate of significance.

In supplementary tests using the pairs distance metric, I also estimate Equations (1b) and (1c) allowing for clustering of the error term separately by i and j following the double-clustering algorithm outlined in Cameron, Gelbach, and Miller (2011) and Peterson (2008). Significance levels estimated using double clustering are very similar to those from the permutation placebo tests.[16]

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[16] I focus on permutation tests because double-clustering standard errors cannot fully account for correlated error structures across paired observations. For example, consider three pairs of individuals: (A B), (A C), and (B C). Double clustering will allow for correlations between the first and second pairs and the second and third pairs, but not for the first and third pairs. I thank Mitchell Peterson for this observation.

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