



Pay Harmony: Peer Comparison and Executive Compensation

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Pay Harmony: Peer Comparison and Executive Compensation

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Abstract:

This study suggests that peer comparison affects both wage setting and productivity within firms. We report three changes in division manager compensation following a 1991-1992 controversy over executive pay. We argue that this controversy increased wage comparisons within firms, particularly those with geographically-dispersed managers – managers with the greatest information frictions. Following the controversy, pay in dispersed firms co-moves more and is less sensitive to individual performance. Relatedly, pay disparity between managers located in different states decreases relative to that of co-located managers. Finally, division productivity falls in dispersed firms, particularly among managers at the low end of the wage distribution.

JEL: J33, J44, M12, M52

Keywords: Executive Compensation, Pay-for-Performance, Internal Labor Markets, Peer Comparison, Firm Geography

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I. Introduction

Individuals care not only about their absolute income, but also about their income relative to others (e.g., Frank, 1985; Akerlof and Yellen, 1990; Luttmer, 2005).¹ Recent field experiments have demonstrated this effect by showing the impact of relative income comparison on employee satisfaction (e.g., Card et al., 2011). Beyond satisfaction, however, there is limited research on how income comparison affects other factors, including pay itself, particularly among top managers within firms, a group of highly competitive individuals at the extreme of the pay distribution. While one recent exception is Shue (2013), who shows that CEO pay responds to pay shocks of peers, to our knowledge no research has considered the effect of comparisons between “like” managers on wage contracts *within firms*. In this paper, we explore this question using a rich panel data on division manager pay and a controversy over CEO pay that occurred during 1991-1992 that culminated in a regulatory shock to pay disclosure. We propose that this controversy, combined with the increased transparency of executive pay, induced greater wage comparisons within firms, particularly by those that were unlikely to have shared information prior to these events. Our findings suggest that firms account for peer comparisons when designing executive wage contracts and also that these comparisons have productivity consequences for firms.

While firms may focus on an executive’s outside option to retain talent, internal labor markets can be equally, if not more, important (e.g., Baker, Gibbs, and Holmstrom, 1994; Lazear and Oyer, 2004, Tate and Yang, 2012). Whether managers care about relative income for behavioral reasons or whether they interpret pay differentials as an informative signal about future pay prospects or current ranking, peer

¹ Peer comparison of pay can be illustrated by the following (non-executive) anecdote related in Baron and Kreps (1999): “We recall an eminent labor economist who, while doing his stint as chair of his economics department – one of the best in the world – remarked in somewhat mystified fashion that his best-paid colleagues seemed particularly concerned not with how their annual raises compared with inflation, but instead how they stacked up with the raises earned by their other highly paid colleagues.” (pg. 256). Closer to our setting, Nickerson and Zenger (2008) relate how Harvard University was forced to reduce compensation of high-performing fund managers of the Harvard Management Company, the subsidiary managing the university’s \$27 billion endowment. Harvard took these actions largely due to faculty and alumni uproar over fund manager pay that, while appropriate for the external finance market, was several orders of magnitude greater than typical faculty salaries.

comparison can affect motivation and effort provision (Charness and Kuhn, 2007).² In practice, compensation consulting firms that specialize in designing executive pay emphasize the importance of “pay harmony” among managers within a firm.³ To ensure some semblance of internal equity, firms’ internal pay structures are commonly based on systems that specify pay ranges by job and level (e.g., Hay points) (Baron and Kreps, 1999). At the same time, there has been an increased emphasis in performance-based executive pay over the past several decades (e.g., Hall and Murphy, 2003; Cunat and Guadalupe, 2009; Frydman and Jenter, 2010; Murphy, 2012), offsetting this effort to maintain harmony. As a result, compensation consultants cite a critical tradeoff in structuring executive pay between the incentive effects of a strong pay-for-performance compensation system and the effects of peer comparison within firms.⁴ Given these multiple factors, the primary objective of our study is to explore whether top manager pay is, in fact, influenced by peer comparison within firms.

The primary empirical challenge that we face is to isolate the effects of peer comparison from unobserved factors that could explain our results,⁵ such as production interdependencies between managers or divisions, selection effects, and common factors in the firm’s environment (e.g., local labor-market conditions). Recent studies of peer influence, facing similar challenges, have used exogenous disclosures of information, such as peer pay and productivity rankings, that increase peer comparison without simultaneously affecting other factors in the environment (Card et al, 2012; Charness and Kuhn, 2005; Barankay, 2012). Our paper uses a related approach by exploiting a highly public 1991-1992 controversy over CEO pay that culminated in a 1992 SEC proxy ruling that mandated increased disclosure of executive pay.

² In this paper, we are agnostic about whether relative pay concerns arise for behavioral or informational reasons. Card et al. (2012) specifically focus on isolating these two mechanisms and find support for behavioral explanations. Other papers, such as Cohen, Frazzini and Malloy (2010) have focused on the information effects of peer groups.

³ The term *harmony* derives from the Greek *ἀρμονία* (*harmonía*), meaning “joint, agreement, concord.”

⁴ This tradeoff is similar to the observation by Rebitzer and Taylor (2011) that inequality aversion results in lower optimal incentive pay within firms.

⁵ This challenge is a version of the reflection problem discussed by Manski (1993) which focuses on empirical issues arising from attributing similar observed behavior within groups to peer (or social) effects.

Our empirical strategy is based on two assumptions. First, division managers that work closer to one another are more likely to come into direct contact with one another and share information by word-of-mouth. Consider, for example, two investment bankers working at the New York City headquarters of a large U.S. investment bank. Consistent with research on word-of-mouth effects (e.g., Hong, Kubik and Stein 2005), these co-located bankers are more likely to share information about each other's pay than a banker located in the firm's Los Angeles office.⁶ Second, the 1991-1992 pay controversy increased the sharing of wage information and this effect was more pronounced for geographically-dispersed managers who had more informational frictions and were therefore less likely to compare pay prior to the controversy. That is, as disclosure of top executive pay increased and discussions of pay became commonplace as a result of the 1991-1992 controversy, the Los Angeles bankers became likelier to seek out comparative pay information from the New York bankers. Our basic prediction is that wage comparisons will be more pronounced after the controversy in firms where managers are located at a distance from one another in comparison to firms where managers are in close proximity.

To explore this prediction and the potential effects of peer comparison on pay, we conduct three sets of differences-in-differences analyses at the division level. First, we define *pay-referent* sensitivity (PRS) as the degree to which division manager pay changes with the average pay of all other division managers within the firm.⁷ Consistent with the executive compensation literature, we measure *pay-performance* sensitivity (PPS) as the degree to which division manager pay changes with changes in division performance. Our first differences-in-differences analysis measures the change in PRS and PPS

⁶ This is based on a story as told to us by an executive who was the Chief Development Officer (CDO) and Managing Director (MD) at a large U.S. investment bank in the early to mid-1990s. Two MDs at NYC headquarters had similar jobs (e.g., number of accounts, number of travel days) and similar performance. On the same day, near the end of the fiscal year, the MDs were individually informed by the CDO about their own annual compensation (bonuses and proposed salary increases). The following day, the lower-paid MD was in the office of the CDO to ask: "Why am I paid less?" This dynamic was much less common with MDs working in the firm's international offices. For example, the MD in Hong Kong was less informed about pay differences (due to delays in information sharing about pay) since they didn't bump into each other in the hallways of headquarters. More broadly, the notion that pay is more likely to be shared between managers that are geographically proximate was confirmed based on interviews with compensation consultants and senior executives, as well as consistent with psychological studies that show that social comparison is influenced by physical propinquity (Baron and Kreps, 2013).

⁷ We incorporate a modified linear-in-means term into a standard wage equation framework. This approach involves several immediate challenges that we discuss later in the paper.

before and after the CEO pay controversy within geographically-dispersed firms relative to concentrated firms. We find increases in PRS (greater pay co-movement) and decreases in PPS in dispersed firms after 1990 (the beginning of the controversy), and no change in concentrated firms. Notably, these effects occur entirely within the years during and immediately following the pay controversy.

While pay co-movement is one measure of sensitivity to referent pay, it not necessarily informative about pay disparity within firms. To capture disparity, we conduct a second differences-in-differences analysis that examines the mean distance between the excess pay of pairs of division managers within a firm. We find that, while overall pay distance increases during and after the controversy (consistent with the overall trend toward more performance pay and greater pay disparity during the period), distance increases less between managers of divisions located in different states than between managers located in the same state.

Finally, we conduct an analysis of division productivity to look for a performance impact on dispersed firms after the controversy. We find that division productivity increases less after the controversy in dispersed firms than in concentrated firms and that this effect is most pronounced for managers at the low end of the wage distribution of division managers within the firm. While the productivity findings may have a number of alternative explanations and are only suggestive, they are consistent with managers expending less productive effort upon discovering that their pay is low relative to peers, a potential real cost of peer comparison.

We argue that these results, taken together, support the notion that firms account for horizontal peer comparison when designing executive wage contracts and that this comparison imposes real costs on firms. The findings are also consistent with a tradeoff between the incentive effects of performance pay and effects of peer comparison that arise from unequal pay.⁸

⁸ An interesting question is why we might find a tradeoff between these two effects: performance pay vs. peer comparison. If firms provide equal pay opportunity to division managers (in ex ante contracts), but performance differences result in differences in pay (ex post), managers should be compensated according to their marginal product of labor and not complain about unfairness in pay policies. However, division manager performance is difficult to measure for a long list of reasons (e.g., multi-tasking, transfer prices, uncontrollable events), in contrast to using performance of a sales representative, for example. This measurement problem ultimately leads to greater

We also investigate several alternative explanations for our results. One concern is that unobserved, time-varying trends may affect dispersed firms differently than concentrated firms. At odds with this explanation, we observe the timing of the changes in pay co-movement and find a discrete, significant increase in PRS and decrease in PPS in the two-year period immediately following the CEO pay controversy and no pre- or post-trends. We also conduct a set of placebo tests around the timing of the passage of the rule as well as run our main specification with division state-year fixed effects (to absorb changes in local market conditions), and replicate our main results using a propensity-score matched sample of firms. The findings from this series of tests are inconsistent with long-run secular trends that do not have a specific break during the 1991-1992 period and affect dispersed firms differently than concentrated firms.

Another alternative explanation is that the 1991-1992 controversy provided better pay information on positions at the top of the hierarchy and that the increase in pay co-movement and reduced disparity in division manager pay was in response to a *vertical* comparison of pay, and not a comparison between “like” managers (or horizontal comparison of peers). Specifically, the focus on CEO pay led division managers to compare their pay to the CEO and, by narrowing the vertical gap, pay increased for *all* division managers. This increase then resulted in greater co-movement and reduced disparity of division manager pay, giving the appearance of horizontal pay comparisons. We find no evidence supporting this prediction, and if anything find the opposite (i.e., the pay gap widens and CEO-division manager pay co-moves less).

We also consider several specific alternative explanations, including different trends in IT productivity, increased horizontal rotation of division managers, different recovery following the 1990-91 recession, and whether our measures of geographic concentration coincide with industry or headquarter-

discretion in the determination of pay. *We argue that it is this discretionary or subjective component of pay that could give rise to a sense of inequity even with equivalent ex ante opportunity in pay.* Finally, even in the absence of measurement problems, there may be a tradeoff between the effects of performance pay and peer comparisons in the presence of preferences based on inequality aversion (e.g., Rebitzer and Taylor, 2011).

state affiliation. Altogether, we do not find compelling evidence that these alternative explanations are driving our results.

The unique contribution of the paper is to provide evidence of how firms' pay policies respond to concerns about peer comparison and internal equity, which, to our knowledge, has not been documented elsewhere. This is important because our findings suggest that access to pay information can increase the importance of internal relative to external labor markets in determining firms' wage policies.

Our findings relate to several literatures in addition to the extensive research on executive compensation (e.g., Murphy, 2012). The findings contribute to the research on peer effects and social comparison in behavioral economics (e.g., Camerer and Malmendier, 2007; Rebitzer and Taylor, 2011; Shue, 2013) and management/strategy (e.g., Nickerson and Zenger, 2008; Larkin, Pierce, and Gino, 2012), as well as the effect of disclosure and feedback of relative standing on employee motivation and productivity (e.g., Barankay, 2012; Marino and Ozbas, 2013). Our study suggests that peer comparison is an important attribute of internal labor markets, specifically at the top levels within a firm, with consequences for pay and potentially for productivity.

Our approach to manager behavior also builds on several earlier works in finance that explore the geographic proximity of agents and the impact on information sharing and social comparisons between agents. For example, Hong, Kubik and Stein (2005) show that proximate mutual fund managers are more likely to buy/sell the same stock and explain their findings by investors sharing information about stocks by word-of-mouth. Landier, Nair and Wulf (2009) show that divisional employees that are located closer to headquarters receive preferential treatment through fewer layoffs, especially in small communities, suggesting social factors are important in corporate decisions. Malloy (2005) shows that geographically-proximate equity analysts are more accurate than other analysts suggesting a geographic component to agency problems in the industry.

Finally, this paper contributes to the ongoing policy debate on the consequences of transparency and mandatory information disclosure to investors (e.g., Hall, and Murphy, 2003; Greenstone, Oyer and Vissing-Jorgensen, 2006; and Hertzberg, Liberti, and Paravisini, 2011), and contributes to the literature

on pay secrecy (e.g., Lawler, 1965; Bewley, 1999) which suggests that employers may have an incentive to not disclose pay. The remainder of the paper is organized as follows. Section II briefly discusses theory related to executive pay. Section III describes our empirical strategy and the data. Section IV outlines and discusses our results. Section V concludes.

II. Theoretical Background: Pay-for-Performance vs. Peer Comparison

The principal-agent model is a workhorse in economics that has implications for how firms set executive pay (c.f., Murphy, 2012; Jensen and Meckling, 1976). The main theoretical implication for firms is that, to elicit optimal effort from agents, principals should link pay to performance. Wage contracts are typically assumed to have the following linear form, in which pay-performance sensitivity is defined as b_1 and the optimal choice of b_1 depends on the underlying model:

$$(1) \quad w = a + b_1 y$$

where w is the wage and y is the measurable output (or performance) of the agent. Generally, a is determined by time-invariant personal and market characteristics, as well as time-varying factors that affect the individual's participation constraint (and bargaining power) within a firm. The magnitude of b_1 is chosen to elicit the optimal effort from the agent. Pay setting within a firm, therefore, is a function of both external and internal markets (e.g., Baker, Gibbs and Holmstrom, 1994; Lazear and Oyer, 2004; Tate and Yang, 2012) and the underlying utility of the agent.

However, while there is extensive research in economics on the determinants of executive pay, in practice, the process is more social than the existing research suggests.⁹ Social comparison has long been studied in the social psychology literature (e.g., Festinger, 1954). One of the key questions in this literature is: who is the salient referent? Festinger's original proposition is that similar others would be

⁹ This is despite an early mention of fairness in wages dating back to Hicks (1963) in *The Theory of Wages* (pg. 317). "The labor market is . . . a very special kind of market which is likely to develop 'social' as well as purely economic aspects. . . . For the purely economic correspondence between wages paid to a particular worker and his value to the employer is not a sufficient condition of efficiency: it is also necessary that there should not be strong feelings of injustice about the relative treatment of employees since these would diminish the efficiency of the team."

frequently chosen as referents. In our setting, we propose that division managers in a multi-business firm represent each other’s salient referents—i.e., they are in roles of similar responsibility within the firm and compete for the same pools of resources and positions for promotion.

To provide a structure to guide our analysis, we offer the following highly-simplified utility function, in which an agent cares about both absolute and relative pay:¹⁰

$$(1a) U(w, \bar{w}_{-d}) = u(w - c) + Iv(w - \bar{w}_{-d})$$

where $u(\cdot)$ is the agent’s utility from absolute pay and $v(\cdot)$ is utility from relative pay. w represents wages and \bar{w}_{-d} is the reference point for peer wages. While \bar{w}_{-d} can be defined in many ways, here, we simply assume that the reference point is defined as the mean peer pay.¹¹ c is the cost of effort, and I is a measure ranging from 0 to 1 of the degree of information available on peer pay.. We consider the simplest case, in which $u(\cdot)$ and $v(\cdot)$ are linear,¹² the wage contract continues to take the linear form $w = a + b_1y$ and the participation constraint binds. In this setup, $w - c + I(w - \bar{w}_{-d}) = 0$ and wages vary with peer wages as follows: $\frac{dw}{d\bar{w}_{-d}} = \frac{I}{1+I}$.

We can represent the linear contract, then, as

$$(2)w = a' + b_1y + b_2\bar{w}_{-d}$$

where $a = a' + b_2\bar{w}_{-d}$ and $b_2 = \frac{I}{1+I}$. b_2 is a positive, concave function of I , such that pay increasingly co-moves with peer pay as the available information on peer pay increases. There are two classes of practical explanations for this relationship. First, managers may care directly about relative income in

¹⁰For an analysis and a review of the principal agent model from a behavioral perspective and the relationship to employee relationships and labor markets, refer to Rebitzer and Taylor (2011).

¹¹ In a typical linear-in-means model of social comparison, individuals gain utility if their actions match the mean action of their referent group, generally simplified as a linear function of the overall group choice. This model, if implemented literally, is likely to be significantly biased (Manski, 1993). In the empirical design section, we discuss how we address this challenge.

¹² For simplicity, and since it is not the focus of our paper, we generally assume that utility from information about relative pay is linear. This abstracts away from the literature on asymmetric preferences—e.g., Fehr-Schmidt preferences (Fehr and Schmidt, 1999) and loss aversion—which would imply that the disutility from being below mean pay is greater than the utility from being above mean pay. However, in our productivity analysis, we explore asymmetries and whether divisional productivity varies by position in the distribution of wages. See Rebitzer and Taylor (2011) for an illustration of how asymmetric inequality aversion in a principal agent model requires the incentive pay parameter to “to do the ‘double duty’ of eliciting work effort and determining the extent of expected pay inequality in the firm. As a result, the firm must compromise along an important dimension by lowering incentive pay and reducing the effort level elicited from workers.” (pg. 730)

addition to absolute income for strictly behavioral reasons (e.g, Frank, 1985; Camerer and Malmendier, 2007; Rebitzer and Taylor, 2011). Second, managers may use information about pay differences to: (i) update expectations of future pay and career prospects—for example, through a promotion (e.g., Card et. al. 2012); (ii) infer performance relative to peers;¹³ or (iii) most generally, become more informed about pay levels.

From equation 2 and the associated discussion, we explore several possible implications for pay setting. First, consistent with peer comparison, executive pay should exhibit pay-referent sensitivity or PRS. Specifically, b_2 in equation 2 should be positive and significant, particularly when pay information about peers is more readily accessible or when peers are immediately salient (for example, geographically proximate).¹⁴ Second, the existence of both agency behavior and peer comparison within firms leads to a different optimal wage contract for firms, as discussed in Rebitzer and Taylor (2011) and observed empirically by Encinosa, Gaynor and Rebitzer (2007) in physician contracts. Finally, an increase in peer comparison, all else being equal, should compress intra-firm pay.

III. Empirical Strategy

III.A. Division Manager Pay in Multidivisional Firms

¹³ One variant of this information story is that managers use increased pay information to infer their standing when compensation depends on relative performance evaluation (Holmstrom 1982) and that, as a consequence, they increase collusion or sabotage activities (Gibbons and Murphy 1990). While this RPE argument is broadly consistent with our information story, we do not believe that it is a large factor in our context because this subterfuge is likely to be detectable over the long run since firms have a median of only four division managers, most of which have substantial career tenures at each firm. These costs of RPE also did not arise in any of our practitioner interviews.

¹⁴ One implicit assumption of this discussion, consistent with the practitioner concept of “pay harmony,” is that the effect of increased pay disclosure is a net increase in disutility in the manager population that leads to increased pay co-movement and less disparity (although our empirical setup allows for the opposite result to be obtained). A positive estimate of b_2 implies that any additional utility accrued to the top performers from learning about their above-mean pay is more than offset by the disutility incurred by the lower-paid managers. This assumption diverges from Lazear (1989, 1991), who points out that it may be “more important to keep the best workers happy than the worst ones” (However, if this were true, then why wouldn’t firms disclose pay, which in practice, is uncommon?). On the other hand, it is consistent with aggregate Fehr and Schmidt (1999) asymmetric preferences, where the disutility incurred by workers from being below the reference point is greater than the utility gained from being above the same point. While asymmetric responses are not the main focus on this paper, we do investigate them later when looking at productivity responses to pay disclosure.

To investigate the effect of peer comparison on firm pay policies, we use a proprietary dataset based on a confidential compensation survey conducted by Hewitt Associates, a leading human resources consulting firm specializing in executive compensation and benefits (described in detail in section III.D). We analyze pay for multiple, domestic division manager positions inside large U.S. firms over a 14-year period. A division manager in our data, also known as a business unit head, is the most senior manager in a division with overall P&L responsibility for that business unit.

Our data and setting are advantageous in evaluating the effects of peer comparison within firms for a number of reasons. First, our panel data include multiple “like” positions within firms—a well-defined reference group—that allow comparisons of similar, standard positions. It is important to note that division heads are senior positions with bargaining power over pay-setting. Second, we have detailed pay data for multiple division managers within a firm and performance data for their respective divisions. These data allow us to estimate pay-performance sensitivity (PPS) and pay-referent sensitivity (PRS), as well as pay distance between manager pairs. Third, we are able to observe and exploit variation in the geographic location of divisions in our sample. Fourth, according to Hewitt, the market for division managers is generally a national, rather than local, labor market, and so local market effects should be less of a challenge in this setting than when studying less-senior employees. Finally, the types of firms in our sample and the period covered allow us to exploit the 1991-1992 pay controversy to identify peer comparison. Our sample includes large, publicly-traded U.S. firms (300+ of Fortune 500)—precisely the target of the media attention and the associated 1992 SEC proxy disclosure ruling, which, fortuitously, occurs roughly in the middle of our sample period (1986-1999).

III.B. Empirical Design

The fundamental empirical challenge that we face is to isolate peer influence from unobserved factors that cause co-movement of division manager pay. The most obvious unobserved factors in our setting are production interdependencies, selection effects (i.e., firm-manager matching), and common shocks to the firm or changes in the firm’s environment that influence pay and performance across

divisions. This challenge is similar to that raised by Manski (1993) in his discussion of the reflection problem endemic to research on social effects.¹⁵ Ideally, information on peer pay would be randomly assigned across employees within firms, similar to Card et al (2012). However, since this treatment is unrealistic given our context, we instead attempt to address the reflection problem by employing a differences-in-differences research design in which we argue that (i) the increased public attention on and associated regulation of the CEO pay had a greater effect on non-proximate managers and (ii) these managers were not simultaneously affected by unrelated (unobserved) trends that would lead to similar changes in pay. We discuss this logic in more depth below and stress it in a series of robustness analyses at the end of the paper.

III.B1.1 The 1991-1992 Pay Controversy

During 1991 and 1992, public and media attention on executive pay dramatically increased. This increase was driven by the confluence of several factors that are summed up as follows by a 1992 *Wall Street Journal* article:

“The anger about high chief executive pay has been building slowly for years. But with the recession, big layoffs and a coming presidential election, executive pay has become an explosive issue lately. From the start, presidential candidates Bill Clinton, Bob Kerrey and Paul Tsongas regularly inveighed against excessive CEO pay.” (WSJ, 4/22/92)

The attention by presidential candidates was supplemented by high profile books and media articles on CEO pay, summarized below (Murphy, 1999, pg. 50):

“Although the business press had followed CEO pay for decades, CEO pay did not really become a public “issue” until 1991. Feature stories on CEO pay aired on the nightly news broadcasts of the three major networks in the Spring of 1991, and CNN, *60 Minutes* and *Nightline* devoted segments to CEO pay. The controversy heightened with the November 1991 introduction of Graef Crystal’s (1991) expose on CEO pay, *In Search of Excess*, and exploded following President George Bush’s ill-timed pilgrimage to Japan in January 1992, accompanied by an entourage of highly paid US executives.”

¹⁵ See Blume et al. (2011) for a review of identification in social interactions.

This trip to Japan was particularly galling during an election year because Japanese companies at the time were widely perceived as outperforming American firms and yet American executives were paid substantially higher compensation than their Japanese counterparts.

To validate the timing of heightened media attention, in Figure 1, we plot the number of articles appearing in the *Wall Street Journal* between 1986 and 1999 with any of the following phrases: “executive compensation,” “executive pay,” “CEO compensation,” and “CEO pay.” As the figure illustrates, the peak period of media coverage was in 1992, with the run-up beginning in the prior year.¹⁶

<< Insert Figure 1 about here >>

In October 1992, in response to the heightened visibility of CEO pay, the Securities and Exchange Commission (SEC) “announced sweeping new rules affecting the disclosure of top executive compensation in the annual proxy statement” of publicly-traded firms (Murphy, 1999, pg. 50) particularly performance-based pay. The two major changes required by the ruling were (i) substantially more detailed compensation information for top executives in a standardized tabular format and (ii) the addition of a narrative section within the proxy in which the level of executive compensation for that year had to be justified in the context of firm performance.

III.B1.2 Empirical approach

Although we do not develop a formal model, our logic and its key implications are relatively straightforward. In our setup, there are two types of division managers: proximate managers who work in close range to their peers and non-proximate managers who work at a distance. Both types gain the same utility from possessing information about peer pay but have different fixed costs of obtaining the information. For proximate managers, word-of-mouth effects lead to a relatively low cost relative to managers at a distance, for whom the likelihood of chance meetings and strength of informal social relationships are lower.

¹⁶ This pattern is similar to that reported by Kuhn and Niessen (2012) who employ a more detailed approach to measure public opinion about executive compensation by analyzing all newspaper articles and using linguistic software to capture the negativity of press coverage of CEO pay.

This setup has two implications: first, we should observe stronger evidence of wage comparison within firms with a higher proportion of proximate managers, all else equal. Second, an increase in the value of knowing peer pay will induce relatively more wage comparison in non-proximate managers. The intuition behind this second point is that, whereas proximate managers will already exchange pay information due to low pre-existing sharing costs, non-proximate managers require higher benefits to overcome the frictions of sharing over a distance.

What form might an exogenous increase in the value of peer pay information take? We propose that one possible form is through additional public attention to top manager pay, as occurred during the 1991-1992 pay controversy. This increase in the value of information about peer pay can be realized either by increasing the utility of the information or reducing the costs of acquiring it. With greater public attention to pay, the utility from higher relative income can rise for purely behavioral reasons (because, for example, income is tied to social status) or for informational reasons (because, for example, the informativeness of pay as a signal of relative ability increases as the link between pay and an executive's intrinsic value is publicly debated). Alternately, the cost of acquiring information may fall as information frictions are reduced. This may occur again for behavioral reasons - social taboos on discussing pay fall, which in turn eases communication between managers with fewer chance meetings and weaker interpersonal relationships - or simply because the frequency of discussing pay on aggregate increases, thereby increasing the word-of-mouth effect even for managers at a distance.

It is important to note that, in our setting, public attention to pay primarily focused on CEO compensation, rather than pay within firms. From an empirical perspective, therefore, it will be important to distinguish between vertical pay comparisons (between manager and CEO pay) and horizontal comparison between peers. While both may be present within the firms in our sample, our findings do not appear to be driven by vertical comparison alone.

III.B2 Empirical Models

In this section, we describe the models we use to implement the differences-in-differences analyses.

III.B2.1 Wage Equation

Equation 2, translated into a form that can be empirically tested, becomes a standard wage model augmented by a modified linear-in-means specification to capture peer influence:

$$(3) w_{dt} = \alpha + \beta_1 s_{dt} + \beta_2 \bar{w}_{-dt} + D'_{dt} \beta_{31} + F'_{dt} \beta_{32} + \eta_d + d_t + \epsilon_{dt}$$

Here, d indexes divisions within a firm at time t . A division manager's (log) wage in period t is a function of division performance, s_{dt} , and referent pay, defined as the average (log) wage of all *other* division managers within the firm, excluding own wage, \bar{w}_{-dt} .¹⁷ A vector of both division, D'_{dt} , and firm characteristics, F'_{dt} , are included as controls. We also include year fixed effects, d_t , and cluster standard errors at the firm level. Our basic specification includes division fixed effects, η_d , which control for unobserved, time-invariant, division or firm heterogeneity and allow us to interpret coefficients as the change in manager pay with changes in the independent variables. This division fixed effects specification analyzes similarities in pay *changes*.¹⁸ However, to explore peer comparison in pay *levels*, we also estimate regressions without division fixed effects.

The main coefficient of interest in this specification is β_2 , which captures pay-referent sensitivity (PRS). As mentioned above, the main empirical challenge in estimating β_2 is to separate peer influence—which we refer to hereafter as ψ —from unobservable factors—hereafter θ , —that also cause pay co-

¹⁷ Linear-in-means specifications typically use the average of the whole group, with the underlying assumption that any one participant has a small marginal impact on the whole group. Because that assumption is not valid in our context, we use the average of all other division managers to calculate our mean pay variables. This definition complicates the interpretation of the standard errors because of the potential for correlated errors between observations within a given firm-year. In addition to clustering by firm and using division fixed effects for our primary specifications, we also run simulations to test whether this calculation can introduce spurious correlations and significance levels into the data. We found no such biases or efficiency distortions.

¹⁸ While we know the state in which a division is located, the inclusion of division fixed effects subsumes fixed effects for the state of location and, as such, controls for time-invariant local labor-market conditions. Moreover, our results are robust to inclusion of division state * year fixed effects. While we have rich information about division manager positions, we know little about the individual manager filling the position. We do know the tenure of the manager in the position and, hence, can estimate our regressions with manager fixed effects instead of division fixed effects. The results are qualitatively similar.

movement. We also estimate changes in β_1 to explore a proposition by Rebitzer and Taylor (2011) that the presence of peer comparison within firms results in lower powered incentive contracts.

We make the following identifying assumption: the 1991-1992 pay controversy did not differentially increase θ in dispersed versus concentrated firms. So, for example, we assume that the controversy did not cause dispersed firms to hire more similarly-productive managers than did concentrated firms. More generally, we assume that there were no other concurrent secular changes around the time of the controversy that caused differential increases in θ in dispersed versus concentrated firms. We discuss potential scenarios in which these assumptions may be violated in a later section on alternative explanations. We also present a more formal discussion of our identification strategy in Appendix B.

Turning to our empirical models, we compare estimates of β_1 and β_2 for firms operating in different information environments. We assume full information about pay in concentrated firms ($I=1$) and, specifically, that proximate managers are informed about each other's pay and engage in peer comparison. We likewise assume no information in dispersed firms ($I=0$). We then employ a differences-in-differences approach (described above) that compares the changes in β_1 and β_2 as responses to the controversy in dispersed and concentrated firms. To isolate the effect of ψ , we assume that increases in information from the controversy are greater in dispersed firms than in concentrated firms ($\Delta I_{disp} > \Delta I_{conc} \sim 0$) and, consistent with the identifying assumptions discussed above, that any changes in θ at the time of the controversy are no different in concentrated and dispersed firms ($\Delta\theta_{disp} = \Delta\theta_{conc}$).¹⁹

Note that defining the precise timing of the pay shock is a challenge, since the controversy spanned two calendar years and the effects could have extended beyond those two years as well. We

¹⁹ Our differences-in-differences wage methodology classifies concentrated firms for the entire period and dispersed firms after 1990 as full information environments. Note, then, that this approach should lead to a conservative analysis of peer effects since it is likely that, because sharing pay information increased in concentrated firms post 1990, because there was some sharing of pay information within dispersed firms prior to 1990, or because of incomplete sharing of pay information in dispersed firms after 1990.

define the “post” period as 1991 onwards, which marks the post-period as running from the first year of the controversy. We will examine the specific timing of the observed effect as part of our first analysis.

III. B2.2. Pairs Distance Analysis and Pay Disparity

While our wage regressions aim to identify the effect of peer comparison on pay co-movement, they are not informative about intra-firm pay disparity. To address this, building on Shue (2012), we conduct a second analysis that measures distances between pay of pairs of division managers within firms.²⁰

The analysis compares the mean absolute difference in pay residuals between two division managers operating in the same state within a firm with the difference between two managers operating in different states. We again exploit the 1991-1992 pay controversy and measure changes in pay distance of same- and different-state managers. As a response to the general push for increased pay for performance, overall pay distance between managers may have increased from 1991 onwards as firms increased overall performance-based pay. However, as with concentrated firms in our earlier analysis, we assume that managers operating in the same state shared more pay information before the controversy than did managers in different states. Therefore, we expect pay disparity to increase less between managers in different states where wage comparison increases. Estimation follows a two-stage procedure similar to that in Shue (2012).

$$(4) \text{ 1}^{\text{st}} \text{ Stage: } w_{dt} = \gamma_0 + \gamma_1 X_{dt} + \hat{w}_{dt}$$

$$(5) \text{ 2}^{\text{nd}} \text{ Stage: } |\hat{w}_{dt} - \hat{w}_{et}| = \delta_0 + \delta_1 * post90 + \delta_2 diff_{state} + \delta_3 diff_{state} * post90$$

As earlier, d indexes division managers within a firm and t indexes firm years in the panel data. Observations in the first stage are unique at the manager-position, firm-year level. The first-stage regression (equation 4) is similar to the wage regressions in the earlier analysis (equation 3), with one important difference—it does not include the referent pay measure. Division manager wages w_{dt} are

²⁰ This method is analogous to that in Shue (2012). She compares section vs. class cohorts of randomly-assigned HBS MBA students. We compare pay for division managers within a firm, who are located in the same state vs. different states, before and after the pay controversy.

regressed on X_{dt} , which includes manager, division, firm, industry, and time controls. The objective of the first stage is to estimate “abnormal” wages beyond those explained by observable determinants of division manager pay (e.g., division size and tenure in position). As such, the residuals \hat{w}_{dt} from the first-stage regression measure this unexplained component of w_{dt} and are used in the second stage.

In the second stage, we create all possible pairs of division managers within the firm in a given year. Note that division manager positions in different firms are never paired; nor are division managers across years. The unit of observation in the second stage is a pair of division manager positions within a firm in a given year. The pair absolute difference is then regressed on several dummy variables and associated interactions: $diff_{state}$ for whether d and e are located in different states and $post90$ to designate years after increased pay visibility from the CEO pay controversy.²¹

Referring to equation 5, δ_0 represents the mean distance in pay residuals in the pre-period (1986-1990) between two managers in a firm that are located in the same state, while $\delta_0 + \delta_1$ are post-1990 distances for same-state managers. Similarly, $\delta_0 + \delta_2$ is the pre-period mean distance between two different-state managers, while $\delta_0 + \delta_1 + \delta_2 + \delta_3$ is post-1990 distance for different-state managers. Evidence in support of peer influence is a negative and statistically significant estimate of δ_3 , which indicates that the change in mean distances for different-state managers *within the same firm* before and after 1990 is smaller than the change for same-state managers within the same firm. We summarize the empirical implications from the above discussion in Table A1 (Appendix).

III. C. Data

The primary dataset used in this study includes a panel of more than 300 publicly-traded U.S. firms over the years 1986-1999, spanning a number of industries. The data are collected from a

²¹ Calculation of significance levels is complicated by each manager appearing in multiple pair-wise observations in a given year. To address this correlation issue, we follow methods in Shue (2012) and estimate standard errors and significance levels using Monte Carlo simulations that employ non-parametric permutation tests. For each permutation, managers are shuffled into random states and manager-pairs are shuffled into random post90 designations. Both the state and post90 assignments match the underlying distributions for each firm. State and firm assignments persist for the entire placebo test to account for autocorrelation and firm-specific factors. We generate 10,000 placebo estimates and calculate 2-sided standard errors and p-values.

confidential compensation survey conducted by Hewitt Associates, a leading human resources consulting firm specializing in executive compensation and benefits.²² The survey is exceptionally broad in that it collects data on many senior and middle management positions, including both operational (e.g., Chief Operations Officer and Division CEO) and staff (e.g., Chief Financial Officer and Head of Human Resources) positions. The survey typically covers all the positions at the top of the hierarchy and a sample of positions lower down. In this paper, we focus on the most-senior position in a division, which is defined in the survey as “the lowest level of profit center responsibility for a business unit that engineers, manufactures and sells its own products.” We focus on US-based division managers because we have multiple observations per firm and it is a managerial position that is consistently defined across firms. The dataset is rather unique because it allows us to identify changes in pay within division manager positions over a 14-year period that is characterized by significant change in pay practices.

The data for each position include all components of compensation, including salary, bonus paid, ex ante valuation of annual grants of restricted stock and stock options, and other forms of long-term incentives (e.g., performance units).²³ An observation in the dataset is a division managerial position within a firm in a year. To ensure consistency in matching these positions across firms, the survey provides benchmark position descriptions and collects additional data for each position, leading to a dataset rich in position characteristics. As a result, in addition to data on all aspects of compensation for multiple division manager positions, the dataset includes division-specific characteristics, such as: job title; the title of the position to whom the position reports (i.e., the position’s boss); division sales; number of employees under the position’s jurisdiction; industry of operation; geographic state of location; number of positions between the division manager position and the CEO in the organizational hierarchy (division

²² For a detailed description of the survey, please refer to Appendix A. Based on several analyses, we conclude that the survey sample is most representative of Fortune 500 firms.

²³ The ex-ante values of all components of long-term incentive pay are computed by Hewitt. For example, stock option grants are valued using a modified version of Black-Scholes that takes into account vesting and termination provisions in addition to the standard variables of interest rates, stock price volatility, and dividends. As is standard practice among compensation consulting firms, the other components of long-term incentives are valued using an economic valuation similar to Black-Scholes that takes into account vesting, term provisions, and the probability of achieving performance goals.

depth); an indicator of the incumbent's status as a corporate officer; and the manager's tenure in the position.

The above data are supplemented with financial and headquarters location information from Compustat and firm-level information technology investments from Harte-Hanks. Finally, we construct a number of variables that are used as controls and that we will describe in the results section.

In Table 1 (Panel A), we present descriptive statistics for the firms and divisions in the sample. While the dataset includes more than 300 firms, the exact number varies over the period, as firms enter and exit as survey participants. The firms in the sample are large, well-established and profitable, with average size of sales of \$8.5 billion, market capitalization of \$9.4 billion, 44,000 employees and return on assets of five percent. The average number of divisions reported in the survey for the sample firms is 4.6. Next, turning to divisional statistics, the mean size of divisions is \$752 million in sales and approximately 3000 employees. The average tenure in position is 42 months; approximately 23 percent of the division managers are corporate officers; and there are 1.4 positions between the CEO and division managers, on average. Average annual division manager pay is \$209K (salary), \$300K (salary plus bonus paid), and \$460K (total compensation defined as the sum of salary, bonus paid, and the ex ante valuation of annual grants of restricted stock, stock options and other long-term incentives). Finally, the sample firms span many industrial sectors of the economy, with some concentration in the food, paper, chemical, machinery, electrical, transportation equipment, instrumentation, communications and utilities industries.

<< Insert Table 1 about here >>

Using the information on division state of location from the Hewitt dataset and headquarters' state and county of location from Compustat, we attempt to characterize divisional proximity to headquarters. We construct a firm-level measure of geographic dispersion by computing the proportion of divisions in the same state as headquarters (mean of 0.48 for the sample). We then construct quintiles of geographic concentration using this firm measure and assign values ranging from 1 (least concentrated) to 5 (most concentrated). In Table 1 (Panel B), we split the sample into concentrated firms (4th and 5th quintiles) and dispersed firms (1st and 2nd quintiles) and report key statistics for these two subsamples. We

exclude the middle quintile in our analyses which, while reducing the power of the tests, enables us to make sharper comparisons between firms at the geographic extremes.

As can be seen from this table, firms in these two categories are roughly similar in several characteristics. The biggest difference is that concentrated firms have fewer and bigger divisions than dispersed firms, based on means. However, in comparing median sales and employees, division size is generally comparable across firm geography.²⁴

Finally, Figure 2 depicts aggregate changes in pay disparity within firms over time. It plots the median coefficients of variation (std dev/mean) for the three main pay measures across both concentrated and dispersed firms. The figure shows that, for dispersed firms, pay variance declines in the years during and just after the 1991-1992 pay controversy for all three pay measures. In contrast, there is no comparable decline in concentrated firms. These patterns are broadly consistent with our claim that dispersed firms were differentially affected by the increased pay visibility and are consistent with more peer comparison leading to less pay disparity (or more pay compression). We now turn to the results of our multivariate analysis.

<< Insert Figure 2 about here >>

IV. Results

We begin by documenting both pay-performance sensitivity (PPS) and pay-referent sensitivity (PRS) in our sample of firms in standard, position-level, wage regressions (Table 2). We then present our first differences-in-differences analysis of how pay co-movement within concentrated and dispersed firms changes before and after the 1991-1992 pay controversy (Tables 3 and 4). Our second differences-in-differences analysis looks at how pay distance changes in proximate (same state) and non-proximate (different state) manager pairs within firms (Table 5). We then present our exploratory analysis of the effect of pay disclosure on division productivity (Table 6). We conduct placebo tests in Table 7. Finally,

²⁴ To address concerns about unobserved differences in dispersed and concentrated firms, we find robust results when we estimate models using propensity score methods (reported in Table A2).

we present evidence at odds with a purely vertical comparison explanation (Table 8) and consider several alternative explanations of our results (Tables in Appendix). We use three pay measures—salary, salary plus bonus, and total compensation—throughout our analysis.

IV.A. Pay-Referent Sensitivity (PRS) and Pay-Performance Sensitivity (PPS)

We begin by estimating a standard wage equation for division manager positions (equation 3). In Table 2, columns 1-3, we regress the logarithm of the three pay measures on division and firm performance, while controlling for a set of covariates common to wage regressions. All regressions include division manager fixed effects (and cluster standard errors by firm); thus, the coefficients can be interpreted as correlations between the changes in pay and changes in the independent variables. Division performance is measured as log division sales, which can be interpreted as sales growth in our fixed effects specifications. We use two measures of firm performance: return on assets (%) and log firm sales, also interpreted as changes in those measures in our fixed effect specifications.²⁵

Consistent with pay-for-performance contracts, we find evidence that firms link pay to both division (local) and firm (global) performance. The coefficient on division sales represents the *pay-division* performance sensitivity, which is positive and significant for all three pay measures. The coefficients on firm performance measures represent the *pay-firm* performance sensitivity and are positive and significant for salary plus bonus and total compensation measures, although not for salary. While annual bonuses and long-term incentives are related to firm performance measures, salary increases (i.e., raises) appear to be driven primarily by division performance. The correlations with the additional controls are as expected. Pay is higher for managers with longer tenure in the job, managers that are officers, and managers closer to the top of the organizational hierarchy (lower depth). We find no relation with the number of other divisions in the firm.

²⁵As a robustness test, we also include growth in market capitalization (change in logarithm of market capitalization). Our results are qualitatively similar. We exclude this variable from the basic specifications for ease of exposition.

We now turn to the primary focus of our analysis: within-firm pay co-movement. In Table 2, columns 4 through 6, we add both referent pay (the log of the firm-wide average pay of other division managers) and an external benchmark (log of industry average pay for all division managers in the division's industry, but external to the firm). The most notable result is that division manager pay increases as the firm-wide average pay of *other* division managers increases. The coefficients on referent pay are positive and significant for all pay measures. So, in addition to pay-performance sensitivity (PPS), we find evidence for pay-referent sensitivity (PRS). Our PRS measure (the coefficient on referent pay in a log-log specification) can be interpreted as the elasticity of a manager's pay with respect to referent pay. In column 4, the elasticity of base salary is 0.28 (column 4)—which means that a doubling of the average salary of other division managers inside the firm is associated with a 28 percent increase in the salary of an individual division manager. The corresponding PRS measures for the other two pay measures are 0.44 (column 5) and 0.55 (column 6), respectively.

<< Insert Table 2 about here >>

Positive PRS is consistent with peer influence (ψ in our discussion above), as well as with omitted factors that also can drive pay co-movement (θ in our discussion above). As discussed earlier, one factor is division managers sharing a common performance shock or a common change in their environment that is not captured by existing controls. Indeed, coefficients on all measures of firm performance drop dramatically with the addition of referent pay in columns 5 and 6 (less so in column 4 because salary is sticky), while those on division performance are stable, evidence that pay co-movement reflects broader firm-level performance.²⁶ Note that other covariates are relatively stable.

²⁶One other notable point is that the standard principal-agent model also predicts that external benchmarks are important in setting wages, as firms may match outside options for executives to retain talent. Consistent with this, we find some (weak) evidence that salaries are matched to the external market (positive and weakly significant coefficient in column 4). While firms use compensation surveys to set executive pay (in fact, the main purpose of the Hewitt survey), the criteria used to set pay can vary substantially: division industry (the measure we use), firms used as peers, wages in local labor markets, firm or division size, or a combination of these criteria. As mentioned earlier, and as claimed by Hewitt consultants, the labor market for executives at the division manager level is more of a national market than a local market.

IV.B. Pay, Geography, and the 1991-1992 Pay Controversy

In Table 3, we show results of our first differences-in-differences analysis, which measures changes in PRS and PPS within firms before and after 1990 by geographic dispersion:

$$(6) w_{dt} = \alpha + \beta_1 s_{dt} \cdot disp_{dt} \cdot Post90 + \beta_2 \bar{w}_{-dt} \cdot disp_{dt} \cdot Post90 + D_d' \beta_{31} + F_d' \beta_{32} + \eta_d + \epsilon_{dt}$$

In Table 3 Panel A, we implement the above specification by splitting the sample by firm geography. We report results for concentrated firms (highest two quintiles of geographic concentration) and dispersed firms (lowest two quintiles of geographic concentration). We include the same set of controls as in Table 2 and report two sets of regressions for each pay measure: without division fixed effects (columns 1, 3 and 5) and with division fixed effects (columns 2, 4, and 6).²⁷ Across both types of firms, we see that all three measures of division manager pay are sensitive to referent pay (PRS) and division performance (PPS). We also see that PRS before 1990 is substantially higher for concentrated firms relative to dispersed firms (e.g., in column 4, 0.57 vs. 0.28), while PPS is lower in concentrated firms relative to dispersed firms (e.g., in column 4, 0.071 vs. 0.114).

<< Insert Table 3 about here >>

Next, we examine how pay sensitivity changes after the 1991-1992 pay controversy. In concentrated firms, there are no differences in any measure of PRS or PPS after 1990 (the interaction terms with *Post90* are generally statistically insignificant). As such, concentrated firms do not appear to respond to increases in the public attention to pay. In contrast, pay practices in dispersed firms are significantly different after 1990. Most importantly, the coefficient on the interaction between referent pay and the *Post90* indicator is positive and significant (and economically meaningful) for all three pay measures. Also, the coefficient on the interaction between division sales and the *Post90* indicator is negative and significant (and economically meaningful) for all three pay measures.

²⁷ In this specification, *disp* is a dummy variable for whether the firm is dispersed (bottom two quintiles of concentration) or concentrated (top two quintiles). For this and all subsequent analyses, all direct and pair-wise interactions are included in our specifications. Additionally, any variable that is interacted with division performance is also interacted with our firm-wide performance measures.

In Table 3 Panel B, we confirm that the differences between firm types across time periods are statistically significant by combining our sub-samples of concentrated and dispersed firms, and estimating triple interactions—e.g., *PRS*Post 1990*Dispersed*. We find statistically significant differences for PRS after the pay controversy in all specifications and for PPS in select specifications (and the correct signs in all specifications).

The results of a substantially stricter test are presented in Table 4, in which we explore the dynamics of pay changes over time to see if the changes are consistent with the timing of the 1991-1992 pay controversy. We focus on the dispersed firms and include a set of dummy variables representing different time periods with 1990 as the reference year. We would expect to see no pre-existing trend before 1990, limited effects during the 1991-1992 controversy, followed by a discrete jump after the 1992 SEC proxy ruling and little change thereafter. In Table 4, we observe changes in PRS consistent with these predictions. The estimates of PRS interactions in the 1993-1994 periods are significantly different from the reference year (1990) in all specifications. Notably, there is limited change in PRS in the period preceding the controversy (1990), and the interactions in the 1995-1996 and 1997-1999 periods are generally not statistically different from the 1993-1994 period.²⁸ Note that we find minimal changes in PRS over time in concentrated firms (unreported) and, in particular, no corresponding discrete change between the 1990 and 1993-1994 periods. Similarly, estimates of PPS are lower in the 1993-1994 period relative to 1990, a difference that is statistically significant in all specifications.

<< Insert Table 4 about here >>

Taken together, this evidence is consistent with greater pay visibility associated with the 1991-1992 pay controversy intensifying peer comparison across non-proximate division managers. More generally, the wage regression results and patterns of pay co-movement suggest a role for peer influence

²⁸ In the cross-sectional specification, the coefficients on the 1986-87 indicators are negative and significant; however, these results are not replicated in the fixed effects regression and these years are known to have the most noise in the survey. The fixed effects regression show that the increases in PRS began during the 1991-1992 period, which is consistent with the increase of wage comparisons commencing during, and not after, the pay controversy.

in wage setting within firms. We also find that PPS is generally lower in dispersed firms after 1990 which is consistent with a tradeoff between PPS and PRS in dispersed firms.

IV.C. Pairs Distance Analysis and Pay Disparity

We now turn to the pairs distance analysis, which measures whether the mean absolute distance in pay between proximate (same-state) managers differs from that of non-proximate (different-state) managers. We estimate the second-stage equation 5 and report the results in Table 5. We use two different first-stage regressions to calculate pay residuals. For the “minimum” specification, we regress pay on division performance (sales) and a minimum set of controls (tenure, officer corp, and division depth) and year fixed effects. Results for this specification are reported in columns 1, 3, and 5 of Table 5. For the “maximum” specification, we add firm performance (return on assets and revenue), as well as firm fixed effects and division state fixed effects, and report the results in columns 2, 4 and 6. Together, these analyses aim to capture two measures for peer influence—i.e., peer similarities in pay beyond what can be explained by observable similarities in division manager positions and in time-invariant firm characteristics and division location. We are agnostic about which first-stage specification is more appropriate.²⁹

A similar pattern holds for all pay measures but is most pronounced for total compensation. Several general findings are notable. First, we find that mean distance in pay residuals between same-state divisions (constant) is lower than that of different-state divisions in each time period and across all pay measures, and is statistically significant in all columns (except 2). That is, there is generally less pay disparity in proximate divisions, consistent with stronger peer comparison between division managers who are geographically close. Second, we find that mean distance increases after 1990 for both pair types and for all pay measures, which we interpret as more pay disparity, consistent with greater performance-based pay throughout the decade. .

²⁹ Although one could argue that the residuals calculated from a first stage that excludes firm and local geographic fixed effects may include pay that is not rightly considered “excess” by managers who are aware of firm and local pay practices.

Finally, and most importantly for our analysis, the increase in distance after 1990 is *less* in different-state divisions than in same-state divisions. We find a negative coefficient on the interaction term, *diff_state*post90*, suggesting a lower increase in pay disparity for non-proximate division managers, particularly in the pay measures that include performance pay. This finding is consistent with more-pronounced peer comparison in non-proximate than in proximate divisions after the 1991-1992 pay controversy. The magnitude of this coefficient is also economically meaningful. Looking at total compensation in column 6, we see that the increase in distance for same-state divisions is 6.11 percent between periods, while the comparable increase for different-state divisions is 2.49 percent (6.11-3.62). That is, the increase in distance after 1990 for non-proximate divisions is approximately half of that for proximate divisions.

<< Insert Table 5 about here >>

Our finding of increasing pay disparity after 1990 is consistent with more performance-based pay after the pay controversy. However, despite the general trend toward more performance pay, we find relatively lower increases in pay disparity between non-proximate managers after 1990. Analogous to the findings of the wage regression analyses, these findings are consistent with firms facing a tradeoff between higher-powered incentive contracts and less pay disparity in the face of peer comparison.

IV. D. Pay Comparison and Division Productivity

In the prior analyses, we argue that peer comparison affects pay setting in firms. Here, we explore whether peer comparison has any productivity impact within firms. If we assume that firms optimize wage contracts and that pay comparison affects this equilibrium, then we might expect divisions in dispersed firms to experience some form of performance impact after the 1991-1992 pay controversy. Identifying this effect is extremely difficult, particularly in an equilibrium context in which principals simultaneously choose wage contracts and absorb any performance impact of pay comparison. Moreover, it is particularly unclear about the timing of any effect. As such, this section is purely exploratory and simply asks: Do we see evidence consistent with pay comparison affecting performance? For our

performance variable, we use division productivity (defined as sales/employee), which has the advantage of being at the division level and, therefore, has fewer firm-level factors to confound the measure.

While there is a growing body of evidence suggesting that relative pay affects utility, the theoretical prediction about an agent’s effort response to information about relative pay is ambiguous. On the one hand, when an agent discovers that she is paid less than her peers, one potential response is to reduce effort as a consequence of becoming disgruntled because of perceived unfair pay. Alternately, low relative pay could have the opposite effect: an agent might expend more effort because she infers greater opportunity for pay raises or because she is fearful of being fired or has generally become less complacent. For the manager paid more than her peers, again, the response is unclear. Top-paid managers might slack off because of limited opportunities for raises or become more motivated when they discover that they are “stars” and work harder. Finally, pay disclosure may not affect manager effort at all (see Charness and Kuhn (2007) for some evidence of this null result from a lab experiment). Peer pay may simply be used to negotiate better pay with senior management and has no effect on effort provided for productive purposes, particularly if wages are adjusted appropriately in response.

In this section, we explore the productivity response of divisions within dispersed and concentrated firms around the period of the 1991-1992 pay controversy. These results are intended to be suggestive only since there could be alternative explanations for our findings that are independent of peer comparison or reduced effort by managers. Since we are agnostic about the timing effects of the controversy on productivity, we run the analysis with two definitions of the “post” period: post 1990 (from the beginning of the controversy) and post 1992 (from the peak of the controversy and the 1992 SEC proxy ruling). We find results only with the latter definition and report those results here.

The productivity regression that we estimate takes the following form:

$$(7) \text{Log div Sales/empl}_{dt} = \alpha + \beta_1 \text{disp}_{ft-1} + \beta_2 \text{Post92} + \beta_3 \text{disp}_{ft-1} * \text{Post92} + \eta_f + d_t + \epsilon_{dt}$$

We regress the log of division productivity in period t on prior-period indicator variables for whether the firm is dispersed, whether the year is after 1992, and an interaction term between the two, as well as firm and year fixed effects.³⁰ Results are shown in Table 6, Panel A. We find a negative and statistically significant coefficient on the interaction term—evidence that is consistent with divisions in dispersed firms exhibiting reduced productivity after 1992 relative to divisions in concentrated firms. The magnitude of the effect is significant. For example, the coefficient -0.1483 represents 30% of a “within” standard deviation of productivity. The coefficient is larger with a higher level of significance when we restrict the sample to 1990-1997, a smaller window that excludes outlying years.

Next, in Table 6, Panel B, we use the restricted sample to evaluate whether divisional productivity differs by whether the manager is below or above the mean salary in the year prior to the year in which productivity is measured. We find that the division managers at the bottom end of the wage distribution largely drive the lower productivity in the dispersed firms (Column 1). In fact, when we split the sample into above- and below-mean division managers, we find no productivity differences between dispersed and concentrated firms among division managers paid above-mean wages, while we see large differences within the below-mean sample (Columns 2 and 3). This result indicates that the relative change in productivity between dispersed and concentrated firms after 1992 is driven by a relative drop in productivity among the low-wage earners in dispersed firms.

<< Insert Table 6 about here >>

These results suggest that division productivity declined after 1992 within the firms most affected by increased pay visibility, particularly for managers at the low end of the pay scale. While there may be many alternative explanations, the evidence is consistent with managers reducing productive effort when they discover they are paid less than their peers.

IV.E. Alternative Explanations

³⁰ We use prior-period indicator variables in this analysis because we assume that the effort response lags realized pay by one period. Note that the sample size is reduced by approximately 50 percent because of this restriction.

In this section, we explore alternative explanations for our results. We describe the tests and report select findings in Table 7 and 8 (with remaining results in the Appendix).

One of the most significant concerns in interpreting our wage regression and productivity results is that dispersed firms differ from concentrated firms in time-varying, unobservable firm characteristics, and we are simply picking up differential trends in dispersed firms. A number of our previously reported findings already counter this concern. First, we find in Table 4 that the changes in pay in dispersed firms occur primarily in the two-year period after the pay controversy. Any alternative explanation that appeals to trends differentially affecting dispersed firms would also have to exhibit this distinctive time pattern.³¹ In addition, in Table A2 we report robust results using propensity score-matched firms in order to address concerns about unobservable differences between dispersed and concentrated firms. The matching method and variables used in the match are described in the table footnotes. Furthermore, the pairs distance analysis in Table 5 does not use the geographic dispersion of firms but, rather, the proximity of manager pairs within firms, regardless of firm dispersion. Again, an alternative explanation would have to account for this second treatment approach. Finally, our asymmetric productivity results in Table 6 Panel B find that the productivity drop in dispersed firms was entirely driven by low-wage workers, a result that may be difficult to explain by macroeconomic, industry or IT trends during this period.

We also run several additional tests to specifically address this concern. Since differential trends should not exhibit a specific break in 1990, one general test for these trends is to conduct tests of placebo breaks. We split the sample by the 1990 break and pick a placebo break year in the middle of each subpanel. For the 1986-1990 subpanel, we create a dummy indicator to represent a 1987 placebo break year, while for the 1993-1999 subpanel, we create a dummy indicator to represent a 1995 placebo break (we exclude 1991 and 1992 in this analysis). We then recreate the differences-in-differences analysis of Table 3 Panel B with the placebo years and the two subpanels. Table 7 Panel A reports the results as triple

³¹ In unreported tables, we also show that PRS and PPS changes are more pronounced within the subsample of dispersed firms that disclosed less prior to the 1992 SEC ruling because they paid a greater proportion of performance-based pay that was not mandatory to disclose. This “intent-to-treat” approach would have to be explained by any alternative story.

interactions for space purposes. Columns 1 through 3 and 4 through 6 report the results for the 1989 and 1995 breaks, respectively. The coefficients on PRS are negative and generally insignificant, and the PPS estimates are also generally insignificant. These results are not consistent with alternative secular trends that differentially affect dispersed firms unless they, too, affect firms only in the years immediately following the 1991-1992 period. Table 7 Panel B reports a similar result for the productivity analysis, with no significant interactions with the placebo breaks.

<< Insert Table 7 about here >>

Next, we conduct a number of tests in Table 8 to evaluate whether our results are in fact driven by vertical comparison between the division managers and the CEOs.³² Although it is hard to completely rule out a vertical comparison effect, it is possible to pull together several pieces of evidence, taken collectively, suggesting that vertical comparison is not the whole story behind our results.

If the 1991-1992 pay controversy led division managers to consider the CEO as the most salient referent and, as a result, pay increased for *all* division managers, we should expect greater pay co-movement between the CEO and division managers within a firm after 1990, regardless of geographic proximity. In Table 8 Panel A, we regress division manager pay on CEO pay for the whole sample and include division fixed effects to estimate the relationship between changes in division manager pay and CEO pay (i.e., co-movement of pay within the firm). Not surprisingly, CEO and division manager pay co-move (the coefficient on CEO pay is positive and significant), however, there is no increase after 1990 for the pooled sample (or within the concentrated or dispersed subsamples, not reported).

In Panel B, we investigate whether the increase in PRS in dispersed firms after 1990 (one key result from Table 3) is robust to the inclusion of CEO pay. If the effect is due purely to a vertical comparison, this result should disappear. We repeat the division fixed effects regressions Table 3 (columns 2, 4 and 6) for dispersed firms and include CEO pay directly and interacted with the Post 1990

³²Relatedly, Wade, O'Reilly and Pollock (2006) show that over- or underpayment of CEOs cascades down to lower managerial levels with the effect diminishing at lower organizational levels.

dummy. The coefficient on PRS*Post 1990 declines somewhat in two of the three specifications (and becomes marginally insignificant in one), but generally the results from Table 3 still hold.

Finally, in Table 8 Panel C, we investigate whether the CEO-division manager pay gap narrows after the controversy -- which would be consistent with a vertical comparison -- and whether this differs by geographic proximity. We define the pay gap as the logarithm of the ratio of CEO Pay / Division manager pay for all pay measures and estimate division fixed effects regressions that include both post 1990 and dispersed dummy variables and their interaction. The results suggest that the gap widened after 1990, i.e., CEO pay increased at a faster rate relative to division manager pay regardless of geographic proximity. These results suggest that vertical comparisons may play some role, but cannot be the whole story behind our findings.

<< Insert Table 8 about here >>

Next we consider several alternative explanations not related to peer comparison that could explain the increase in PRS in dispersed firms in the post-1990 period. In the first alternative explanation, IT productivity improved over the same period as the pay controversy and SEC rule implementation, facilitating greater team production (increase in θ), especially in dispersed firms.³³ Adoption, for example, of networking and telecommunications infrastructure, fax machines, email and common operating systems and software enabled division managers to coordinate their activities during the 1990s to a greater degree than during earlier periods.³⁴

Again, the results in Table 4 show that the pay changes occur primarily in the period just after the 1991-1992 controversy, which is at odds with the IT explanation. We also look to test this explanation directly, using IT productivity (measured by number of PCs per employee in the firm). If IT intensity

³³ IT may also improve communication about pay, which is another related explanation for our results. This falls under the category of a technological change that affects the difference in information between geographically-dispersed and concentrated firms.

³⁴ Theoretically, increased IT productivity may have the opposite impact—namely, greater monitoring of agents. Under this argument, IT enables principals (CEOs, group managers, directors) to monitor the activities of division managers more closely and, therefore, understand their individual contributions with greater accuracy, negating the free-riding aspects of team production or the distortions from multitasking that occur with less-accurate monitoring. However, we do not consider this scenario, as it would bias the results away from our findings.

drives increased pay co-movement, we would expect to see greater pay co-movement post-1990 in more IT-intensive firms. In the subsample of dispersed firms, we interact the referent pay*1990 interaction with IT intensity and predict a positive coefficient if IT drives co-movement. In all specifications, the coefficients are negative and generally insignificant, particularly in the models with division fixed effects (reported in Table A3 in the Appendix).

The second alternative is that the practice of rotating division managers through multiple divisions, particularly in dispersed firms, increased during the 1990s as a method of grooming future leaders for roles as top managers. This increased rotation, in turn, may have led to increased co-movement in division manager pay, due not to peer comparison, but simply to convergence in job characteristics between division managers and to the stickiness of compensation plans. To explore this, we investigate whether tenure decreased relatively more in dispersed firms after 1990, reflecting increased job rotation. We do not find evidence of this (reported in Table A4 in the Appendix). Moreover, based on our interviews with Hewitt Associates, the practice of horizontal rotations for division managers was uncommon during this time frame. It is also not clear how horizontal rotation would explain stronger effects in the years just following the rule change.

Another alternative story is that the 1991-1992 pay controversy coincided with the end of the 1990-91 recession coincided with the pay controversy, and dispersed firms responded differently to the recovery than concentrated firms. This alternative is compelling because it shares similar timing characteristics with the main explanation. We argue, however, that this story is unlikely to drive our findings for three reasons: First, it does not appear that the recession differentially affected our two groups of firms.³⁵ Second, if the recession differentially affected dispersed firms, we should also see symmetric and opposite effects during the actual 1990 recession year. Specifically, the 1990 indicators in Table 4 and the 1987 placebo break test in Table 6 should produce significant estimates, but they do not. Finally, it is hard to articulate recession-based explanations for i) the pairs distance results in Table 5 that

³⁵ In an unreported figure, we plot five firm performance metrics over time by geographic concentration. Dispersed and concentrated firms exhibit similar patterns during the recession and early recovery period across all five measures.

use an alternative treatment that does not rely on overall geographic dispersion of the firm; and ii) the asymmetric productivity result in Table 6 Panel B that shows that the productivity drop in dispersed firms was driven by managers at the low end of the wage spectrum. We argue that these points, taken together, make a recession-based alternative story unlikely.

The last alternative story is that our measures of dispersed and concentrated firms are inadvertent proxies for underlying time-varying industry or geographic effects. For example, if most concentrated firms are technology firms based in California, and most dispersed firms are manufacturing firms based in the Midwest, then, perhaps, we are capturing relative California-Midwest trends or technology-manufacturing trends. Figure A1 plots the distribution of concentrated and dispersed firms by industry. While there are some differences visible at the two-digit SIC level, on aggregate, the industry composition broadly matches between the two groups. As an additional check, we rerun the main specifications, excluding industries that are highly skewed to either concentrated or dispersed firms, and find that the results remain, as do the results of the propensity-score matched sample, which matches on industry in addition to other variables (Table A2).³⁶ Another possible story is that our measure of dispersion is picking up characteristics affiliated with the location of firm headquarters. Figure A2 shows the distribution by headquarters location. As with industry, there is no broad regional skew between the two types of firms, although there are state-level differences. Similar to the industry analysis, when we rerun our analysis excluding skewed states and run the matched analysis, we obtain economically similar and statistically significant results.³⁷ Finally, we run the specification in Table 3 with division state*year fixed effects and the results are essentially unchanged (unreported).

In sum, we do not find compelling evidence that these alternative explanations are driving our results.

³⁶ We did this in two ways. First, we excluded the highly-skewed industries (high levels of concentrated or dispersed firms) 2-digit SIC codes 28, 36, 37. We also ran the analyses including all industries that were between 15- and 85-percent concentrated, excluding the tails on both ends.

³⁷ In this analysis, we exclude California, Massachusetts, Michigan, Minnesota and Virginia, states that were highly skewed toward either concentrated or dispersed firms.

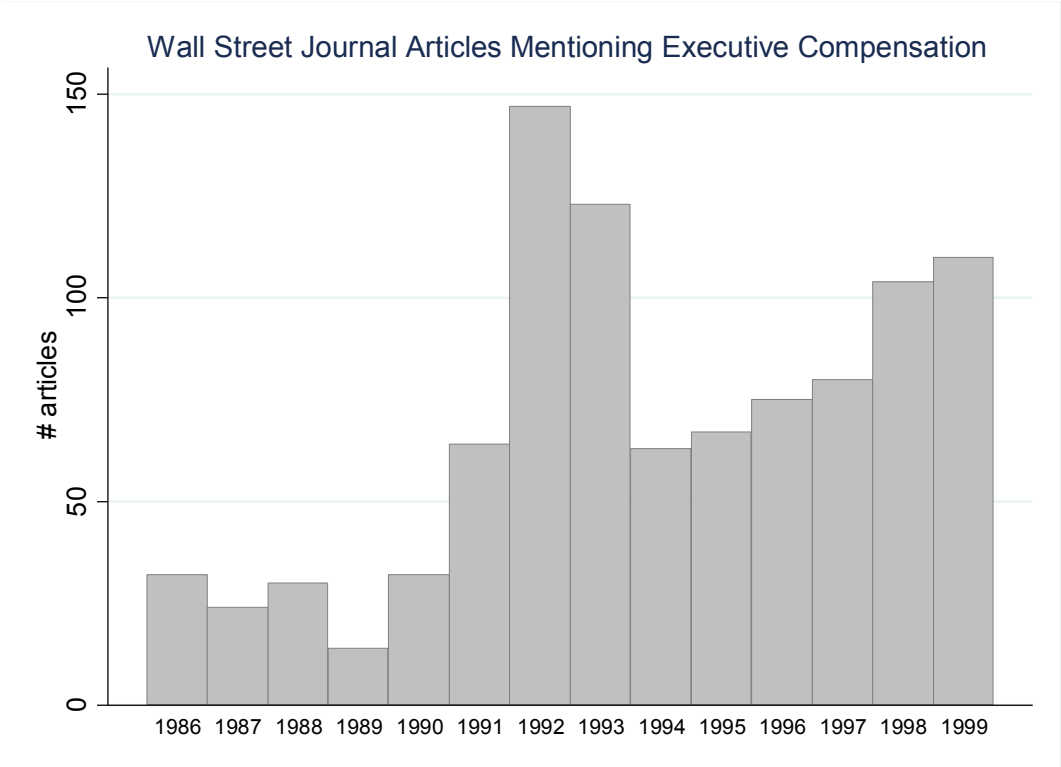
V. Conclusion

In this study, we find evidence consistent with peer comparison influencing pay policies for executives inside firms. Our underlying approach is to measure changes in pay co-movement, pay disparity and productivity by exploiting the public controversy about CEO pay that occurred in 1991 and 1992. We argue that the increased media coverage on CEO pay and the associated SEC ruling that mandated greater pay disclosure led to greater awareness of pay and, hence, greater peer comparison throughout all managerial ranks, particularly between non-proximate managers who had greater information frictions.

We present the results of three difference-in-difference analyses that, taken together, support the argument that firms' pay policies respond to peer comparison and concerns about internal equity. In general, we find evidence that pay distance between managers and division productivity increased during this period. However, these measures increased *less* among firms and managers that were more affected by the 1991-1992 pay controversy. Specifically, after the controversy, we find increases in PRS (pay-referent sensitivity)—or greater co-movement of division manager pay—and decreases in PPS (pay-performance sensitivity) in geographically-dispersed firms, but not in concentrated firms. Notably, these changes occur in the two-year period following the controversy, with no observed pre or post trends. In our pay distance analysis, we find that distance in residual pay increases less between pairs of division managers located in different states relative to pairs of managers located in the same state, who were likelier to have been sharing pay information prior to the controversy. Finally, we find that division productivity in dispersed firms increases less after the pay controversy relative to divisions in concentrated firms and that this effect is driven by managers at the low end of the wage distribution for division managers. Altogether, our findings suggest that wage comparisons between peers within firms and concerns for “pay harmony” affect firms' policies on setting pay for executives. The results are also consistent with principals facing a tradeoff between the incentive effects of performance pay and the effects of peer comparison that arise from unequal pay.

The unique contribution of the paper is that it demonstrates how firms' pay policies respond to concerns about internal equity, which, to our knowledge, has not been documented elsewhere. This research also raises questions for future research on the costs of pay disclosure and on labor markets more generally. What are the equilibrium consequences of the changes in wage contracts resulting from increased access to pay information? From the firm's perspective, these consequences may range from pay ratcheting to aggregate shifts in worker effort or firm-specific investments and turnover. Each of these changes, in turn, may have performance consequences for firms. From the employee's perspective, increased pay information may influence decisions to join firms and shift the relative importance of internal and external benchmarks, thereby having larger labor-market consequences. Aside from the contributions in this paper, these areas represent potentially fruitful avenues for further research as we broaden our understanding of peer influence within firms.

Figure 1: Press mentions



Annual counts of articles appearing in the *Wall Street Journal* between 1986 and 1999 with any of the following phrases: “executive compensation,” “executive pay,” “CEO compensation,” and “CEO pay.” Source: Factiva.

Figure 2: Intra-firm Variance in Pay (coefficient of variation) over time: Dispersed vs. Concentrated Firms



Median coefficient of variation (sd/mean) of Base salary, Salary + bonus and Total compensation. The figure has been locally smoothed using a lowess estimator with a bandwidth of 0.45. The sample includes all firms with more than ten years of observations in the panel and for which divisions appear, on average, in at least 50 percent of the years. The x-axis represents fiscal-year ends (not calendar-year time) so that, for example, the data at 1992 represent compensation granted at year end for work completed during 1992. The figure excludes outlying years of the survey (1986, 1998 and 1999).

Table 1 Panel A: Summary Statistics: Firm and Division

Variable	Obs	Mean	Std. Dev.
Firm			
Sales (\$millions)	2,315	8,508	13,548
Market capitalization	2,248	9,366	16,502
Employees (000s)	2,307	44.12	69.46
Assets (\$millions)	2,319	8,868	17,933
Return on assets	2,315	0.0512	0.0674
Average no. div / firm	2,362	4.55	4
Proportion div in HQ state	2,362	0.48	0.39
Division			
Sales (\$millions)	9,871	752	1,448
Employees (000s)	9,790	3.02	10.14
Productivity (sales/emp)	9,373	590	1435
Division manager			
Tenure (months)	8,899	42.23	42
Officer corp	10,731	0.23	0.42
Division depth	10,706	1.42	0.81
Base salary (\$)	10,731	208,849	82,439
Base plus bonus (\$)	10,731	300,088	151,459
Total compensation (\$)	10,731	459,640	357,865

Table 1 Panel B: Concentrated v Dispersed firms (Means)

Variable	Concentrated	Dispersed
Firm		
Obs	1,003	865
Firm sales (\$millions)	8992.64	7607.12
Market capitalization	9349.69	7757.12
Firm employees (000s)	44.91	44.39
Firm assets	9764.51	7507.56
Return on assets	0.0554	0.0443
Average no. div / firm	3.83	5.1
Proportion div in HQ state	0.89	0.05
Division		
Obs	3,837	4,346
Sales (mean) (\$millions)	919	562
Sales (median)	379	258
Employees (mean) (\$000s)	3.3	2.6
Employees (median)	0.9	1.1
Productivity (mean) (\$000s)	881	399
Productivity (median)	319	190
Division manager		
Tenure (months)	40.33	43.66
Base salary (\$)	216,397	197,225
Base plus bonus (\$)	312,907	280,139
Total compensation (\$)	498,911	415,225

Firm statistics are obtained from Compustat and Hewitt Associates for 296 firms with 2572 divisions between 1986 and 1999. Division and division manager statistics are from Hewitt. Concentrated firms are defined as firms in the top two quintiles of firms with the highest proportion of divisions in the same state as HQ, and dispersed firms are those firms in the bottom two quintiles. *Tenure* refers to the number of months a division manager has been in the position. *Officer corp* is equal to one if the division manager is also an officer of the firm. *Division depth* is the number of levels between the division and the CEO. *Base salary* refers to annual salary, *Base plus bonus* refers to salary plus annual bonus and *Total compensation* refers to salary plus bonus paid plus (ex ante valuation of) annual restricted stock grants, stock option grants, and other forms of long-term incentive pay (LTIP: performance units). Reported in 1996 dollars.

Table 2: Division Manager Pay-Performance Sensitivity (PPS) and Pay-Referent Sensitivity (PRS)

Log pay type:	Total compensation			Base salary (4)	Base + bonus (5)	Total compensation (6)
	Base salary (1)	Base + bonus (2)	compensation (3)			
PRS and PPS						
Log referent pay (PRS)				0.2838*** (0.0351)	0.4413*** (0.0388)	0.5511*** (0.0276)
Log division sales (PPS)	0.0751*** (0.0071)	0.0935*** (0.0102)	0.1106*** (0.0121)	0.0715*** (0.0068)	0.0873*** (0.0091)	0.1022*** (0.0106)
Firm Performance						
Return on assets	-0.0211 (0.0607)	0.5472*** (0.1285)	0.6307*** (0.1513)	-0.0065 (0.0479)	0.3059*** (0.0766)	0.2853*** (0.0746)
Log firm revenues	0.0377** (0.0150)	0.0820*** (0.0250)	0.1446*** (0.0351)	0.0236** (0.0116)	0.0391*** (0.0150)	0.0564*** (0.0181)
Other Controls						
Tenure	0.0009*** (0.0001)	0.0011*** (0.0001)	0.0011*** (0.0001)	0.0009*** (0.0001)	0.0011*** (0.0001)	0.0010*** (0.0001)
Officer corp	0.0879*** (0.0134)	0.1052*** (0.0204)	0.1525*** (0.0250)	0.0792*** (0.0118)	0.0937*** (0.0155)	0.1253*** (0.0178)
Division depth	-0.0501*** (0.0065)	-0.0597*** (0.0091)	-0.0895*** (0.0117)	-0.0436*** (0.0057)	-0.0474*** (0.0072)	-0.0639*** (0.0085)
Number of non-focal divisions in firm	-0.0014 (0.0011)	-0.0006 (0.0015)	-0.0006 (0.0021)	0.0010 (0.0009)	0.0035*** (0.0012)	0.0056*** (0.0016)
Log industry pay				0.0384* (0.0196)	0.0045 (0.0201)	-0.0083 (0.0195)
Constant	12.0891*** (0.1059)	12.3131*** (0.1433)	12.6738*** (0.3616)	8.6564*** (0.4310)	6.8895*** (0.4995)	5.6629*** (0.4111)
R-squared	0.274	0.252	0.364	0.315	0.352	0.511
Division and year FE	Y	Y	Y	Y	Y	Y
Observations	10312	10312	10312	10312	10312	10312

Standard errors clustered by firm. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.1$. *Log referent pay* refers to the log mean pay of division managers, excluding the focal division, within a given firm and year. Referent pay is calculated using base salary in column (4), base+bonus in (5) and total compensation in (6). *Number of non-focal divisions* refers to the number of divisions in a firm-year, excluding the focal division. *Log industry pay* refers to the log mean pay of all division managers outside the firm in the focal manager's Fama-French industry. Refer to footnote in Table 1 for additional definitions.

Table 3 Panel A: Effect of 1991-1992 Pay Controversy on PRS and PPS in Concentrated and Dispersed Firms

Log pay type:	Base salary		Base + bonus		Total compensation	
	(1)	(2)	(3)	(4)	(5)	(6)
Concentrated firms (N=3620)						
Log referent pay (PRS)	0.5769*** (0.0565)	0.4030*** (0.0603)	0.6481*** (0.0471)	0.5686*** (0.0582)	0.7111*** (0.0359)	0.6661*** (0.0368)
Log division sales (PPS)	0.0873*** (0.0110)	0.0610*** (0.0107)	0.1006*** (0.0135)	0.0714*** (0.0134)	0.1171*** (0.0144)	0.0752*** (0.0159)
<i>Post 1992 Interactions</i>						
Log referent pay (PRS)*Post 1990	0.0885 (0.0548)	-0.0127 (0.0318)	0.0253 (0.0470)	-0.0361 (0.0331)	0.0355 (0.0369)	-0.0543** (0.0261)
Log division sales (PPS)* Post 1990	-0.0131 (0.0107)	-0.0089 (0.0088)	-0.0095 (0.0131)	-0.0045 (0.0110)	-0.0106 (0.0154)	0.0027 (0.0121)
R-squared	0.657	0.622	0.677	0.658	0.754	0.738
Dispersed firms (N=4225)						
Log referent pay (PRS)	0.5390*** (0.0483)	0.1124* (0.0587)	0.5593*** (0.0430)	0.2840*** (0.0572)	0.5975*** (0.0454)	0.4303*** (0.0357)
Log division sales (PPS)	0.1173*** (0.0098)	0.0916*** (0.0106)	0.1488*** (0.0125)	0.1141*** (0.0138)	0.1740*** (0.0144)	0.1314*** (0.0151)
<i>Post 1990 Interactions</i>						
Log referent pay (PRS)* Post 1990	0.2275*** (0.0482)	0.1115** (0.0525)	0.1861*** (0.0467)	0.0870* (0.0487)	0.1650*** (0.0462)	0.1039*** (0.0364)
Log division sales (PPS)* Post 1990	-0.0337*** (0.0104)	-0.0182** (0.0072)	-0.0491*** (0.0127)	-0.0276** (0.0107)	-0.0542*** (0.0144)	-0.0305** (0.0123)
R-squared	0.701	0.605	0.691	0.641	0.759	0.729
Firm and division controls, year FE	Y	Y	Y	Y	Y	Y
Firm performance * Post 1990	Y	Y	Y	Y	Y	Y
Division FE	N	Y	N	Y	N	Y

Standard errors clustered by firm. *** = p<0.01, ** = p<0.05, * = p<0.1. *Log referent pay* refers to the log mean pay of division managers, excluding the focal division, within a given firm and year. Referent pay is calculated using base salary in column (1, 2), base+bonus in (3, 4) and total compensation in (5, 6). Concentrated (dispersed) firms are those in the highest (lowest) two quintiles of the proportion of divisions in the same state as headquarters. All pair-wise interactions and direct effects included in specification. Firm and division controls are the same as those in Table 2. Refer to footnote in Table 1 for additional definitions.

Table 3 Panel B: Effect of 1991-1992 Pay Controversy on PRS and PPS, Triple Interactions

Log pay type:	Base salary		Base + bonus		Total compensation	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>N</i> = 7845						
Log referent pay (PRS) * Post 1990 * Dispersed	0.1306* (0.0723)	0.1189** (0.0587)	0.1506** (0.0666)	0.1188** (0.0580)	0.1075* (0.0615)	0.1624*** (0.0441)
Log division sales (PPS) * Post 1990 * Dispersed	-0.0236 (0.0155)	-0.0086 (0.0111)	-0.0437** (0.0191)	-0.0214 (0.0152)	-0.0495** (0.0222)	-0.0322* (0.0168)
R-squared	0.688	0.627	0.689	0.651	0.761	0.737
Firm and other controls, Year FE	Y	Y	Y	Y	Y	Y
Firm performance* Post 1990*Dispersed	Y	Y	Y	Y	Y	Y
Division FE	N	Y	N	Y	N	Y

Standard errors clustered by firm. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.1$. *Log referent pay* refers to the log mean pay of division managers, excluding the focal division, within a given firm and year. Referent pay is calculated using base salary in column (1, 2), base+bonus in (3, 4) and total compensation in (5, 6). Concentrated (dispersed) firms are those in the highest (lowest) two quintiles of the proportion of divisions in the same state as headquarters. All pair-wise interactions and direct effects included in specification. Firm and division controls are the same as those in Table 2. Refer to footnote in Table 1 for additional definitions.

Table 4: Effect of 1991-92 Pay Controversy on PRS and PPS in Dispersed Firms Over Time

Log pay type: N = 4225	Base salary		Base + bonus		Total compensation	
	(1)	(2)	(3)	(4)	(5)	(6)
PRS						
Log referent pay (PRS) in 1990 (ref year)	0.6069*** (0.0634)	0.0604 (0.0574)	0.6194*** (0.0593)	0.2572*** (0.0573)	0.6596*** (0.0575)	0.4143*** (0.0398)
Log referent pay (PRS)*(1986-87)	-0.1675** (0.0749)	0.0329 (0.0577)	-0.1629** (0.0729)	0.0071 (0.0621)	-0.1838** (0.0726)	-0.0194 (0.0552)
Log referent pay (PRS)*(1988-89)	-0.0501 (0.0410)	0.0050 (0.0357)	-0.0252 (0.0521)	0.0275 (0.0533)	-0.0096 (0.0450)	0.0002 (0.0371)
Log referent pay (PRS)*(1991-92)	0.0134 (0.0417)	0.0771** (0.0321)	-0.0141 (0.0496)	0.0726* (0.0378)	-0.0042 (0.0432)	0.0654** (0.0316)
Log referent pay (PRS)*(1993-94)	0.1935*** (0.0596)	0.1751*** (0.0524)	0.1728*** (0.0579)	0.1405*** (0.0419)	0.1386** (0.0577)	0.1119*** (0.0373)
Log referent pay (PRS)*(1995-96)	0.1689*** (0.0629)	0.1568** (0.0617)	0.1385** (0.0689)	0.1597*** (0.0572)	0.0913 (0.0741)	0.1175** (0.0529)
Log referent pay (PRS)*(1997-99)	0.1592** (0.0666)	0.2052*** (0.0671)	0.1160* (0.0643)	0.1654*** (0.0583)	0.0840 (0.0653)	0.1584*** (0.0496)
PPS						
Log division sales (PPS) in 1990 (ref year)	0.1174*** (0.0130)	0.0925*** (0.0138)	0.1418*** (0.0149)	0.1046*** (0.0166)	0.1784*** (0.0199)	0.1308*** (0.0185)
Log division sales (PPS)*(1986-87)	0.0138 (0.0180)	0.0087 (0.0139)	0.0259 (0.0205)	0.0204 (0.0163)	0.0118 (0.0252)	0.0045 (0.0201)
Log division sales (PPS)*(1988-89)	-0.0050 (0.0116)	-0.0081 (0.0080)	0.0009 (0.0149)	-0.0016 (0.0117)	-0.0089 (0.0185)	-0.0083 (0.0145)
Log division sales (PPS)*(1991-92)	-0.0096 (0.0116)	-0.0085 (0.0071)	-0.0115 (0.0134)	-0.0132 (0.0105)	-0.0292* (0.0169)	-0.0312** (0.0120)
Log division sales (PPS)*(1993-94)	-0.0341** (0.0161)	-0.0179** (0.0085)	-0.0423** (0.0178)	-0.0211* (0.0120)	-0.0597*** (0.0224)	-0.0324** (0.0147)
Log division sales (PPS)*(1995-96)	-0.0298 (0.0198)	-0.0283** (0.0139)	-0.0320 (0.0234)	-0.0129 (0.0185)	-0.0407 (0.0295)	-0.0105 (0.0214)
Log division sales (PPS)*(1997-99)	-0.0410** (0.0168)	-0.0220 (0.0181)	-0.0530** (0.0210)	-0.0189 (0.0246)	-0.0676** (0.0278)	-0.0207 (0.0303)
R-squared	0.706	0.600	0.696	0.639	0.763	0.728
Firm and other controls, Year FE	Y	Y	Y	Y	Y	Y
Firm performance* year groups	Y	Y	Y	Y	Y	Y
Division FE	N	Y	N	Y	N	Y

Standard errors clustered by firm. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.1$. *Log referent pay* refers to the log mean pay of division managers, excluding the focal division, within a given firm and year. Referent pay is calculated using base salary in columns (1, 2), base+bonus in (3, 4) and total compensation in (5, 6). Sample includes only geographically-dispersed firms defined as those in the lowest two quintiles of the proportion of divisions in the same state as headquarters. All pair-wise interactions and direct effects included in specification. Firm and division controls are the same as those in Table 2. Refer to footnote in Table 1 for additional definitions.

Table 5: Pay Disparity (Pairs Distance) between Managers in Same State versus Different States within Firms

Distance between pay residuals by log pay type:		Base salary		Base + bonus		Total compensation	
		Min	Max	Min	Max	Min	Max
<i>N=29,007 DM-DM pairs within firm</i>		(1)	(2)	(3)	(4)	(5)	(6)
Different State		0.0229** (0.0095)	0.0111 (0.0122)	0.0315*** (0.0097)	0.0193* (0.0106)	0.0406*** (0.0000)	0.0231** (0.0104)
Post 1990		0.0261*** (0.0067)	0.0260*** (0.0073)	0.0411*** (0.0000)	0.0423*** (0.0000)	0.0610*** (0.0000)	0.0611*** (0.0000)
Different State * Post 1990		-0.0074 (0.0069)	-0.0032 (0.0044)	-0.0203** (0.0090)	-0.0142 (0.0092)	-0.0425*** (0.0129)	-0.0362** (0.0137)
Cons		0.1598*** (0.0000)	0.1587*** (0.0000)	0.1947*** (0.0000)	0.1923*** (0.0000)	0.2073*** (0.0000)	0.2050*** (0.0000)
R-squared		0.0074	0.0065	0.0075	0.0082	0.0079	0.0085
First stage controls & year FE		Y	Y	Y	Y	Y	Y
First stage firm FE		N	Y	N	Y	N	Y
First stage division state FE		N	Y	N	Y	N	Y

Each observation represents manager-manager pairs within a given firm and year. Significance is represented by: *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.1$. Standard errors calculated by non-parametric permutation tests implemented by Monte Carlo simulation (10,000 runs per model). Details about the permutation tests are described in more detail in Footnote 24. *Different state* represents whether the division managers in the same firm are located in different states from each other. The dependent variable, *Pairs distance between pay residuals* is calculated according to equation 5 as the absolute value difference between pay residuals calculated from a first stage model described in equation 4. Columns (1), (3), (5) show results for residuals calculated using the minimum specification in the first stage (without firm and division state FE) and Columns (2), (4), and (6) show results for residuals calculated with the full first stage specification.

Table 6 Panel A: Divisional Productivity Differences

Log (Sales/employee)	All years		1990-1997	
	(1)	(2)	(3)	(4)
Dispersed* Post 1992	-0.1483*	-0.1524**	-0.1776**	-0.1940**
	(0.0793)	(0.0774)	(0.0863)	(0.0834)
Dispersed	-0.0006	-0.1441**	0.1244	-0.1236
	(0.0681)	(0.0590)	(0.1307)	(0.0925)
Post 1992	0.3322***	0.3288***	0.3635***	0.3253***
	(0.0811)	(0.0811)	(0.1042)	(0.1026)
R-squared	0.017	0.050	0.013	0.057
Year FE	Y	Y	Y	Y
Firm FE	Y	N	Y	N
Observations	4723	4723	3139	3139

Table 6 Panel B: Divisional Productivity Differences – Asymmetric Effects

Log (Sales/employee)	1990-1997		
	All firms	At or above mean pay	Below mean pay
	(1)	(2)	(3)
Dispersed*Post 1992*Below mean pay	-0.2706**		
	(0.1156)		
Dispersed*Post 1992		-0.0757	-0.2554**
		(0.0908)	(0.1103)
R-squared	0.019	0.015	0.011
Year FE	Y	Y	Y
Firm FE	Y	Y	Y
Observations	3139	1667	1472

Standard errors clustered by firm. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.1$. Dependent variable, log *Sales/employee*, is calculated as division sales divided by the number of division employees. *Below mean pay* is a dummy equal to 1 if division manager pay is below mean pay, where mean pay is defined as mean base salary of division managers in the year prior to the year in which productivity is measured. Firm and division controls are the same as those in Table 2. Refer to footnote in Table 1 for additional definitions. All dependent variables lagged by one year and, therefore, sample only includes division observations with adjacent one-year lagged observation.

Table 7 Panel A: Alternative Explanations: PRS and PPS and Placebo Break

Log pay type:	1987 break (1986-1990)			1995 break (1993-1999)		
	Base salary	Base + bonus	Total compensation	Base salary	Base + bonus	Total compensation
Log referent pay (PRS)*Post break*Dispersed	(1) -0.0414 (0.0989)	(2) 0.0189 (0.0928)	(3) -0.0061 (0.0948)	(4) -0.2002** (0.0829)	(5) -0.0120 (0.0991)	(6) 0.0189 (0.1017)
Log division sales (PPS)*Post break*Dispersed	(1) -0.0199 (0.0184)	(2) -0.0020 (0.0201)	(3) -0.0548** (0.0265)	(4) -0.0144 (0.0193)	(5) -0.0197 (0.0257)	(6) -0.0414 (0.0311)
R-squared	0.463	0.422	0.446	0.279	0.269	0.360
Year FE	Y	Y	Y	Y	Y	Y
Division FE	Y	Y	Y	Y	Y	Y
Observations	2829	2829	2829	3821	3821	3821

Log pay type:	1989 break (1986-1992)			1995 break (1993-1999)		
	Base salary	Base + bonus	Total compensation	Base salary	Base + bonus	Total compensation
Log referent pay (PRS)*Post break*Dispersed	(1) -0.0449 (0.0836)	(2) -0.0166 (0.0814)	(3) -0.0420 (0.0750)	(4) -0.2002** (0.0829)	(5) -0.0120 (0.0991)	(6) 0.0189 (0.1017)
Log division sales (PPS)*Post break*Dispersed	(1) 0.0142 (0.0161)	(2) -0.0020 (0.0201)	(3) 0.0030 (0.0235)	(4) -0.0144 (0.0193)	(5) -0.0197 (0.0257)	(6) -0.0414 (0.0311)
Year FE	Y	Y	Y	Y	Y	Y
Division FE	Y	Y	Y	Y	Y	Y
Observations	4024	4024	4024	3821	3821	3821
Adjusted R-squared	0.419	0.389	0.434	0.279	0.269	0.360

Table 7 Panel B: Alternative Explanations: Divisional Productivity and Placebo breaks

Sales/employee	1987 break (1986-1990)		1995 break (1993-1999)	
	(1)	(2)	(3)	(4)
Dispersed*Post break	0.0709 (0.1087)	-0.0367 (0.1358)	-0.1141 (0.1077)	-0.0927 (0.1126)
Dispersed*Post break*Below mean pay		0.2346* (0.1282)		-0.0543 (0.1618)
R-squared	0.077	0.053	0.002	0.001
Year FE	Y	Y	Y	Y
Division FE	Y	Y	Y	Y
Observations	1683	1683	2098	2098

Standard errors clustered by firm. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.1$. *Post break* is a dummy equal to 1 for all years after the designated break year (1987 or 1995). In Panel A *Log referent pay* refers to the log mean pay of division managers, excluding the focal division, within a given firm and year. Referent pay is calculated using base salary in columns (1, 4), base+bonus in (2, 5) and total compensation in (3, 6). In Panel B, the dependent variable, *Sales/employee*, is calculated as division sales divided by the number of division employees. *Below mean pay* is a dummy equal to 1 if division manager pay is below mean pay, where mean pay is defined as mean base salary of division managers in the year prior to the year in which productivity is measured. All pair-wise interactions and direct effects included in specification. Firm and division controls are the same as those in Table 2. Refer to footnote in Table 1 for additional definitions.

Table 8: Alternative Explanation: Vertical Comparison to CEO Pay**Panel A: CEO-Division Manager Pay Co-movement**

Log Division Manager pay type: <i>N=10312</i>	Base salary (1)	Base + bonus (2)	Total compensation (3)
Post 1990	0.5019 (0.5086)	0.1737 (0.4350)	0.2184 (0.3747)
Log CEO pay	0.0880*** (0.0319)	0.2559*** (0.0330)	0.3411*** (0.0278)
Log CEO pay * Post 1990	-0.0282 (0.0387)	-0.0069 (0.0311)	-0.0086 (0.0265)
R-squared	0.383	0.431	0.536
Firm and division controls	Y	Y	Y
Firm performance * Post 1990	Y	Y	Y
Division FE	Y	Y	Y

Panel B: Robustness of 1991-1992 Pay Controversy Effect on PRS in Dispersed Firms

Log Division Manager pay type: <i>N=4225</i>	Base salary (1)	Base + bonus (2)	Total compensation (3)
Log CEO pay	0.0149 (0.0436)	0.1352*** (0.0415)	0.1453*** (0.0385)
Log referent pay (PRS)	0.1111* (0.0589)	0.2271*** (0.0582)	0.3575*** (0.0430)
Log division sales (PPS)	0.0916*** (0.0109)	0.1075*** (0.0155)	0.1287*** (0.0150)
<i>Post 1990 Interactions</i>			
Log CEO pay * Post 1990	0.0126 (0.0439)	0.0083 (0.0402)	-0.0150 (0.0372)
Log referent pay (PRS) * Post 1990	0.1048* (0.0532)	0.0692 (0.0523)	0.1031** (0.0464)
Log division sales (PPS) * Post 1990	-0.0177** (0.0073)	-0.0218** (0.0103)	-0.0284** (0.0122)
R-squared	0.608	0.635	0.736
Firm and division controls, year FE	Y	Y	Y
Firm performance * Post 1990	Y	Y	Y
Division FE	Y	Y	Y

Panel C: Effect of 1991-1992 Pay Controversy on CEO-Division Manager Pay Ratio, Dispersed versus Concentrated Firms

Log pay type: Pay ratio: <i>N=7845</i>	Base salary CEO/Div (1)	Base + bonus CEO/Div (2)	Total compensation CEO/Div (3)
Post 1990	0.3034*** (0.0699)	0.1890** (0.0767)	0.2200** (0.1080)
Dispersed	0.0235 (0.0332)	0.0741 (0.0538)	0.1559** (0.0611)
Dispersed * Post 1990	-0.0449 (0.0298)	-0.0589 (0.0444)	-0.0272 (0.0506)
R-squared	0.316	0.235	0.351
Firm and division controls	Y	Y	Y
Firm performance * Post 1990	Y	Y	Y
Division FE	Y	Y	Y

Standard errors clustered by firm. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.1$. *Log CEO pay* refers to CEO base salary in Columns (1), CEO salary+bonus in Columns (2) and CEO total compensation in Columns (3). All pair-wise interactions and direct effects included in specification. Firm and division controls are the same as those in Table 2. Refer to footnote in Table 1 for additional definitions.

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Appendix A: Data Description and Sample Representativeness

The primary dataset used in this study includes a panel of more than 300 publicly-traded U.S. firms over the years 1986-1999, spanning a number of industries. The data are collected from a confidential compensation survey conducted by Hewitt Associates, a leading human resources consulting firm specializing in executive compensation and benefits. The survey is the largest private compensation survey (as measured by the number of participating firms), and the participants are typically leaders in their sectors. More than 75 percent of the firms in the dataset are listed as Fortune 500 firms in at least one year, and more than 85 percent are listed as Fortune 1000 firms. In general, Hewitt survey participants also participate in other compensation consulting firm surveys (e.g., Hay Associates, Mercer, Towers Perrin, to name a few) and do so primarily to receive information about pay practices to use as a competitive benchmark in evaluating their own compensation programs. It is important to note that the sample includes many more firms than Hewitt's consulting client base, with at least 50 percent of the firms as survey participants with no other relationship to Hewitt.

We believe the survey data are accurate for several reasons. First, Hewitt personnel are knowledgeable about survey participants because they are assigned to specific participants for several years. Furthermore, while the participating firms initially match their positions to the benchmark positions in the survey, Hewitt personnel follow up to verify accuracy and spend an additional eight to ten hours on each questionnaire evaluating the consistency of responses with public data (e.g., proxy statements) and across years. Finally, participants have an incentive to match positions correctly and provide accurate data because they use the survey results to set pay levels and design management compensation programs.

Clearly, an important issue in datasets such as this one is the question of sample selection and whether the firms in the dataset are distinct from, or representative of, employers of similar size in their industry. The survey participants are typically the leaders in their sectors and, in fact, more than 75 percent of the firms in the dataset are listed as Fortune 500 firms in at least one year. We evaluate the representativeness of the broader sample by comparing key financial measures of our survey participants to a matched sample from Compustat. We begin by matching each firm in the Hewitt dataset to the Compustat firm that is closest in sales within its two-digit SIC industry in the year the firm joins the sample. We then perform Wilcoxon signed rank tests to compare the Hewitt firms with the matched firms. While the firms in the Hewitt dataset are, on average, have slightly larger sales than the matched sample, we find no statistically significant difference in employment and profitability (return on sales). We also find no statistically significant difference in sales growth, employment growth, or annual changes in profitability for all sample years. We also calculate financial measures for the sample of Compustat firms with 10,000 employees or greater over the period from 1986 to 1999 (excluding firms operating in

financial services). On average, survey participants are more profitable, but growing at a slower rate than those in the sample of large Compustat firms. This is consistent with our observation that the firms in our sample are likely to be industry leaders (hence, slightly more profitable) and also large (hence, the slightly slower growth). In sum, the survey sample is most representative of Fortune 500 firms (for more details, see Rajan and Wulf, 2006).

Appendix B: Identification strategy

To be more precise and to illustrate our identification strategy, we introduce the following two (unobservable) variables: ψ and θ . $\psi(I) \in [0,1]$ represents the strength of peer influence on pay as a function of I , the degree of pay information available in the environment, where $\frac{\partial \psi}{\partial I} > 0$. As such, the elasticity of pay with respect to referent pay can be represented as $\beta_2 \psi(I)$. $I=1$ is an environment with full information about peer pay; $I=0$ is an environment with no information.

The variable θ represents all unobservable factors that simultaneously affect pay across division managers that are not captured through existing controls (i.e., team production, selection, common shocks). Critically, $\rho_{w\theta} = \text{corr}(\bar{w}_{-dt}, \theta) > 0$; that is, an increase in θ is correlated with an increase in peer pay. This correlation creates our challenge in separately identifying the effect of ψ and θ on pay and will bias upward the estimate of β_2 in equation 3 such that $\hat{\beta}_2 = \beta_2 + \rho_{w\theta} \frac{\sigma_\theta}{\sigma_w}$.

In an attempt to address this problem, we estimate β_2 for firms operating in different information environments. More specifically, consider the following equation that includes θ and an unbiased β_2 :

$$w_{dt} = \alpha + \beta_1 s_{dt} + \beta_2 \psi(I) \bar{w}_{-dt} + \beta_\theta \theta + \dots,$$

where β_θ measures the association between wages and unobservable factors unrelated to peer influence. We assume $\psi(1)$ is full information sharing and $\psi(0)$ is no information sharing and that $\psi(1) > \psi(0)$, leading to the following two equations:

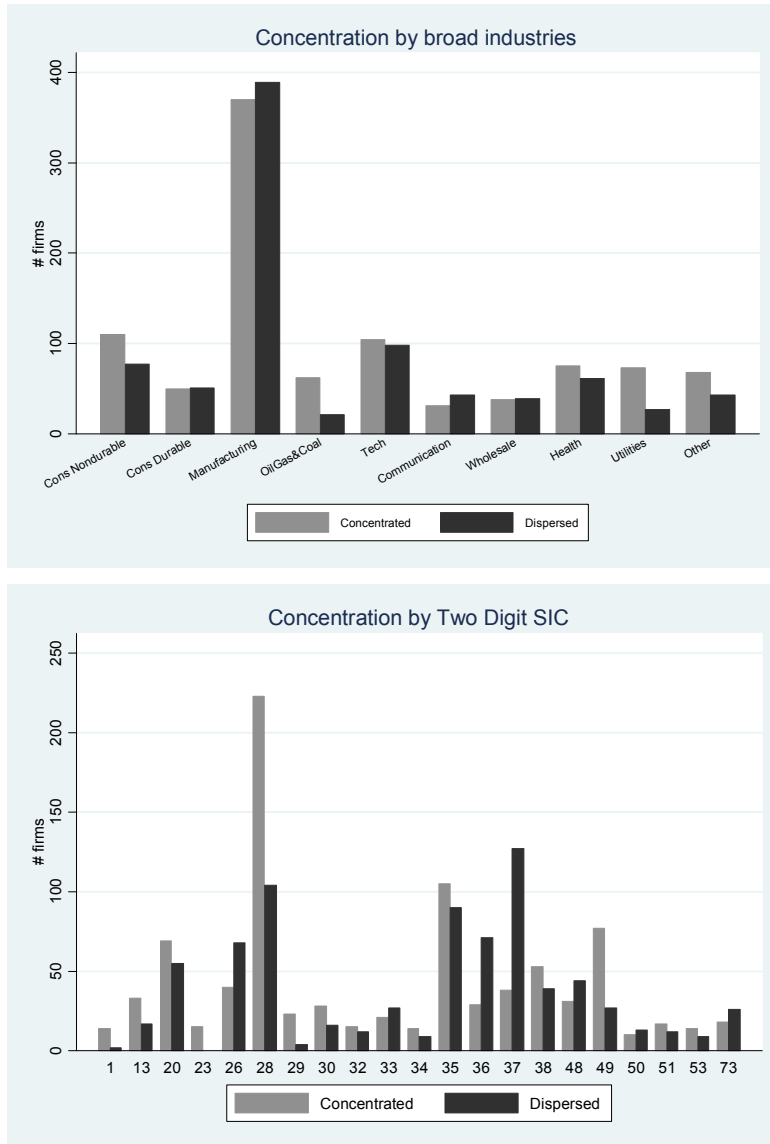
$$w_{dt} = \dots \beta_2 \psi(1) \bar{w}_{-dt} + \beta_\theta \theta + \dots$$

$$w_{dt} = \dots \beta_2 \psi(0) \bar{w}_{-dt} + \beta_\theta \theta + \dots$$

In our differences-in-differences models, we subtract the two equations and eliminate $\beta_\theta \theta$, giving us an unbiased estimate of $\beta_2 \psi$. To simplify and without loss of generality, if we define $\psi(0)=0$ and $\psi(1)=1$, then subtracting these two equations will yield an unbiased estimate of β_2 . Similar logic applies for estimates of β_1 (PPS).

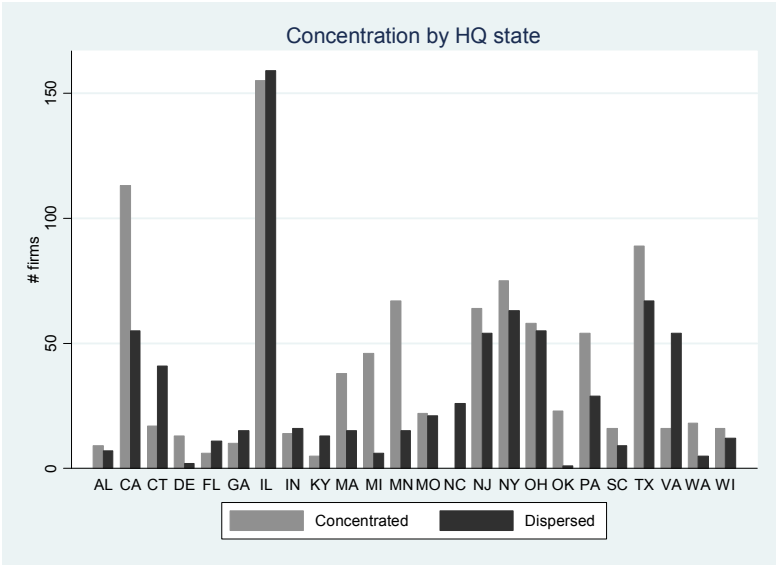
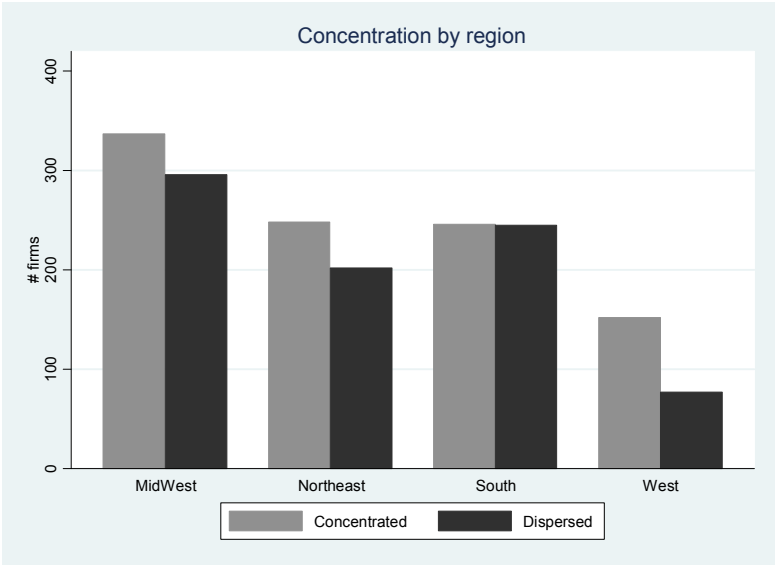
APPENDIX – Additional Figures

Figure A1: Distribution of Geographic Concentration by Industry



Notes: Bottom panel includes categories with 15 or more total firm-year observations. Two-digit SIC codes are as follows: 1- Agriculture Production-Crops; 13 – Oil and gas extraction; 20 – Food and kindred product; 23 – Apparel & other finished product; 26 – Paper and allied products; 28 – Chemicals and allied products; 29 – Petroleum refining and related industries; 30 – Rubber and misc plastics; 32 – Stone clay glass and concrete products; 33 – Primary metal industries; 34 – Fabricated metal products; 35 – Industrial and commercial machinery; 36 – Electronic and other electrical equipment; 37 – Transportation equipment; 38 – Measuring and analyzing instruments; 48 – Communications; 49 – Electric gas and sanitary services; 50 – Wholesale trade – durable goods; 51 – Wholesale trade – nondurable goods; 53 – General merchandise stores; 73 – Business services

Figure A2: Distribution of Geographic Concentration by HQ State



Notes: Top panel regions classified according to U.S. Census Bureau. Bottom panel includes categories with 15 or more total firm-year observations.

APPENDIX – Additional Tables

Table A1: Empirical Implications for Peer Comparison

Panel 1: Wage Regressions: PRS and PPS

<u>Pay Measures</u>	<u>Sample Split</u>	<u>Hypotheses</u>		<u>Mechanism & Interpretation</u>
	<u>Geography</u>	<u>PRS*Post 90</u>	<u>PPS*Post 90</u>	
All Measures	Dispersed	$\beta_2 > 0$	$\beta_1 \neq 0$	<ul style="list-style-type: none"> • Greater access to information facilitates peer comparison • Trade-off between PRS & PPS ($\beta_1 < 0$) • Overall push to link pay to performance ($\beta_1 > 0$)
	Concentrated	$\beta_2 = 0$	$\beta_1 = 0$	<ul style="list-style-type: none"> • No Difference post-90

Panel 2: Pairs Distance Analysis: Pay Disparity

	<u>Same State</u>	<u>Different State</u>	<u>Peer comparison</u>	<u>Mechanism & Interpretation</u>
Pre 90	δ_0	$\delta_0 + \delta_2$	$\delta_2 > 0$	<ul style="list-style-type: none"> • Greater access to information facilitates peer comparison <ul style="list-style-type: none"> ◦ Less distance btw same-state divisions pre 90
Post 90	$\delta_0 + \delta_1$	$\delta_0 + \delta_1 + \delta_2 + \delta_3$	n/a	<ul style="list-style-type: none"> • Performance-based pay increasing across all firms <ul style="list-style-type: none"> ◦ More distance for all pairs post 90
Difference-in-Difference (post 90 – pre 90)	δ_1	$\delta_1 + \delta_3$	$\delta_3 < 0$	<ul style="list-style-type: none"> • Difference between before and after 90 less for different-state divisions

**Table A2 Panel A:
Effect of 1991-1992 Pay Controversy on PRS and PPS in Concentrated and Dispersed Firms, Propensity-Score Matched Sample**

Log pay type:	Base salary		Base + bonus		Total compensation	
	(1)	(2)	(3)	(4)	(5)	(6)
Concentrated firms (N=2333)						
Log referent pay (PRS)	0.6121*** (0.0569)	0.3909*** (0.0740)	0.6813*** (0.0486)	0.5552*** (0.0728)	0.7335*** (0.0415)	0.6409*** (0.0453)
Log division sales (PPS)	0.0873*** (0.0109)	0.0573*** (0.0125)	0.1003*** (0.0134)	0.0705*** (0.0160)	0.1169*** (0.0140)	0.0744*** (0.0190)
<i>Post 1992 Interactions</i>						
Log referent pay (PRS)*Post 1990	0.0466 (0.0537)	-0.0487 (0.0344)	-0.0486 (0.0471)	-0.0953*** (0.0347)	-0.0378 (0.0433)	-0.0897*** (0.0302)
Log division sales (PPS)* Post 1990	-0.0121 (0.0125)	-0.0077 (0.0094)	-0.0007 (0.0149)	0.0014 (0.0116)	-0.0011 (0.0176)	0.0092 (0.0133)
R-squared	0.670	0.611	0.683	0.653	0.761	0.740
Dispersed firms (N=2097)						
Log referent pay (PRS)	0.5405*** (0.0619)	0.0712 (0.0688)	0.5729*** (0.0480)	0.2568*** (0.0620)	0.6240*** (0.0524)	0.3909*** (0.0740)
Log division sales (PPS)	0.1083*** (0.0127)	0.0862*** (0.0138)	0.1387*** (0.0154)	0.1055*** (0.0168)	0.1593*** (0.0171)	0.0573*** (0.0125)
<i>Post 1992 Interactions</i>						
Log referent pay (PRS)* Post 1990	0.1454*** (0.0500)	0.1791*** (0.0432)	0.0990* (0.0517)	0.0950** (0.0425)	0.0767 (0.0492)	0.1092*** (0.0400)
Log division sales (PPS)* Post 1990	-0.0146 (0.0110)	-0.0127 (0.0105)	-0.0236* (0.0131)	-0.0139 (0.0135)	-0.0231 (0.0161)	-0.0115 (0.0155)
R-squared	0.678	0.605	0.691	0.652	0.765	0.741
Firm and division controls, year FE	Y	Y	Y	Y	Y	Y
Firm performance * Post 1990	Y	Y	Y	Y	Y	Y
Division FE	N	Y	N	Y	N	Y

Table A2 Panel B: Effect of 1991-1992 Pay Controversy on PRS and PPS, Triple Interactions, Propensity-Score Matched Sample

Log pay type:	Base salary		Base + bonus		Total compensation	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>N</i> = 4430						
Log referent pay (PRS) * Post 1990 * Dispersed	0.0972 (0.0777)	0.2188*** (0.0564)	0.1461** (0.0728)	0.1831*** (0.0576)	0.0931 (0.0673)	0.1880*** (0.0512)
Log division sales (PPS) * Post 1990 * Dispersed	-0.0117 (0.0165)	-0.0086 (0.0111)	-0.0354* (0.0195)	-0.0147 (0.0176)	-0.0352 (0.0235)	-0.0199 (0.0202)
R-squared	0.674	0.597	0.686	0.643	0.763	0.736
Firm and other controls, Year FE	Y	Y	Y	Y	Y	Y
Firm performance* Post 1990*Dispersed	Y	Y	Y	Y	Y	Y
Division FE	N	Y	N	Y	N	Y

Standard errors clustered by firm. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.1$. Subsample based on propensity-score matched data. Match based on pre-1992 number of divisions, firm revenue, year and two-digit SIC, without replacement. *Log referent pay* refers to the log mean pay of division managers, excluding the focal division, within a given firm and year. Referent pay is calculated using base salary in column (1, 2), base+bonus in (3, 4) and total compensation in (5, 6). Concentrated (dispersed) firms are those in the highest (lowest) two quintiles of the proportion of divisions in the same state as headquarters. All pair-wise interactions and direct effects included in specification. Firm and division controls are the same as those in Table 2. Refer to footnote in Table 1 for additional definitions.

Table A3: Alternative Explanation: IT Intensity Changes

Log pay type:	Base salary (1)	(2)	Base + bonus (3)	(4)	Total compensation (5)	(6)
<i>N</i> =3190						
Log referent pay (PRS) * Post 1990 * IT Intensity	-0.1256*** (0.0416)	-0.0232 (0.0397)	-0.0564 (0.0431)	-0.0093 (0.0399)	-0.0584 (0.0433)	-0.0250 (0.0277)
Log division sales (PPS) * Post 1990 * IT Intensity	0.0057 (0.0092)	-0.0008 (0.0064)	0.0001 (0.0109)	-0.0007 (0.0089)	-0.0017 (0.0121)	-0.0001 (0.0101)
R-squared	0.693	0.563	0.668	0.694	0.665	0.738
Firm and other controls, Year FE	Y	Y	Y	Y	Y	Y
Division FE	N	Y	N	Y	N	Y

Standard errors clustered by firm. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.1$. *IT Intensity* is defined as the quintile for each firm in a given year of ratio of PCs/employee. *Log referent pay* refers to the log mean pay of division managers, excluding the focal division, within a given firm and year. Referent pay is calculated using base salary in columns (1, 2), base+bonus in (3, 4) and total compensation in (5, 6). All pair-wise interactions and direct effects included in specification. Firm and division controls are the same as those in Table 2. Refer to footnote in Table 1 for additional definitions.

Table A4: Alternative Explanation: Horizontal Job Rotation

	Tenure in position (in months)	
<i>N</i> =6424	(1)	(2)
Dispersed* Post 1990	-4.1655 (4.0667)	-2.3651 (4.2742)
Dispersed	8.0865 (4.9134)	4.5353 (3.8029)
Post 1990	3.8150 (4.2263)	-1.4083 (4.0636)
R-squared	0.007	0.005
Year FE	Y	Y
Division FE	Y	N

Standard errors clustered by firm. *** = $p < 0.01$, ** = $p < 0.05$, * = $p < 0.1$. Dependent variable is *Tenure in position*. Refer to footnote in Table 1 for additional definitions. *Dispersed* is defined as those firms in the lowest two quintiles of the proportion of divisions in the same state as headquarters.