Peer group comparisons and pay spillovers in the CEO labor market*

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Abstract

This paper examines the peer benchmarking process in executive compensation and its effects on CEO pay. Previous studies have shown that firms manipulate the benchmarking process to raise CEO pay by selecting high-paying peer firms. We study whether this peer group manipulation by one firm has effects on CEO pay at other firms. Using data on disclosed compensation peer groups from 2006 to 2023, we decompose peer compensation into a fundamentals component, captured by predicted peer-group pay, and a deviation component, captured by the peer pay effect (PPE). We then instrument average peer pay with the average PPE of those peers, isolating the component of peer compensation driven by their own strategic benchmarking. We find that pay changes at one firm propagate through the benchmarking network to other firms. Dyadic tests show that effects are significantly stronger for actual disclosed peers than for predicted but non-selected peers, consistent with transmission through the formal benchmarking network. Results are robust across first-differences, spatial autoregressive, and alternative peer-prediction specifications. Even firms with strong corporate governance and no apparent peer group manipulation see their CEO pay inflated by the actions of other firms. This implies that peer group manipulation need not reflect opportunistic rent-seeking to generate externalities: firms selecting higher-paying peers for arguably legitimate reasons, such as rewarding a well-performing manager, may inadvertently contribute to pay increases elsewhere in the network. The findings illustrate how benchmarking in imperfectly competitive markets can systematically raise compensation levels overall.

1 Introduction

Compensation committees routinely benchmark CEO pay to peer firms when designing pay contracts. Referencing peer firms allows boards to incorporate information about wages and outside options in the CEO labor market (Murphy and Zabojnik 2004; Gabaix and Landier 2008). Benchmarking also provides an observable justification for compensation decisions, helping address shareholder concerns about managerial rent-seeking or concerns from the broader public about inequality and fairness (Bebchuk et al. 2002; Hermanson et al. 2012).

While benchmarking is often described as a mechanism for incorporating objective information into compensation decisions, firms retain substantial discretion in the pay-setting process. A key source of this discretion lies in defining the group of peers that constitute the relevant comparison set. Existing research has highlighted evidence of systematic bias in the peer selection process. Firms show a tendency to select larger or higher-status peers with higher levels of pay (Faulkender and Yang 2010, 2013; Albuquerque et al. 2013; de Vaan et al. 2019), which contributes to upward pressure on their own pay. This strategic manipulation of peer groups could reflect an efficient response by corporate boards aiming to retain talented CEOs (Albuquerque et al. 2013; Schneider 2021) or opportunistic rent extraction by powerful managers (Bebchuk et al. 2002) or some combination of the two. The evidence on the rent extraction channel indicates that this opportunistic behavior is exacerbated by weak governance mechanisms (Faulkender and Yang 2010, 2013; Bizjak et al. 2011; de Vaan et al. 2019; Kalpathy et al. 2024).

These earlier findings raise important questions regarding the aggregate or spillover effects of this benchmarking practice. Because of the interconnected and overlapping nature of compensation peer groups, benchmarking creates interdependencies in pay-setting decisions. When a firm adjusts its peer group in ways that support higher pay, by adding larger firms or excluding lower-paying firms, this adjustment alters not only its own compensation benchmark but also the compensation reference points observed by other firms that benchmark against it. From this perspective, peer selection decisions may have consequences that extend beyond the selecting firm, potentially generating spillovers across firms linked through benchmarking relationships. And while stronger governance could help constrain a firm's own peer group manipulation, it may not fully insulate the firm from the effects of strategic behavior occurring within the firms in its peer group. The question is whether pay changes actually transmit through this channel, or whether firms recognize and filter them out.

In this paper, we examine whether changes in CEO pay at one firm propagate to other firms through the peer benchmarking network. We focus specifically on innovations to pay driven by the manipulation of benchmarking peer groups and analyze whether the corresponding effects on pay at one firm have spillover effects on other firms that are benchmarking against it. Empirically identifying pay spillovers is challenging due to an inherent endogeneity problem. CEO compensation is correlated across firms for many reasons that are unrelated to the benchmarking channel, such as common industry shocks, macroeconomic conditions, shared labor market trends, or compensation consultant overlap. This is especially true for firms that are connected through the benchmarking network, since firms are intentionally choosing peers they believe to be similar.

To analyze pay spillovers while addressing these concerns, we employ an instrumental variables strategy that isolates the component of CEO pay driven by strategic manipulation of peer groups. Building off the existing literature on peer benchmarking, we construct a firm-specific measure of peer group manipulation called the "peer pay effect" (Albuquerque et al. 2013). The peer pay effect (PPE) captures the difference in CEO pay between the set of peers actually chosen by the firm and pay at a group of predicted peers that we identify based on observable characteristics (following the approaches of Faulkender and Yang 2010 and Bizjak et al. 2011). A positive PPE indicates that the firm has selected peers with higher pay than would be expected based on observable characteristics. To test for evidence of spillovers through the benchmarking network, we instrument average pay in a firm's peer group using the average PPE *of those peer firms* and then study the impact on the focal firm's level of pay. The key identifying assumption required to support a causal interpretation is that, conditional on observables, the extent to which a peer firm manipulates its own peer group is uncorrelated with unobserved determinants of pay at the focal firm. Intuitively, while firm A may select firm B as a peer because the two firms share certain characteristics, the degree to which firm B has strategically chosen *its own* peers should be unrelated to firm A's unobservables.

We find that the average PPE of a firm's peers is highly correlated with that firm's own level of pay, indicating that peer group manipulation at one firm is positively associated with CEO pay at other firms that benchmark against it. In terms of magnitudes, our preferred IV specifications indicate that a 10% increase in average pay at a firm's peers—driven by their own strategic benchmarking behavior—is associated with an increase in the focal firm's pay of between 5.1-8.8%. We show that these results are not driven by peer group manipulation at the focal firm itself. Our results remain robust across a range of alternative methods for selecting peer groups, including using a set of non-parametric machine-learning classification models to construct predicted groups. We also examine the sensitivity of the results to the specific identifying assumptions by estimating several alternative specifications, including: first-differences panel regressions that exploit changes in peers' PPE over time; dyad-level regressions that compare the spillover effects of chosen peers to similar firms that are not selected; and spatial autoregressive models that exploit the network structure for identification of pay spillovers. Across all of these different specifications, we find consistent evidence of pay spillovers through the benchmarking channel.

We also investigate potential mechanisms underlying these effects. We find some evidence suggesting that spillovers may be larger for focal firms with weaker protection for shareholder rights, though these effects are not always statistically significant. If the spillovers are indeed higher among firms with weaker governance, it would suggest that the spillovers could be exacerbated by managerial opportunism at the focal firms. Likewise, some of the effect could be driven by a labor market competition channel in the presence of imperfect information, whereby firms with stronger governance are better able to filter out the effects of strategic manipulation by their peers, regardless of whether those peers are motivated by opportunistic rent-seeking or the desire to retain talented CEOs. Nonetheless, we continue to find significant pay spillovers even for firms with strong governance characteristics, providing evidence in support of the benchmarking channel in which anchor points or norms play a role in determining pay levels.

Our results suggest that the consequences of determining CEO pay extend beyond the level of a single firm. A firm that selects a higher-paying peer group in order to justify higher pay for its own CEO does not internalize the impact this has on raising the reference points for other firms in the benchmarking network. And while governance mechanisms may constrain a firm's own peer manipulation, they do not appear to insulate the firm from externally-generated changes in its benchmarks. Cross-firm externalities are particularly consequential if CEO labor markets are imperfectly competitive, because in the absence of a single market-clearing wage, external reference points do not merely reveal pay levels, they help determine them.

The remainder of the paper proceeds as follows. Section 2 provides a brief review of the related literature and our contributions. Section 3 describes our data sources. In Section 4, we describe our empirical strategy, identifying assumptions, and potential threats to validity. In Section 5, we present our main results, along with a discussion of several alternative specifications and robustness checks. Section 6 concludes.

2 Literature Review

A large literature examines the role of peer benchmarking in executive compensation and its implications for CEO pay. Prior research documents that firms exercise considerable discretion in constructing their compensation peer groups and that this discretion is systematically associated with higher pay. Faulkender and Yang (2010) and Bizjak et al. (2011) show that firms tend to select peers that are larger or higher-paying than themselves. Albuquerque et al. (2013) interpret this pattern as reflecting compensation for unobserved CEO talent, while Cadman and Carter (2014) show that certain economic determinants can explain much of the bias. More recent work emphasizes the persistence of upward bias in peer selection even in the presence of disclosure requirements (de Vaan et al. 2019) and highlights the role of aspirational benchmarking in driving compensation outcomes (Schneider 2021; Kalpathy et al. 2024). Across these studies, the analytical focus is at the level of the firm making the decisions about peer selection. Our paper draws on this literature but shifts the focus to whether the consequences of benchmarking decisions at one firm extend to others.

Several existing studies examine the topic of spillovers in executive compensation and find evidence consistent with cross-firm transmission of compensation shocks. Bereskin and Cicero (2013) show that governance changes associated with Delaware law reforms led to broader increases in CEO pay, including at firms not directly affected by the reforms. Shue and Townsend (2017) show that higher pay at firms that grant a fixed number of options (rather than a fixed dollar value) resulted in higher executive pay at predicted peer firms.[1] These studies identify patterns consistent with pay spillovers, but the underlying channels remain ambiguous, leaving open the extent to which observed spillovers operate through the benchmarking network and whether they can be explained by broader labor market or competitive forces.

More recent work provides direct evidence that compensation benchmarking plays a role in facilitating pay spillovers. Two studies that are closely related to ours are the papers by Çolak et al. (2017) and Denis et al. (2020).[2] Both papers exploit the peer benchmarking relationships disclosed by firms and show that shocks to pay at some firms spill over onto other firms through the benchmarking network. Denis et al. (2020) show that negative say-on-pay outcomes at one firm are associated with reductions in compensation at firms that

[1] In addition to these empirical studies, Gabaix and Landier (2008), Acharya and Volpin (2010), and Dicks (2012) all present models where pay increases at a subset of firms can affect pay at other firms.

[2] DiPrete et al. (2010) also examine spillovers related to compensation benchmarking. Like us, they argue that strategic behavior by some firms to increase their pay results in higher pay at other firms, though their analysis uses imputed peer groups and relies on simulations.

include it in their peer groups, consistent with boards updating compensation practices in response to governance signals. Çolak et al. (2017) document that increases in CEO pay following a firm's addition to the S&P 500 index also contribute to increases in pay at other firms that are connected through the peer network.

Our distinct contribution lies in the nature of our shock. In these earlier studies, the underlying sources of variation are discrete, externally-generated events, and the shocks are arguably salient enough to influence perceptions of market conditions. Our analysis builds on this literature by examining whether routine pay-setting behavior at one firm influences pay at other firms. We highlight the role of peer benchmarking not just as a propagation mechanism for these spillovers but also as the source of pay shocks. We show that pay changes attributable to idiosyncratic manipulation of peer groups, rather than discrete external shocks, give rise to systematic spillovers in CEO compensation. This distinction allows us to move from an event-based view of compensation contagion to a more general characterization of benchmarking as a continuous mechanism through which compensation norms are transmitted across firms. The pay spillovers in our context are difficult to explain through a pure learning channel or competitive labor market conditions alone; instead, they highlight that firms have substantial discretion in determining compensation levels.

These results contribute to the broader debate on the determinants of executive compensation. Many existing theories of executive compensation implicitly share a common assumption: that benchmarking helps reveal an underlying market wage. This includes theories of efficient contracting, in which pay reflects market forces for managerial talent (Gabaix and Landier 2008), as well as theories of managerial power, in which compensation reflects rent extraction by executives (Bebchuk et al. 2002). Our analysis shifts attention from firm-level governance failures to systemic issues of governance and market structure. We draw on the broader literature in labor economics emphasizing that wage-setting in imperfectly competitive markets is not fully determined by market-clearing conditions (Manning 2003; Card 2022). In such settings, reference points and peer comparisons play an important role in shaping outcomes, as workers and firms evaluate compensation relative to observed wages elsewhere (Akerlof and Yellen 1990; Card et al. 2012). And in the context of rent-sharing between firms and workers (Card et al. 2018; Kline et al. 2019), relative bargaining power determines the division of the surplus. While this literature focuses primarily on rank-and-file workers, many of the insights extend to the CEO labor market, which is likewise characterized by pervasive market failure, relative comparisons, and fairness concerns (Bebchuk et al. 2002; Hermanson et al. 2012; Edmans et al. 2017; Cziraki and Jenter 2022; Edmans et al. 2023). Viewed in this way, our findings suggest that benchmarking may play a more fundamental role in shaping compensation outcomes by influencing the reference points and norms that guide pay-setting decisions.

3 Data Description

Our main source of data is the ISS Incentive Lab academic dataset, which contains information on executive compensation and peer comparison groups for about 1000 firms per year for the years 2006 through 2023. For most peer firms, the dataset contains the CIK (the SEC's central index key) and ticker symbol, in addition to the name of the peer firm. For cases where only the peer's name is available, we attempt to merge in the CIK by matching on the names. Because our main analysis requires information on the CEO compensation and peer group of each firm and of each of its peers, we restrict the final dataset to only those firm-peer pairs for which both the firm and the peer are contained in the original dataset. This means we drop firm-peer pairs for which no information is available on the peer group of the peer firm. Therefore our dataset does not include the universe of all peers for every firm.[3]

For those explanatory and control variables not included in the ISS database, we obtain information from Compustat and CRSP and merge the data based on CIK and fiscal year.

[3] See Appendix Figure A1 for a graphical representation of the benchmark compensation network for a single year within our sample.

This includes data on industry, sales, assets, market cap, index membership, return on assets, market-to-book ratio, and geographic and business segments.

As part of our investigation into potential mechanisms, we also use data on the Entrenchment Index (or E index) developed by Bebchuk et al. (2008), which we use as a proxy for the quality of corporate governance.[4] The E index operationalizes corporate governance as a function of managerial entrenchment. Six measures comprise the index: the presence of staggered boards, poison pills, golden parachutes, and limits to the ability of shareholders to propose bylaw amendments as well as supermajority requirements for amending the corporate charter and for merger proposals. Each of these measures is associated with more entrenched managers and negatively impacts firm governance.

4 Empirical Framework

In order to understand the effect of pay decisions at one firm on pay at other firms, we are interested in estimating a peer effects model of the following type:

$$y_i = \beta_0 + \beta_1 \bar{y}_{B(i)} + \beta_2 X_i + u_i \tag{1}$$

where y_i represents the log of total CEO compensation at firm i, X_i includes a set of firm characteristics, and $\bar{y}_{B(i)} = \frac{1}{n_i} \sum_{j \in B(i)} y_j$ is the average compensation across the n_{i} peer firms included in firm i's benchmarking peer group $B(i)$. We are interested in obtaining a consistent estimate of β_1, representing the elasticity of CEO pay with respect to pay at peer firms. Unfortunately, using OLS to estimate the above equation is unlikely to yield an unbiased estimate of the causal effect of $\bar{y}_{B(i)}$, since there are multiple reasons to believe that the error term in the above equation is correlated with $\bar{y}_{B(i)}$. For one, firms are explicitly choosing peers that they believe to be similar in many ways, causing many firm characteristics to be correlated across peers; it is likely that some of these characteristics have an effect on pay but are not included in X_{i}, leading to omitted variables bias. In addition, some of

[4] A link to download these data is available at https://pcg.law.harvard.edu/data/.

the firms in i's peer group may themselves be benchmarking their own pay to firm i; this reciprocal benchmarking may contribute to additional simultaneity bias.

To address this identification challenge, we employ an instrumental variables (IV) strategy where we instrument for pay at a firm's peers using the "peer pay effects" of those peer firms. The peer pay effect (PPE) measures the degree to which a firm's chosen peer group differs from a peer group that we select based on observable characteristics. A higher value for the PPE means that the firm chose higher-paying peers than expected. If firms are selectively choosing higher-paying peers in order to justify higher pay for their own CEO, we would observe a positive first-stage relationship between a firm's CEO pay and its PPE. Our IV strategy aims to detect whether this first-stage effect spills over onto other firms via the benchmarking network. The key identifying assumption is that while firm A's decision to benchmark to firm B may reflect similarities between them, the extent to which firm B manipulates its own peer group is unrelated to firm A's unobserved characteristics.

In the following subsections, we first describe how we construct predicted peer groups for each firm in the sample, and we use these predicted groups to construct each firm's peer pay effect. Then we present our main estimating equation, which uses the PPE at peer firms as an instrument for pay at those firms. We also discuss potential threats to validity and briefly outline robustness checks and alternative estimation strategies.

4.1 Constructing predicted peer groups

Our identification strategy relies on innovations to the benchmarking network driven by differences between the peer groups actually chosen by firms and the set of expected peer groups that we predict based on observable characteristics. To determine the predicted peer groups for our baseline specifications, we follow Faulkender and Yang (2010) and estimate the following probit specification for all possible (i, j) firm pairs:

$$\text{chosen}_{ijt} = \gamma_0 + \gamma_1 X_{ijt} + \gamma_2 n_{it} + \epsilon_{ijt} \tag{2}$$

where the dependent variable is an indicator for whether firm i has chosen firm j in its peer comparison set in year t. X_{ijt} contains a series of indicator variables representing the following relationships between firms i and j: same 2-digit industry, same 3-digit industry, sales within 50-200% of each other, assets within 50-200%, market cap within 50-200%, both in Dow 30 index, both in S&P 400, both in S&P 500, both have CEO who is also board chair, both have CEO who is not board chair, and an indicator for recent top-5 executive talent flows between the firms. n_{it} measures the total number of firms in firm i's peer comparison group.[5]

After estimating the model, we use Equation (2) and the corresponding parameter estimates to obtain the predicted probability of firm j being chosen as a peer by firm i, for all firms in our dataset (i.e., for all $j \neq i$). We then select the n_i firms with the highest predicted probability of being chosen, where n_i is the number of peers in firm i's actual peer group. This set of firms represents the *predicted peer group* for firm i in year t. By construction, the number of peers in each firm's predicted group matches the number of peers in its actual group.

We also estimate a version of Equation (2) that includes the log CEO compensation at potential peer j as an additional explanatory variable. The corresponding coefficient estimate is positive and highly significant, indicating that a higher level of CEO pay increases the likelihood of firm j being selected in another firm's peer group, which is consistent with the findings of Faulkender and Yang (2010) and Bizjak et al. (2011).[6]

For our baseline results, we estimate Equation (2) over rolling three-year windows, such that the predicted groups and PPE for year t are based on data from years $t-3$ through $t-1$, but the results are similar if we estimate a single specification for the entire sample period.

[5] The estimates from this specification are displayed in Appendix Table A1. The sample in columns 1 and 2 includes data from fiscal years 2006 and 2007 only, which corresponds to the period examined by Faulkender and Yang (2010) for comparison purposes. Columns 3 and 4 includes data for our entire sample period spanning fiscal years 2006 through 2023.

[6] The results are presented in Appendix Table A1. We also estimate a specification interacting pay at potential peer j with year fixed effects to examine how this effect varies over time; the results in Appendix Figure A2 indicate this effect has remained relatively consistent over time.

In addition to the specification above, we use several alternative methods for predicting peer groups to examine the robustness of our results. We estimate the specification from Bizjak et al. (2011), which uses a different set of covariates to predict the peer groups. We also run the analysis using several different machine-learning algorithms. These analyses are discussed in more detail in Section 5.4.4, where we show that our findings remain consistent across all of these prediction methods.

4.2 The peer pay effect

Once we have determined the predicted peer group for each firm, we can compare it to the group that is actually selected. Following Albuquerque et al. (2013), we define the *peer pay effect* (PPE) for an individual firm as follows:

$$\text{PPE} = \frac{\text{actual peer group pay} - \text{predicted peer group pay}}{\text{predicted peer group pay}} \tag{3}$$

where *actual peer group pay* represents the median CEO pay among the group of peers actually selected by the firm, and *predicted peer group pay* represents the median CEO pay among the group of peers that we select based on observable characteristics. The greater the deviation in pay between these two groups, the greater the peer pay effect. A positive peer pay effect means that the firm has chosen a group of peers with higher pay than expected (and vice versa).

For some firms, the actual and predicted groups are identical (resulting in a PPE of 0), and for others, there is no overlap between the two groups. For most firms, however, the predicted peer group contains some but not all of the firms in the actual peer group (and vice versa). Figure 1 displays a plot of median CEO compensation and the peer pay effect over our sample period. The median PPE is positive in every year in our sample, indicating that firms are systematically choosing peers with higher levels of compensation than expected. That is, when firms select peers in a way that deviates from our predictions, the chosen peers are more likely to have higher levels of pay relative to the predicted-but-not-selected peers.[7]

[7]The distribution of PPE values is displayed in Appendix Figure A3, and additional descriptive statistics

The figure also shows a steady decline in PPE over the first 10 years of the sample, which coincides with the time period immediately following the 2006 rule change mandating peer group disclosure. These results are consistent with the findings of de Vaan et al. (2019), who show that the bias in peer group selection has declined over time while the effect of this bias on pay has increased; the figure is also consistent with the findings of Jochem et al. (2025) that cross-firm pay dispersion has declined in recent years.

4.3 The first stage relationship: modeling pay as a function of PPE

Before outlining our estimating equations, we first show how the first-stage relationship between a firm's CEO pay and its peer pay effect can be derived from a simple model of compensation benchmarking. Suppose that CEO compensation at any particular firm is influenced by the characteristics of the firm and the individual CEO, along with external factors related to the CEO labor market. Since firms do not have full information about the characteristics of the CEO labor market, they instead choose a proxy for this information: the pay contracts at a set of peer comparison firms. We approximate the pay-setting function at firm j using the following equation:

$$\log(\text{pay})_j = \delta_0 \log\left(\text{actual peer group pay}_j\right) + \delta_1 X_j + \text{error}$$

where our measure of firm j's peer group compensation, *actual peer group pay*, is the median CEO compensation among its chosen peers, and X_j includes additional firm-level characteristics.

From the definition of the peer pay effect in Equation (3) above, we can decompose *actual peer group pay* into a function of *predicted peer group pay* and the *peer pay effect*:[8]

$$\text{actual peer group pay} = (1 + \text{PPE}) \cdot \text{predicted peer group pay} \tag{4}$$

are presented in Appendix Table A2.

[8] Since actual $\cdot \frac{\text{predicted}}{\text{predicted}} = \left(1 + \frac{\text{actual}-\text{predicted}}{\text{predicted}}\right) \cdot \text{predicted} = (1 + \text{PPE}) \cdot \text{predicted}$.

Substituting Equation (4) into the pay-setting function above, and using the approximation $\log(1 + \text{PPE}) \approx \text{PPE}$, we arrive at the following equation for CEO pay at firm j:

$$\log(\text{pay})_j = \gamma_0 \text{PPE}_j + \gamma_1 \log\left(\text{predicted peer group pay}_j\right) + \gamma_2 X_j + e_j \tag{5}$$

Here we are modeling CEO pay at firm j as a function of median pay in firm j's predicted peer group and firm j's own peer pay effect. Ultimately, we are interested in the impact of pay at firm j on pay at firm i, where firm i has selected j in its peer group, so the relationship in Equation (5) is part of the *first stage* of our empirical strategy.

The coefficient estimates for this relationship are shown in Table 1. We show results for multiple specifications with different combinations of covariates. The simple bivariate relationship in column 1 indicates that the firm's peer pay effect (PPE) is only weakly correlated with its CEO pay. But once we condition on the firm's predicted peer group pay, both the magnitude and significance of the PPE effect increase substantially: the estimates in column 2 indicate that pay at an individual firm is strongly related to its own peer pay effect (PPE), a result that is in line with the existing literature.[9] Firms that select a peer group with higher average pay than their predicted peer group tend to have higher pay themselves. We continue to observe strongly significant effects after controlling for additional firm-level characteristics (column 3) and industry fixed effects (column 4). In the subsequent analyses below, our aim is to examine the relationship between pay at one firm and pay at its peers; we do so by making use of this result that pay at a peer firm is strongly influenced by that peer firm's PPE.

4.4 Estimating equation

Equation (5) models pay at an individual firm j as a function of its own PPE, its predicted peer group pay, and firm-level characteristics. Since our goal is to estimate the effect of *peer*

[9] Both PPE and predicted group pay are positively correlated with own pay, but negatively correlated with each other. So omitting the predicted group pay would be expected to bias the estimate on PPE downward.

pay on a focal firm's compensation, we aggregate Equation (5) across all firms in firm i's peer group $B(i)$ to obtain our main estimating framework. Specifically, our two-stage empirical setup is as follows:

$$y_i = \beta_0 + \beta_1 \bar{y}_{B(i)} + \beta_2 X_i + u_i \tag{6a}$$

$$\bar{y}_{B(i)} = \gamma_0 + \gamma_1 \frac{1}{n_i} \sum_{j \in B(i)} \mathrm{PPE}_j + \gamma_2 X_i + \epsilon_i \tag{6b}$$

for all firms i and j such that firm i has selected firm j to be in its benchmark compensation peer group $B(i)$. The dependent variable y_i is the log of total CEO compensation at firm i, and $\bar{y}_{B(i)} = \frac{1}{n_i} \sum_{j \in B(i)} \log(\mathrm{pay}_j)$ represents average log pay at the firms in i's peer group. In order to obtain a consistent estimate of β_1, we instrument $\bar{y}_{B(i)}$ using $\overline{\mathrm{PPE}}_{B(i)} = \frac{1}{n_i} \sum_{j \in B(i)} \mathrm{PPE}_j$, the average peer pay effect across the firms in i's peer group. Equation (6b) follows from averaging the firm-level relationship in Equation (5) across the members of $B(i)$. The vector X_i represents a set of control variables that includes the average predicted peer group pay of firms in $B(i)$, firm i's own PPE, additional firm-level characteristics, and industry fixed effects. The identifying assumption is that, after conditioning on observable characteristics, the average PPE of firm i's peers is uncorrelated with unobservable factors contained in u_i. To address potential simultaneity arising from reciprocal benchmarking, we exclude firm i from the calculation of j's PPE where applicable.

We also estimate reduced form specifications that regress pay at firm i directly on the instrument:

$$y_i = \eta_0 + \eta_1 \overline{\mathrm{PPE}}_{B(i)} + \eta_2 X_i + e_i \tag{7}$$

The coefficient of interest is η_1, which captures whether a focal firm's CEO pay is related to the degree of peer group manipulation at its peers. The reduced form specification provides a direct and transparent test for the existence of pay spillovers. Relative to the IV specification, it is also more easily adapted to the alternative specifications discussed below.

4.5 Threats to validity

Our identification strategy relies on the assumption that the average PPE of a firm's peers is uncorrelated with unobserved determinants of the firm's own pay. After presenting our main results below, we also provide results from several alternative specifications that partially address potential threats to the validity of this assumption. The robustness of our results across different identification strategies lends support for a causal interpretation.

A key concern is that the similarity between connected firms extends to unobserved characteristics that may be correlated with a peer's PPE. To probe the extent to which correlated unobservables can explain our findings, we estimate two additional sets of specifications. First, we estimate dyadic regressions at the (i, j) pair level that include both actual and predicted peers in the estimation sample. This allows us to compare the estimated effect of a potential peer's PPE depending on whether that firm was actually chosen or merely predicted as a likely peer. If the association between own pay and peer PPE is driven entirely by correlated unobservables, we would expect similar estimates for both groups; a significantly larger effect for actual peers would be consistent with a causal spillover operating through the benchmarking network. Second, we estimate a spatial autoregressive (SAR) model that exploits the structure of the benchmarking network itself as a source of identification, providing a complementary approach to identification. We also combine the spatial-weighting estimator with the PPE-based instrument to estimate standard errors that account for spatial correlation in the benchmarking network.

A second concern is that a firm's own strategic selection of peers could bias the results. A firm seeking to justify higher CEO pay may deliberately choose peers with high levels of compensation and, therefore, potentially high PPE, creating a correlation between own pay and peer PPE that reflects the firm's own manipulation rather than a spillover from its peers. We address this concern in several ways. In our main specifications, we can explicitly control for the focal firm's own PPE and also explore how the estimated pay spillovers vary with the degree of the focal firm's own peer group manipulation. In addition, we estimate an alternative specification that replaces the average PPE of the firm's chosen peer group with that of its predicted peer group. Because the predicted group is determined entirely by observable characteristics and is not influenced by the firm's own selection decisions, this specification helps to remove bias introduced by the firm's strategic choice of peers.

5 Results

We begin by presenting our main reduced form and instrumental variables results, which provide evidence that peer group manipulation at one firm spills over to affect CEO pay at other firms. We then turn to a series of alternative specifications designed to probe the robustness of these findings and the extent to which they can be interpreted as causal, following the empirical strategies outlined in Section 4.5. Finally, we examine potential mechanisms through which these spillover effects may operate.

5.1 Main Results

Table 2 presents results from our main reduced form specification, where we regress CEO pay at firm i directly on the average PPE of its chosen peers. In column 1, we control for the average *predicted peer group pay* of firm i's chosen peers, along with year fixed effects. The coefficient estimate of 0.528 indicates that a 10 percentage point increase in the peer group's average PPE is associated with an increase in the focal firm's CEO compensation of approximately 5.3 percent.[10] Subsequent columns include additional control variables: in column 2, we control for the focal firm's own PPE, and columns 3 and 4 add controls for additional firm-level characteristics and 3-digit industry fixed effects. Across these specifications, the estimated coefficient on the peer group's average PPE remains positive and highly significant. In our preferred specification (column 4), which includes the full set of controls,

[10] Recall that the PPE is defined as a percentage relative to predicted pay: a PPE of 0.1 means that actual peer group pay is 10% higher than predicted peer group pay (Equations 3 and 4). So the reduced form parameter estimates can be interpreted as elasticities.

the estimated coefficient is 0.419.[11]

In Table 3, we present results from the instrumental variables specification, where we instrument average pay in a firm's peer group using the average PPE of those peers.[12] These results provide an estimate of the elasticity of own pay with respect to peer pay. Across all specifications, the estimates are highly significant and the magnitudes suggest a high degree of correlation in pay across firms. In our preferred specification in column 4, the estimates suggest that a 10% increase in average peer group pay—driven by manipulation of the peer groups of those peer firms—leads to an 8.8% increase in pay at the focal firm. Comparing the specification in column 1 to the specification in column 4 that includes the full set of controls, the magnitude of the estimated effect is very similar. In contrast, the results from uninstrumented (and likely endogenous) versions of this regression are far more sensitive to inclusion of controls: adding control variables causes the magnitude to fall substantially, from 0.89 to 0.34 (Appendix Table A5).

To the extent that the PPE reflects manipulation of the benchmarking process, the results in Tables 2 and 3 suggest that pay manipulation by one firm has spillover effects on pay levels at other firms that have chosen that firm in their peer group. These spillovers represent an unintended consequence of the benchmarking process: pay decisions that may be locally justified at one firm can propagate through the network and raise pay levels at other firms. This also suggests that even if governance mechanisms successfully constrain peer group manipulation at a given firm, that firm may still be affected by manipulation occurring elsewhere in the benchmarking network.

[11] We also examine whether the estimated effects vary over time by interacting average peer PPE with year fixed effects (Appendix Figure A4) and by restricting the analysis to different time periods (Appendix Table A3). We observe significant effects throughout the sample period with no clear trend over time.

[12] The table also reports first-stage F statistics for tests of the excluded instrument (average peer group PPE). The complete first-stage regression estimates are displayed in Appendix Table A4.

5.2 Peer selection bias and controlling for own-firm manipulation

As discussed in Section 4.5, a potential concern with our baseline results is that a firm's own strategic selection of peers could generate a spurious correlation between own pay and peer PPE. A firm with high PPE may be seeking to justify higher CEO pay by deliberately choosing peers with high levels of compensation, and those highly-paid peers may have high PPEs themselves. The firm's own manipulation could therefore lead to a positive correlation between its pay and the PPE of its peers, unrelated to any spillover effects.

In our preferred specification in Table 2 above, we control directly for the focal firm's own PPE. Including this control has very little effect on the estimated coefficient on peer PPE: the estimate remains strongly significant and, if anything, increases slightly in magnitude. This suggests that our results are not driven by correlations across peer firms in their degree of peer group manipulation. In this section, we present additional analyses to further investigate this issue. Since the implications of pay spillovers across firms differ fundamentally from those of within-firm strategic manipulation, it is important to rule out the latter as an explanation of our findings.

First, we examine how the estimated peer spillover effects vary with the focal firm's own level of peer group manipulation. To do this, we include an interaction between the firm's own PPE and average peer PPE. Figure 2 plots the average marginal effect of peer PPE as a function of the firm's own PPE. A firm whose predicted and actual peer groups coincide would have an own PPE equal to zero; higher values reflect firms that have selected peers with higher pay than expected. The results show that the effect of peer PPE is positive and significant over the entire range of own PPE, even for firms with an own PPE of zero. In other words, the estimated spillover from peer manipulation is present regardless of whether the focal firm itself appears to be manipulating its own peer group.[13]

Second, we estimate an alternative specification that does not depend on the actual peer

[13] We find similar results using an alternative method for examining the heterogeneity of this effect. Instead of including a linear interaction term, we divide the focal firms into deciles based on their own PPE and estimate the effect of peer PPE by decile (Appendix Figure A5).

group selection of the own firm. Instead of using the average PPE of the *chosen* peer group, we calculate the average PPE of the firm's *predicted* peer group. Because the average firm ends up choosing some (but not all) of the firms in their predicted group, there is a strong correlation between the characteristics of the predicted group and actual group. But since the predicted group is determined based on observable characteristics and is not influenced by the firm's own peer selection, this specification helps to remove any bias introduced by the firm's strategic choice of peers.

The results of this analysis are presented in Table 4. The first two columns show the first-stage relationship between average CEO pay among the firm's *chosen* peer group and the characteristics of its predicted peer group, including the average PPE and predicted peer group pay of those peers. The significant coefficient on predicted peer PPE indicates that the degree of peer group manipulation among a firm's predicted peers is a strong predictor of pay levels among its actual chosen peers. Columns 3 and 4 display the reduced form estimates, which remain positive and highly significant, though smaller in magnitude than the estimates in Table 2 using the actual peer groups. This decline in magnitude could be due to two different reasons: the baseline estimates in Table 2 could be biased upward if we are not adequately controlling for strategic behavior by the focal firm; or the estimates in Table 4 could be attenuated due to the predicted group being a noisy proxy for the actual group. To distinguish between these explanations, we present results from the instrumental variables specifications in columns 5 and 6, where we instrument average CEO pay in the (actual) peer group using the PPE of the predicted peer group. These IV results are similar in magnitude to the estimates in Table 3, where the instrument was the PPE of the firm's chosen peers. The fact that we obtain similar estimates when instrumenting with characteristics of the predicted peer group, which is not influenced by the firm's own selection decisions, suggests that the extent of any upward bias in our baseline estimates due to the firm's own peer selection is small.

5.3 Chosen vs. non-chosen peers

A key concern with our identification strategy is that firms connected via the benchmarking network are likely to be similar in unobserved ways. If these unobserved characteristics are correlated with a peer's PPE, the positive association between own pay and peer PPE could reflect correlated unobservables rather than a causal spillover.

To investigate the extent to which this concern could explain our findings, we estimate dyadic regressions at the (i, j) pair level, where the dataset includes the peers actually chosen by the firm along with potential peers from the predicted group that were not selected. We can then estimate the effect of $\mathrm{PPE_j}$, the PPE of potential peer j, on the focal firm's CEO pay, allowing this effect to differ depending on whether the potential peer was actually chosen. Specifically, we interact $\mathrm{PPE_j}$ with $\mathrm{Actual_{ij}}$, an indicator for whether firm j is selected in firm i's actual peer group.

In our baseline specifications above, identification relies on comparing focal firms whose peers have different levels of average PPE, under the assumption that this variation is uncorrelated with unobservables after conditioning on X_{i}. The dyadic specification provides a complementary approach with a potentially more transparent counterfactual. Because predicted-but-not-chosen peers are observably similar to actual peers, they serve as a natural counterfactual allowing us to identify the effect of being connected through the benchmarking network. The identifying assumption here is different: rather than assuming that peer PPE is uncorrelated with unobservables, we assume that any bias from correlated unobservables is similar for actual and predicted-but-not-chosen peers. Under this assumption, a significantly larger effect of the PPE for actual peers could be attributed to the benchmarking link itself.

There are also some tradeoffs associated with this approach. Because we are exploiting the firm's choice of peers, the estimates could reflect strategic selection of high-paying, high-PPE peers. While we continue to control for the focal firm's own PPE, the current strategy and the one from Table 4 cannot be easily combined, since the latter removes the firm's actual peer selection from the analysis entirely. Another potential concern is that, given the network structure associated with these dyadic pairs, the conventional asymptotic standard errors could be biased or inconsistent. We address this concern by instead basing our inference on p-values that we estimate using randomization inference, where we randomize the Actual_{ij} indicator over the combined set of potential (actual plus predicted) peers.

The results are displayed in Table 5. The interaction between $\mathrm{PPE_j}$ and $\mathrm{Actual_{ij}}$ is positive and highly significant across all specifications, indicating that the estimated effect of a peer's PPE on the focal firm's pay is substantially larger when that peer is actually included in the benchmarking group.[14] We also see that the magnitude of the estimate is relatively unaffected when we control for the own firm's PPE (in column 3), suggesting that peer group manipulation on the part of the focal firm is unlikely to be driving the effect.

The positive coefficient for the base category indicates that there is a positive relationship between own pay and the PPE of predicted peers even when those peers are not actually selected (and the effect is statistically significant based on the asymptotic standard errors). This may reflect some residual endogeneity, where unobserved characteristics that are correlated across similar firms could generate a positive association between pay and PPE even in the absence of a direct benchmarking link. At the same time, the estimated effect is substantially smaller for these non-chosen peers relative to actual peers: in our preferred specification in column 4, the estimated effect of an actual peer's PPE is 0.23 ($= 0.163 + 0.067$), which is over 3 times larger than the effect for a predicted-but-not-chosen peer. In contrast, the differential effect is much smaller for the "fundamentals" component of peer pay: the coefficient on predicted group pay is only about 17% larger for actual peers than for non-chosen peers. This suggests that the differential effect for actual peers is specific to the manipulation component of peer pay, consistent with a spillover that operates through the benchmarking process rather than through broader similarities between firms.

[14]In contrast to the specifications above in Table 2, which included the average PPE across all of a firm's peers, the coefficients in this table correspond to the estimated effect of a change in PPE at *one* of the firms in the peer group.

5.4 Alternative specifications and robustness

In this section, we present several additional analyses that examine the robustness of our findings to alternative specifications, estimation methods, and sample definitions.

5.4.1 First-differences panel regressions

Our baseline specifications are estimated as pooled cross-sectional regressions with year fixed effects. Given the panel structure of the dataset, a natural extension might be to include firm fixed effects to control for time-invariant unobserved heterogeneity. However, one concern with this approach is that changes in a firm's peer group PPE over time are likely to be driven in large part by changes in the composition of the peer group, rather than simply reflecting changes in PPE within those peer firms. As a result, including firm fixed effects in our baseline specification could exacerbate the bias related to the firm's own strategic choice of peers. Instead, we estimate the model in first differences and use a consistent set of peers to construct each difference. Rather than differencing the average PPE across firm i's peer group between periods, we first compute the change in PPE within each peer firm j and then average these within-peer differences across the members of the peer group. As a result, the change in peer group PPE in each period represents the change in PPE within a consistent group of peers, ensuring that the differences do not reflect compositional changes in the peer group.

The results are displayed in Table 6. The reduced form estimates in columns 1 and 2 show that changes in PPE among a firm's peers are positively related to changes in the firm's CEO pay. For the IV estimates in columns 3 and 4, we instrument changes in average pay among the firm's peers using changes in their PPE.[15] The results from these IV regressions continue to show a positive and significant effect of pay at peer firms, though the magnitudes are smaller than the corresponding cross-sectional estimates (Table 3). One possible explanation is that the cross-sectional estimates reflect the cumulative effect of spillovers that propagate

[15] See Appendix Table A6 for the corresponding first stage estimates.

through the benchmarking network over time, while the first-differences estimates capture only the short-run impact of year-over-year changes in peer PPE. The estimated effects in Table 6 remain economically meaningful: the point estimate in column 4 implies that a 10% increase in average pay among a firm's peer group corresponds to an increase of 5.1% in the firm's own pay. The results from these panel regressions indicate that our main findings remain robust to specifications that difference out time-invariant unobservables.

5.4.2 Chosen vs. non-chosen peers: Restrict to RPE peers only

In this section, we estimate the dyad-level specification from Section 5.3 but use an alternative set of counterfactual peers. Previously we constructed a set of potential peers using the union of the firm's actual and predicted peer groups. In this section, rather than predicting the counterfactual group ourselves, we instead focus on a set of potential peer firms identified by the firm itself: the relative performance evaluation (RPE) peer group. In addition to the benchmark (BM) compensation peer groups that we have been analyzing, many firms also disclose a set of RPE peers used to evaluate CEO performance for incentive compensation purposes. While there is often substantial overlap between these two peer groups, they are not necessarily identical, such that some firms included in the RPE group are omitted from the BM group.[16] This allows us to compare the estimated spillover effect of a potential peer's PPE depending on whether that peer is also part of the firm's BM group.

We estimate dyadic regressions similar to those in Table 5. The sample of focal firms is restricted to only those firms that disclose a RPE peer group, and the set of "potential peers" is limited to only the firms appearing in the RPE group. We construct an indicator for whether the RPE peer (i.e., the potential BM peer) is also selected in the BM peer group. The results are presented in Appendix Table A7. Across all specifications, the interaction between the potential peer's PPE and the BM indicator is positive and highly significant. The results indicate that the pay spillovers are concentrated among peers connected through

[16]In our sample, approximately 22% of RPE peers are excluded from the BM compensation peer group.

the compensation benchmarking network, as opposed to simply reflecting general similarities between the firm and its RPE peers.

This specification relies on a different set of identifying assumptions than the dyadic regressions in Table 5, which used predicted-but-not-chosen peers as the counterfactual. Here, both the RPE and BM groups are chosen by the firm, so peers in both groups are likely to be similar to the focal firm along observable and unobservable dimensions. The differential effect for BM peers is therefore unlikely to be explained by unobserved similarities between the firm and its peers, providing further support for a causal interpretation of the spillover effect operating through the benchmarking channel. The estimated magnitudes are similar to those in Table 5 above, despite relying on different counterfactuals and identifying assumptions, suggesting that both approaches are identifying a consistent spillover effect.

5.4.3 Spatial autoregressive model

Our baseline model uses information about a firm's peers to construct characteristics of the peer group, but it does not explicitly account for the overlapping and interconnected nature of the network. Identification relies on the exclusion restriction with respect to peer PPE along with the fact that there is imperfect overlap across peer groups.

We complement this baseline approach with a spatial autoregressive (SAR) model that more explicitly exploits the structure of the benchmarking network as a source of identification. The SAR model estimates a peer effects specification in which a firm's CEO pay depends on the pay of the firms it is connected to through the benchmarking network. The key identifying assumption is that spillovers must follow the structure of the network: a firm's pay can directly affect only those firms that have chosen it as a peer, but these direct effects can propagate indirectly through the network to peers of peers, and so on.

We can represent the SAR model using the following equation:

$$y = \mathbf{W}y\beta_1 + \mathbf{X}\delta + \epsilon \tag{8}$$

where y is log CEO pay; $\mathbf{W}$ is the spatial weighting matrix, a row-normalized adjacency matrix for the benchmarking network; and $\mathbf{X}$ is a set of firm-specific control variables. The term $\mathbf{W}y$ is the "spatial lag" of the dependent variable, and the parameter of interest, β_1, identifies the spillover effect of peer pay on own pay. We estimate the model using the Generalized Spatial Two-Stage Least Squares (GS2SLS) estimator of Drukker et al. (2013), in which the exogenous variables in $\mathbf{X}$ are used in the identification of β_1.

Table 7 presents the results for fiscal year 2007. Because the network can change each year as firms update their peer groups, the model is estimated as a cross-section for a single year. (The corresponding results for additional years are presented in Appendix Tables A8 and A9.) Panel (a) displays the results of estimating Equation (8) using different combinations of the explanatory variables to identify the peer effects. Across all specifications, we continue to observe positive and highly significant effects of peer pay, with a 10% increase in peer pay associated with a 5–9% increase in own pay. In terms of magnitudes, the estimates in panel (a) can be compared to our earlier IV estimates, as they represent an elasticity of own pay with respect to the average pay at peer firms.

We also utilize the same GS2SLS estimator to confirm that our baseline results are robust to adjusting the standard errors to explicitly account for the spatial structure of the network. In panel (b), we estimate a reduced form specification relating pay at a focal firm to the PPE of its peers. We replace the spatial lag of pay in Equation (8) with the spatial lags of the peer pay effect and predicted group pay, which are included as exogenous regressors. There is no spatial autoregressive term in this specification. Instead, this specification is analogous to our baseline reduced form, except that we construct the peer group variables using the network weighting matrix and account for spatial correlation in the error term. We continue to observe positive and significant effects of peer PPE, confirming that our reduced form findings are robust to explicitly accounting for the network structure in the estimation.

5.4.4 Alternative peer group comparisons

Our baseline results rely on predicted peer groups constructed using the probit specification from Faulkender and Yang (2010). In order to examine whether our findings are sensitive to this particular modeling choice, we use several alternative approaches to construct the predicted peer groups. First, we use the same probit model from our baseline specification but pool the data for the entire sample period and estimate a single specification (instead of the rolling three-year windows used for our baseline results). Second, we replace the probit with a gradient boosted classification tree: after comparing a set of machine-learning classifiers (including logit, random forest, and gradient boosted trees), we selected this model based on out-of-sample predictive accuracy, measured as the fraction of actual peers appearing in the predicted group. Third, we use the logit specification from Bizjak et al. (2011), which includes a broader set of variables capturing industry linkages, relative performance, credit ratings, and organizational structure. Finally, we estimate a Lasso-penalized logistic regression over the combined set of covariates from both Faulkender and Yang (2010) and Bizjak et al. (2011), using five-fold cross-validation to select the regularization parameter.

We estimate our main reduced form specification using these alternative prediction methods and display the results in Appendix Table A10. Across all five columns, the estimated coefficients on average peer PPE remain highly significant and similar in magnitude, ranging from 0.42 to 0.48. These results indicate that our findings are not driven by the choice of a particular method to construct the predicted peer groups. Together with the results using RPE peers above, these alternative specifications also help address the concern raised by Cadman and Carter (2014) that our baseline model may omit important economic determinants of peer selection.

5.5 Potential mechanisms

Finally, we run additional specifications to examine potential channels through which these pay spillovers occur. We analyze two specific mechanisms: shared compensation consultants and governance characteristics at the focal firm.

Existing research has identified overlapping compensation consultants as a possible factor leading to increasing similarity in compensation packages across firms (Gallani 2016). Shared compensation consultants could be either a potential transmission channel for the spillovers we have identified or a source of unobserved heterogeneity biasing our estimated spillover effects. We find little evidence of either. To examine the potential effects of shared compensation consultants, we estimate dyadic specifications similar to those in Section 5.3, but we include an additional indicator for whether firms i and j use the same compensation consultant. We interact the *Shared Consultant* indicator with all of the right-hand-side variables and display the corresponding estimates in Appendix Table A11.

As before, the coefficient on PPE × Actual remains highly significant; this term represents the spillover effect for the base group of peer firms that do not share a compensation consultant with the focal firm. When we interact this term with the indicator for shared consultant, the estimated effect is positive in most specifications but small in magnitude and usually not statistically significant. We only observe a statistically significant coefficient in the specification that includes the complete set of control variables. In this specification, the estimated effect of a peer's PPE is 0.153 for the base group, very close to the estimate of 0.163 observed in Table 5; the corresponding estimated effect for peers using the same consultant as the focal firm is slightly higher at 0.195. Overall these results suggest that overlap between compensation consultants is not a main transmission channel for the spillover effects that we document.

We next examine whether the spillover effects vary with the quality of corporate governance at the focal firm. If the spillovers are partly driven by managerial opportunism, we might expect to see larger effects at firms with weaker governance, as entrenched managers take advantage of the inflated benchmarks to justify higher pay for themselves. We test for this channel by interacting the average PPE of the peer group with a proxy for the managerial power at the focal firm. Specifically, we use the Entrenchment Index (E index) of Bebchuk et al. (2008), where higher values of the index indicate greater managerial entrenchment and weaker shareholder protections. We construct an indicator variable, High-E-index$_i$, equal to 0 for firms with E index values of 0–2 and equal to 1 for E index values of 3–6 (a group that includes approximately 51% of firms in the estimation sample). Because the E index is most appropriate for firms without dual-class stock, we present results both excluding dual-class firms and including all firms. The E index data are available only through 2006, so we restrict the sample to fiscal years 2006–2010 to limit the time elapsed since the governance variables were measured.

The results are presented in Table 8. In the specification with the full set of controls (column 2), the interaction between the high-E-index indicator and the average PPE of peers is positive and marginally significant (at the 10% level, $p = 0.06$), suggesting that pay spillovers may be stronger for focal firms with weaker governance. The estimated magnitude of the effect of peer PPE for high-E-index firms is approximately twice as large as the effect for low-E-index firms. Depending on the specification, however, this interaction effect is not always significant, so we interpret these results as suggestive. If the spillovers are indeed larger for firms with weaker governance, it suggests that weak governance may amplify the transmission of pay spillovers. These firms could be less capable of filtering out the effects of manipulation at peer firms; alternatively, they may take advantage of the higher pay at peer firms in order to justify higher levels of pay for their own CEO.[17]

At the same time, the base effect of peer PPE (representing the effect for low-E-index firms) remains positive and significant across all specifications, suggesting that pay spillovers persist even for firms with stronger governance. These findings indicate that even firms with relatively strong shareholder protections are not insulated from the pay spillovers generated by peer group manipulation at other firms. The fact that the spillover effects are present even in the absence of apparent managerial entrenchment is consistent with a reference point

[17]Notably, the interaction between the E index and the average predicted group pay of peers is small and insignificant across all specifications, indicating that any differential effect of governance is specific to the manipulation component of peer pay rather than the fundamentals-driven component.

channel in which peer benchmarking helps to shape overall norms about pay across all types of firms.

6 Concluding Remarks

This paper documents evidence of cross-firm spillovers in CEO pay and examines the corresponding role of the compensation benchmarking network. We show that one firm's choice of how much to pay its CEO affects the pay at other firms that benchmark against it, highlighting how the benchmarking network serves as a transmission mechanism for pay spillovers. In addition to transmitting pay shocks to other firms, the benchmarking process also plays a separate role in originating shocks to pay: many firms use the benchmarking process in a strategic manner, selecting their peer firms in a way that can help explain or justify their desired pay levels.

Specifically, we find that a firm's strategic manipulation of its benchmarking peer group not only influences pay at that firm, it also spills over onto other firms in the network. We find consistent evidence of these spillovers across a range of identification strategies, including instrumental variables specifications, first-difference panel regressions, dyad-level comparisons of chosen versus non-chosen peers, and spatial autoregressive models that exploit the network structure directly.

On average, the strategic manipulation of benchmarking peer groups results in higher-than-expected pay at those firms. While not all firms engage in this sort of strategic behavior, the subsequent spillover effects can lead to higher pay for all types of firms, including firms that select their own peer groups in accordance with standard practice and firms with strong shareholder protections. Taken together, our results suggest that the practice of compensation benchmarking contributes to higher levels of CEO pay overall.

Our findings have implications for how we understand the role of compensation benchmarking. Under perfect competition, peer benchmarking could address an information problem by helping boards learn about the competitive market wage. We would expect shocks to labor supply or labor demand to influence wages and to observe those wage effects propagate through the market. But in the case of idiosyncratic, firm-specific shocks to pay at peer firms, such as those driven by managerial capture or the desire to retain a particularly effective CEO, we would expect other firms to filter those out. The fact that they do not suggests that benchmarking does more than convey information about market prices; it also determines reference points that shape what is perceived as appropriate compensation. This normative role of benchmarking is especially consequential in a setting where CEO labor markets are imperfectly competitive: without a single market-clearing wage, peer comparisons do not simply reflect prevailing pay levels but actively influence them, and the externalities we document are unlikely to be self-correcting.

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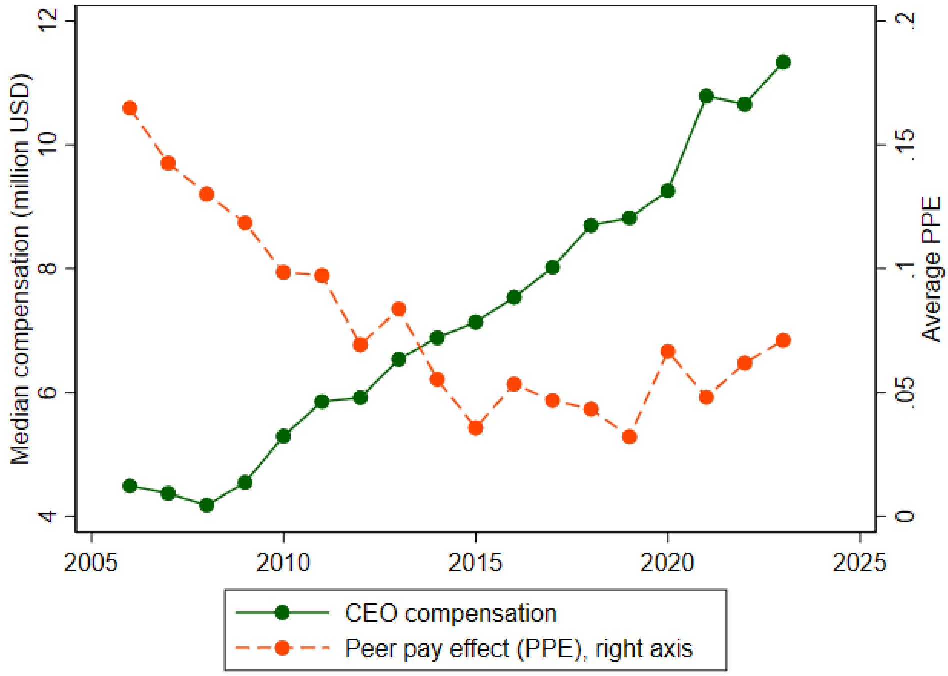
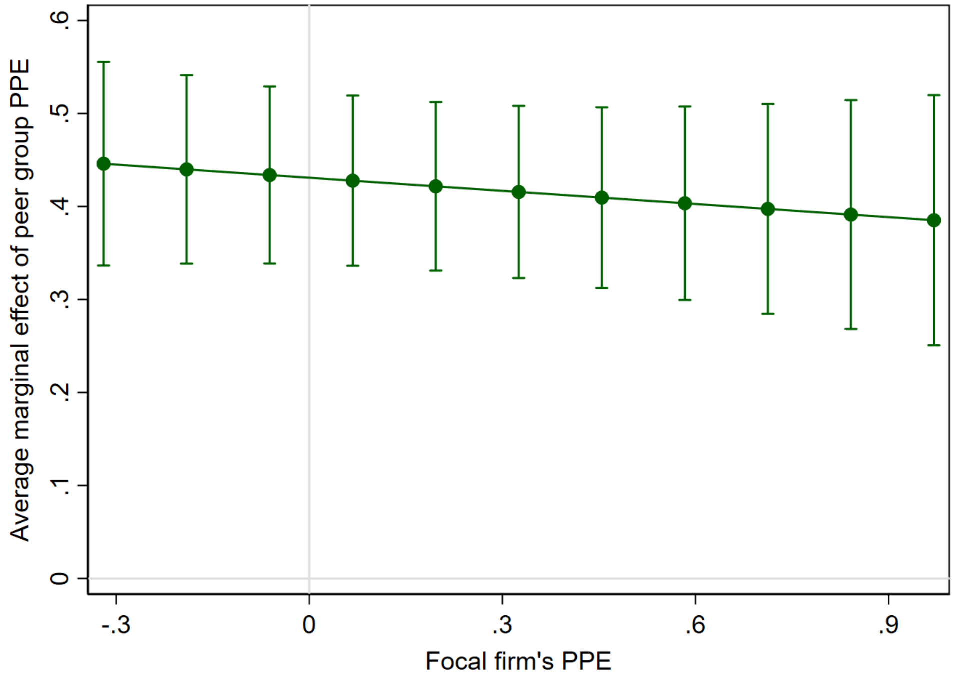
Figure 1: Median CEO compensation and peer pay effect (PPE) over time



Notes: Median compensation and median peer pay effect for all firms in the estimation sample. Median PPE is greater than zero in every year, indicating that firms systematically select higher-paid peers when they deviate from the predicted peer choices. The magnitude of this positive bias is declining over time; in recent years, pay at a firm's selected peer group more closely aligns with pay at its predicted peers.

Figure 2: Effect of peer group PPE, by own PPE



Notes: The figure plots the estimated marginal effect of the average PPE of a firm's peer group ("Average PPE of peers") on the firm's log CEO compensation, as a function of the focal firm's PPE ("Own PPE"). The estimated effects are from a specification similar to Equation (7) and column 4 of Table 2, except that we additionally include the following interaction terms: "Own PPE $\times$ Average PPE of peers" and "Own PPE $\times$ Average predicted group pay of peers." Estimates are displayed for focal firm PPE values (horizontal axis) ranging from the 5th to 95th percentiles. Point estimates and 95% confidence intervals are displayed; standard errors are clustered at the firm level.

Table 1: Log CEO pay vs. own firm's peer pay effect (PPE)

	(1)	(2)	(3)	(4)
Own peer pay effect (PPE)	0.0596*	0.473***	0.373***	0.287***
	(0.0315)	(0.0259)	(0.0248)	(0.0242)
Own predicted group pay		1.055***	0.682***	0.469***
		(0.0238)	(0.0378)	(0.0392)
R^2	0.128	0.400	0.445	0.474
Observations	18446	18446	18413	18388
No. of firms	1755	1755	1755	1753
Additional controls			Yes	Yes
Industry FE				Yes

Notes: The dependent variable is the (log) total compensation of the firm's CEO. These specifications are similar to the "first stage" of our estimation framework but are estimated at the firm level; true first stage results (showing the relationship between average peer group pay and average peer group PPE) are shown in Appendix Table A4. "Additional controls" include log sales; log total assets; indicators for membership in the Dow 30, S&P 500, and S&P 400 indices; and an indicator for whether the CEO is also board chair. "Industry FE" include fixed effects for 3-digit NAICS. Firm-level pooled cross-section regressions for the years 2006-2023. All specifications include year fixed effects. Standard errors are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Effect of peer pay on own pay: Reduced form effect of average peer group PPE

	(1)	(2)	(3)	(4)
Average PPE of peers	0.528***	0.606***	0.553***	0.419***
	(0.0499)	(0.0497)	(0.0475)	(0.0464)
Average predicted group pay of peers	1.254***	1.276***	0.800***	0.554***
	(0.0328)	(0.0333)	(0.0482)	(0.0505)
Own PPE		-0.129***	-0.0291	0.0116
		(0.0247)	(0.0228)	(0.0223)
R^2	0.384	0.387	0.443	0.472
Observations	18356	18356	18344	18319
No. of firms	1750	1750	1750	1748
Additional controls			Yes	Yes
Industry FE				Yes

Notes: The dependent variable is the (log) total compensation of the firm's CEO. The table displays results from the reduced form specification in Equation (7). See notes to Table 1 for information on control variables. Firm-level pooled cross-section regressions for the years 2006-2023. All specifications include year fixed effects. Standard errors are clustered at the firm level. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table 3: Effect of peer pay on own pay: Instrument average peer group pay using average peer group PPE

	(1)	(2)	(3)	(4)
Average pay of peers	0.877***	1.192***	1.142***	0.878***
	(0.0801)	(0.0924)	(0.0984)	(0.0966)
Average predicted group pay of peers	0.228**	-0.0881	-0.314***	-0.263***
	(0.0905)	(0.101)	(0.0908)	(0.0850)
Own PPE		-0.319***	-0.253***	-0.169***
		(0.0293)	(0.0330)	(0.0324)
R^2	0.303	0.297	0.325	0.323
Observations	18356	18356	18344	18319
No. of firms	1750	1750	1750	1748
First stage F stat	734.7	542.0	587.7	570.8
Additional controls			Yes	Yes
Industry FE				Yes

Notes: The dependent variable is the (log) total compensation of the firm's CEO. The average compensation of the firm's peer group is instrumented using the average PPE of the peer group. The reported F-statistics are from the first stage F-test of the excluded instrument. Firm-level pooled cross-section regressions for the years 2006-2023. All specifications include year fixed effects. Standard errors are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Effect of peer pay on own pay: Use PPE of predicted peer group

	First Stage: Avg peer pay		Reduced Form: Own pay		IV (2SLS): Own pay	
	(1)	(2)	(3)	(4)	(5)	(6)
Average PPE of predicted peers	0.473***	0.229***	0.433***	0.212***		
	(0.0323)	(0.0212)	(0.0580)	(0.0423)		
Average pay of actual peers					0.915***	0.924***
					(0.108)	(0.186)
Average predicted group pay of predicted peers	0.974***	0.776***	1.147***	0.501***	0.256***	-0.216
	(0.0169)	(0.0203)	(0.0333)	(0.0482)	(0.0921)	(0.133)
Own PPE		0.479***		0.176***		-0.266***
		(0.0125)		(0.0213)		(0.0942)
R^2	0.633	0.820	0.348	0.469	0.304	0.313
Observations	18359	18323	18359	18323	18359	18323
No. of firms	1752	1750	1752	1750	1752	1750
First stage F stat					214.7	116.9
Additional controls		Yes		Yes		Yes
Industry FE		Yes		Yes		Yes

Notes: The dependent variable in columns 1 and 2 is the average log total compensation among the focal firm's chosen peer group, and in columns 3-6, it is the (log) total compensation of the firm's CEO. In columns 5 and 6, we instrument average log pay among the chosen peer group using the average PPE of the firm's predicted peer group. The reported F-statistics are from the first stage F-test of the excluded instrument. Firm-level pooled cross-section regressions for the years 2006-2023. All specifications include year fixed effects. Standard errors are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Compare effects of chosen peers to potential peers

	(1)	(2)	(3)	(4)
Potential Peer's PPE × Actual	0.211***	0.232***	0.224***	0.163***
	(0.0188)	(0.0165)	(0.0160)	(0.0130)
	[< 0.001]	[< 0.001]	[< 0.001]	[< 0.001]
Potential Peer's PPE	-0.137***	0.149***	0.147***	0.0672***
	(0.0148)	(0.0142)	(0.0139)	(0.0104)
Peer's predicted group pay × Actual		0.104***	0.0906***	0.0702***
		(0.0122)	(0.00936)	(0.00844)
Peer's predicted group pay		0.684***	0.693***	0.348***
		(0.0219)	(0.0216)	(0.0187)
Own Firm PPE			0.0566*	0.108***
			(0.0299)	(0.0276)
R^2	0.133	0.272	0.272	0.352
Observations	340155	340155	340155	338917
No. of firms	1754	1754	1754	1752
Additional controls				Yes
Industry FE				Yes

Notes: Pair-level pooled cross-section regressions for the years 2006-2023; each observation corresponds to an ordered firm-pair dyad (i, j) in a single year. The dependent variable is the (log) total compensation of focal firm i's CEO. In addition to firm i's chosen peers, the sample of potential peers also includes peer firms from the predicted group who were not selected in the actual peer group. These predicted-but-not-selected peers comprise the base category; "Actual" is an indicator variable for whether potential peer j was actually selected to be in firm i's peer group. All specifications include year fixed effects. The values in brackets correspond to the p-value from a randomization inference procedure where we randomize the "actual" indicator over the combined set of peers. The values in parentheses are the conventional asymptotic standard error estimates, adjusted for multi-way clustering at the level of the firm (i) and the (unordered) firm pair $\{(i, j), (j, i)\}$. The set of "Additional controls" is listed in column 1 of Appendix Table A1. "Industry FE" are fixed effects for 3-digit NAICS for both firm i and firm j. All specifications include year fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Effect of peer pay on own pay: First-differences panel regressions

	Reduced Form		IV (2SLS)	
	(1)	(2)	(3)	(4)
Average ΔPPE of peers	0.200***	0.188***		
	(0.0397)	(0.0397)		
Δ Average pay of peers			0.463***	0.508***
			(0.0958)	(0.111)
Avg Δ predicted group pay of peers	0.357***	0.328***	-0.0425	-0.0914
	(0.0597)	(0.0575)	(0.0737)	(0.0789)
Δ Own PPE		-0.0156		-0.126***
		(0.0160)		(0.0308)
R^2	0.014	0.027	-0.020	-0.012
Observations	16163	16119	16163	16119
No. of firms	1641	1638	1641	1638
First stage F stat			198.7	181.3
Additional controls		Yes		Yes
Industry FE		Yes		Yes

Notes: Firm-level first-differences panel regressions for the years 2007-2023. The dependent variable is the year-over-year change in (log) total compensation of the firm's CEO. We construct the change in peer group PPE ("Average ΔPPE of peers") by first computing the change in PPE within each peer firm j and then averaging these within-peer differences across the members of firm i's peer group. The change in average predicted group pay of peers is constructed analogously. Columns 1 and 2 present reduced form estimates, where the change in CEO pay is regressed on the average change in peers' PPE. Columns 3 and 4 present instrumental variables estimates, where the change in average pay among the chosen peer group peer is instrumented using the average change in peer group PPE. The reported F-statistics are from the first stage F-test of the excluded instrument; the corresponding first stage estimates are reported in Appendix Table A6. All specifications include year fixed effects. Standard errors are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Spatially-weighted regressions: SAR and reduced form effects

(a) Spatial autoregressive (SAR) model: Own pay versus peer pay

	(1)	(2)	(3)	(4)	(5)
Average peer pay	0.510***	0.510***	0.481***	0.553***	0.527***
	(0.0787)	(0.0794)	(0.113)	(0.0929)	(0.126)
Observations	1017	1017	1017	1017	1017
Additional controls	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Peer PPE		Yes		Yes	
Peer predicted group pay			Yes		Yes
Own firm PPE				Yes	Yes

(b) Reduced form: Own pay versus peer PPE

	(1)	(2)	(3)	(4)
Average PPE of peers	0.521***	0.582***	0.489***	0.388***
	(0.103)	(0.109)	(0.104)	(0.110)
Average predicted group pay of peers	1.297***	1.319***	0.779***	0.681***
	(0.0516)	(0.0508)	(0.0915)	(0.112)
Observations	1017	1017	1017	1017
Additional controls			Yes	Yes
Industry FE				Yes
Own firm PPE		Yes	Yes	Yes

Notes: The dependent variable is the (log) total compensation of the firm's CEO in fiscal year 2007. The table displays results from spatially-weighted models, where a firm's CEO compensation is modeled as a function of its own characteristics as well as the weighted characteristics of other firms. The weighting matrix used is the (row-normalized) network adjacency matrix defined by the actual compensation benchmarking network, such that element (i, j) equals 1 if firm i has included firm j in its peer group and 0 otherwise. Panel (a) displays results from a spatial autoregressive (SAR) model, where the relationship between pay at a focal firm and pay at its peers is identified using the network structure, under the assumption that pay spillovers across firms must follow the benchmarking network. In panel (b), we combine information about the PPE at other firms together with the peer network structure in order to identify the reduced form effect. Instead of lagging the dependent variable, these specifications include the spatial lag of the peer pay effect (PPE) and predicted group pay. All models are estimated using the Generalized Spatial 2SLS estimator of Drukker et al. (2013). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

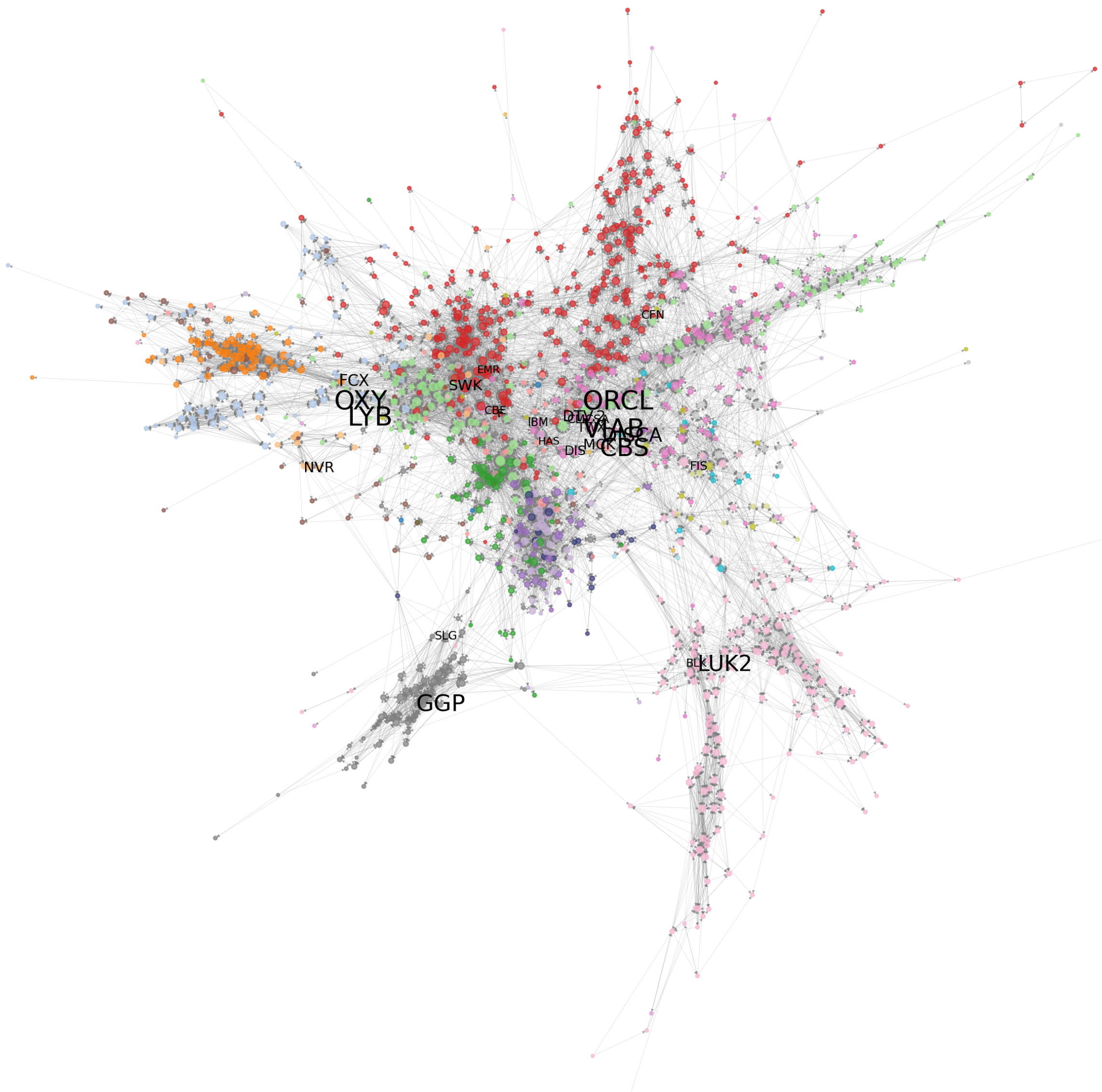
Table 8: Mechanisms: interact with governance measures

	Exclude dual-class		All firms	
	(1)	(2)	(3)	(4)
High E index × Average PPE of peers	0.151	0.278*	0.189	0.317**
	(0.167)	(0.148)	(0.155)	(0.140)
Average PPE of peers	0.480***	0.280***	0.497***	0.277***
	(0.109)	(0.102)	(0.106)	(0.0988)
High E index × Average predicted group pay of peers	-0.136	0.0240	-0.109	0.0592
	(0.0930)	(0.0907)	(0.0903)	(0.0920)
Average predicted group pay of peers	1.325***	0.495***	1.272***	0.433***
	(0.0703)	(0.0961)	(0.0692)	(0.0928)
High E index	2.088	-0.429	1.655	-0.980
	(1.452)	(1.415)	(1.408)	(1.433)
High E index × Own Firm PPE		0.0142		-0.00615
		(0.0652)		(0.0621)
Own Firm PPE		0.0152		0.0408
		(0.0436)		(0.0423)
R^2	0.393	0.520	0.369	0.498
Observations	3031	3022	3428	3419
No. of firms	727	724	828	825
Additional controls		Yes		Yes
Industry FE		Yes		Yes

Notes: The dependent variable is the (log) total compensation of the firm's CEO. The sample is restricted to fiscal years 2006-2010 to limit the time elapsed since the governance variables were last measured. "High E index" is an indicator variable equal to 1 for firms with E index values of 3–6 (indicating higher levels of managerial entrenchment) and equal to 0 for values of 0–2. Columns 1 and 2 restrict the sample to firms without dual-class stock; columns 3 and 4 include all firms. All specifications include year fixed effects. Standard errors are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

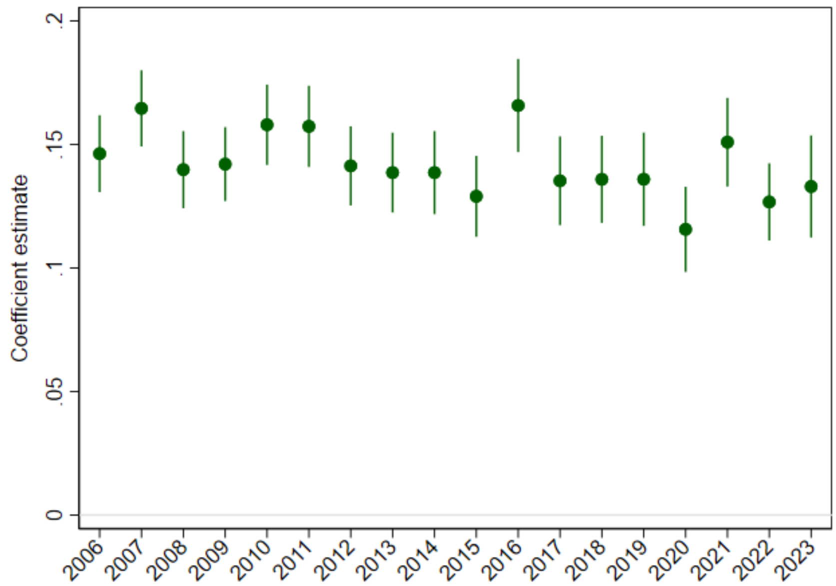
Appendix A Additional Results

Figure A1: Benchmark compensation network in 2010



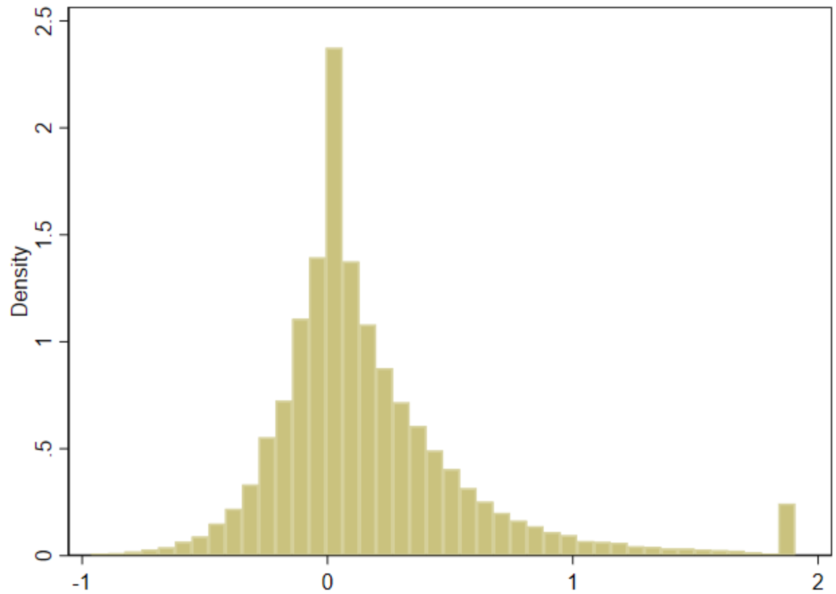
Notes: The figure displays the network of firms in our dataset in 2010. Each node is a firm, and a link indicates that at least one of the firms includes the other in its compensation peer group used to benchmark executive pay. Colors represent 2-digit industries; node size reflects the number of peer groups to which the firm belongs; ticker symbols are shown for selected firms with highly-paid CEOs in 2010 with the font size scaled by total CEO compensation.

Figure A2: Likelihood of being chosen as another firm's peer: estimated effect of own CEO compensation, by year



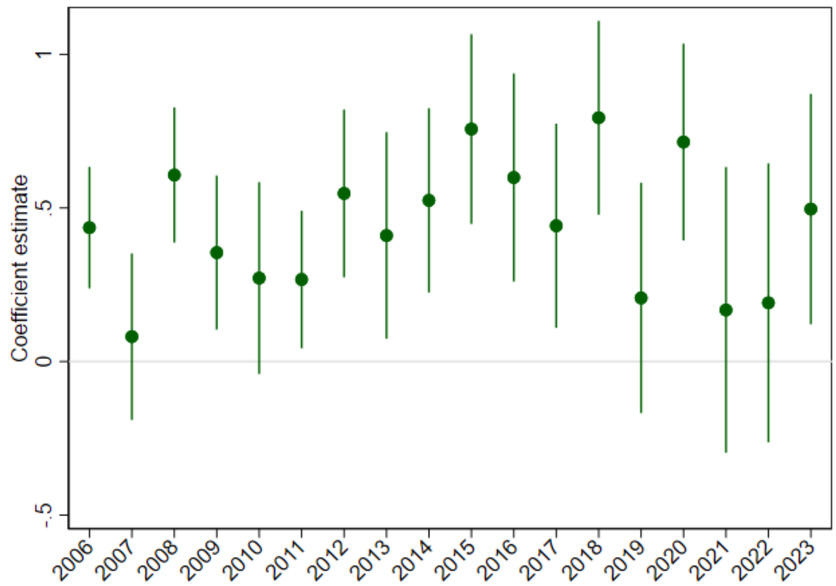
Notes: This specification is similar to column 4 of Table A1, except that we include year fixed effects and interact the *peer's compensation* variable with all of the year effects. The figure displays the point estimates and confidence intervals from the interaction terms. We do not include *peer's compensation* separately as a base effect, so the coefficients on each interaction can be interpreted as the estimated effect of *peer's compensation* in that year. In each year, a higher level of CEO compensation is associated with a higher likelihood of being included in another firm's peer comparison group. Point estimates and 95% confidence intervals are displayed; standard errors are clustered at the firm level.

Figure A3: Histogram of peer pay effect (PPE)



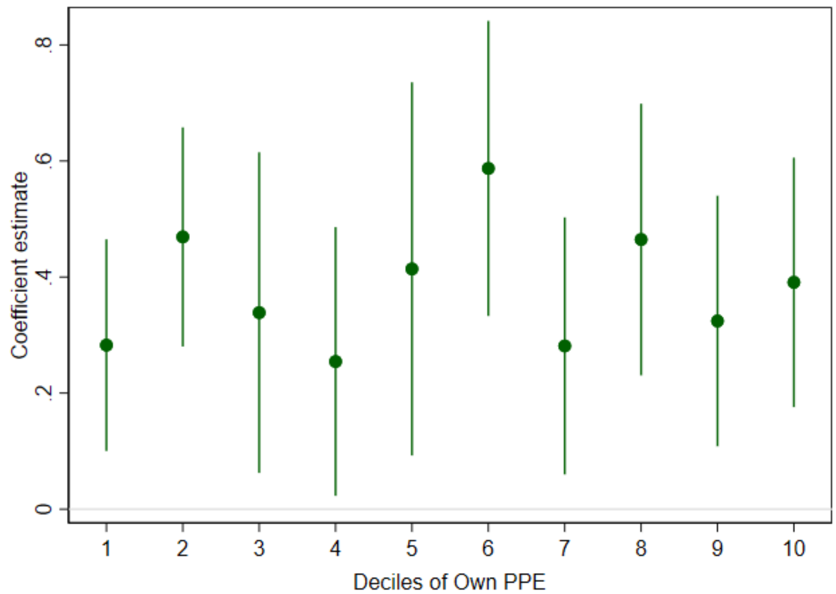
Notes: The figure displays a histogram of the peer pay effect (PPE) for all firm-years in the sample. The PPE is defined as the difference in median pay between the actual and predicted peer groups divided by median pay in the predicted group. A positive value indicates that the chosen peer group has higher pay than the predicted group; a value of 0.5 indicates that median pay in the actual group selected is 50% greater than in the predicted group.

Figure A4: Effect of peer group average PPE by year



Notes: The figure plots the estimated effect of the average PPE of a firm's peer group on the compensation of its CEO, estimated separately by year. The estimates are obtained from a specification similar to Equation (7) and column 4 of Table 2, except that "Average PPE of peers" is interacted with year fixed effects, and the corresponding coefficients are displayed in the figure. All other controls, including average predicted group pay of peers, own PPE, additional firm-level controls, and 3-digit NAICS industry fixed effects, are included. Point estimates and 95% confidence intervals are displayed; standard errors are clustered at the firm level.

Figure A5: Effect of peer group average PPE, by own PPE



Notes: The figure plots the estimated marginal effect of the average PPE of a firm's peer group on the compensation of its CEO, by deciles of the focal firm's own PPE. The estimates are obtained from a specification similar to Equation (7) and column 4 of Table 2, except that "Average PPE of peers" is interacted with indicators for the decile of the focal firm's own PPE, and the corresponding coefficients are displayed in the figure. Point estimates and 95% confidence intervals are displayed; standard errors are clustered at the firm level.

Table A1: Modeling peer group selection based on firm-pair observable characteristics

	Dec 2006 - Nov 2007		All years	
	(1)	(2)	(3)	(4)
Peer's compensation		0.166***		0.139***
		(0.00814)		(0.00489)
Same 2-digit industry	1.088***	1.093***	1.155***	1.159***
	(0.0309)	(0.0310)	(0.0231)	(0.0234)
Same 3-digit industry	0.709***	0.734***	0.606***	0.614***
	(0.0320)	(0.0318)	(0.0242)	(0.0244)
Sales within 50-200%	0.416***	0.424***	0.498***	0.503***
	(0.0135)	(0.0134)	(0.00863)	(0.00859)
Assets within 50-200%	0.274***	0.282***	0.310***	0.314***
	(0.0128)	(0.0126)	(0.00747)	(0.00738)
Market cap within 50-200%	0.0926***	0.111***	0.118***	0.131***
	(0.0140)	(0.0136)	(0.00755)	(0.00729)
Both Dow 30	1.562***	1.456***	1.341***	1.268***
	(0.0954)	(0.0956)	(0.0896)	(0.0899)
Both S&P 500	0.543***	0.442***	0.530***	0.460***
	(0.0173)	(0.0176)	(0.0118)	(0.0116)
Both S&P 400	0.174***	0.183***	0.142***	0.161***
	(0.0224)	(0.0224)	(0.0125)	(0.0124)
Both CEO is chair	0.129***	0.1000***	0.115***	0.113***
	(0.0152)	(0.0149)	(0.00873)	(0.00854)
Both CEO is not chair	-0.00672	0.0214	-0.0171**	-0.0195**
	(0.0152)	(0.0152)	(0.00784)	(0.00783)
Number of peers	0.0187***	0.0189***	0.0191***	0.0197***
	(0.00132)	(0.00131)	(0.000957)	(0.000976)
Talent flows	1.006***	0.983***	1.106***	1.071***
	(0.250)	(0.252)	(0.0500)	(0.0498)
Pseudo R^2	0.286	0.295	0.295	0.300
Observations	1208525	1208525	22041449	22041449
No. of firms	949	949	1759	1759

Notes: Probit model of peer group selection, following Faulkender and Yang (2010). The dependent variable is an indicator for whether firm i has selected firm j in its peer comparison set in year t. We use the parameter estimates from specifications similar to columns 1 and 3 to predict peer group membership and derive each firm's "peer pay effect"; see Section 4.2 for details. We estimate the model for all possible (i, j) firm-pairs in the dataset. Columns (1) and (2) are restricted to fiscal year 2006-07 to facilitate comparison with the results in Faulkender and Yang (2010). Standard errors adjusted for clustering at the firm i level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Descriptive statistics

(a) Panel A: Firm-level variables

	N	Mean	Std. Dev.	p25	Median	p75
Log CEO total compensation	18,446	15.69	0.86	15.22	15.77	16.25
Own peer pay effect (PPE)	18,446	0.17	0.43	-0.06	0.07	0.31
Average (log) pay of peers	18,446	15.82	0.56	15.48	15.85	16.21
Average PPE of peers	18,356	0.17	0.20	0.05	0.13	0.26
Log total assets	18,446	8.90	1.56	7.86	8.81	9.86
Log sales	18,413	8.21	1.52	7.26	8.19	9.17
Dow 30 member	18,446	0.03	0.16	0.00	0.00	0.00
S&P 500 member	18,446	0.42	0.49	0.00	0.00	1.00
S&P 400 member	18,446	0.26	0.44	0.00	0.00	1.00
CEO is board chair	18,446	0.40	0.49	0.00	0.00	1.00
E index	3,452	2.49	1.25	2.00	3.00	3.00
High E index (= 1 if E index $\geq$ 3)	3,452	0.51	0.50	0.00	1.00	1.00
Number of firms	1755					

(b) Panel B: Pair-level indicator variables

	count	mean	sd
Same 2-digit industry	380,265	0.631	0.483
Same 3-digit industry	380,265	0.407	0.491
Sales within 50-200%	380,265	0.669	0.471
Assets within 50-200%	380,265	0.608	0.488
Market cap within 50-200%	380,265	0.525	0.499
Both Dow 30	380,265	0.022	0.147
Both S&P 500	380,265	0.404	0.491
Both S&P 400	380,265	0.113	0.317
Both CEOs are board chair	380,265	0.232	0.422
Neither CEO is chair	380,265	0.331	0.470
Talent flows	380,265	0.009	0.094
Number of firms	1755		

Notes: The table reports descriptive statistics for the main variables used in the analysis. Panel A presents firm-level variables, with one observation per firm-year. Panel B presents pair-level indicator variables constructed at the ordered firm-pair (i, j) level, where each observation corresponds to a firm i and a potential peer j in a given year; the sample includes both actual peers and predicted-but-not-chosen peers used in the dyadic specifications (see Table 5). Because the E index is available only through 2006, the summary statistics for the E index variables are reported for the subsample used in Table 8 (fiscal years 2006-2010). For the remaining variables, the sample covers fiscal years 2006 through 2023.

Table A3: Robustness: effects over time

	(1) 2006-2009	(2) 2010-2013	(3) 2014-2017	(4) 2018-2021	(5) 2020-2023
Average PPE of peers	0.387***	0.374***	0.555***	0.422***	0.356***
	(0.0625)	(0.0874)	(0.0940)	(0.111)	(0.130)
Avg pred grp pay of peers	0.454***	0.420***	0.558***	0.626***	0.641***
	(0.0723)	(0.0877)	(0.0967)	(0.0970)	(0.105)
Own PPE	0.0218	0.00777	0.0161	0.100**	0.0341
	(0.0315)	(0.0388)	(0.0414)	(0.0482)	(0.0523)
R^2	0.459	0.444	0.418	0.377	0.330
Observations	3950	4281	4156	4006	3934
No. of firms	1259	1227	1235	1158	1120
Additional controls	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is the (log) total compensation of the firm's CEO. The table displays results from the reduced form specification in Equation (7) and column 4 of Table 2, estimated separately for different sub-periods. Each column corresponds to a four-year window, with column 5 partially overlapping column 4 to include the most recent years of available data. Also see Figure A4 for year-by-year estimates. All specifications include year fixed effects and 3-digit NAICS industry fixed effects. Standard errors are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: First stage relationship: Peer group's average pay vs. peer group's average PPE

	(1)	(2)	(3)	(4)
Average PPE of peers	0.602***	0.508***	0.485***	0.477***
	(0.0222)	(0.0220)	(0.0202)	(0.0202)
Average predicted group pay of peers	1.169***	1.142***	0.976***	0.932***
	(0.0104)	(0.0101)	(0.0142)	(0.0178)
Own PPE		0.157***	0.194***	0.204***
		(0.00943)	(0.00980)	(0.00951)
R^2	0.813	0.826	0.842	0.853
Observations	18356	18356	18344	18319
No. of firms	1750	1750	1750	1748
Additional controls			Yes	Yes
Industry FE				Yes

Notes: The dependent variable is the average log CEO compensation across the firms in the focal firm's peer group. The table displays the first-stage results corresponding to the instrumental variables specifications in Table 3. Firm-level pooled cross-section regressions for the years 2006–2023. All specifications include year fixed effects. Standard errors are clustered at the firm level. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table A5: OLS (instrumented) relationship between own pay and average peer pay

	(1)	(2)	(3)	(4)	(5)	(6)
Average pay of peers	0.892***	0.959***	0.384***	0.563***	0.673***	0.331***
	(0.0240)	(0.0241)	(0.0332)	(0.0353)	(0.0372)	(0.0355)
Average predicted group pay of peers				0.580***	0.481***	0.176***
				(0.0493)	(0.0505)	(0.0511)
Own PPE		-0.265***	-0.0252		-0.203***	-0.0217
		(0.0254)	(0.0241)		(0.0254)	(0.0242)
R^2	0.378	0.393	0.473	0.399	0.408	0.473
Observations	18446	18446	18388	18360	18360	18323
No. of firms	1755	1755	1753	1751	1751	1749
Additional controls			Yes			Yes
Industry FE			Yes			Yes

Notes: The dependent variable is the (log) total compensation of the firm's CEO. These specifications correspond to the uninstrumented counterparts of the IV results reported in Table 3. The first 3 columns exclude average predicted peer group pay, while columns 4-6 include this variable as a control. For each case, we show specifications with different combinations of the other control variables to examine the influence on the coefficient of interest (average pay of peers). Firm-level pooled cross-section regressions for the years 2006–2023. All specifications include year fixed effects. Standard errors are clustered at the firm level. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table A6: First stage: First-differences panel regressions

	First Stage	
	(1)	(2)
Average ΔPPE of peers	0.433***	0.369***
	(0.0307)	(0.0274)
Avg Δ predicted group pay of peers	0.863***	0.825***
	(0.0357)	(0.0365)
Δ Own PPE		0.217***
		(0.00942)
R^2	0.194	0.304
Observations	16163	16119
No. of firms	1641	1638
Additional controls		Yes
Industry FE		Yes

Notes: Firm-level first-differences panel regressions for the years 2007-2023. The dependent variable is the year-over-year change in average (log) CEO compensation across the firms in the focal firm's peer group. The table shows the first-stage estimates corresponding to the IV first-differences specifications in columns 3 and 4 of Table 6. All specifications include year fixed effects. Standard errors are clustered at the firm level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Relative performance evaluation (RPE) peers: Differential effect of including RPE peer in the benchmark compensation (BM) peer group

	(1)	(2)	(3)	(4)
RPE Peer's PPE × BM	0.0888**	0.245***	0.239***	0.188***
	(0.0413)	(0.0498)	(0.0492)	(0.0345)
RPE Peer's PPE	0.0582	0.184***	0.182***	0.0269
	(0.0369)	(0.0508)	(0.0505)	(0.0294)
RPE Peer's predicted group pay × BM		0.399***	0.394***	0.331***
		(0.0661)	(0.0644)	(0.0464)
RPE Peer's predicted group pay		0.395***	0.395***	-0.0184
		(0.0735)	(0.0725)	(0.0462)
Own Firm PPE			0.0515	0.0984**
			(0.0512)	(0.0454)
R^2	0.154	0.344	0.344	0.478
Observations	42725	42716	42716	42685
No. of firms	677	677	677	677
RI p-value	< 0.001	< 0.001	< 0.001	< 0.001
Additional controls				Yes
Industry FE				Yes

Notes: Pair-level pooled cross-section regressions for the years 2006-2023; each observation corresponds to an ordered firm-pair dyad (i, j) in a single year. The dependent variable is the (log) total compensation of focal firm i's CEO. The sample is restricted to the subset of focal firms that report a relative performance evaluation (RPE) peer group, and the set of potential peers includes only those firms in the RPE peer group. "BM" is an indicator variable equal to 1 if the RPE peer is also included in the firm's benchmark (BM) compensation peer group. The base category therefore comprises RPE peers that are not in the BM group, while the interaction term captures the additional effect of BM group membership. The "RI p-value" corresponds to the p-value from a randomization inference procedure where we randomize the "Actual" indicator over the set of RPE peers. The values in parentheses are conventional asymptotic standard error estimates, adjusted for multi-way clustering at the level of the firm (i) and the (unordered) firm pair $\{(i, j), (j, i)\}$. "Additional controls" include the covariates in column 1 of Table A1. "Industry FE" are fixed effects for 3-digit NAICS for both firm i and firm j. All specifications include year fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Spatially-weighted regressions: SAR and reduced form effects (2012)

(a) Spatial autoregressive (SAR) model: Own pay versus peer pay

	(1)	(2)	(3)	(4)	(5)
Average peer pay	0.548***	0.519***	0.575***	0.714***	0.898***
	(0.0852)	(0.0908)	(0.128)	(0.121)	(0.147)
Observations	1061	1061	1061	1061	1061
Additional controls	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Peer PPE		Yes		Yes	
Peer predicted group pay			Yes		Yes
Own firm PPE				Yes	Yes

(b) Reduced form: Own pay versus peer PPE

	(1)	(2)	(3)	(4)
Average PPE of peers	0.597***	0.812***	0.802***	0.754***
	(0.126)	(0.140)	(0.137)	(0.155)
Average predicted group pay of peers	1.404***	1.456***	1.069***	0.910***
	(0.0602)	(0.0624)	(0.131)	(0.161)
Observations	1061	1061	1061	1061
Additional controls			Yes	Yes
Industry FE				Yes
Own firm PPE		Yes	Yes	Yes

Notes: Same specifications as in Table 7, but using data and network for 2012. See notes to Table 7 for details. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Spatially-weighted regressions: SAR and reduced form effects (2017)

(a) Spatial autoregressive (SAR) model: Own pay versus peer pay

	(1)	(2)	(3)	(4)	(5)
Average peer pay	0.560***	0.522***	0.297**	0.464***	0.308**
	(0.0868)	(0.0903)	(0.121)	(0.104)	(0.124)
Observations	1025	1025	1025	1025	1025
Additional controls	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes
Peer PPE		Yes		Yes	
Peer predicted group pay			Yes		Yes
Own firm PPE				Yes	Yes

(b) Reduced form: Own pay versus peer PPE

	(1)	(2)	(3)	(4)
Average PPE of peers	0.887***	0.916***	0.809***	0.620***
	(0.132)	(0.142)	(0.140)	(0.159)
Average predicted group pay of peers	1.412***	1.418***	1.127***	0.818***
	(0.0583)	(0.0581)	(0.0971)	(0.134)
Observations	1025	1025	1025	1025
Additional controls			Yes	Yes
Industry FE				Yes
Own firm PPE		Yes	Yes	Yes

Notes: Same specifications as in Table 7, but using data and network for 2017. See notes to Table 7 for details. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table A10: Robustness to alternative methods for constructing predicted peer groups

	(1) Baseline	(2) All years	(3) GradBoost	(4) BLN	(5) Lasso CV
Average PPE of peers	0.419***	0.428***	0.417***	0.447***	0.450***
	(0.0460)	(0.0471)	(0.0474)	(0.0489)	(0.0459)
Average predicted group pay of peers	0.554***	0.551***	0.555***	0.557***	0.490***
	(0.0504)	(0.0508)	(0.0512)	(0.0516)	(0.0468)
Own PPE	0.0116	0.0109	0.0120	0.0105	0.0302
	(0.0222)	(0.0223)	(0.0223)	(0.0227)	(0.0221)
R^2	0.472	0.472	0.472	0.472	0.466
Observations	18319	18319	18318	18289	18083
No. of firms	1748	1748	1748	1747	1741
Additional controls	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is the (log) total compensation of the firm's CEO. Each column re-estimates the baseline reduced-form specification using a different prediction method to construct the group of potential peers. Column 1 is identical to column 4 of Table 2; this baseline model uses the probit specification from Faulkender and Yang (2010) estimated over rolling three-year windows, where the predicted groups and PPE for year t are based on data from years $t-3$ through $t-1$; for the first three sample years, the rolling window uses data from fiscal years 2006–2008. See Section 4.1 for details. Column 2 estimates the same probit model pooling the entire sample period. Column 3 uses a gradient boosted classification tree, estimated on the full sample. Columns 1–3 all use the Faulkender and Yang (2010) covariates (listed in column 1 of Table A1). Column 4 uses the logit specification from Bizjak et al. (2011) with the following set of covariates: same Fama-French 49 industry; Fama-French industry return correlation; positive and negative differences in log sales, ROA, and market-to-book ratio; and indicators for talent flows, both in S&P 500, both not in S&P 500, same credit rating, both with single/multiple business segments, and both with single/multiple geographic segments. Column 5 uses a Lasso-penalized logistic regression with five-fold cross-validation, estimated on the full sample using the combined set of covariates from both the Faulkender and Yang (2010) and Bizjak et al. (2011) specifications. Firm-level pooled cross-section regressions for the years 2006–2023. All specifications include year fixed effects. Standard errors are clustered at the firm level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A11: Mechanisms: Shared compensation consultants

	(1)	(2)	(3)	(4)
Shared Consultant × Potential Peer's PPE × Actual	-0.000393	0.0314	0.0306	0.0420*
	(0.0259)	(0.0250)	(0.0244)	(0.0218)
Potential Peer's PPE × Actual	0.210***	0.225***	0.217***	0.153***
	(0.0202)	(0.0177)	(0.0172)	(0.0143)
Shared Consultant × Potential Peer's PPE	-0.0171	-0.00541	-0.00486	-0.0209
	(0.0212)	(0.0211)	(0.0208)	(0.0187)
Potential Peer's PPE	-0.133***	0.148***	0.146***	0.0709***
	(0.0157)	(0.0156)	(0.0152)	(0.0111)
Shared Consultant × Peer's predicted group pay × Actual		0.0348	0.0324	0.0280
		(0.0217)	(0.0202)	(0.0182)
Peer's predicted group pay × Actual		0.0994***	0.0862***	0.0652***
		(0.0135)	(0.0108)	(0.00947)
Shared Consultant × Peer's predicted group pay		-0.0190	-0.0167	-0.0219
		(0.0234)	(0.0229)	(0.0249)
Peer's predicted group pay		0.683***	0.691***	0.349***
		(0.0234)	(0.0231)	(0.0193)
Shared Consultant × Own Firm PPE			0.00403	0.0253
			(0.0301)	(0.0281)
Own Firm PPE			0.0553*	0.104***
			(0.0316)	(0.0293)
R^2	0.136	0.273	0.273	0.355
Observations	340155	340155	340155	338915
No. of firms	1754	1754	1754	1752
RI p-value	0.984	0.230	0.174	0.030
Additional controls				Yes
Industry FE				Yes

Notes: Pair-level pooled cross-section regressions for the years 2006-2023; each observation corresponds to an ordered firm-pair dyad (i, j) in a single year. The dependent variable is the (log) total compensation of focal firm i's CEO. The specifications extend the dyadic regressions in Table 5 by interacting all covariates with "Shared Consultant," an indicator equal to 1 if firms i and j report using the same compensation consultant. The coefficient of interest in the top row ("Shared Consultant × Potential Peer's PPE × Actual") represents the additional effect of the PPE for "Actual" (i.e., chosen) peers who also share a compensation consultant with the focal firm. This estimate captures whether the spillover effect operating through the benchmarking network differs for peers sharing a compensation consultant with the focal firm; it is interpreted relative to the base category in the second row ("Potential Peer's PPE × Actual"). The "RI p-value" corresponds to the p-value (on the coefficient in the top row) from a randomization inference procedure where we randomize the "Shared Consultant" indicator over the set of potential peers. The values in parentheses are conventional asymptotic standard error estimates, adjusted for multi-way clustering at the level of the firm (i) and the (unordered) firm pair $\{(i, j), (j, i)\}$. See notes to Table 5 for additional details. All specifications include year fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.