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CEO AND AVERAGE EMPLOYEE PAY DURING THE 1980S: FIRM-LEVEL DETERMINANTS, DEVELOPMENTS, AND EFFECTS

By

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ABSTRACT

CEO AND AVERAGE EMPLOYEE PAY DURING THE 1980S:
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This dissertation presents two separate analyses of chief executive and average employee compensation. For both studies, data on executive compensation collected by the author from archived proxy statements are matched with firm-level compensation, balance sheet, and market data provided by Standard and Poor's Compustat. The first study looks at the determinants of executive compensation, focusing primarily on how firm-level unionization impacts executive pay. The second study looks at relative pay developments over the 1980s and how those developments affected employee output.

The implicit regulation hypothesis proposed by Jensen and Murphy (1990) suggests that CEO pay may be lower and less sensitive to firm performance than is optimal due to political constraints posed by negative public reactions to high executive pay. Chapter 1 uses firm-level measures of unionization in a sample of manufacturing firms to test the predictions of the implicit regulation hypothesis. Results suggest that executives in densely unionized firms have significantly lower pay than executives in nonunion and less unionized firms. However, the performance sensitivity of CEO pay in unionized firms is higher than that in nonunion firms. Additionally, it is shown that at high levels of unionization, unions successfully decrease the difference between compensation expenditures made to chief executives and to average employees.

It is also shown that the pay of chief executive officers in the United States has grown at a much faster rate than the per-employee compensation expenditures in the same firms. Chapter 2 examines whether the divergence in pay levels has had an impact on the productivity of firm employees. According to equity theory in industrial psychology, workers will withdraw productive work effort as the wages for workers in comparison groups grow at a relatively faster rate. According to tournament theory in economics, however, relatively high wages for chief executives are consistent with efficient incentive mechanisms. The impact of pay inequality, measured as the difference between executive and average employee pay, is gauged by including measures of relative pay as explanatory variables in a production function framework. Efforts are made to address potential sources of bias in the estimates, including bias associated with the simultaneous determination of pay and productivity and bias associated with unmeasured worker quality. Empirical tests do not indicate that a high ratio of executive to average employee pay is associated unequivocally with either lower or higher per worker output.

For Lian, remember that there are many ways to think about the world...

RETIRED MINERS

In Dr. Capalletti's office,
crippled and wheezing:

"if any guy   tells you
he got rich   through hard work
ask him   whose?"

From *Miracle Mile* by Ed Ochester, published by Carnegie-Mellon University Press, Pittsburgh, 1984. Used with the author's permission.

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CHAPTER 1

UNIONS AND CEO COMPENSATION: ARE UNIONS IMPLICIT REGULATORS?

Introduction

Recent economic analysis of the compensation packages received by CEOs in large US corporations has explored the possibility that CEO pay is affected by its public disclosure. Jensen and Murphy (1990) have proposed that political forces both inside and outside the firm engage in "implicit regulation" of CEO pay at least in part due to SEC requirements that the pay of executives be recorded in annual proxy statements, which are sent to shareholders. This paper explores the relationship between firm-level measures of unionization and executive compensation to determine if the effects of unionization on executive pay are consistent with the predictions generated by the implicit regulation hypothesis.

According to the implicit regulation hypothesis, average CEO pay is both lower and less sensitive to firm performance than it would be in the absence of public disclosure due to the negative reactions that greet announcements of high executive pay levels. Such reactions, from shareholders and "employees, labor unions, consumer groups, congress, and the media (Jensen and Murphy, p.254, 1990)," are believed to insinuate themselves into the pay determination process for CEO's, making it difficult for boards of directors to devise mechanisms which reward executives with extraordinary pay levels for extraordinary market performance. The truncation at the high end of the executive pay distribution, in turn, mitigates the possible "punishments" boards can mete out for poor executive performance, resulting in a downward limit to executive pay and less risk of executives losing their jobs.

Implicit regulation, according to Jensen and Murphy, may explain their finding that executive pay is relatively insensitive to changes in the market valuation of firms. This insensitivity raises questions concerning the importance of executives to firm economic performance and, consequently, the optimality of executive pay. Since economic theory suggests that, especially for chief executives, optimal labor contracts will tie employee compensation to firm performance, failure to find economically substantial links between them may imply that markets disregard the inputs of chief executives and that boards of directors therefore have little reason to tie executive pay to firm performance[1]. Jensen and Murphy, among others, find this interpretation unsatisfying based in part on empirical evidence showing that the market valuation of firms changes significantly with unexpected changes in firm leadership.

Since markets appear to pay attention to firm leadership, implicit regulation is arguably a better explanation for the observed relationship between performance and pay. Jensen and Murphy find some support for this explanation by comparing executive pay levels and sensitivity during the 1970s and 1980s to the higher levels and greater sensitivity of executive pay during the 1930s, a period when firms were not required to publicize the pay of their top executives.

Testing a similar hypothesis using executive pay over the 1970s and 80s, Joskow, Rose, and Shepard (1993) show that the extent of government economic regulation

[1] Other economists question Jensen and Murphy's interpretation of their findings either on the basis of econometric misspecification (Rosen, 1992, and Holmstrom, 1992) or on theoretical grounds (Haubrich , 1994).

impacts the levels of executive pay and its sensitivity to firm market performance. Executives of firms subject to firm-level price regulation, such as electric utilities, are compensated dramatically less than executives of nonregulated firms. Furthermore, these same executives have pay that is much less sensitive to firm performance than executives of firms in nonregulated industries. Joskow, Rose, and Shepard propose that this may be due to the political-economic nature of regulatory agencies, with regulatory officials being especially sensitive to public disapproval of high executive pay and especially opposed to pay schemes which focus executives' attention on increasing shareholder value at the expense of consumer surplus.

This paper proceeds down a similar avenue of analysis by using firm measures of union density to determine if unions affect the levels and manner in which executives get paid. Arguably, if there is a political effect on executive pay independent of regulatory authorities, it will be stronger in unionized firms. The motivation for this assertion is provided by a combination of analyses presented in the theoretical and empirical analyses of unions. Specifically, median voter models of unionization suggest that, provided union members have a general preference for equality between their pay and that of executives, the compensation of executives in unionized firms should be lower due to the enhanced voice mechanism of unions allowing members to assert their preferences (Freeman and Medoff, 1984).

Related economic reasons stem from the fact that management in unionized firms must repeatedly interact with unions in contract negotiations. High executive pay may serve as a signal to unions of the financial health of the firm and as a symbol of the allocation of profits between capital and labor, making directors of unionized firms wary of pay schemes which result in huge payoffs for chief executives. High executive pay, Jensen and Murphy (1990) argue, may "provide emotional justification for increased labor demands in labor negotiations." Finally, unions play an additional political-economic role in society through lobbying campaigns and policy advocacy (Addison and Hirsch, 1986). If executive pay in unionized firms becomes too far out of line with union members' preferences, then unions may be more inclined to make efforts to curtail it through political channels[2].

Results presented in this paper do not support the notion that unions, *per se*, play a regulatory role in the pay of chief executives. In fact, executives of firms with moderate levels of unionization seem to make more than the executives of nonunion firms. However, executives in firms with very high levels of unionization are paid significantly less than executives in nonunion and less unionized firms. Like the results in Joskow, Rose, and Shepard, it is difficult to attribute this exclusively to a "regulatory" effect, since unions may also decrease the potential impact that executives can have on firm performance, though the bias associated with unobserved executive productivity is likely to be a less important factor for these nonregulated firms. Fixed effects procedures suggest that unobserved firm heterogeneity explains the negative impact of unions on executive pay, though a negative impact on pay relative to less unionized firms remains. Fixed effect results are statistically inconclusive, however, and their usefulness may be limited due to data limitations and measurement error. There is also evidence that unions

[2] See, for example, the International Union of Operating Engineers' World Wide Web site, entitled "America Needs a Raise," at *www.ioue.org/html/oe5.htm*; and the AFL-CIO's "Executive Paywatch," at *www.aflcio.paywatch.org/ceopay/front.html*, which greets users with the statement, "Welcome to Executive Paywatch -- a working family's guide to monitoring and curtailing the excessive salaries, bonuses and perks in CEO compensation packages."

are able to narrow the pay difference between the average worker and the chief executive when they have organized a sufficient proportion of workers.

While unions are shown to impact the pay levels of chief executive officers, there is not evidence that they impact the pay-performance relationship in the way predicted by the implicit regulation hypothesis. The estimated semi-elasticity associated with market returns is larger in unionized firms than in nonunion firms, and among unionized firms there is little indication that this relationship declines as firms become increasingly unionized. This result suggests that the formulation of the implicit regulation hypothesis suggested by Jensen and Murphy may not fully capture the preferences that unions have regarding how executives are paid.

While the results presented in this paper do not provide unequivocal support for the implicit regulation hypothesis, neither do they lend themselves to direct conclusions regarding the welfare effects of chief executive pay packages. Previous analysis has cited concern in the popular media that high pay for executives in large U.S. corporations reflects executive capture and control of boards of directors, not relative productivity (Joskow, Rose, and Shepard (1993) and Joskow, Rose, and Wolfram (1996)). It cannot be concluded from this analysis, for example, that the high executive pay in some unionized firms is inefficient or unmerited on the basis that executives are participants in rent sharing arrangements, since the executives may be responsible for identifying, maintaining, or capturing those rents. Nevertheless, the results do show that executives in unionized firms are paid differently, and in some cases more, than the executives in nonunion firms.

The following analysis proceeds by reviewing the economic literature on executive pay. In addition, relevant research on unionization is reviewed, with special attention paid to their effects on wages and relative wages. The data used in this analysis are then introduced, followed by the econometric model used and predictions for the estimates in the model. Statistical results are then presented, followed by concluding comments.

The compensation of chief executive officers and the regulation of pay

Gomez-Meija and Balkin (1992) would impose a moratorium on the analysis of executive pay pending the emergence of data superior to those currently available from traditional sources. Other authors, however, have noted that the compensation of executives in large US corporations probably merits the academic attention it has received given the enormous levels of resources at the command of these executives[3]. This argument points out that there are substantial welfare implications from devising efficient incentive mechanisms for these employees. Rosen classifies the economic analysis of executive pay in terms of three fundamental problems which an efficient executive labor market must solve: "the distribution and control among executives; providing performance incentives; and identifying talent and reassigning control in the course of career development (1992, p.182)." These "problems" have been provided a

[3] Rosen (1992), Joskow, Rose, and Shepard (1993).

theoretical framework through models examining marginal productivity in hierarchical organizations, principal-agent theories, and tournament theories, respectively[4].

Executive productivity

Competitive labor markets will result in workers being paid their marginal product. The high pay of chief executive officers reflects the higher productivity of these individuals as well as the importance of their positions. Generally, this is illustrated in two ways. First, models incorporating the hierarchical nature of production show that individuals sorted into the top positions of firms have higher pay because their actions are magnified through all subordinate levels of the hierarchy. Because of this magnification of productivity, decisions made at the top are especially important and there is strong incentive for the hierarchy of the firm to sort the most talented people into the highest positions, where "a little extra talent...can have enormous effects on total output (Rosen, 1992, p.185)." Thus, when there are complementarities between individual talent and control of resources, the people in charge of those resources will receive substantially more pay than others in the same organization, the most talented people will be allocated into the top positions of the largest firms, and the returns to devising efficient incentives will be highest in large firms.

Additionally, the high concentration of pay in the top positions of firms reflects the need to structure pay mechanisms which induce greater effort from younger executives as they move through their career paths. Rosen (1986) presents a model where

[4] See Gomez-Meija and Balkin (1992) for a multidisciplinary discussion of executive compensation.

players vie for promotion through several stages of a tournament. Promotion into the next stage increases the remuneration of the winner and allows her to continue to compete for further promotions. As individuals proceed toward the top of the hierarchy, the number of possible promotions decreases which decreases the incentive effect of participating in further contests. Thus, firms must increase the payoff to promotions in the final stages of play to offset the decline in the future income stream associated with fewer potential promotions. That is, the payoff must increase so that incentives are maintained for individuals near the top of the promotion ladder, who may otherwise "rest on their laurels" and reduce effort. This framework explains the positive skew in the distribution of pay among top executives, where CEO's frequently make substantially more than even the second highest paid executive.

Empirical tests of these theories have taken several forms. In his review of the literature, Rosen (1992) finds that estimates of the size elasticity for chief executive pay is consistently estimated at around .25, implying that the pay of executives increases 2.5 percent on average for every 10 percent increase in firm size. Tests of tournament models are less numerous, though papers exploring the structure of the pay hierarchy among executives and its relationship to firm performance have found mixed results[5].

Executive incentives

Recently, economists have expended much energy exploring the mechanisms which can be used to tie managerial behavior to firm performance. In principal-agent theory, the objectives of managers can be brought into alignment with those of owners using the appropriate incentives. The primary result of principal-agent models is that nonowning managers whose compensation is based on organizational performance will be more diligent in maximizing shareholder value. Typically in these models, informational asymmetries between the principals and agents regarding the agents' effort levels (or circumstances leading to some observed action) necessitate pay structures which result in risk-averse agents exerting the effort which will maximize the expected value of the principal's assets. In absence of these pay structures, agents will be free to shirk or engage in other utility maximizing behavior at the expense of firm performance.

With risk neutral agents, the efficient behavior is ensured by the transfer of ownership from the principal to the agent. With risk averse agents, principals must insure agents against risk by paying some base salary but will otherwise make pay contingent on performance. Empirical analysis has measured the pay performance relationship for chief executives by including accounting or market rates of return on the right hand side of earnings regressions. Positive and significant results are interpreted as indications that managers' pay is aligned with the interests of the firm.

Regulation of executive pay and welfare analysis

In their analysis of the pay-performance relationship, Jensen and Murphy argue that the evidence suggests that executives have a only a small portion of their pay at risk for poor performance. While principal agent theory provides no definitive rule regarding the optimal portion of pay which should be tied to the performance of the firm, estimates provided by Jensen and Murphy suggest that executives receive (or forego) only $.30 in lifetime wealth for every $1000 generated (or lost) in shareholder value. Adding in

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[5] See Main, O'Reilly, and Wade (1993) and Leonard (1990).

another $.45 for the estimated increase in executive turnover after bad performance brings the total to 75 cents for every $1000 change in shareholder value. Their high end estimates, including the effect of poor performance on the value of personal holdings in firm shares, brings the total to $3.25 per $1000 change.

Jensen and Murphy argue that, while the pay-performance relationship is positive and significant, these adjustments to pay (and employment) for good or bad performance constitute very little incentive for executives relative to their base pay. Their empirical evidence indicates that on average the pay-performance relationship for CEOs in large US firms has little economic significance. This is not due to the importance of the executives to firm performance, since there remains an active takeover market which disciplines against managerial performance becoming too inefficient. Rather, they argue that implicit regulation may be decreasing pay levels and muting its performance sensitivity.

Joskow, Rose, and Shepard (1993) use interindustry variation in regulatory behavior to identify the impact of economic regulation on executive pay. For example, executive pay in the electric utilities industry is subject to firm-level regulation, and pay levels are therefore subject to the scrutiny of state regulatory agencies. Owners of regulated firms in the absence of this scrutiny presumably would engage the same incentive mechanisms used in nonregulated industries in order to prompt efficient executive performance. Social welfare maximizing agencies, on the other hand, may not approve of high payoffs, or at least of rewards based on stock performance, because those agencies must act in the interest of consumers. Recognizing this conflict, regulatory agencies may use their authority over rate setting or cost allowance decisions to prohibit firms from establishing compensation packages that result in high payoffs. Testing these hypotheses, they find substantial cuts in the pay levels for executives in regulated industries, as well as significantly lower pay-performance sensitivity for these firms.

The finding that regulatory agencies strongly impact the levels and performance sensitivity of CEO pay relative to nonregulated firms does not necessarily imply that the effect comes fully from political constraints on executive pay. Such an interpretation depends on whether the pay of executives in nonregulated markets is efficient or merely indicative of how adept CEOs are at controlling the board of directors. Joskow, Rose, and Wolfram observe, "to the extent that the political constraints become binding, they may either limit 'excessive' CEO compensation (i.e. reduce any shareholder rents appropriated by the CEO) or distort CEO performance incentives and the allocation of managerial talent by reducing compensation from efficient levels (p 166, 1996)." If the latter is the case, then lower executive pay in regulated industries may be attributable to productivity differences between executives in regulated versus nonregulated industries. This will be true if one of the effects of regulation is to decrease the impact that executives can have on the performance of the firm, making it efficient for boards of directors "to pay CEO's less, to tie pay less closely to firm performance, *and* to hire less able CEO's (Joskow, Rose, and Wolfram, p.166, 1996)."

The effect of economic regulation on CEO pay is consistent with the predictions of implicit regulation. However, it is difficult to discern if these effects occur because of political restraints operating through the formal regulatory process or because regulation decreases the relative productivity of CEOs in regulated firms. This paper uses a measure of political variation across firms which presents a potentially cleaner test of the implicit regulation hypothesis in that most firms are not subjected to external authorities which have control over price setting and cost recovery. Jensen and Murphy (1991) propose several sources of political regulators, including employees, unions, the media, and the threat of legislative responses. The latter of these presumes that nonregulated firms independently regard such a threat as real, though the magnitude of the benefits to pay incentives and the costs of plausible government responses to high pay are not well understood[6]. It is likely that in a Nash equilibrium sense, it would be suboptimal for any firm to respond to the threat of a government response to a potentially lucrative pay contract if all other firms are responding to the threat, especially if benefits to such a contract are substantial. There is also little indication that the public responds negatively to high executive pay, at least in terms of purchasing decisions. The responses of employees and unions are likely to be more important considerations in the determination of executive pay, especially in the context of deteriorating or stagnating standards of living.

Unions and compensation

Compensation specialists frequently discuss the various needs for firms to develop compensation strategies which incorporate and balance the often conflicting elements of equity, stability, and responsiveness to market forces[7], while noting that unions will frequently impact compensation systems, work design, and management strategies. In this context, the direct impact of unionization on CEO pay is of interest for several reasons. First, unions are by their nature political organizations which seek to

[6] For analysis of the benefits of pay that is highly sensitive to performance, see Abowd (1990).

increase the welfare of their members. Second, union members seem to prefer pay equality[8], at least among members in particular bargaining units. Evidence of desire for greater pay equality between executives and their employees is more anecdotal, but is nonetheless a frequently voiced concern which is linked to both considerations of fairness and productivity. According to Milkovich and Newman, "others, including unions, have long held the belief that more egalitarian pay structures support team workers, high commitment to the organization, and improved performance (p.52, 1992)." Finally, managers of unionized firms must accommodate work rule constraints imposed by collective bargaining agreements, as well as respond to the added variables of union rent seeking and the strike threat.

Unions, the median voter, and relative wages

According to the exit-voice theory of unionization (Freeman and Medoff, 1984), unions impact a firm's industrial relations policies by providing a "voice" to members which allows them to communicate collectively with management. This contrasts with nonunion employees, who may only be inclined or able to communicate with management by leaving, or exiting, the employment relationship[9]. In the absence of unions, firms will construct pay packages, work rules, and other employment policies in order to attract the marginal worker, though these policies may be undesirable to existing

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[7] See Levine (1993) for a discussion and empirical analysis of these.

[8] See Parsons (1991) for indirect evidence of this, and see discussion below.

[9] The exit-voice distinction is useful to the extent that the preferences of the average worker in a potential collective bargaining unit are different from the preferences of the marginal worker, which is likely to be the case if mobility differs by worker characteristics like tenure and skill specificity.

employees. In such cases, workers have the option of leaving the employment relationship or establishing a collective bargaining unit.

In cases where the benefits of unionization to the median worker are greater than the costs, unions may be established, becoming the democratic representatives of covered workers. As such, unions are thought to assert the preferences of average, or inframarginal, workers in negotiations with management over pay, employment, and work rules. The democratic nature of unions reasonably assures that the preferences of average workers are pursued subsequent to union certification. Hirsch and Addison (1986), argue that the empirical evidence is generally supportive of the median voter model of unions, suggesting that they do indeed pursue the interests of their average workers.

In a test of the median voter model which pertains to the wages of workers, Farber and Saks (1980) show that an individual's position in a potential bargaining unit's earnings distribution is negatively related to her likelihood of voting for a union in certification elections. Unions, through various functions[10], tend to pursue "egalitarian" compensation policies through increasing the mean wage and narrowing the distribution of earnings. They accomplish this by shifting the left tail of the earnings distribution toward the center. Thus employees in the lower end of the wage distribution are likely to gain substantially more in income due to unionization than those in the upper end. In context of median voter models, this interpretation implies that firms whose production units include low-skill majorities, that is firms with many workers in the left tail of the

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[10] For example, through rate standardization across and within establishments. See the discussion in Freeman (1980).

earnings distribution, will be more likely to vote for unions due to their self interested pursuit of union wage gains, which is what Farber and Saks find.

However, Parsons (1991) has shown that the relatively egalitarian wage policies of unions are not merely a function of the skill distribution of union members. Parsons demonstrates that, while firms with a high concentration of highly skilled workers are less likely to vote for unions, they are not likely to change the tendency of unions to redistribute rents disproportionately to low wage workers once unions are established. One explanation of Parson's results is that union employees are frequently concerned with matters of fairness in the distribution of wages, perhaps to preserve solidarity with low-wage workers in order to maintain a tenable strike threat.

Unions also tend to close the pay gap between union and nonunion workers within firms (Freeman, 1980). In Freeman's estimates, unionized workplaces in the manufacturing sector have a white collar-blue collar pay differential that is 30 percent lower on average than that in nonunion workplaces. It is not clear from his analysis if the reduction in the occupational wage differential comes fully from increases in union wages, or if it comes partly through decreases in white collar wages. That is, unions could have a negative or positive spillover effect on the wages of white collar workers within firms if wages among white collar workers in those firms are not determined purely by the market[11].

Unions also decrease the use of performance incentives among members. This may be due to perceptions by union members that management is incapable of

[11] Hirsch and Addison (1986) note that union wage gains must come from reductions in returns to capital, product price increases, or lower pay to nonunion labor. The high magnitude of the Freeman estimate, however, suggests that unions on average have a negative spillover effect.

impartially evaluating individual performance, perhaps because of favoritism, team oriented production technology, or because such mechanisms may weaken the strike threat by introducing competition among workers and negatively impacting solidarity. Farber and Saks (1980) present evidence that workers who believe that unions will improve fairness, *vis a vis* their treatment by supervisors, are more likely to vote for unions. Again, to the extent that such adjustments in the compensation of union members are generalized to white collar workers in the same firm, executives in unionized firms will likely have a weaker pay-performance relationship[12].

Unions and firm performance

If political factors impact the pay of executives, then the union voice function will likely affect the pay of executives in the manner predicted by the implicit regulation hypothesis. However, the voice mechanism is not the only factor at work which will lower the pay of executives. As in the case of economic regulation, unionization may impact the decisions that executives make, thereby decreasing their relative productivity. Hirsch (1991) presents a model of union rent seeking behavior where unions negatively impact the investment and research and development decisions that are made in unionized firms. Unions, through the exercise of the strike threat and myopic behavior due to the nontransferability of union membership rights, will try to capture the rents generated through such expenditures and investments. Thus, unions essentially impose a tax on investments, resulting in lower equilibrium levels of investment in union firms. In

[12] Indirect evidence of this comes from the "Multi-City Study of Urban Inequality," where recent hires hired into firms with collective bargaining units are substantially less likely to receive pay increases based on performance. This is true, as well, for individuals hired into managerial and professional jobs whether the job requires a college degree.

empirical tests, Hirsch shows that the level of unionization negatively affects the capital investments a firm makes (though unions will still increase the capital labor ratio), its research and development expenditures, and a host of other related "behavioral" outcomes dependent on executive decisions[13].

As in the problem outlined in other papers (Joskow, Rose, and Shepard, 1992, and Joskow, Rose, and, Wolfram, 1996), identification of a negative union impact on executive pay may imply a regulatory effect or a productivity effect. To the extent that boards of directors recognize the impact on executive behavior imposed by union rent seeking, they will adjust compensation packages accordingly and hire lower quality executives. Again, this distinction is important to the extent that one would like to use variation in political institutions to identify rent appropriation on the part of chief executive officers. That is, if executive talent is roughly equal across firms that differ only in their levels of unionization, higher executive pay in unionized firms may indicate that those executives are successfully appropriating shareholder rents. Practically, measuring all the variables necessary to make such a *ceteris paribus* comparison is likely to be prohibitively expensive and is not undertaken in this analysis.

Countervailing factors

The effect of union voice on executive pay will also be difficult to identify since there are likely to be numerous other unobserved factors that are correlated with unionization and which may increase the pay of the executives in union firms. For example, executives with the skills needed to interact effectively with a unionized work

[13] For example, investment intensity, patents per dollar spent on research and development, advertising intensity, and debt-equity ratios.

force may command a premium in the market for executives. Or rather, the added responsibilities of negotiating with unions may necessitate a compensating differential be paid to executives in unionized firms. Additionally, firms that are targets of union organization may also be those with more able managers. Managers that are especially adept at generating extraordinary economic profits through acquisition of market power, or maintenance of other quasi-rents, may be paid more *and* be the leaders of firms that are likely to be targets of certification drives. If such is the case, higher executive pay can be expected in unionized firms. Hirsch and Addison (1986) also note that management can become more productive after employees have established collective bargaining units, citing evidence that managers respond to decreased profit expectations after the establishment of unions by cutting costs, improving monitoring, and making other productivity enhancing adjustments. Thus, one might expect higher executive pay in unionized firms, which may be merited based on the higher productivity of those managers.

Alternatively, high executive pay in unionized firms may reflect rent sharing with union members due to product market power or the existence of quasi-rents that are not necessarily related to the quality of the current executive. Firms in less competitive product markets, with high fixed-cost production technology, or with lower cost structures may be more likely to be targets of successful union membership drives. Executives in such firms may be paid more than their counterparts in more competitive product markets due to rent sharing arrangements among the various stakeholders in the firm.

These various possibilities make it difficult to predict the effect of unionization on CEO pay. CEO pay should be negatively affected by the presence of unions through the influence of social pressures or through their impact on executive investment behaviors and the financial performance of the firm. On the other hand, differences in unobserved ability and rent sharing may result in higher executive pay in unionized firms. In the absence of good measures of executive ability and firm-specific rents, it may be difficult to discern the possible political impact that unions have on executive pay. With these limitations in mind, the goal of this paper is to assess the direct effect of unionization on CEO pay and to explore the possibly different avenues through which the CEOs of more unionized firms are paid relative to those of less unionized firms.

Data

Data for this analysis are drawn from several sources. Executive compensation data were collected from firm proxy statements filed with the SEC and archived by the Q-sup data company. When proxy statements were unavailable, form 10-k were checked for executive compensation information. Annual data on CEO compensation were collected for 12 years (1980-91), with a mean of 9.8 years per firm available. Additional information on the percentage (if greater than 1 percent) of the firm's common shares owned by both the CEO and the board of directors was collected, as well as information on the CEO's tenure as CEO, his age, and the number of years as a board member. Missing observations in these data are the result of missing microfiche, proxy statements not archived by Q-sup data company, firm attrition from the sample due to mergers, bankruptcies and closings, or some other event which removed the firm from SEC reporting requirements.

Measures of unionization come from a 1987 mail-phone survey of publicly traded manufacturing firms conducted by Barry Hirsch. Hirsch asked firms the following question: "To the best of your knowledge, approximately what percentage of your corporation's total North American work force is covered by collective bargaining agreements (p.26, 1991) ?" Respondent were also asked to recall as best as they could the proportion of covered employees in their firm in 1977 (as it existed then). The measures of unionization and sample methods are explained in greater detail in his book, *Labor Unions and the Economic Performance of Firms* (1991). In total, executive compensation information and unionization measures were matched for 249 firms. Of these, roughly 150 firms had observations for executive compensation in both 1980 and 1987. For the bulk of analysis below, the sample is constituted of these 150 firms, with the 1977 measure of unionization assigned to 1980 measures of executive compensation and the 1987 measure matched to 1987 measures of executive compensation.

The third source of data is Standard and Poor's Compustat. Compustat provides financial information from 10K forms, quarterly reports, and annual reports submitted to the SEC by publicly traded firms. A wide variety of information is available from this source. The following variables are used for the present analysis: annual sales, annual market return, number of employees, labor related expenditures, state where corporate headquarters are located, and SIC. All dollar measures are adjusted to 1991 values using the consumer price index for urban areas.

Econometric Model and Predictions

The model used here to analyze the compensation of chief executive officers follows that presented in Joskow, Rose, and Shepard (1993). The following equation will be used to estimate the determinants of chief executive pay,

$$SB_{ift} = \alpha_0 + \alpha_1\, SIZE_{ft} + \alpha_2\, PERF_{ft} + \alpha_3\, HC_{ift} + \alpha_4\, OWN_{ift} +$$

$$\alpha_5\, OUT_{ift} + \alpha_6\, UNION_{ift} + \alpha_7\, X_{ift} + \varepsilon_{ift}\,, \quad (1)$$

where *i* denotes chief executive, *f* denotes firm, and *t* denotes year. *SB* represents the natural log of chief executive salary plus bonus. The total of salary plus bonus must be used, because salaries and bonuses were tabulated separately in only 18 percent of proxy statements. While this is a standard problem when using information from proxy statements (or other sources which use proxy statement as their source), it is unfortunate since the use of base salary versus bonuses might be substantially different between union and nonunion firms.

Also, this measure excludes noncash compensatory rewards associated with long term incentive programs, such as the cash value of stock appreciation rights, stock options, and other related stock grants. Reporting requirements during the sample period make it difficult to identify annual measures for this variable. The SEC required that companies report the number and value of options granted as well as the realized value from previously awarded options separately from other forms of compensation, though companies frequently failed to tabulate the awards and were allowed to report cumulative awards over several years. This particular shortcoming is especially important given recent findings by Hall and Liebman (1997) that failure to incorporate the valuation of longterm awards results in a serious understatement of the sensitivity of executive wealth to firm performance. The literature on executive compensation has long recognized the inherent difficulty in combining long term awards with annual compensation, though Jensen and Murphy (1990) and Hall and Liebman (1997), among others, have successfully merged them.[14]

SIZE denotes scale variables, which will be represented by the natural log of annual sales in the following analysis. In the empirical literature, scale is controlled for in several ways. Joskow, Shepard, and Rose (1993) use the natural log of employees and the natural log of total assets together to control for scale. They argue that these measures better control for the heterogeneity in the capital-labor ratio among the different industries in their sample. Since the sample used in this analysis is constituted only of manufacturing firms and is therefore more homogeneous in terms of production technology, the sales measure will be used to control for scale effects. With these data, specifications that include assets and employees instead of sales generally have a worse fit in terms of R^2 than the specifications that include the sales variable. Annual sales is predicted to have a positive effect on the pay of executives, while there is no *a priori* reason to believe that the scale elasticity will be affected by unionization.

PERF represents performance measures and is measured by annual market return. Annual market return is provided by Compustat and is defined as the percentage change in closing share price from the previous year, accounting for per share dividends paid. Results incorporating accounting rates of return are not presented at this time, though

[14] The conversion of long term awards to current cash value is not possible with these data, however, due to missing information on the number of outstanding awards. For the present, it is assumed that the error in measurement for the dependent variable is uncorrelated with the measure of unionization, so that estimates presented below are unbiased. Whether in fact this is the case is an interesting empirical matter.

such measures are likely to be relevant sources of information regarding the performance of chief executive officers (Rosen, 1992, and Holmstrom, 1992). Performance measures are hypothesized to have a positive effect on executive salary. However, the implicit regulation hypothesis suggests that unionized firms should have a weaker pay-performance relationship since performance based pay should result in larger payoffs for executives and higher average pay.

Human capital variables are represented by HC. The linear and quadratic measures of tenure in the position of CEO and potential experience (defined as CEO age minus 22) are used in this analysis. Both tenure and experience are expected to have a positive effect on compensation and are interpreted in the usual Mincer (1974) framework, with the coefficient on experience representing returns to general human capital, and the coefficient on tenure representing the returns to job-specific human capital. Joskow, Rose, and Shepard (1993) find that the return to tenure in regulated firms is twice that in nonregulated firms. They argue that this may represent a shift to more bureaucratic forms of pay, for which the public has less objection. An alternative, though not contradictory, explanation is that increased returns to tenure with a simultaneous decline in the strength of the pay-performance relationship represents a shift from the use of performance based incentive mechanisms to the use of bonding mechanisms.

Such a model has been presented by Lazear (1979), who argues that firms may induce effort from employees by paying them less than their productivity early in their careers and more than their productivity later in their careers. This will result in the slope of the pay-tenure relationship being steeper than the slope of the productivity-tenure relationship. In such a scenario, an employee essentially posts a bond early in her career by working for less than she is worth. If she is caught shirking, she will be fired and consequently have to forego the higher pay late in her career. Thus, if unions perform an implicit regulatory role as described by Jensen and Murphy (1990) and have less objection to predefined, or bureaucratic, increases in pay, the tenure-pay relationship should vary with unionization[15].

OWN represents two sets of dummy variables for various levels of chief executive common stock ownership and the common stock ownership of the board of directors. The sign of the CEO variables is difficult to determine, *a priori*. Since high levels of CEO ownership represent a substantial vested interest in the success of the firm, executives who own more of the firm's stock should have greater incentive to perform well and therefore should have higher pay. However, failure to find a positive association between these variables and pay may not accurately reflect the relative productivity of high ownership executives since they may have greater lifetime wealth which annual pay measures are not accurately reflecting. It may be that for high ownership variables, pay in firm shares substitute for annual cash compensation. An alternative interpretation of these variables is that very high levels of ownership represent executive entrenchment. Presumably, executives can enter their positions in the firm through various avenues. Theoretically, the market for executives sorts the most productive people into their most productive positions. Executives can enter into jobs less efficiently, however, through mechanisms such as *old boy networks* or nepotism. Also, executives who are firm founders may stay in their positions long after their value in the

[15] This prediction follows from Joskow, Rose and Shepard's (1993) findings. Theoretically, it is not clear to what extent one might expect one form of incentive mechanism to substitute for others. In fact, the use of one mechanism may be complementary to the use of others, or they may be independent.

market for executives has declined by virtue of their control over large portions of firm stock[16]. The market for corporate control should discipline such inefficiencies through takeover bids, though takeovers are likely to have substantial transactions costs and will only be taken if the benefit of acquiring the firm's assets (the increase in the firm's market valuation resulting from the takeover) exceeds the associated transactions costs.

Ownership variables for the directors (net of CEO ownership) is expected to increase pay. This is due to the likelihood that directors with a large portion of their personal assets vested in the performance of the firm will be more diligent in searching for skillful executives. Thus, the OWN variables should proxy for executive effort and ability.

OUT is a dummy variable indicating whether the executive officer was a member of the board of directors for three or less years before acquiring his position as CEO. Since the average board tenure before becoming CEO is roughly six years (though the median is slightly more than 4 years), short board tenure may be an indication of executive quality. This variable measures whether the executive is on a "fast track" and should indicate some extraordinary ability. It is predicted to have a positive effect on executive compensation, and may mitigate any omitted variable bias from executive quality which the measures for unionization pick up[17].

UNION represents the measure of unionization previously described. The variable, following Hirsch, is included in the following analysis in its continuous form

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[16] The current data set does not have information either on whether the CEO is the founder of the company or on the familial relations among the members of the board of directors.

[17] This will be the case if the variable is also positively correlated with unionization, that is if unionized firms are more likely than nonunion firms to select executives from sources outside the boardroom.

and in several categorical breakdowns. The implicit regulation hypothesis suggests that the pay of chief executives in unionized firms should be lower due to union social influence and union effects on firm performance. As outlined above, however, the sign of this measure is difficult to determine, *a priori*, since factors such as unobserved executive ability and union selectivity suggest a positive relationship between unionization and executive pay.

The remaining control variables, represented by *X*, include year controls, industry controls (9 categories), and a dummy variable for the first, incomplete year of tenure for the chief executive.

Econometric problems

The most important issues in the following analysis are related to measurement error in the independent variable and omitted variable bias, most notably pertaining to unmeasured executive ability and unmeasured firm-specific rents. Measurement error in explanatory variables causes bias in OLS estimates. In the present analysis, this may be especially important given there are three possible sources of measurement error in the measure of unionization. First, the measure of unionization for 1977 is used with other variables which are measured in 1980. Thus, there will be error in the measure of unionization for those firms whose unionization rate changed between 1977 and 1980. Second, both of the measures of unionization were acquired in a 1987 mail and phone survey. Since the variable was directed to individuals in surveyed firms, as opposed to objective measures of unionization, the measure is subject to respondent error (since any two respondents in the same firm are likely to give different estimates). The third source of measurement error is due to recall error, since the 1977 measure of unionization was collected in a 1987 survey. Measurement error in OLS regressions is generally thought to bias the estimated coefficient toward zero and may cause bias in the estimated effects of variables not measured with bias. The first of these problems will not be addressed in this paper and the second does not seem to be a problem.

Estimating differences in compensation levels among different groups based on some observable characteristic which varies across those groups is problematic whenever unobserved characteristics exist that are correlated with both the dependent and independent variables. As outlined above, potential sources of omitted variable bias may come from factors at both the executive and firm level. At the firm level, the existence of firm-specific rents will be correlated with unionization and with the compensation of executives. Since the regressions reported here do not control for such rents[18], the estimates on unionization will be biased upward. At the level of the executive, if there are unobserved executive characteristics positively correlated with firm performance and unionization, then the coefficient on unionization will be biased upward if that ability is not adequately measured by observable variables. Omitted variable bias can be addressed by several methods. To the extent that those variables are fixed over time, they can be controlled for using firm-level or executive-level fixed effects.

The existence of union density measures for two years affords the panel nature of the data to be exploited for this purpose. In such a case, the error term in Equation 1 is assumed to take the structure $\varepsilon_{ift} = \phi_f + \upsilon_{ift}$, where ϕ_f represents the unobserved, firm-

[18] Rents are inherently difficult to identify, *ex ante*, and are generally inferred from performance or wage changes following changes the economic environment. For example, see Rose (1983).

specific component. Fixed effects estimates are eliminated by differencing the time varying variables or including firm-specific dummy variables in regressions. In order to control for firm-level omitted variable bias, firm fixed effects estimates are presented below.

Effect of Unions on Chief Executive Pay

Table 1.1 presents sample means and standard deviations for compensation variables. Results are presented for all firms and broken down by unionization categories, where union categories are defined as of 1977. The sample consists of averages on both executive and average employee compensation for the years between and including 1980 and 1987. The dollar values are adjusted to 1991 dollars using the consumer price index for urban areas. The sample for chief executive officers includes observations for only those years where the CEO worked the entire year, and the growth estimates for CEO's are CEO specific. Sample sizes differ between levels and changes because of missing values, and they differ from regression samples in subsequent tables because the years between 1980 and 1987 are included in Table 1.1.

On average, executives in this sample earned a little over $700 thousand per year, while average compensation expenditures for other employees averaged $41 thousand per annum, a ratio of 17 to 1. The ratio of CEO to average pay increased over the years, going from 15 to 1 in 1980 to 19 to 1 in 1987, which is reflected in the percentage change measures.

Levels of CEO pay differ across unionization category, though much of this difference can be accounted for by firm size. Still, among unionized firms, executive pay does decline as unionization increases, though the differences are not especially large. Per employee expenditures also vary by union category, with the highest level occurring in the most densely unionized firms. Interestingly, compensation expenditures per employee do not rise monotonically with union density, since firms with 1 - 40 percent of their work force unionized have lower average expenditures than nonunion firms.

Results in Table 1.1 also show that CEO compensation grew over the sample period at an annual rate of 7 percent on average. This contrasts with the .5 percent growth in average employee compensation expenditures during the same period. This sharp contrast in the growth of pay understates the true difference in compensation expenditures since the measures for executive compensation do not include the realized or awarded value of stock grants and options for the chief executives. By unionization, growth in pay differs for both executives and employee expenditures. Executives in firms with medium levels of unionization had the highest rate of growth, as did the pay of their employees. Employees in high union firms had negative compensation growth, though they had the highest average pay levels. These results suggest that unions decrease the executive employee pay ratio, but that the ratio is increasing faster than firms with less unionization.

Table 1.2 presents summary statistics for firm and executive characteristics. In this table, it is interesting to note the nonunion firms seemed to enjoy the greatest success in terms of sales, employment, and asset growth during the sample period. These firms also had higher average market returns than the unionized firms. Firms with medium unionization, on the other hand, had the most dramatic drop in sales and employment, suggesting negative demand shocks, though this is in contrast to the positive wage growth reported in Table 1.1. Densely unionized firms also had negative average sales and employment growth. Nonunion firms are smallest on average. Means on densely unionized firms indicate they are the largest, though standard deviations suggest that the means are heavily affected by outliers.

Consistent with the results presented in Becker (1987) and Hirsch (1991), unionization is negatively associated with market returns. Average market return decreases monotonically with the level of unionization. Executives of nonunion firms take their positions at an earlier age and have longer tenure, though this result is likely to be a function of firm size. Likewise, executives and board members of nonunion firms hold a large portion of the firm stock than executives and board members in unionized firms. Among unionized firms, there is no clear pattern of share ownership which emerges. Since differences in the summary statistics are only suggestive (and frequently not statistically significant), the differences evident in tables 1 and 2 must be examined further using regression analysis.

Table 1.3 presents results from OLS regressions where the dependent variable is the log of annual salary plus bonus for chief executive officers. The sample size is smaller than in Tables 1 and 2 because it includes data from 1980 and 1987 only. This change is due to the fact that there are only two years of observations for the union variable. Four specifications are presented, with the first excluding unionization; the second including the linear measure of unionization; the third including the linear and the quadratic measures of unionization; and the fourth including categorical variables for unionization.

Estimates of the size elasticity are .26 and strongly significant. This estimate matches the .25 - .30 values presented in other analyses (Rosen, 1992), and says that the pay of chief executives increases roughly 2.6% with every 10% increase in firm size. This elasticity changes little across the OLS specifications presented. The sensitivity of executive pay to changes in the market value of the firm is estimated at .06, implying a .6 percent increase in executive pay for every 10 percentage point increase in market returns. This point estimate is smaller than expected, being about half that estimated in other papers that use roughly contemporaneous data. Joskow, Shepard, and Rose (1993), for example, report pay sensitivity estimates for market performance of around .10 from their OLS estimates. Rosen reports estimates from the literature generally range from .10 - .15 in both OLS and fixed effects estimates. The low estimated market return increases to .12 when pooling across additional years of data, suggesting that the selection of years may have some impact on this estimate. Estimates on human capital variables are the expected sign and a reasonable magnitude. Estimates suggest that returns to tenure peaks at 28 years for executives, while returns to experience peak at 36 years.

The relationship between unionization and executive pay in column two suggests that unionization is negatively associated with executive pay. The OLS estimate suggests that going from zero to a work force that is fully unionized results in a decline in executive pay of roughly 10 percent, though the estimate is only marginally significant. The negative coefficient is consistent with the implicit regulation hypothesis, though it is also consistent with executives in unionized firms being less productive, perhaps because union rent-seeking reduces the impact executives can have on firm success.

Results in Column 2 mask a nonlinear relationship between unionization and executive pay that is apparent in closer inspection of the data. Column 3 presents results from regressions including the continuous measure of unionization and its squared value. The inclusion of the squared measure of unionization has little impact on the estimates of other variables, though it does suggest a rather strong nonlinear relationship between the level of unionization and executive pay. The estimate on the linear term of unionization is positive and significant at the 10 percent level (the linear and quadratic terms are jointly significant at the 5 percent level of significance). Estimates suggest that executive pay rises sharply with unionization of the work force, until it is 28 percent unionized, at which point further unionization starts to decrease executive pay. The relationship between unionization and executive pay falls quickly, though it remains positive until roughly 60 percent of the work force is unionized[19]. This nonlinear relationship suggests that unions, *per se*, do not have a negative impact on executive pay until they reach fairly high levels of union density. This has also been found by DiNardo, Hallock, and Pischke (1997) in an analysis similar to this one.

The fourth column of Table 1.3 inspects the effect of unionization on CEO compensation using categorical variables for unionization. The four categories of unionization are nonunion firms, firms with 1 to 40 percent of their work force unionized, firms with 41 to 70 percent of their work force unionized, and firms that are more than 70 percent unionized. Results reflect those presented in Column 3, since there appears to be

[19] Plotting the residuals of a regression excluding any of the union variables against unionization reveals that the negative relationship between unionization and executive pay is not a function of outliers. Eliminating one observation that has an especially negative error term (a firm with a high level of unionization) changes the point estimates somewhat, but the results are qualitatively the same.

a strong nonlinear relationship between union density and the pay of executives. OLS results show that firms with 1 - 40 percent of their work force unionized tend to pay their executives seven percent more on average (with marginal significance) than nonunion firms. Firms with 41 to 70 percent unionization pay their executives roughly the same as nonunion firms, and firms with the highest levels of unionization pay their executives significantly less than the executives of nonunion firms. Executives in highly unionized firms are being paid nearly 17 percent less than executives in nonunion firms (the entire set of dummy variables are jointly different from zero at 1 percent confidence level).

The initial positive association between unionization and executive compensation is not likely to reflect a causal relationship. Such an interpretation would require that either unions are increasing the productivity of chief executive officers, or, at least, that boards of directors must compensate executives for the disutility of interacting with unions. Neither of these explanations would explain why the initial positive effect decreases with further increases in unionization. More plausible explanations of high executive compensation with low levels of unionization are that unions target firms with more capable managers or that the presence of unionization indicates that there are unobserved rents that are also increasing executive pay. The two sets of ownership variables and the dummy variable for short board tenure are intended to proxy for executive ability. Since they are not likely to be correlated with unionization, they will not control for those characteristics which unions presumably target. In fact, their exclusion has little impact on the presented estimated effects for unions.

The initial positive association between unionization and executives most probably reflects the existence of unobserved rents, while the decline in executive pay with further unionization indicates unions capturing those rents. Whether this eventual negative impact of unions is also due to unions exerting a regulatory effect on the pay of chief executives or a productivity effect is not immediately apparent. Arguably, the role of productivity differences between executives within the manufacturing industry is likely to be less serious than that difference between executives in regulated and nonregulated industries (although the estimated difference is smaller, as well). In regulated industries, the cost allowance and price setting functions of regulatory agencies may allow those monopoly firms to overcome the impact that relatively poor executive performance has on the firms' financial market performance. Manufacturing firms do not have such latitude, since poor performance which results in price increases or the decline in returns to assets will quickly drive firms from the market. The fact that chief executives in unionized firms seem to behave differently in terms of investment decisions and other matters does not necessarily imply that they are less productive in maximizing shareholder value, since maximizing shareholder value implies that executives will engage in only those activities where the marginal value exceeds the marginal cost. Knowing which projects *not* to undertake is as important an knowing which projects to undertake. Thus the negative impact of unions on executive pay is likely to be the result of a regulatory effect.

To discern whether the second prediction of the implicit regulation is at work in unionized firms, separate regressions were run by unionization category on a model similar to that presented in Table 1.3[20]. The implicit regulation hypothesis predicts that the pay of executives will be less tied to the performance of the firm as political forces

[20] The model exclude ownership variables and the dummy variable for less than three years board tenure. The union categories are those presented in Table 3, Column 4.

exert pressure on firms to temper executive pay. The sample for these regressions includes all observations from 1980 through 1987 for those firms where reported levels of didn't change categories unionization (with the levels defined as in Column 4 of Table 1.3). The results of both OLS and fixed effects regressions suggest that pay-performance sensitivity is highest for executives with the highest levels of unionization and lowest for nonunion firms, though the difference between them is not statistically significant[21]. Low and medium unionized firms do have pay that is more sensitive to performance than is the pay of executives in nonunion firms, at marginal levels of significance (the point estimates lie below the levels of the most unionized firms, however).

These results suggest that relative to nonunion firms, executives in unionized firms have stronger pay performance relationships in the measure of salary plus bonus, in direct contradiction to the implicit regulation hypothesis. Among unionized firms the pay-performance relationship doesn't seem to fall substantially with unionization, though estimates initially fall and then become statistically weaker as firms become more unionized. On the other hand, the coefficient on the tenure variable does increase monotonically with the level of unionization in both OLS and fixed effects results.

The combination of results from these regressions suggests that boards of highly unionized firms may use Lazear-type incentive mechanism as pay for performance relationships become less reliable. In this sense, the results are weakly consistent with the

[21] The estimates are (standard errors in parentheses):

		Nonunion	Low	Medium	High
OLS:	Tenure	.003 (.009)	.017 (.012)	.034 (.015)	.043 (.027)
	Return	.057 (.043)	.205 (.037)	.153 (.026)	.248 (.171)
FE:	Tenure	.016 (.006)	.026 (.006)	.052 (.014)	.068 (.037)
	Return	.067 (.025)	.147 (.027)	.126 (.039)	.269 (.114)

steeper tenure profile for executives in regulated firms found by Joskow, Rose, and Shepard (1993). The results may also indicate that the complexity of the job for executives increases with unionization, and that executives in these firms experience much on the job training.[22]

The finding that the pay of executives in unionized firms is more sensitive to performance also suggests that the implicit regulation hypothesis needs to be further formalized. It is not necessarily the case that union members will not want the pay of executives to be insensitive to performance, especially if making pay sensitive to performance is an effective method by which firms generate extraordinary profits. If this is the case, then unions can capture some of these rents through exercise of the strike threat. Of course, reconciling higher pay variability with lower average pay is problematic, since on average greater income risk should be offset by higher average executive pay.

The preceding analysis must be qualified with respect to the failure to incorporate long term incentive pay into the dependent variable. As Hall and Liebman point out, "the large literature that measures pay-to-performance sensitivity with salary and bonus elasticities should be interpreted with the important caveat that salary and bonus sensitivity is only the tip of the iceberg (p.30 1997)."

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[22] This result implies that, *ceteris paribus*, the union pay differential closes over time, which is exactly the case. Executives in the firms with the highest level of unionization and with greater than the median tenure have a smaller wage differential than executives in high union firms with low levels of tenure. The reason for this is not immediately clear and merits further consideration, though the use of Lazear-type bonding mechanisms seems a compelling explanation.

Unionization and pay equality

Table 1.4 presents results for the effect of unionization on the relative pay of firm employees. Freeman has shown that unions tend to decrease the pay differential between unionized workers and white collar workers by 30 percent. Joskow, Rose, and Shepard (1993) note that the pay of blue collar workers in regulated industries is generally higher than the pay in nonregulated industries, and that regulatory agencies tend to be more tolerant of relatively high compensation for average employees (presumably for efficiency wage types of reasons, because of regulator capture, or because of redistributive preferences of regulators) than for executives. Thus, part of the regulatory effect may be in the emphasis on decreasing the relative pay of executives within firms. High pay for chief executive officers may be palatable to union members if their pay is also relatively high. In order to examine the effect that unions have on pay inequality within the firm, the following equation will be used to estimate the firm level-determinants of compensation expenditures per employee,

$$PAY_{ft} = \beta_0 + \beta_1\, SIZE_{ft} + \beta_2\, UNION_{ft} + \beta_3\, X_{ft} + \mu_{ift} \quad (2)$$

where *f* represents firms and *t* represents years; PAY is per employee labor related expenditures; SIZE is the log of annual sales, and is used to control for firm size, UNION represents the three measures of unionization which were used in the analysis of executive compensation; and X represents dummy variables for year, industry, and region of firm headquarters. These estimates are then subtracted from the corresponding estimates presented in Table 1.3, which is comparable to regressing the log of the ratio of executive pay to average employee compensation on the control variables[23].

[23] In fact, regressing the log of the ratio on the variables in (2) changes the point estimates

Results in Column 1 show that the pay of employees is positively and significantly related to firm size, with the point estimate roughly consistent to that found in other studies (for example, Hansen, 1996). This effect is attributable to a number of explanations, such as the use of efficiency wages to solve monitoring problems in large organizations, rent sharing arrangements, or union threat effects.

Including the linear measure of unionization decreases the size elasticity by .008. The coefficient on unionization is nearly positive 20 percent, reflecting the differences in employee pay levels reported in Table 1.1. However, this specification also masks an apparent nonlinearity in the effect of unionization on average employee pay. Linear and quadratic estimates suggest that the effect of increasing union density from zero is to initially decrease the average compensation expenditures, suggesting that there is not necessarily a strong union threat effect within firms. Alternativey, this may indicate that at low levels of unionization, management is able to adjust its behavior to eliminate inefficiencies elsewhere in the firm in order to maintain firm profitability (Addison and Hirsch, 1986). However, as union density increases (estimates suggest that the average pay is at its lowest when 30 percent of the work force is unionized), further unionization increases average compensation expenditures. Estimates in Column 4 underscore this pattern, with low levels of unionization decreasing average compensation expenditures (though insignificantly) and high levels increasing the average (with marginal significance).

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little, though sample size falls and standard errors are larger. Also, regressing CEO compensation on the variables in (2) and subtracting the estimates for average employees changes the results little.

It is immediately apparent that the effects of unions on the pay differential between the executives and average employees is going to reflect the results in Table 1.3. Subtracting the coefficients from those results gives the effect of the variable on increasing or decreasing relative pay. These results, and their standard errors (calculated as $(var_{CEO}+var_{EMP})^{.5}$), are given in columns 1' - 4'. Estimates suggest that firm size increases executive pay faster than it does the pay of average employees. For unionization, the estimate in Column 2' suggests that unionization dramatically decreases the inequality between average and executive pay. This occurs both through decreasing executive pay and increasing the pay of average employees. The estimate presented in Column 2' of -.31 is close to the effect reported in Freeman (1980) for the decrease in the union employee-white collar pay differential. Column 3' estimates include both the linear and squared measure of unionization. These results indicate that relative pay in firms with low unionization is higher than in nonunion firms. It is also substantially higher than in highly unionized firms. The categorical variables also reflect this relationship, with the highest union category closing the gap between executives and their employees by 28%, which is nearly the same as the Column 2 estimate. These results suggest that unions may act as implicit regulators by decreasing the pay gap between union members and executive officers. To the extent that the pay of unionized employees in low-union firms is increased to the levels reflected in densely unionized firms, there may be little union pressure to restrain executive pay in those firms.

Fixed effects estimates

The results presented above indicate that at high levels of unionization, unions have moderately strong negative effects on the pay levels of chief executive officers, while at low levels there is a positive association between unionization and chief executive pay. The positive results in these lower categories may be biased due to unmeasured variables correlated positively with both unionization and executive pay. To the extent that such variables are time invariant, they can be controlled for using firm fixed effects techniques.

Before turning to the results from fixed effects regressions, however, it is useful to inspect the degree of variation in the unionization variable over time. Appendix Table 1.1 provides summary statistics on the changes in unionization that were reported by the firms in the sample. For the sample in Table 1.3, changes in unionization are given in Panel A. Of the 147 firms with observations on all variables in both 1977 and 1987, the mean change in unionization was a decline of nearly 5% (given in Panel A.1). Forty eight percent of firms reported a decline in unionization, while only 12% reported an increase. In total, 60 percent of firms reported a change in unionization over the sample period.

Changes in categories are given by the cross tabulation in Panel A.2. Of the 147 firms in both years, 30 of them reported a change which put them into a different category. Of these, 8 firms increased in category, while 22 firms reported a decrease in category. It is worth noting that changes between categories can be smaller than changes within categories. For example, firms that report a decline in unionization from 41% to 39% will be coded as changers, while firms that report a decline from 39% to 1% will not. On average, however, the changers between categories experienced much larger changes than those that remained in their categories. For example, the average change for firms that declined categories was a 23 percentage point drop, while the mean for those firms which did not change categories, but which reported a decline in unionization, was -10 percentage points.

In fixed effects estimates, the effect of an explanatory variable on the dependent variable is identified only by those firms that report a change. Thus, much rides on the way the regression is specified. In this case, using the continuous variable of unionization will provide relatively more changers (88 firms report a change) than the specification which uses the categorical variables (30 changers). However, intuitively it seems as though large changes are going to have more impact on the "political" influence unions wield, or cease to wield, in determining chief executive pay than small changes in unionization. In the regressions in Table 1.5, the results are presented for the corresponding specifications in Table 1.3 and encompass both types of changers.

An additional concern in fixed effects estimates is the assumption that the structural relationship between unionization and executive compensation is stable over time. The fact that the measures of unionization are ten years apart may be some cause for concern, especially given the dramatic changes which occurred in collective bargaining outcomes during the 1980s[24]. Since the estimates presented in this paper presume to measure a "political" effect on the compensation of executives, it is questionable whether the political impact was the same in 1987 as it was in 1980 given the important developments that took place during that time in the outcomes of union contract agreements. OLS regression results by year suggest, however, that the estimates

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[24] See Bell (1989) for an analysis of collective bargaining outcomes during the 1980s.

are relatively stable both in terms of sign patterns and magnitudes. F tests on the entire model and for the unionization variables do not reject pooling over the two years[25].

Column 2 presents results from including the linear measure of unionization in fixed effects estimates. Relative to ordinary least squares, the point estimate changes sign and the standard error increases dramatically. The result indicates that the negative effect in OLS estimates is biased downward and that small changes in unionization have little impact on the pay of chief executive officers. The standard error increases in magnitude dramatically, indicating that the measure of unionization is very noisy, has too little variance, or that there are two few changers to get reliable estimates. Given the survey methods used to gather the measure on unionization and the fact that the initial year of compensation does not correspond to the 1977 measure of unionization in these regressions, it is unsurprising to find such a noisy estimate. Furthermore, fixed effects estimates may be less reliable than OLS estimates in the presence of measurement error (Hsiao, 1986). That is, the bias associated with measurement error in OLS is exacerbated in fixed effects estimates. Unfortunately, the data set used here affords only two years of data on unionization and therefore provides little opportunity to address this issue.

Including the quadratic term allows the firms who experience big changes in unionization to have more impact on the estimated effect. Consequently, the results seem to reflect those in Table 1.3, Column 3 in that there remains a positive coefficient on the linear term and a negative term on the quadratic, though the standard errors have increased dramatically in size. The relationship between unionization and executive

[25] The only variable for which pooling is rejected over 1980 and 1987 is the measure of market performance. In 1980 the effect is positive and significant, while for 1987 it is negative and indistinguishable from zero.

compensation is positive initially, but it peaks and eventually declines. Notably, these results suggest that the effects of unionization on executive pay are positive over the entire range of unionization relative to nonunion firms. Thus, the fixed effects estimates retain the upside-down U shaped relationship exhibited in Table 1.3, but it is much flatter than OLS estimates indicate with the flatness resulting from a moderate decline in the positive effect and a relatively large increase in the previously negative effect. Fixed effects estimates indicate that the relatively low pay in highly unionized firms is less a function of unions than a function of firm heterogeneity, though such an interpretation of this bias is tempered given that the estimates are likely to be seriously biased toward zero due to measurement error. In addition, given that the standard errors in the fixed effects estimates are rather large, too much should not be read into them.

This relationship is also reflected in Column 4 estimates. Although the point estimates are much higher than the estimates presented in Table 1.3, they do increase initially and then slowly decline. The standard errors remain very large in this specification. Together, fixed effects estimates suggest that OLS estimates are biased. The bias story does not fit well with that hypothesized: that firm level rents associated positively with both unionization and executive pay were biasing the estimates of the effect of low levels of unionization on executive pay upward. In fact, while the estimated impact of unions does seem to shift down somewhat at lower levels of unionization, the effect at higher levels of unionization become more positive, and at the highest levels of unionization the effect becomes positive. For categorical measures, the effect become larger for all levels of unionization, and becomes positive for firms with high levels of unionization (though only one firm enters that level of unionization over the time period, while seven leave). To the extent that rents are time invariant, then the results from fixed effects regressions suggest that OLS estimates are not biased upward due to unobserved rents. However, if rents vary over time, then estimates from both fixed effects and OLS regressions are biased.

Fixed effects estimates control for unobserved firm heterogeneity. Also of interest is how changes in unionization effect the pay of executives. To explore the effect that increases or decreases in unionization, or large versus small changes, have on the pay of executives, the following model was estimated:

$$\Delta SB_{if} = \beta_0 + \beta_1 \Delta X_{if} + \beta_2 \Delta UNION_f + v_{if}, \quad (3)$$

where ΔSB represents the difference in the log of executive pay between 1987 and 1980, ΔX represents the difference in other time varying variables (here X represents log of sales, market return, tenure, and experience and their squared terms), and $\Delta UNION$ represents various specifications of the changes in unionization. For example, $\Delta UNION$ may represent the simple difference in the measures of unionization between time periods. More importantly, $\Delta UNION$ is specified as dummy variables indicating whether the change in unionization was positive or negative, or whether the change was sufficient to change the category of unionization (for the present analysis, categories are defined differently than above, with the categories here being nonunion, 1 - 50 percent union, and greater than 50 percent).Using this specification allows one to isolate the effects that changes of different direction and magnitudes had on the pay of chief executives.

Results from this exercise suggest that there is a difference between firms that change categories and firms that report changes which don't result in categorical changes, though t-statistics rarely reach traditional magnitudes of significance. Specifically, executives of firms that report that the level of unionization declined experienced a 3 percent (s.e.=.06) increase in pay, while those in firms experiencing an increase in unionization reported a decrease in pay of 3 percent (s.e.=10), both relative to firms that report no change in unionization. Executives of firms that declined a category, however, experienced a decrease in pay of 10 percent (s.e.=.09), while increasing a category resulted in a 7 percent decrease in pay[26]. In specifications distinguishing between categorical changers and firms reporting simple changes[27] the effects of reporting a change large enough to change categories is not so negative, while within category changes resulted in a slightly higher increase in pay. In all, these results suggest that moderate decreases in reported levels of unionization result in higher executive pay. This result is consistent with the implicit regulation hypothesis, though such an interpretation is unsatisfying to the extent that the political effect associated with small changes in unionization is likely to be negligible. The negative effect of categorical declines in unionization is inconsistent with implicit regulation, though the variable may be indicating firms that are experiencing negative economic shocks which cause both the level of unionization and the pay of executives to fall.

A final specification uses 4 dummy variables indicating whether the firm reported a large change (positive and negative) or a small change (positive and negative), irrespective of the categories for unionization. Results from these regressions indicate

[26] Several other specifications have been analyzed, including an exhaustive representation of the cross tabulation matrix of possible changes (9 categories) and their interactions with continuous changes. Due to small sample sizes, these specifications frequently provide very noisy estimates.

[27] That is, a specification with four dummy variables (with no overlap between them) taking the form (standard errors in parentheses):

-.076 * Dec. Cat + .051 * Dec - .029 * Inc - .047 * Inc. Cat.
(.099) (.067) (.119) (.140)

that executives of firms reporting the largest declines in unionization experienced a 1 percent increase in pay (relative to executives in firms reporting no changes in unionization), while those experiencing the largest increases in unionization experienced a decrease in pay of four percent. Both results are not statistically different from zero. Point estimates suggests that the magnitude of the change is less important than whether the change put the firm in a different category for unionization. Since the categorical break points were not formally determined in this analysis and there are not statistically important differences between specifications, it is difficult to reach strong conclusions from these results.

Running fixed effects on per employee compensation results in similar patterns as described for fixed effects on executive compensation. Panel B in Appendix Table 1.1 gives the means on changes in unionization for the 48 firms with all observations for this variable in both 1980 and 1987. The mean change is a decline of 4 percentage points in the proportion of workers covered. Fifty four percent of firms reported a decline in unionization, while 8 percent of these reported an increase. While these numbers are reasonably close to the changes in the executive sample, the number of firms that changed categories decreases dramatically. Of the 47 firms, only 5 reported a change in category. Thus, fixed effects estimates are ignored for specifications using categorical measures of unionization.

The estimate on changes in unionization for the linear measure is -.18 (s.e.=.17) indicating that higher unionization results in lower per employee compensation. Including the quadratic term changes the linear estimate to -.49 (s.e.=.42), while the quadratic term equals .48 (s.e.=.60). The pattern of the results of the linear and quadratic specification, while not statistically significant, follow those of OLS estimates. The point estimates suggest that the effect of unionization is to lower employee expenditures initially, though further unionization eventually increases pay. The estimates are dubious, however, given that they imply lower compensation expenditures for unionized firms through the entire range of unionization. Again, the estimates are imprecise and likely to suffer from substantial bias toward zero due to measurement error.

Fixed effects estimates, overall, do not provide precise estimates of the effect of unionization on executive pay. Due to data problems, the fixed effects estimates are likely to be less reliable than OLS estimates. Nonetheless, in specifications which include the quadratic term of unionization, the overall pattern of the estimates is the same as those in OLS. Thus, the implication remains that among unionized firms additional unionization eventually decreases the pay of executives, consistent with the prediction that implicit regulators negatively impact the pay levels of chief executive officers. Similarly, unionization eventually increases the pay of average employees, thereby decreasing the pay difference between employees and executives. In closing this gap, unions seem to act analogously to regulatory agencies.

Conclusions

Other authors (Jensen and Murphy (1990), Joskow, Rose, and Shepard (1993) and Joskow, Rose, and Wolfram (1996)) have argued that political considerations decrease the pay of executive officers and decrease the sensitivity of pay to firm performance. This paper incorporates firm measures of unionization into the analysis of executive compensation as a source of political variation between firms: economic research on unionization frequently models unions as political organizations which pursue the interests of their average members. Findings show that only the pay levels of executives in highly unionized firms earn significantly less in terms of annual salary plus bonus than executives in nonunion and less unionized firms.

Other estimates do not indicate that performance sensitivity for CEO salary plus bonus declines with the level of unionization. In fact, point estimates for performance sensitivity are the highest for the most unionized firms, though differences from less unionized firms are not statistically significant. All categories of unionized firms have more performance-sensitive pay than nonunion firms, suggesting that the implicit regulation hypothesis does not fully capture the preferences of unionized employees. Findings also suggest that the executives in unionized firms have steeper earnings-tenure profiles, the strongest indication that unions have an impact on pay analogous to that of regulatory agencies found in Joskow, Rose, and Shepard (1993). Other results suggest that the pay differential between executives and the average employee is substantially narrowed by unionization, also consistent with previous findings in the literature. The results suggest that unions can impact how much, and the way, executives are paid, but they do not indicate unions have preferences for making executive pay less contingent on firm performance.

Fixed effects estimates, however, seem to indicate that executive pay in unionized firms is greater than the pay of executives in nonunion firms, though those estimates are statistically imprecise and subject to several sources of measurement error. Despite the imprecision, the results for some specifications are roughly consistent with OLS estimates in that the most densely unionized firms have lower executive pay relative to the executives in the low and moderately unionized firms.

Research of the effect of unions on firm performance has shown that unions impact the performance of firms and the decisions that managers make (Hirsch, 1991). This indicates possibility that individual productivity differences between executives may account for the estimated differences in pay. While this is arguably less a concern than in regulated industries where regulatory agencies can buffer firms from the effects of relatively poor executive performance, it remains an important consideration as an explanation of the negative impact of unionization on executive pay. The current data set affords little opportunity to address these concerns, though future analysis should take greater care in identifying instruments that will control for productivity differences among executives.

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Table 1.1: Means (Standard Deviations) of Compensation Levels and Compensation Growth for CEOs and Average Employees in Manufacturing Firms from 1980 to 1987

	CEO salary + bonus		Compensation expenditures per employee	
	Levels	%Δ	Levels	%Δ
All firms	707.6 (455.5)	.070 (.263)	41.1 (10.5)	.005 (.080)
Sample size	1277	893	581	521
Nonunion firms	494.6 (303.6)	.055 (.206)	39.2 (7.2)	.007 (.076)
Sample size	325	231	63	55
Percentage of employees unionized:				
0 < unionization ≤ 40%	805.3 (389.2)	.068 (.211)	38.3 (10.7)	.005 (.065)
Sample size	492	360	248	219
40 < unionization ≤ 70%	775.2 (457.5)	.089 (.355)	41.3 (9.9)	.012 (.070)
Sample size	310	201	165	151
Unionization > 70%	709.0 (709.2)	.076 (.324)	48.3 (9.5)	-.008 (.120)
Sample size	150	101	105	96

All values are adjusted to 1991 dollars using the Consumer Price Index for Urban Areas.

Table 1.2: Means (Standard Deviations) of Firm and CEO Characteristics

A. Firm Characteristics

	Annual sales		Employees		Total assets		Market return	
	Levels	%Δ	Levels	%Δ	Levels	%Δ	Levels	%Δ
All firms	5270.1	.005	32.8	-.004	4308.9	.028	.194	--
	(13811)	(.160)	(71.7)	(.152)	(10451)	(.203)	(.384)	
Nonunion firms	2506.5	.033	20.8	.023	2447.5	.061	.221	--
	(8066)	(.189)	(57.4)	(.175)	(8485)	(.236)	(.459)	
Percentage of employees unionized:								
0 < U ≤ 40%	6374.6	.009	34.1	.001	5083.0	.030	.200	--
	(16231)	(.150)	(48.0)	(.141)	(11785)	(.227)	(.339)	
40 < U ≤ 70%	4687.1	-.020	30.3	-.032	3831.0	.003	.177	--
	(5718)	(.140)	(32.6)	(.130)	(4860)	(.150)	(.389)	
U > 70%	8772.4	-.018	57.7	-.019	6736.1	.008	.157	--
	(22810)	(.157)	(158.8)	(.168)	(16013)	(.129)	(.329)	

B. Executive Characteristics

	Total	Nonunion	0 < U ≤ 40	40 < U ≤ 70	U > 70
Tenure	9.0	11.6	8.0	8.6	7.2
Age at appointment	48.5	44.5	49.8	49.7	49.8
CEO ownership					
1 ≤ shares < 3%	.132	.220	.093	.115	.104
3 ≤ shares < 9%	.114	.198	.076	.076	.139
Shares ≥ 10%	.103	.178	.072	.099	.052
Board Ownership					
1 ≤ shares < 3%	.294	.156	.369	.346	.243
3 ≤ shares < 9%	.270	.370	.243	.256	.179
Shares ≥ 10%	.188	.312	.109	.152	.249

Standard deviations of continuous variables are reported in parentheses. Assets and sales are in millions of 1991 dollars and employees are thousands.

Table 1.3: Determinants of Annual CEO Salary + Bonus, OLS Estimates

	1	2	3	4
Scale:				
Log(annual sales)	.263	.268	.262	.262
	(.015)	(.015)	(.016)	(.015)
Performance:				
Market return	.060	.059	.061	.059
	(.050)	(.050)	(.050)	(.049)
Human Capital:				
Tenure	.012	.012	.013	.012
	(.007)	(.007)	(.010)	(.007)
$\text{Tenure}^2 \times 10^{-3}$	-.218	-.232	-.220	-.223
	(.179)	(.179)	(.178)	(.177)
Experience	.032	.028	.024	.023
	(.019)	(.019)	(.019)	(.019)
$\text{Experience}^2 \times 10^{-3}$	-.439	-.373	-.339	-.318
	(.271)	(.275)	(.274)	(.274)
CEO common stock ownership:				
1 - 3%	-.009	-.008	-.020	-.017
	(.060)	(.060)	(.060)	(.059)
3 - 9%	-.072	-.070	-.074	-.075
	(.071)	(.071)	(.070)	(.070)
> 10%	-.138	-.137	-.156	-.161
	(.080)	(.080)	(.080)	(.079)
Board common stock ownership:				
1 - 3%	.037	.035	.024	.015
	(.052)	(.052)	(.052)	(.052)
3 - 9%	.051	.048	.041	.038
	(.057)	(.057)	(.057)	(.056)
> 10%	.057	.060	.060	.062
	(.064)	(.064)	(.064)	(.063)

(continued)

Table 1.3: Determinants of Annual CEO Salary + Bonus, OLS Estimates (continued)

	1	2	3	4
Less than 3 years board tenure before becoming CEO	.007 (.039)	.008 (.039)	.004 (.039)	.009 (.039)
Unionization:				
Union	--	-.111 (.078)	.399 (.238)	--
$Union^2$	--	--	-.703 (.311)	--
1 - 40%	--	--	--	.070 (.050)
40 - 70%	--	--	--	.028 (.056)
> 70%	--	--	--	-.166 (.075)
R^2	.695	.697	.702	.707

Sample size is 361. Standard errors are reported in parentheses. All specifications include industry and year dummy variables and a dummy variable for the first year of CEO tenure.

Table 1.4: The Effect of Unionization on Per Employee Pay and the CEO-Average Employee Pay Differential

	Per Employee Compensation Expenditures				Difference Between Coefficients of CEO and Per Employee Pay			
	1	2	3	4	1′	2′	3′	4′
Log(annual sales)	.050 (.014)	.042 (.014)	.048 (.013)	.046 (.014)	.213 (.021)	.226 (.021)	.214 (.021)	.216 (.021)
Unionization:								
Union	--	.195 (.076)	-.573 (.235)	--	--	-.306 (.109)	.972 (.334)	--
Union^2	--	--	.949 (.276)	--	--	--	-1.652 (.416)	--
1 - 40%	--	--	--	-.043 (.058)	--	--	--	.113 (.077)
41 - 70%	--	--	--	-.003 (.063)	--	--	--	.031 (.084)
> 70%	--	--	--	.118 (.071)	--	--	--	-.284 (.103)
R^2	.382	.407	.451	.415				
N	169	169	169	169				

Regressions in Columns 1 - 4 are for the log of per employee labor related expenditures. They also include year and industry dummy variables and dummy variables indicating the region of the firm's headquarters. Table entries in Columns 1′ - 4′ are the difference in the coefficients from the corresponding OLS estimates in Table 3 and Columns 1 - 4. Standard errors are reported in parentheses.

Table 1.5: Determinants of Annual CEO Salary + Bonus, Fixed Effects Estimates

	1	2	3	4
Scale:				
Log(annual sales)	.357	.352	.351	.350
	(.064)	(.067)	(.068)	(.070)
Performance:				
Market return	.142	.140	.141	.145
	(.059)	(.060)	(.060)	(.060)
Human Capital:				
Tenure	.012	.012	.013	.014
	(.010)	(.010)	(.010)	(.010)
$\text{Tenure}^2 \times 10^{-3}$	-.468	-.469	-.474	-.499
	(.277)	(.228)	(.231)	(.229)
Experience	.022	.021	.020	.021
	(.025)	(.025)	(.025)	(.026)
$\text{Experience}^2 \times 10^{-3}$	-.200	-.195	-.186	-.198
	(.365)	(.367)	(.371)	(.378)
CEO common stock ownership:				
1 - 3%	.093	.093	.090	.094
	(.084)	(.084)	(.085)	(.084)
3 - 9%	.013	.012	.009	-.011
	(.123)	(.124)	(.126)	(.125)
> 10%	.129	.128	.125	.105
	(.140)	(.141)	(.142)	(.142)
Board common stock ownership:				
1 - 3%	.130	.129	.129	.123
	(.074)	(.074)	(.074)	(.075)
3 - 9%	.167	.167	.169	.160
	(.089)	(.090)	(.090)	(.093)
> 10%	.213	.215	.220	.219
	(.110)	(.110)	(.114)	(.116)

(continued)

Table 1.5: Determinants of Annual CEO Salary + Bonus, Fixed Effects (continued)

	1	2	3	4
Less than 3 years board tenure before becoming CEO	.009 (.060)	.011 (.061)	.010 (.061)	.014 (.060)
Unionization:				
Union	--	.041 (.210)	.146 (.615)	--
$Union^2$	--	--	-.138 (.761)	--
1 - 40%	--	--	--	.158 (.112)
40 - 70%	--	--	--	.109 (.132)
> 70%	--	--	--	.105 (.178)
R^2	.657	.654	.655	.656

Sample size is 361. Standard errors are reported in parentheses. All specifications include industry and year dummy variables and a dummy variable for the first year of CEO tenure.

Table 1.A1: Changes in Unionization Between 1977 and 1987

A.) Executive compensation sample

	Mean change	Firms reporting: Decline	No change	Increase
1.) Unionization	-.046	.480	.397	.123

Mean changes if firms report: Decline	No change	Increase
-.140	--	.166

2.) Union Categories:

		1987 Nonunion	$0 < U \leq 40$	$40 < U \leq 70$	$U > 70$	Total
	Nonunion	33	5	2	0	40 (.272)
1977	$0 < U \leq 40\%$	3	49	0	0	52 (.354)
	$40 < U \leq 70\%$	1	11	25	1	38 (.258)
	$U > 70\%$	0	2	5	10	17 (.116)
	Total	37 (.252)	67 (.456)	32 (.218)	11 (.075)	147

Mean changes if firms change category:

Decline category	No change in category: Decline	No change in category: Increase	Increase category
-.231	-.098	.089	.263

(continued)

Table 1.A1: Changes in Unionization Between 1977 and 1987
(continued)

B.) Per employee compensation sample

	Mean change	Firms reporting: Decline	Firms reporting: No change	Firms reporting: Increase
1.) Unionization	-.042	.542	.375	.083
		Mean changes if firms report: Decline	Mean changes if firms report: No change	Mean changes if firms report: Increase
		-.104	--	.175

2.) Union Categories:

		1987				
		Nonunion	0 < U ≤ 40%	40 < U ≤ 70%	U > 70%	Total
	Nonunion	6	0	1	0	7 (.145)
1977	0 < U ≤ 40%	1	19	0	0	20 (.417)
	40 < U ≤ 70%	0	2	10	0	12 (.250)
	U > 70%	0	0	1	10	9 (.188)
	Total	7 (.145)	21 (.438)	12 (.250)	8 (.167)	48

Proportions of firms in that category are reported in parentheses.

Table 1.A2: Changes in Other Categorical Variables Between 1980 and 1987

Executive compensation sample

A.) Executive common share ownership:

		1987				
		S < 1	1 ≤ S < 3	3 ≤ S < 10	S ≥ 10	Total
	S < 1%	77	12	1	2	92 (.626)
1977	1 ≤ S < 3%	7	9	0	0	16 (.109)
	3 ≤ S < 10%	6	3	10	2	21 (.143)
	S ≥ 10%	3	0	1	14	17 (.122)
	Total	93 (.633)	24 (.163)	12 (.082)	18 (.122)	147

B.) Board of directors' common share ownership:

		1987				
		S < 1	1 ≤ S < 3	3 ≤ S < 10	S ≥ 10	Total
	S < 1%	25	8	0	1	34 (.231)
1977	1 ≤ S < 3%	11	17	6	1	35 (.238)
	3 ≤ S < 10%	4	18	19	2	43 (.293)
	S ≥ 10%	2	4	10	19	35 (.238)
	Total	42 (.286)	47 (.320)	35 (.238)	23 (.157)	147

Proportions of firms in that category are reported in parentheses.

CHAPTER 2

WIDENING PAY GAPS AND FIRM PRODUCTIVITY: DOES FAIRNESS MATTER?

Introduction

The pay difference between CEOs and average workers in the United States is much larger than similar measures from other industrialized countries (Abowd and Bognanno 1995). The wide differential does not escape public commentary, suggesting that the relatively lucrative compensation arrangements enjoyed by CEOs in publicly traded firms are unpopular with the media, many shareholders, and the general public.[28] While not a particularly disinterested party when it comes to matters of pay, the AFL-CIO has devoted a web page to "curtailing" what it deems "excessive" CEO compensation packages, reflecting the skepticism that many people have for CEO pay levels.[29]

This paper explores whether relatively high levels of CEO pay are related to employee productivity. Evidence emerging from several recent surveys of compensation directors and other human resource practitioners supports assumptions used in motivating various efficiency wages models of the macroeconomy. Of particular interest in this study is that variant of the efficiency wage model that argues wage rigidity and persistent inter-industry wage differentials are caused in part by employee concerns with *fairness* in

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[28] Jensen and Murphy (1990) argue that public scrutiny likely suppresses CEO pay levels and changes the way they are paid.

[29] See the AFL-CIO's "Executive Paywatch" web page at *www.paywatch.org*. See White (1981) for a discussion of union motives in emphasizing worker-management pay differentials.

relative earnings. Both Blinder and Choi (1990) and Campbell and Kamlani (1997) find evidence that compensation specialists believe employees will respond with effort reductions to unfair pay developments.

The specific focus of this paper is to determine if there is empirical evidence that supports the fundamental assumption motivating the fair wage-effort hypothesis:[30] namely, that workers reduce effort when wage differentials increase. At least two other papers attempting to identify efficiency wage effects have used a framework similar to the one used here.[31] Both papers include a measure of relative wages in an augmented production function and find that firms paying wages that are high relative to their industry also have higher output. This paper extends such analysis by including a firm-specific measure of relative pay as an explanatory variable in a productivity equation.

The relative pay measure used here captures the difference in pay between chief executives and average employees in the same firm. The intent is to determine if fairness concerns, at least over this particular dimension of a firm's compensation system, manifest themselves in per worker measures of sales or value added. As in the other studies, the relationship between pay and performance estimated here is likely to be biased due to the simultaneous determination of productivity and performance and because adequate measures of worker quality have not been included. Attempts are made below to address these issues.

In all, the results of this exercise do not provide solid support for the assertion that effort reductions occur when pay differences increase. Neither do they point

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[30] See Akerlof and Yellen (1990). See Levine (1991) for a model incorporating similar assumptions about employee behavior, and see Hamermesh (1975) for an early treatment of these issues in a production function framework. Summaries are presented in table 1.
[31] See Levine (1992) and Wadhwani and Wall (1991).

unequivocally to models of workplace tournaments, which suggest that pay differentials will be positively associated with at least some measures of firm performance. Relatively large contemporaneous differences in pay are associated with higher productivity, though it is unlikely that the estimated relationship represents causality. One year lagged measures are also positively associated with higher productivity, with the effect being especially large. Using a two year lag, the effect of widening pay dispersion is negative and frequently statistically significant. Estimates on lagged measures are difficult to interpret theoretically, however. Instrumental variables results also fail to point unambiguously toward a potential negative impact from increasing pay inequality.

It is worth noting that this paper analyzes the effect of increasing pay differences on per employee output, as opposed to firm profitability. By focusing on productivity, important considerations regarding firm financial performance may be missed. For example, a wage reduction imposed on employees may result in decreased productivity, but the increase in profits due to reduced labor costs may more than offset the reduction in productivity. Therefore, one cannot conclude, *a priori*, that profit maximizing firms will not make negative adjustments in wages or relative wages even if such adjustments lead to decreased output. Complementary analysis should therefore examine the effects that wage inequality has on accounting and market rates of return, among other firm performance measures.

The remainder of the paper proceeds as follows: section II reviews the relevant literature linking pay dispersion and productivity. The review is wide ranging, covering theoretical treatments of compensation practices both by economists and by researchers outside of the profession. Section III presents the data and econometric specification used and discusses some of the problems that arise in estimating the pay difference-productivity relationship. Section IV presents the statistical results and analysis and, Section V concludes the paper.

Pay, relative pay, and productivity: theory and evidence

Compensation systems and equity theory

A significant body of literature considers the question of whether pay dispersion is associated with higher or lower productivity. Notably, this issue does not arise in the textbook model of the firm, since standard neoclassical economic theory assumes that profit-maximizing firms pay individuals according to their observable marginal contribution to production. Therefore, wage variation only reflects interpersonal differences in productivity.[32]

However, in fields such as human resource management and industrial psychology, pay is not regarded strictly in terms of market mechanisms. Compensation systems are viewed as integral components of firms' administrative policies, since they are used, for example, to prompt work effort or to direct the flow of employees through internal labor markets. These elements of pay, which are somewhat familiar to economists, also imply social relationships, since pay confers status among employees and control over the pay of others entails issues of authority and conflict (White 1981).

In such a framework, social comparisons are likely to take place. According to equity theory in industrial psychology,[33] individuals derive utility based in part on the

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[32] See Filer, Hamermesh, and Rees (1996) for a discussion of the relationship between ability and income distribution.

[33] See Kenungo and Mendoca (1992) for a detailed discussion of equity theory. Other theories of work motivation and job satisfaction abound, such as *reinforcement theory*,

relationship of their pay to the pay of others. Equity is presumably linked to the workplace since the utility or disutility people feel regarding their relative pay affects their morale and induces behavioral responses. Equity theory predicts that as wage differentials increase, relatively underpaid employees will feel less committed to the success of the firm, resulting in decreased work effort, lower output quality, increased quit rates, and potential acts of sabotage. As Kahneman, *et al* put it, "the rules of fairness define the terms of an enforceable implicit contract: *firms that behave unfairly are punished in the long run* (1986, p.728, italics added)."

From a practical standpoint, the fact that the workplace is subject to sociological and psychological phenomena does not help to explain how interpersonal comparisons take place. Most obviously, the question arises of how comparison groups are selected. Equity in the workplace has *horizontal* and *vertical* dimensions and these dimensions have *internal* and *external* facets.[34] Encinosa, Gaynor, and Rebitzer (1997), drawing on experimental results in social psychology, emphasize "saliency" in the determination of comparison groups. Saliency refers to the increasing effect of interpersonal comparisons "with an individual's similarity, proximity, and exposure to the reference group (p.6)."

Presumably, saliency will be strongest among workers doing similar tasks within the same firm, though "similarity" is not the sole determinant of comparison groups.

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expectancy theory and *facet satisfaction theory*. See Kenungo and Mendoca (1992) and Harpaz (1982) for discussions.

[34] Horizontal and internal dimensions of pay equity are generally emphasized by human resource specialists (Milkovich and Neuman 1996, Gomez-Meija and Balkin 1992, and Levine 1993). The internal aspects of horizontal equity refer to the dispersion of pay among employees doing the same or similar work within the firm, while external concerns refer to the relative levels of pay among workers in relevant labor markets outside of the firm. Equity can conceivably have intertemporal facets, too. That is, an individual may have concerns about her current wage relative to her wages in previous years.

Given the nature of the variable used in this analysis, it is of interest to know whether vertical comparisons matter in determining morale. Martin (1982) confirms that workers make comparisons to workers in superior positions. In a survey of blue-collar workers, almost all made comparisons to those earning more than themselves, and the relative earnings of supervisors were more important than other blue-collar workers' earnings in determining job satisfaction. In Campbell and Kamlani's (1997) survey, 86 percent of respondents thought workers would withdraw effort either a moderate amount (35 percent) or a great deal (51 percent) if the firm increased the pay of highly paid workers without changing the pay of low wage workers. On the other hand, White's (1981) review of evidence suggests that at most only 20 percent of work stoppages or production slow downs, measured by such incidents as strikes or employees' systematic use of work to rule strategies and overtime refusals, were attributable to conflicts over relative earnings disparities.

Several authors have incorporated equity, *per se*, into economic models of production and the macroeconomy. Notably, Akerlof and Yellen (1990), Levine (1989), and Hamermesh (1975) have all motivated models of the macroeconomy with the assumption that workers prefer greater wage equality and will withhold effort if inequality increases. These models are frequently cited as the *fairness* variants of the efficiency wage hypothesis, and have been used to explain a host of macroeconomic and microeconomic phenomena that are difficult to explain using conventional economic theory. Examples of such real-world anomalies include persistent interindustry wage differentials (Krueger and Summers 1986), the relatively small variance in pay within job grades *vis a vis* the variation in individual productivity (Medoff and Abraham 1981), involuntary unemployment (Akerlof and Yellen 1990, Levine 1989, Hamermesh 1975), and tempered adjustments to increases in the minimum wage (Hamermesh 1975).

In Levine's model, wage compression between groups of workers produces *cohesion*.[35] The optimal relative wage levels set the elasticity of cohesiveness with respect to wage compression equal to the elasticity of output with respect to labor. That is, the gains from increased cohesiveness are balanced against the marginal productivity of an additional worker employed at the market wage. In equilibrium, some firms will pay low-productivity workers wages above their marginal product. While the fair wage-effort models offer a possible explanation of why firm or industry-level pay differences arise and persist over long periods of time, they are motivated by assumptions about potential economic consequences that have gone largely untested.

Pay variation in economics: the role of incentives

In economics, incentive effects of pay variation have been incorporated into formal models. This is of particular importance here since models of workplace tournaments (Rosen 1993 and Lazear and Rosen 1981), which emphasize the beneficial role of pay dispersion, may be especially applicable to executive labor markets. In tournament models, pay hierarchy elicits effort from young employees who compete for the pay raises and increased control of resources that go along with promotions. Winning promotion into the next bracket of competition gives one the right to participate in continued competition for the top position in the firm. Furthermore, the difference in

[35] In Hamermesh (1975) market wage changes are treated as exogenous, while in Akerlof and Yellen's (1990) model, wages are treated as endogenous and are adjusted as a function of reference group wages and the competitive wage.

pay, that is the high payoff for promotion, assures that the most talented individuals continue to compete for promotions.

This construction of organizational compensation helps to explain the distribution of earnings among executives, where the pay difference between the chief executive and the second in command is unlikely to be explained by their relative productivities. The high pay associated with the chief executive position is justified on the grounds that the compensation serves as an incentive for the most able executives to maintain effort as the potential number of promotions dwindles. Skewness in the pay scale assures that the most talented individuals do not opt out of further competition when they near the top of the firm hierarchy.

Within the context of tournament models, however, there are instances when increasing pay differentials could lower productivity (Lazear 1988). This occurs because an agent can conceivably affect her relative performance in two ways: by increasing her own productive work effort or by sabotaging the output of her competitors. High potential payoffs from winning contests may induce contestants to engage in acts of sabotage. With a fixed probability of sabotage being detected, the marginal effect of increased productive effort on winning a promotion eventually is lower than the marginal impact from decreasing the output of opponents, accomplished by destructive effort. Thus, if firms cannot detect the type of effort that employees engage in, raising the payoff to winning a competition may have a net negative impact on productivity. In units of production that rely on teamwork or that otherwise emphasize cooperation among employees, excessive pay differentials are likely to lead to unproductive behavior.

One of the ostensive strengths of tournament models in explaining the function of pay inequality is that they avoid the frequently arbitrary behavioral assumptions regarding interdependence in utility functions that motivate models invoking fairness or morale concerns. Lazear's model, for example provides a non-equity based rationale for pay compression. Whether Lazear's model generalizes to situations where agents within a firm are non-competing but potentially concerned about relative earnings is unclear. It does suggest that care should be taken in identifying characteristics which are likely to make individual incentives fail, and in that sense the model is complementary to those emphasizing the social or psychological aspects of the employment relationship.

However, Lazear's model does underscore troubling theoretical aspects of the fair wage hypothesis. If firms can adjust relative wages to increase productivity, the question becomes, why would firms choose suboptimal relative wage levels? The presumption in economics is that profit-maximizing firms will employ the right amount of morale in production. That is, firms are unlikely to choose a wage structure that will leave them worse off than another possible set of wages. The assumption of profit maximization suggests that concerns about fair wages are theoretically "vacuous".[36]

There may be circumstances when firms will choose the "wrong" combination of wages. Managers may be able to make only *near rational* choices regarding the relative wage structure. For example, managers may not know the magnitude of the response their employees will have to changes in relative pay, or managers may overestimate their ability to deter shirking. With respect to the pay measure used here, boards of directors may choose the wrong compensation level for CEOs because they have been *captured* or

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[36] In his commentary on Katz' (1986) discussion of efficiency wages, Weiss argues that "the 'sociological' arguments (for efficiency wages)...appear vacuous (p.286)."

because they are not privy to all relevant information about executive performance. For example, Joskow, Rose and Wolfram cite the work of Crystal which argues that "compensation typically is controlled de facto by the CEO rather than by the board of directors, and that many CEOs are overcompensated as a result (p.166, 1996)" In such instances, the relative wage structure may be near the optimal level, but measurable effects from relative pay concerns may arise.

Implied in this analysis is the assumption that the maximization objectives of chief executives do not coincide with those of owners (or those of the average employee). Principal agency theory, among several other earlier theoretical treatments of managerial behavior, suggests that executives and managers are likely to have different objectives than owners. For example, chief executives may *** insert tirole stuff here *** Rosen (1993) and Tirole (1990) present surveys of possible managerial objectives. Supposing there is some optimal, profit-maximizing chief executive-average employee pay ratio, one could not assume that it is the ratio of pay that the chief executive, or a "captured" compensation committee, would choose.

Related Evidence

Empirical evidence on the role that pay inequality plays in the determination of performance is sparse, though several compelling examples exist. In addition to direct tests for the effects of relative wages on productivity and other performance measures, there is a growing body of indirect evidence, though little of it derives from attempts to sort out the implications of competing equity and incentive concerns.[37]

Evidence on equity concerns and the effects of pay variation can be divided, roughly speaking, into two categories. The first is characterized by surveys and experiments which attempt to gauge attitudes regarding fairness and how these attitudes are likely to impact economic behavior. The second category includes more data intensive statistical analysis. This category includes explorations for evidence of efficiency wages; studies of the effect of worker morale on output; and studies that include wage variation as an explanatory variable in performance regressions. Table 2.2.1 provides a brief summary of these papers.

Kahneman, et al (1985), survey households about their attitudes regarding market transactions in order to determine the "rules" that constitute fairness. They conclude that attitudes about fairness pose binding constraints on the profit-maximizing behavior of agents in the marketplace. Gorman and Kehr (1992) find that chief executives share, in varying degrees, the same attitudes about fairness exhibited in Kahneman, *et al*'s household survey.

As indicated above, Blinder and Choi (1990) and Campbell and Kamlani (1997) surveyed firms to determine which of several efficiency wage assumptions, if any, are consistent with practitioners views of the labor market and employment relationship. Both find relatively strong evidence that compensation directors are reluctant to make wage changes that violate standards of fairness since they fear workers are likely to

[37] On this latter point, this paper is no exception. See Encinosa, Gaynor, and Rebitzer (1997) for a remarkable exception. In that paper they are concerned primarily with demonstrating theoretically and empirically the role that group norms have in determining optimal compensation practices.

respond by withdrawing effort. Concerns about adverse selection also were cited as important reasons for not cutting wages during recessions.

Evidence presented by Levine (1993) from a survey of compensation executives suggests that compensation policies generally are adjusted in response to changes in market wages only gradually, and that compensation executives take care in maintaining existing relative wage relationships when adjusting pay structures, though he finds less concern with maintaining pay relativities across either narrowly or broadly defined occupational groups. In general, survey evidence reveals widespread agreement about what constitutes fairness. Furthermore, these attitudes are perceived as binding constraints on behavior in the employment relationship.

In an exceptional series of papers, Norsworthy and Zabala (1982, 1983, 1985, 1990) attempt to link an index of worker morale they create from information on wildcat strikes, filed grievances, and other presumed measures of employee dissatisfaction to productivity and production costs in the automotive manufacturing industry. Their findings indicate that the slowdown of productivity growth during the 1960s and through the 1970s is partly attributable to deteriorating worker morale, though Straka (1993) questions the robustness of their findings.

In a model that is closely related to this study, Levine (1992) tests the efficiency wage hypothesis using a measure of relative wages acquired in a survey of manufacturing plants on the right hand side of a productivity regression. He shows that plants paying relatively high wages[38] successfully recoup the payments in increased per worker output, though he is unable to distinguish between efficiency wage and rent-sharing explanations.

Wadhwani and Wall (1991) present similar results for a large panel of UK manufacturing firms. They measure average firm-level pay analogously to the method employed here, which divides total labor expenditures by the number of employees. This variable is then divided by the industry average and included in production function equation. They also find that relatively high pay is associated strongly with higher firm output, and that the estimate remains virtually unchanged when using instrumental variables to correct for potential bias associated with the simultaneous determination of wages and productivity.

In tests for the incentive effects of pay dispersion, Leonard (1990) examines whether the steepness of the executive pay profile impacts firm financial performance. Using the same data, Main, O'Reilly, and Wade (1993) identify firms that presumably are more team-oriented, and therefore more prone to the negative effects of sabotage, to examine the effect of increasing executive wage dispersion. Leonard finds no evidence that the steepness of the executive pay scale increases return on equity. Main, *et al*, however, do find a significantly positive correlation between the coefficient of variation for executive compensation and return on assets, though they do not find evidence of sabotage occurring due to excessive pay dispersion.

Using a particularly rich data set, Pfeffer and Langton (1993) test the effect that pay variation has on several performance measures. Specifically, using a large cross-

[38] The relative wage measure was the respondent's estimate of the firm's average wages relative to its three closest competitors, with respondents instructed to hold the characteristics of employees in comparison firms constant.

sectional survey of faculty in US college and university academic departments they find that research productivity, job satisfaction, and participation in collaborative research are all negatively affected by the department-level coefficient of variation for compensation. They also find that faculty research output is higher the more closely it is tied to pay.

The results below present additional evidence on the effects of pay dispersion on output. The analysis complements the above papers by attempting to identify whether the relatively high levels of pay received by chief executives prompts an equity response from employees, or if it is associated with higher productivity. The data used in this exercise are far from ideal, though they compare favorably to data used in previous analysis. The tests come from a panel of firm-level observations, so that effect of pay dispersion is identified while controlling for unobserved, time-invariant firm heterogeneity. In addition, care has been taken to eliminate bias associated with the simultaneous determination of pay and productivity and to address bias associated with the failure to control for employee characteristics.

Data and estimating framework

The relationship between executive-average employee pay inequality and productivity is estimated using the following augmented translog production function,[39]

$$\ln(Q_{ift}/L_{ift}) = \alpha_0 + \alpha_1 \ln(K_{ift}/L_{ift}) + \alpha_2 \ln(L_{ift}) +$$

$$\alpha_3 \ln(K_{ift}/L_{ift})\times\ln(K_{ift}/L_{ift}) + \alpha_4 \ln(L_{ift})\times\ln(L_{ift}) + \alpha_5 \ln(L_{ift})\times\ln(K_{ift}/L_{ift}) +$$

[39] Alternatively, Cobb-Douglas or constant elasticity functional forms could be used. See Appendix A for a discussion of results from these specifications. The source for the data used in this analysis is Standard and Poor's Compustat unless otherwise indicated. All dollar measures used in this analysis are adjusted to 1991 values using the consumer price index for urban areas.

$$\alpha_6 \ln(CEO_{ift} / LAB_{ift}) + \alpha_7 \ln(COV_{ift}) + \alpha_8 \ln(IND_{it}) + \alpha_9 X_{ift} + \varepsilon_{ift}, \qquad (1)$$

where the subscripts, i, f, and t, represent industry, firm, and year, respectively. Q represents output, consisting of total firm sales or value added. Value added is defined as total sales minus the cost of materials. Value added is recovered for this analysis by subtracting the cost of goods sold (Compustat's *COGS*) from total sales and adding back in labor related expenditures (Compustat's *XLR*).[40] Both dependent variables are then divided by the total number of employees, giving per worker measures of output

On the right hand side of the equation, capital is represented by K, and is measured using the dollar value of property, plant, and equipment net of depreciation (Compustat's *PPEN*).[41] Labor (Compustat's *EMP*) input into production is represented by L. X represents a vector of explanatory variables which presumably shift per worker output, including time dummy variables and 2-digit industry dummy variables representing the primary industry classification for the firm in its last reporting year.

The variables of special interest in this analysis measure inequality of pay. *CEO* represents annual CEO salary plus bonus. Executive compensation data were collected independently by the author from firm proxy statements filed with the SEC and archived by the Q-sup data company. Annual data on CEO compensation were collected for 12 years (1980-91) on 348 firms, with a mean of 9.7 years per firm available. In this analysis CEO compensation is measured as the sum of annual salary plus bonus. It therefore understates total annual executive pay by a substantial amount. A better

[40] Technically, rental payments should also be added to profits. Unfortunately, that variable was not among those collected for this analysis.
[41] Gross property plant and equipment and total assets were also used to measure capital stock. Results using them are similar to the results presented here.

measure would also include the valuation of granted stock options and related incentive pay, but reporting requirements for these elements of CEO compensation made the collection of detailed information prohibitively difficult.[42]

LAB represents compensation expenditures per employee. The average employee pay variable is constructed using labor related expenditures divided by the number of employees. The measure is only available for a small proportion of firms due to the relatively relaxed reporting requirements for this particular variable. This measure was available from Compustat for 157 manufacturing firms, with a mean of 8 observations per firm.

Matching the two sources of compensation information results in 1125 matches for 152 firms. Sample sizes for the analysis below are smaller yet due to missing values and the requirement that CEO pay be measured only in years when the CEO worked the entire year. In some analysis below, the data are differenced so that sample sizes shrink even further due to the elimination of a cross section of observations and because of missing values in adjoining years. In addition, differencing reveals some changes in firm size or output levels that represent rather large magnitudes. Such outliers were eliminated by excluding the top and bottom one percent of observations on the following variables: both dependent variables, changes in capital per employee, and changes in

[42] Frequently, granted and exercised options during this period were reported over three to five year spans, making it difficult to identify the number of granted options in any given years, especially given the large number of missing observations due to missing microfiche. Additionally, executives could be granted stocks from more than one program, which presumably had different rules governing their exercise. Finally, granted and exercised options frequently were reported in text, as opposed to being tabulated, making it very time consuming to collect the information about granted and exercised options. See Hall and Leibman (1997) for a discussion of the issues associated with measurement of executive compensation and as an example of successful incorporation of outstanding stock awards.

employment. The observations were eliminated from cross section and fixed effects estimates, as well, with little impact on estimates.[43]

Since high executive pay may be important in prompting work effort among executives, the coefficient of variation (the standard deviation divided by the mean) for the pay of the top five most highly paid executives is also included, represented in equation 1 by *COV*. This information also was collected from firm proxy statements. Its inclusion in the present analysis follows from models of firm hierarchies, which argue that the productivity of the workers in the top of the hierarchy echo through subordinate levels of the firm (Rosen 1992).[44] If executive pay differentials effectively increase executive effort levels, then one should expect to see higher average productivity in firms where the pay differentials are higher.[45] As Rosen states, hierarchical models imply that "a little extra talent at the top can have enormous effects on total output (p.185, 1992)."

The variable is based only on five observations per firm per year, so it is likely to be an imperfect proxy for the entire executive pay scale. The problem is exacerbated because it is difficult to determine if executives other than the chief executive worked the

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[43] The minimum (maximum) values for these before eliminating the tails were -.64 (1.17) for value added per employee, -.67 (1.18) for sales per employee, -1.34 (.75) for employment, and -.84 (1.14) for capital per employee. After eliminating the tails the values were -.30 (.25), -.26 (.34), -.46 (.24), and -.23 (.36), respectively. The maximum value for value added represents a 220 percent increase in reported output per employee over one year, which is unlikely to be attributable to changes in employee morale.

[44] It should be noted that Rosen presents these models to explain the relationship between executive pay and firm size.

[45] Main, O'Reilly, and Wade analyze the pay of executives in the top 5 *levels* of the executive hierarchy. They include the coefficient of variation for executives over these levels in an analysis of firm financial performance. There in no inexpensive way of knowing how much of the senior executive hierarchy is represented by the five executives in this sample, though it is probably safe to assume that they constitute only the top two or three levels. Executives in sub-CEO positions in this sample generally had job titles of president, chief operating officer, executive vice president, group vice president, senior vice president, vice president, or vice chairman.

entire year. The variable should pick up relevant differences between the CEO and those directly competing for the top position, and it should therefore provide useful information about the incentive component of high chief executive pay. Pay variation over the executive pay scale can also have equity effects, though Main, *et al* (1993) find no evidence of sabotage related to excessive variation in executive pay.

IND represents measures of pay variation from 2-digit industries. The variables are from the Current Population Survey, and are derived by aggregating the wage and compensation information from each month for the years between and including 1980 and 1991.[46] They are included in this analysis as proxy variables for possible horizontal developments in pay taking place in the firm. Since Groshen (1991) has shown that there is substantial between-firm variation in pay levels within industries, the variables are unlikely to serve as good proxies. In addition, Davis and Haltiwanger (1993) present evidence that the variables will measure developments in firm-level pay with substantial error. The variables are retained in some of the analysis below since their inclusion does not materially affect the estimates on other variables and since they may be of general interest independent of their usefulness as proxies for firm pay developments.

Included among *IND* are the following variables: the coefficient of variation and the median wage in industry *i* in year *t* measured separately for *white collar* workers (including managerial, professional-technical, and sales occupations), *blue collar* workers (including craft, operative, and laborer occupations), and *pink collar* workers (including clerical and service occupations).

[46] The measure was constructed using all reported measures of pay available in each year for all workers between and including the ages of 25 and 55. No adjustment were made for worker characteristics.

Econometric problems

Several problems with the proposed estimating framework are apparent. Most importantly, estimates on the relative pay variable from equation 1 will likely be biased because of the simultaneous determination of pay and output and because of the omission of important explanatory variables that also are correlated with the pay measures. It turns out that neither of these problems can be resolved conclusively using the present data set, though the problems will be addressed to the extent possible. The strategy used below is to present a broad range of possible specifications and to discuss their relative merits based on theoretical and econometric considerations.

The inclusion of firm-level measures of pay on the right hand side of equation 1 is problematic since pay and productivity are simultaneously determined. In the present case this source of bias may be especially problematic. Levine (1992), in a similar exercise, discounts simultaneity as a source of bias in his analysis using between-firm measures of relative wages as an explanatory variable in a production function. He points out that firms may face short run positively sloped supply curves, implying that an increase in product demand may prompt the firm to increase wages in order to attract additional workers. He argues, however, that the persistence in relative pay levels across plants over long periods of time makes it unlikely that transitory shocks account for his finding. Furthermore, he demonstrates no substantial negative autocorrelation in the growth of his relative wage variable, which he argues would indicate adjustments in wages back to market levels.

Wadhwani and Wall also acknowledge the problem associated with the simultaneous determination of pay and the output measures used here. Rent sharing arrangements in high productivity firms, they point out, may increase the pay of employees in those firms. Using lagged firm financial variables as instruments, they find that their point estimate is virtually unchanged and that the estimate remains strongly significant when instrumenting.

Given the dimension over which the relative pay variable is measured in this analysis, it is unlikely that the issue of simultaneity can be dismissed. This is especially the case for CEOs, since their pay is usually contractually tied to the performance of the firm. Both dependent variables used in this analysis, per employee measures of value added and sales, are constructed using total firm sales.

The potential positive bias associated with both the numerator and denominator of the pay ratio makes the sign of the bias in the estimated effect difficult to predict. By increasing the denominator, positive shocks to productivity will result in a negative bias. However, there is also a positive bias associated with the numerator, since positive shocks to productivity will also increase executive pay. The "net" bias associated with the variable is likely to depend on which measure of pay is most responsive to changes in productivity.

The pay of executives is likely to be more responsive to productivity shocks than the pay of average employees. This could be for a variety of reasons, such as the nature of labor contracts for average workers. Contracts negotiated by unions generally determine wage levels over relatively long periods. Furthermore, implicit contracts between workers and firms may entail wage stability since firms may agree not to cut wages during bad times in exchange for tempered wage demands during periods of relative prosperity, insuring employees against negative income fluctuations.

Empirically, Akerlof, Dickens and Perry (1996) demonstrate with several data sets that the nominal pay of most employees is downwardly sticky. Blanchflower, Oswald, and Sanfey (1996) show that average pay growth in manufacturing industries increases with industry profitability, though the increases in pay come after a three year lag. Thus for average employees there is some upward responsiveness in wages associated with industry rents, though the response is delayed.

For executives, Rosen (1992) reviews both the studies establishing the theoretical link between firm sales and executive compensation and the studies examining the empirical relationship between them.[47] Regarding the latter, he finds a relationship that is statistically very strong whether the two variables are measured in levels or in changes, with the strength and size of the estimate consistent across a wide range of data sets representing different time periods. Furthermore, the relation of executive salary plus bonus to other performance measures is also strongly established, with executive pay being especially responsive to various accounting measures of profitability (Rosen 1992). Thus, empirical evidence suggests that the pay of executives is more responsive to exogenous productivity shocks, implying that the bias in the estimate on the pay ratio in equation 1 should be positive.

The bias associated with endogeneity will be addressed in two ways. The first approach attempts to attenuate the endogeneity problem by including lagged values of the measure instead of a contemporaneous measure. This solution is satisfying provided the error term in equation 1 is not serially correlated, and to the extent that any of the theoretical effects are persistent over time (as discussed below).

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[47] Sales can represent both a measure of scale, since it is highly correlated with measures of capital stock and total assets, or a measure of performance. Rosen notes that recent

Second, the estimates from an instrumental variables approach are presented. When using instrumental variables, the variable selected to identify the structural effect must be correlated with the relative pay variable but unrelated to the error term in the productivity equation. Even if the variable used in this exercise meets these strict requirements, it does not provide unambiguous insight into the relationship between productivity and pay inequality.

A second problem with the model presented in equation 1 which should be addressed is related to unobserved variables. Namely, α_6 may be biased in cross-sectional estimation, even though the pay ratio is lagged, due to unobserved firm characteristics, such as the quality of the work force or management, which are correlated both with the pay ratio and the dependent variables. To get unbiased estimates, measurements for these firm-level characteristics should be included in the analysis.

Due to the panel structure of the data set, they can be controlled for to the extent that they are time invariant. Including firm-level dummy variables will control for time-invariant firm heterogeneity. Unfortunately, the estimated α_6 may still be biased for two reasons. The first is due to the fact that important omitted variables, such as employee quality, are unlikely to remain fixed over time. For example, a firm cutting its workers' wages, can expect its most productive employees to leave, since they are likely to have the highest alternative wage. Thus, one would expect the wage cut and consequent increase in the gap between executive and average employees to result in lower productivity. By not controlling for the change in worker quality, one cannot identify an independent equity or incentive effect.

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analyses of executive compensation emphasize the "scale" interpretation of firm sales.

Results from attempts to address this problem are presented in the following analysis. Levine (1992) also discusses the problem. Though the survey question producing his relative pay measure instructs respondents to control for the relative quality of firm employees, Levine acknowledges that respondents may not have accurately estimated the relative quality of their employees. However, he argues that even if he were able to control for observable differences in the human capital stock of employees, the resulting estimate would not be substantially different than the one he presents.[48] Wadhwani and Wall argue that differencing the variables controls sufficiently for employee quality.

The second reason for bias in α_6 derives from the fact that fixed-effects estimation reintroduces the bias caused by simultaneity. This point is illustrated clearly by recalling that the inclusion of firm dummies in equation 1 is equivalent to removing the firm mean from each variable. Endogeneity is reintroduced in this process: pay_{ift-1} - $mean_{pay}$ is correlated with ε_{ift} - $mean_{\varepsilon}$, since $\text{cov}(mean_{pay}, mean_{\varepsilon}) \neq 0$. Note that the endogeneity exists using this procedure regardless of the number of years the explanatory variable is lagged. Lagging the pay variable one year and first differencing equation 2 similarly results in correlation between the differenced pay variable and the error term.[49]

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[48] To support this contention he cites an exercise he runs using two firm-level data sets with detailed information on wages, firm characteristics, and human capital variables (both data sets have observations of multiple individuals within firms. One is cross sectional, the other is a panel). For each data set he regresses wages once on firm level variables and once on the same firm variables and additional human capital variables. He argues that the high correlation (greater than .9 for both data sets) between equations in the estimates on firm-level variables imply that the high-wage firms do not pay high wages primarily because they have higher quality workers (conditional on occupational mix).

[49] When first differencing equation 1, the error term becomes $\delta\varepsilon = \varepsilon_t - \varepsilon_{t-1}$, while the pay variables (if lagged one year) become, $\delta pay = pay_{t-1} - pay_{t-2}$, so that δpay is correlated

To control for the bias associated with simultaneity, first differencing equation 1 with second-order lagged measures of the endogenous variable:

$$[\ln(Q_{ift}/L_{ift}) - \ln(Q_{ift-1}/L_{ift-1})] = \alpha_0 + \alpha_1[\ln(K_{ift}/L_{ift}) - \ln(K_{ift-1}/L_{ift-1})]$$

$$+ \alpha_2[\ln(L_{ift}) - \ln(L_{ift-1})] + \alpha_3[\ln(K_{ift}/L_{ift})\times\ln(K_{ift}/L_{ift}) - \ln(K_{ift-1}/L_{ift-1})\times\ln(K_{ift-1}/L_{ift-1})]$$

$$+\alpha_4[\ln(L_{ift})\times\ln(L_{ift}) - \ln(L_{ift-1})\times\ln(L_{ift-1})] + \alpha_5[\ln(L_{ift})\times\ln(K_{ift}/L_{ift}) - \ln(L_{ift-1})\times\ln(K_{ift-1}/L_{ift-1})]$$

$$+ \alpha_6\ \delta\ln(CEO_{if}/LAB_{if})^* + \alpha_7[\ln(COV_{ift}) - \ln(COV_{ift-1})] + \alpha_8[X_{ift} - X_{ift-1}] + [\varepsilon_{ift} - \varepsilon_{ift-1}], \quad (2)$$

where $\delta\ln(CEO_{if}/LAB_{if})^*$ is the difference between $\ln(CEO_{ift-2}/LAB_{ift-2})$ and $\ln(CEO_{if-3}/LAB_{ift-3})$.[50] Estimates of α_6 from equation 2 will provide unbiased estimates of the effect of increasing the pay gap within firms on future productivity growth; that is, α_6, in equation 2 represents permanent changes in the rate of productivity growth that are due to past changes in the ratio of executive to average employee pay.[51]

Finally, to address potential problems in the estimated standard errors that are due to heteroskedasticity, the tables below report robust standard errors. Standard errors are calculated using version 5.0 of the Stata software package, which utilizes the method developed by White (1980).[52]

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with $\delta\varepsilon$ since the covariance of ε_{t-1} and pay_{t-1} is not equal to zero.

[50] Note that in the estimates below, results from contemporaneous and one year lagged changes are also presented.

[51] The implied model is related to the discussion in Pindyck and Rubinfeld (1981) on distributed lags. Extending the length of the lag structure in this data set quickly becomes costly in terms of loss of degrees of freedom, due to frequently missing information on either of the compensation variables and because of the relatively short time frame for which observations are available.

[52] At this time, tests for heteroskedasticity have not been performed.

Predictions

The firm-level relative compensation measure used in this analysis emphasizes vertical equity. Given the public ill will that greets the annual announcements of CEO compensation, it is assumed that this dimension of pay inequality is relevant. Since CEO pay is highly publicized and firm specific, two of Encinosa, *et al*'s criteria for saliency are met.[53] The dramatic growth in CEO pay that occurred during this period--when many household living standards were stagnating--may make the use of CEO pay especially appropriate.

Predictions about the relationship between pay dispersion and performance are ambiguous. If the relationship between pay inequality and productivity is persistent over the time period indicated, $\alpha_6 < 0$ is consistent with an equity response to increasing wage gaps, provided there are sufficient controls for worker quality. Alternatively, a positive estimate on α_6 is consistent with high executive pay prompting increases in average productivity, provided there is not appreciable bias from simultaneity or worker quality.

Empirical results

Summary statistics

Table 2.2 presents summary statistics on compensation levels and growth for the CEOs and average employees in this data set. The mean level of salary plus bonus for chief executive officers is $879 thousand per year. The minimum value for average annual CEO compensation is $162 thousand, and the maximum exceeds $12 million.

[53] Recall, the three elements of saliency they emphasize are *similarity*, *proximity*, and *exposure*. The latter two are especially well met.

Elimination of the 1 percent tails resulted in a slight decrease in the mean level of CEO pay, to roughly $850 thousand.

The average compensation expenditures per employee for the firms in this sample is roughly $41 thousand per year. The range of values spans from $11 thousand to $99 thousand. Clearly, executives in this sample are making substantially more than the average worker, even excluding the value of long term incentive awards. The ratio of executive salary plus bonus to average employee compensation expenditures exceeds 21 to 1.

On average the ratio grew over the sample period, as reflected in the percentage change measures presented in growth columns. Executive compensation grew at an average rate of nearly 9 percent. Even after eliminating the 1 percent tales, the mean growth rate in executive compensation approached 7 percent. On the other hand, pay for average employees grew at only one percent per year. Both the levels and changes presented here for salary plus bonus are similar to the results presented in Hall and Liebman (1997).

In addition to the levels and changes, one can compare the incidence of pay cuts and the distribution of pay growth. Average employees took cuts in real wages in 44 percent of cases, whereas chief executives took real pay cuts in their salaries and bonuses in only 36 percent of cases. However, the pay cuts for executives were more likely to be very large, with executives taking cuts in pay of 20 percent or more in 10 percent of cases.[54] For average employees, the largest pay cuts exceeded 7 percent.[55] Executives

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[54] Again, these developments occur in the measure of salary plus bonus only, which does not include losses due to fluctuations in the valuation of long term incentive awards such as stock appreciation rights and other granted options. Using their more comprehensive measure of CEO wealth, Hall and Liebman (1997) report 24 percent of executives lost

also received large pay increases, with 25 percent of observations exceeding 12 percent. Fully ten percent received raises that exceeded 35 percent. For average employees, 10 percent of employees experienced pay growth of 8 percent or more.

Specifying the pay ratio differently, dummy variables were constructed to indicate whether the direction of the pay change for CEOs matched or diverged from the other employees in their firm. In 36 percent of cases, the reported changes both increased, though in nearly 50 percent of cases the two went in opposite directions. In 28 percent of observations, the pay of the CEO increased, while average employee pay decreased. In 21 percent of cases, the pay of the CEO fell while the pay of the average employee increased. In 16 percent of cases, pay levels decreased for both groups. Table 2.2 provides plenty of evidence at the firm level to suspect that violations of fairness standards occurred during the 1980s. Relative pay developments frequently diverged, and the rate of income growth for chief executives greatly exceeded that for average employees.

Table 2.3 presents summary statistics for the basic set of variables. The table indicates that during the time period, annual sales growth averaged about 1 percent per year,[56] while value added increased at a slightly slower rate. The table also indicates a wide range of firm size, although all firms are relatively large. The number of employees

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money in 1994, with a median loss of $3 million. In this sample, the median loss represents a reduction of $82 thousand, dramatically understating the downside risk to CEOs for poor performance. However, given that the gains and losses in the CEO's portfolio of compensation benefits are largely hidden, the more visible components are probably most relevant to this analysis.

[55] Bell (1989) reported that in the early 80s, union wage concessions resulted in compensation reductions of an average of 6 percent in real terms.

[56] Measures are of differences in logs, so that actual percentage growth will be higher. The log growth in the ratio (see table 3) is close to that reported by Abowd and Bognanno 1995.

in the smallest firm is greater than 800, while the largest firm reports having over 800,000 employees.

Regression results

The first set of regression estimates are presented in table 2.4. Results are from augmented translog production functions where value added per employee and sales per employee are regressed on measures of pay dispersion along with control variables for capital, labor, the coefficient of variation for the top five most highly paid executives, industry-level measures of pay and pay dispersion, years, and 2-digit industries. Estimates from OLS, firm fixed effects, and first difference regressions are presented.

OLS estimates indicate that the relationship between the CEO-average employee pay ratio and output is negative. The coefficient is likely to be negatively biased due to omitted worker quality measures. If workers are paid their marginal product, then firms with the most productive workers will have higher pay on average. Higher average employee pay will decrease the relative pay ratio, resulting in a negative correlation between it and output.

Thus, OLS estimates are probably biased. Turning to the fixed effects and first difference estimates, the estimated relationship between the pay ratio and output becomes positive and, in the case of difference estimates, statistically significant. The estimates for fixed effects are also positive but are not statistically significant. Notably, omitting the coefficient of variation for firm executives from fixed effects results in a larger and statistically significant relationship in both the value added and sales regressions.[57]

[57] See table A3 in appendix A for examples of regressions omitting the executive coefficient of variation.

Leaving the executive coefficient of variation out of the difference equations has no effect on the size of the pay ratio estimate or its standard error.

Because of potential incentive and sabotage effects of pay dispersion among executives, the coefficient of variation for them may have a positive or negative effect on productivity. In fixed effects estimates the effect is positive, but in difference estimates it is negative. None of the estimates for this variable is statistically significant.

As argued above, the estimates on the pay ratio are likely to be biased upward due to simultaneity. One of the proposed ways of dealing with this problem is to lag the endogenous variable and difference the equation. Table 2.5 presents results from such a procedure. Only estimates on the relative pay measure are reported. Panel A presents results from a 1 year lag, panel B gives results using a two year lag, and panel C gives results from regressions including the contemporaneous pay ratio and both lagged measures.

Results indicate that the effect of the 1 year lag is generally positive. The effect is strongest in the differenced value added regression, where it is significantly positive. Notably, as discussed above one would expect the estimate to be biased. However the sign of the bias should be negative given that a positive shock in t-1 decreases the error term in equation 2 and increases the differenced pay ratio.[58] On the other hand, bias on fixed effects estimates should be positive for similar reasons. The estimates for both sales and value added are relatively close to zero for fixed effects estimates.

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[58] Recall that the error term in the difference equation is $\delta\varepsilon = [\varepsilon_t - \varepsilon_{t-1}]$, while the growth in the pay ratio is $\delta pay = (pay_{t-1} - pay_{t-2})$. In fact, using the differenced pay variable from t-2 as an instrument for the I year lag indicates that the bias is downward, though, as argued above, the two year lag may not be a good instrument since it may affect productivity.

The second year lag for the pay ratio indicates a negative relationship between the pay ratio and output. In both fixed effects and difference estimates for value added, the estimates are negative and statistically significant at the ten percent level. These provide the strongest indication that increasing pay differences between chief executives and the average employee will result in decreased output. However, in the specifications that include all three pay ratio variables, difference equations indicate positive and significant effects for contemporaneous and 1 year lags, with no effect from two year lags.

As in table 2.4 these estimates generally indicate that the relationship between pay differences and productivity is positive. The exception is for the 2 year lagged measure of pay differences, which is negatively related to productivity. As outlined above, lagging two years in a difference equation will help eliminate simultaneity bias. It also changes the interpretation of the estimated relationship. Instead of measuring the effect of a contemporaneous change on a change in a dependent variable, the relationship becomes dynamic, with the effect measuring changes in productivity growth rates. While such a specification does not follow directly from the fairness models of efficiency wages presented in table 2.2.1, anecdotal evidence does suggest that changes in morale and labor relations tend to persist over long periods of time.

Industry pay variables

Results from industry-level variables are mixed. Since the measures represent a substantially different level of aggregation than the dependent variable, they are likely to be measured with substantial error. The direction of the resulting bias is not easily predicted. Furthermore, the estimates are quite sensitive to changes in specification, though their inclusion does not change the estimated effects for other variables. Fixed effects estimates corresponding to table 2.4 (see appendix table A3) indicate that the coefficient of variation for all three groups is positively related to output, controlling for the average quality of workers in the industry. However, in moving from fixed effects to difference estimates, the effect for pink collar workers changes from significantly positive at the 5 percent confidence level to significantly negative at the 10 percent confidence level. The results for sales regressions exhibit even less consistency between the fixed effects estimates and difference estimates. Given their unreliability, these variables are not included in further analysis.

Instrumental variables estimation

The problem addressed in this section stems from the simultaneous determination of the pay ratio and productivity. It is likely that the strength of the incentive or equity effect of a given relative pay development is strongest shortly after the change has taken place. Thus, it is desirable to identify the productivity effect contemporaneously with the relative pay development.

One can identify in principle a contemporaneous effect by using an instrumental variables approach (Hsiao 1986). The conditions for identifying the structural effect are that the instrument used for identification be correlated with the endogenous variable but uncorrelated with the error term in the original model.

The instrument used here is the proportion of the firm's common stock owned by the board of directors net of the proportion owned by the CEO. This information is reported to firm shareholders in annual proxy statements and was collected with the executive compensation data. Quite plausibly, ownership may have no relation to productivity independent of its effect on the compensation ratio. Suppose executives are paid according to easily observed outcomes, such as changes in the market valuation of the firm, and otherwise for efforts that are more costly to observe and which don't materially impact the valuation of the firm. Secondly, suppose that reputation serves as an important bonding mechanism for executives so that all executives engage in the difficult to observe aspects of the job regardless of board monitoring.[59] Executives in high ownership boards may be observed and compensated more for these activities especially if high ownership makes boards less fearful of objections by shareholders and the public over relatively high executive pay. To the extent that hard to observe behaviors don't contribute to productivity based on their being observed, then ownership will be related to compensation but independent of productivity[60] In addition, high ownership may impact the denominator of the pay ratio by deterring wasteful compensation expenditures that are otherwise unrelated to employee performance.

Just as easily, however, the association of ownership with executive pay may imply differences in productivity. As outlined above, in models of hierarchical firms productivity in the highest positions "echoes" through the performance of subordinate employees (Rosen 1992). Increasing the quality of the employees at the top of the hierarchy will affect the productivity exhibited throughout the firm. Thus, one might

[59] An executive's concern about his how he is perceived professionally will likely ensure that he expends a large amount of effort regardless of monitoring by board members. See Rosen (1991) for a discussion of reputation as a bonding mechanism and its limitations as such. As Hall and Liebman observe, "CEOs are a self-selected group of high-effort, overachieving individuals, it may be that lack of effort is not a first-order agency issue in this population (p.9 1997)."

[60] Jensen and Murphy (1990) argue that public reactions to high executive pay packages may decrease CEO pay levels.

argue that variables picking up productivity differences between executives should not be excluded from the original model. High board ownership implies that a substantial proportion of board members' personal financial interests are vested in the success of the firm, so that they may have greater incentive to recruit and retain CEO candidates with higher average ability. As Rosen states,

> If...competence has extraordinary marginal product for top management positions in large firms, how incompetence is revealed and handled must be important. Formally the job falls to boards of directors. Yet there is much opinion and some evidence...that boards are controlled by the CEO (p.207).

Board ownership, to the extent that it implies board independence from executive control, will presumably be related to productivity through its impact on the quality of the chief executive, implying that it should not be excluded from the productivity equation.

Table 2.6 presents results from instrumental variables regressions. The ownership variable is specified in two ways, in its continuous form and as a set of dummy variables representing particular intervals of ownership.[61] In either form, board ownership is correlated with pay, though the first stage t statistics for the linear measure are insignificant at conventional levels in both the sales (t=1.44) and the value added (t=1.35) samples. The F statistics for joint significance of the categorical variables are both above 4, with p values of .002.

[61] Reaser (1997) uses the variable in its categorical form in an analysis of the determinants of executive compensation. Since neither of the forms constitutes a "nested" version of the other, a Chow test measuring the difference between the different models is not straightforward (and therefore not carried out here). As an approximation of this, one can compare adjusted R^2's from first stage regressions, since adjusted R^2's give explained variation controlling for available degrees of freedom. The first stage adjusted R^2 for the continuous variable in the regression for value added (sales) is .833 (.834), while for the categorical variables it is .836 (.836). Given these slight differences and the fact that the second stage estimates vary somewhat depending on the variable used, results using both forms are presented below.

A problem does arise in the use of the variable, however. Specifically, the implied direction of the bias on the pay ratio estimate changes depending on the way the ownership variable is specified. Table 2.6 presents results from three regressions each for value added and sales. The first column gives non-IV fixed effects estimates for the contemporaneous measure of the ratio of CEO to average employee pay. This column corresponds to the fixed effect estimates in table 2.4, though sample sizes have changed modestly due to missing values in the instrumental variable. The second gives the IV fixed effects estimate using the continuous form of the ownership variable as an instrument, and the third gives estimates using the set of dummy variables for ownership.[62]

Fixed effects results reflect those of earlier regressions. Using the continuous form of ownership as an instrument, changes in the point estimates imply that estimates in the non-IV fixed effects are downward biased. That is, they imply that the fixed effects estimates understate the benefits of relatively high executive pay. The nonlinear specification for ownership, however, indicates that the bias is in the other direction, with the point estimate for both value added and sales becoming negative.

Given the sizes of the standard errors, neither the differences between the OLS and IV estimates nor the differences between the IV estimates for each output measure are statistically significant. In addition, the point estimates on other variables seem to be

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[62] The four categories are for firms whose boards own less than 1 percent of outstanding firm stock, greater than or equal to 1 and less than 3 percent, greater than or equal to 3 percent and less than 10 percent, and greater than or equal to 10 percent. The variables originally were constructed so that the proportion of observations in each of the three categories with greater than one percent ownership was roughly equal. In the present analysis, roughly 25 percent of observations fall into the 1 to 3 percent category, 21 percent fall into the 3 to 9 percent category, and 16 percent fall into the greater than 10 percent category.

sensitive to the specification of the instrument, though the differences are not statistically precise. Whether the instrumental variable results indicate either an upward or downward bias in the contemporaneous measure of pay inequality, they do not indicate a significant relationship between employee productivity and relatively high pay for chief executives.

Firm size

Along with similarity, the saliency of a reference person is likely to be determined by proximity and exposure (Encinosa, *et al* 1997). It stands to reason that firm size will affect one's proximity and exposure to the chief executive officer. This will be true especially if smaller firms have fewer production facilities and are less geographically dispersed than large firms. Assuming this to be the case, the regression sample was divided by the median number of employees, and production function estimates were acquired for the separate samples. Even in the small-firm sample, firms are still rather large, with the smallest firm having 800 employees and the largest having around 25000 employees.

For small firms, results of estimates on the ratio of CEO to average employee pay are,

value added			*sales*		
1	2	3	1′	2′	3′
FE	FE:IV1	FE:IV2	FE	FE:IV1	FE:IV2
.037	.263	-.332	.020	1.464	-.062
(.032)	(.872)	(.226)	(.033)	(2.240)	(.182)

For large firms, results are,

	value added			*sales*	
1	2	3	1′	2′	3′
FE	FE:IV1	FE:IV2	FE	FE:IV1	FE:IV2
.061	.047	-.163	.063	-.032	-.241
(.036)	(.262)	(.154)	(.032)	(.221)	(.156)

There is no indication of substantial or systematic differences by firm size. Fixed effects estimates without instrumenting indicate that the effect of increasing the relative pay of executives is larger in large firms. However, instrumenting for the likely simultaneous determination of the pay ratio and output indicates results that are substantially the same as in the pooled sample. The implied direction of bias for small firms depends on the way that the instrumental variable is specified. In general, the results for categorical variables in columns 3 and 3′ suggests that the direction of bias in fixed effects without instrumenting is positive, though the estimates do not indicate that the true effect is significantly negative. For small firms, estimates in columns 2 and 2′, using the linear measure for board stock ownership, indicate fixed effects estimates in columns 1 and 1′ understate the true effect, though the standard errors are inordinately large.[63]

Ability to pay

The specifications discussed above presume to measure the effects of *fairness*. There may be alternative specifications that better represent developments in pay which people would characterize as fair or unfair. Campbell and Kamlani (1997) ask respondents specifically if employees will withdraw effort when wages increase for

[63] The first-stage t statistic on the linear version of the instrument for small (large) firms is .74 (1.37), while for categorical variables the F statistic for small (large) firms is 3.33 with a p value of .02 (2.27 with a p value of .08).

highly paid groups but remain stagnant for other employees. They also ask if effort reductions differ depending on firm profitability. In both cases more than 85 percent of respondents agreed that such pay developments would result in moderate to large effort reductions. Kahneman, *et al* (1986) demonstrate that households view nominal compensation decreases during times of firm profitability as unfair.

To examine whether a response by employees is dependent on various qualitative developments in pay and profitability, the four groups of relative pay developments identified in table 2.2 are used in the production function to indicate various pay developments. To recap, the first is constituted of observations where the CEO received an increase in pay *and* the average employee received an increase in pay (36 percent of observations); the second group is made up of observations where the CEO received an increase in pay *but* the average employee received a cut in pay (28 percent); the third group is where the CEO received a cut in pay *but* the average employee received an increase (21 percent); and the fourth is where both the CEO *and* the average employee took a cut in pay (15 percent). Note that these variables are specified such that firms can fall into any of the categories depending on the relative wage developments that take place in a given year.

Presumably, during periods when the executive gains while the average employee takes a cut in pay, the negative effects of fairness should be the strongest. Productivity should be the highest when the developments in pay follow one another or when pay levels converge.

Table 2.7 presents estimates from difference regressions including the dummy variables described above. The specifications include pay developments from the preceding two periods. This specification is chosen because in table 2.5 it is shown that this is the period with the most negative relationship between relative pay and performance. Using the pay-development variables from earlier periods is not more illuminating, while at the same time it is complicated with issues of endogeneity.

The results suggest that per employee value added is the highest when both the CEO and average employee took pay cuts. Surprisingly, productivity growth is lowest when CEO pay decreased *and* average employee pay increased, significantly different than the highest group at the five percent level. When firms widened the pay gap between executives and the average employee, they also experienced low productivity growth. In terms of magnitude, the level of growth for these firms was similar to the level when CEO pay and average employee pay converged. The fact that developments which most people would regard as more fair resulted in still lower growth casts doubt on the interpretation that the low growth in firms with the most unfair pay developments is attributable to an equity effect.

It may be true, however, that firms make such adjustments in pay when they have monitoring technology in place that allows them to deter shirking or sabotage. If that is the case, then monitoring intensity is positively correlated with both wage inequality and productivity, and the point estimates will be positively, suggesting the negative effect for cases of diverging pay levels is understated. Thus, the relative effects presented here may not reflect the difference in employee responses that would occur if one were able to control for monitoring intensity.

An additional test of whether ability to pay matters incorporates financial performance. If a firm is relatively profitable, diverging pay developments may have different effects than when the firm is performing poorly. If fairness is a constraint on behavior, profitable firms that impose pay cuts on employees while increasing the pay of the executives should have lower productivity than when pay developments mirrored one another.

In results not reported the four categories of relative pay developments were interacted with a variable indicating that the firm earned more than the median rate of return (either market return or return on assets) in each of the two preceding years.[64] Eight categorical variables are then generated by interacting the high profit (HP) variable and (1-HP) with the four pay categories. Seven of these variables were included in regressions to determine if productivity growth rates differed depending on the interaction of pay developments and profitability.

Results indicate that in cases when one expects the effect to be the most negative (times when CEO pay increased and average employee pay decreased during periods of high profits), the effect is negative with marginal statistical significance. However, the results for when CEO pay decreased and average employee increased in profitable firms are more strongly negative. Thus, arguments that productivity will fall if the rules of fairness are violated do not seem to be supported by the data.

Employee quality

Typically the efficiency wage production function is specified so that labor input is augmented with an effort function, $e(\cdot)$, which is a function of the wage rate,

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[64] Profitability is measured as the firm being above the median rate of return for a sample of roughly 300 manufacturing firms in each of the two years before the wage changes are measured. That is, in years t-4 and t-3.

$$Q = A\,K^{\theta}\,[e(w) \times L^{\eta}]\,. \tag{3}$$

Wadhwani and Wall (1991) specify the effort function in the following way,

$$e(w) = -a + b\,(w/w^{*})^{\gamma}\,u^{-\phi}, \tag{3a}$$

where w is a worker's own wage, w^{*} is her comparison wage, and u is the unemployment rate.[65] The negative intercept assures that firms cannot acquire positive effort at zero wage, and the functional form ensures that the elasticity of effort with respect to the wage declines with the wage. Substituting equation 3a into equation 3 and taking logs gives approximately,

$$\log(Q) = \log(A) + \theta\,\log(K) + \eta\,\log(L) + \alpha\,\log(w/w^{*}). \tag{4}$$

When L represents homogeneous labor, profit maximizing conditions imply that α will equal η, or that the output gain from increasing the wage will equal the effect of employing an additional worker at the market wage. For the present exercise, the two types of labor are quite different, and the above model is not appropriate.[66]

However, following Wadhwani and Wall, industry-level data can be used to help clarify at least part of the relationship in question. Namely, the above framework implies that one can include the ratio of average employee pay in the firm to the average pay in the industry in the production function. In so doing, one can control for labor quality, insofar as it is manifested in average compensation expenditures, and then attempt to identify the effect of high executive pay. In this section the following equation is

[65] The unemployment rate is disregarded here, though it is intended to capture the incentive effect of unemployment, since workers caught shirking will pay a higher penalty when u is high according to some versions of the efficiency wage hypothesis.

[66] Each of the three models summarized above are of firms that employ heterogeneous labor who have interdependent wage concerns. Given the very specific compensation measure used here and the fact that the high wage labor is composed of one person, those models are not amenable to the present analysis.

estimated,

$$\log(Q) = \log(A) + \theta \log(K) + \eta \log(L) +$$

$$\alpha_1 \log(LAB/w^*) + \alpha_2 \log(CEO) + \upsilon.^{67} \qquad (5)$$

All variables are defined as above, except that the new variable, w^*, measures the average annual income for workers in firm *i*'s industry. The measure is from the Current Population Survey and is equal to the mean wage in industry *i* in year *t* multiplied by the average number of hours per week for full time workers. This number is then multiplied by 52 to get annual income. The mean of the resulting measure is slightly over 75 percent of the mean firm-level compensation expenditure. The variables are correlated at slightly over .50. Of special interest here is the effect of high executive pay when controlling for labor quality or effort. The question of whether α_1 represents rent sharing arrangements, relative labor quality, or an efficiency wage effect is left unexamined.

Focusing on the coefficients for CEO pay, one sees that the pattern in the resulting estimates is largely the same as in previous tables:

value added			*sales*		
FE	FE:IV1	FE:IV2	FE	FE:IV1	FE:IV2
.123	.193	-.053	.099	.817	.025
(.017)	(.345)	(.135)	(.019)	(.698)	(.138)

The fixed effects estimates on CEO pay are strongly positive, with a magnitude that is several times higher than when using CEO as the numerator in the relative pay ratio. Using the same instruments as in table (5), the results of instrumental variables are largely the same as seen earlier. The implied bias depends on the specification of the

[67] The simple Cobb-Douglas form indicated here is used. Estimates on the parameters of interest from translog functions are not materially different. The estimates presented are identical regardless of whether per employee or total output measures are used on the left hand side. Year effects and the executive coefficient of variation are also included.

instrument. However, it is worth noting that the relationship between the linear specification and the endogenous variable is relatively weak, with a t statistic that is around 1.2. The F statistic for the categorical variables in first stage estimates remains high, with a value near 4 and a p value of less than 1 percent. Again, the conclusion from instrumental variables estimates is that the productivity benefit of relatively high executive pay is indistinguishable from zero.

Additional notes on this specification are in order: the relative wage variable, *LAB/w**, has a coefficient that is very close to the estimate on *L*. For example the point estimate in column 1 for relative wages is .83, while for labor input it is .86. They are not statistically distinguishable in this case, though in others, especially for sales, they do diverge a little more. The estimate is consistent with efficiency wage theory, as well as with labor heterogeneity and rent-sharing arrangements. Also, including *LAB* alone when estimating equation 5 results in a coefficient that is similar to the relative wage ratio without appreciably changing the estimates from the other variables, though the difference between the *LAB* coefficient and the *L* coefficient tends to increase.

Productivity levels as a function of changes in pay

A final test of the relationship between executive-average employee pay differences and productivity relates changes in relative pay to output levels. That is,

$$Q = f(K, L, \delta pay), \qquad (6)$$

where $\delta pay = \log(CEO/LAB)_t - \log(CEO/LAB)_{t-1}$, and *Q*, *K*, and *L* are measured in period t and specified as in equation 3.

Focusing on fixed effects estimates, the results for the CEO-average employee pay ratio are somewhat different than earlier specifications. Fixed effects estimates from equation 6 suggest that increasing the rate of growth in the pay ratio will decrease growth in value added and increase sales, though both effects are statistically insignificant and economically small. The specification does not eliminate the correlation of the explanatory variable with the error term, so ideally an instrumental variables approach could help identify if there is an effect. The instrument used in previous sections of this paper, board ownership of the firm, is difficult to relate to the change in the pay ratio, however. Measured in either levels or changes, the board ownership variables perform poorly. Other instruments have not been identified.

Conclusions

The ratio of CEO to average employee pay is used to determine whether widening income differences are associated with higher or lower productivity. Theoretically, pay dispersion can have positive or negative effects on productivity, though the results presented here do not indicate that increasing or decreasing pay differences between average employees and chief executives have a straightforward effect on the productivity of employees.

The strongest evidence of negative effects from increasing earnings inequality come from pay variables that are lagged two years. Several reasons suggest that the two-year lag represents the most reliable estimates presented here. First, contemporaneous measures are likely to be biased toward finding a positive correlation between relatively high CEO pay and measures of performance. Second, measures that are lagged only one year remain correlated with the error term when variables are differenced to eliminate unobserved, time invariant firm effects. Finally, the instrument used here to identify a contemporaneous effect does not provide clear evidence for or against the argument that increased pay inequality leads to lower productivity, and superior instruments have not yet been identified. While it is difficult in context of existing theoretical models to interpret the results from the two year lag, the econometric interpretation is straightforward: increasing the current ratio of executive to average employee pay will decrease future productivity growth.

It is difficult to interpret this relationship in terms of existing theory, though there is reason to believe that these represent the most reliable estimates.

While it may be the case that employee morale manifests itself in growth rates over long periods of time, other evidence presented casts doubt on the relationship being the result of perceived inequity. Namely there is no indication that productivity growth is lower in firms experiencing the most unfair developments in pay. Contemporaneous and 1-year lagged measures produce positive and statistically significant results when controlling for firm-level heterogeneity. Instrumental variable methods do not provide clear indications of the direction, or existence, of bias in contemporaneous measures, much less indicate significant positive or negative effects from increasing or decreasing the relative pay of chief executives.

The firm-level pay variable used here measures changing pay practices over only one dimension of the firm--a dimension which may not be the most relevant in these rather large firms. A more satisfying model would incorporate detailed information on the methods of pay, relative pay levels over other dimensions of the firm, and measures of other determinants of employee morale. Discussions in papers presenting fairness variants of the efficiency wage hypothesis seem to imply such a model: relative wages are shorthand for referring to the potential productivity effects of employing a variety of human resource tools that bolster employee morale.

An additional qualification of these results is that they do not indicate the relationship between relatively high executive pay and profitability. Theories examining the connection between executive pay and performance are concerned with performance as it is measured by firm profitability. Principal-agent theory, for example, is concerned with the mechanisms that align executive income with the owners' interests, implying that the most important relationship is the link between executive compensation and the market valuation of the firm. While productivity and financial performance are related, they are not equivalent. Complementary analysis should evaluate the relationship of pay inequality and profitability.

Relative wage concerns have many dimensions that are difficult to disentangle. Both fairness and incentive concerns are likely to be important aspects of firms' compensation systems. The lack of findings in this paper do not indicate that relative wage concerns do not matter. For example, relative pay developments for workers doing similar work within firms, or in firms attempting to employ more team-oriented production technologies, may have more important consequences than developments occuring between CEOs and average employees. Further research should attempt to isolate firm-level relative pay developments and employee productivity in order to determine the extent to which such concerns determine firm productivity.

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Table 2.1: Literature Review

A. Theoretical Treatments: Equity

Authors:	*Paper:*	*Model:*	*Conclusions:*
Daniel S. Hamermesh	"Interdependence in the Labor Market," *Economica*, 1975.	Employees adjust effort to changes in relative wages: $Y=F[E_1\{g(w_1,w_2)\},E_2]$; $g_1>0, g_2<0$	Equity concerns affect short-run responses by firms to exogenous market wage changes. Firms may employ more high-priced labor when their wages increase.
George A. Akerlof and Janet L. Yellen	"The Fair Wage-Effort Hypothesis and Unemployment," *QJE*, 1990.	Employees supply effort in proportion of own wage to fair wage: $e=\min(w/w^*, 1)$; $w^*=f(w_{i\neq j}, w_c)$	Equity concerns cause involuntary unemployment and explains wage compression.
David Levine	"Cohesiveness, Productivity, and Wage Dispersion," *Journal of Economic Behavior and Organization*, 1991.	Firms increase production by investing in cohesiveness through wage compression: $q = C(w_L/w_H)\ f(H,L)$	For some firms, wages vary less than productivity as firms compress the wage structure to utilize cohesion.

B. Theoretical Treatments: Incentives

Authors:	*Paper:*	*Model:*	*Conclusions:*
Edward P. Lazear and Sherwin Rosen	"Rank-Order Tournaments as Optimum Labor Contracts," *JPE*, 1981	Executive labor market are best characterized by contests where pay is based on order of finishing rather than measured output.	The high pay for first prize induces productivity over an executive's career.
Sherwin Rosen	"Prizes and Incentives in Elimination Tournaments," AER, 1986	Near the top of hierarchies, contestants in tournaments may slack off or rest on their laurels.	Concentration of rewards in top ranks of executive hierarchies assures that incentive effect is maintained during final stages of the game.

Table 2.1: Literature Review (continued)

B. Theoretical Treatments: Incentives (continued)

Authors:	*Paper:*	*Model:*	*Conclusions:*
Edward P. Lazear	"Pay Equality and Industrial Politics," JPE, 1989	In context of tournaments, wide wage disparities may be detrimental to output if worker behavior is ubobservable.	Contestants may be able to increase their chances for promotion by engaging in sabotage. Therefore, wage compression leads to more efficient behavior.

C. Theoretical Treatments: Equity and Incentives

Authors:	*Paper:*	*Model:*	*Conclusions:*
William E. Encinosa III, Martin Gaynor, and James B. Rebitzer	"The Sociology of Groups and the Economics of Incentives: Theory and Evidence on Compensation Systems," Mimeo, Nov. 1997.	Economic analysis of pay system incorporating the concept of "group norms," along with use of incentive pay.	"Group norms" effect the pay system used by medical partnerships. Size of practice is positively related to use of incentive pay systems.

D. Empirical Evidence: Surveys

Authors:	*Paper:*	*Model:*	*Conclusions:*
Daniel Kahneman, Jack L. Knetsch, and Richard Thaler	"Fairness as a Constraint on Profit Seeking: Entitlements in the Market," *AER*, 1986	Uses answers to questions administered in a household survey to analyze attitudes regarding "fair behavior" in product and labor markets.	Finds wide consensus for a "dual entitlements" conception of fairness, that takes into account the
David I Levine	"Fairness, Markets, and the Ability to Pay: Evidence from Compensation Executives" *AER*, 1993	Survey of compensation executives.	Compensation executives place more emphasis on maintaining the relative wage relationships in a firm than on responding to changes in labor market conditions.

Table 2.1: Literature Review (continued)

E. Empirical Evidence: Production Function and Productivity

Authors:	*Paper:*	*Model:*	*Conclusions:*
David I. Levine	"Can Wage Increases Pay for Themselves? Tests with a Production Function," *The Economic Journal*, 1992	Information about the pay of employees in manufacturing plants relative to the pay levels of similar workers in competing firms.	Finds that relatively high wages are associated with more productive employees, and that the productivity gain is sufficient to offset higher wages.
J.R. Norsworthy and Craig A. Zabala	"Worker Attitudes and the Cost of Production," *Economic Inquiry*, 1990 "Effects of Worker Attitudes on Production Costs and the Value of Capital Input," *Economic Journal*, 1985	Proprietary information on union-management grievances and other labor-relations information. Industry productivity information.	Decreasing worker morale accounts for declines in productivity and productivity growth.
John W. Straka	"Is Poor Worker Morale Costly to Firms?" *ILRR*, 1993	Uses Norsworthy and Zabala framework, substituting industrial relation information from BLS.	Finds that Norsworthy-Zabala results are not robust and could be a result of the functional form used.
Peter Capelli and Keith Chauvin	"An Interplant Test of the Efficiency Wage Hypothesis," *QJE*, 1991	Uses plant-level wage information from a geographically dispersed multi-plant firm.	Finds a fewer disciplinary dismissals when wages are high relative to average wages in the geographic area of the plant.

F. Empirical Evidence: Executive Pay Dispersion

Authors:	*Paper:*	*Model:*	*Conclusions:*
Brian G. M. Main, Charles A. O'Reilly III, and James Wade	"Top Executive Pay: Tournament or Teamwork," *JOLE*, 1993	Compensation information for executives in a panel of large firms.	Finds limited evidence that higher pay dispersion is deleterious to financial performance in firms more heavily reliant on team production.

Table 2.2: Pay Levels and Pay Growth for CEO's and Average Employees, 1980 - 1991

	Chief Executive Officer	Average Employee
Mean pay levels	878.9	41.1
	(597.7)	(10.6)
Sample size	1002	1103
Mean pay growth	.087	.009
	(.515)	(.085)
Distribution of pay growth:		
10^{th} percentile	-.197	-.074
25^{th} percentile	-.051	-.027
50^{th} percentile	.039	.008
75^{th} percentile	.126	.044
90^{th} percentile	.351	.084
Proportion with positive pay growth	.636	.560
Proportion of firms where:		
CEO pay increases and average pay increases	.358	--
CEO pay increases and average pay decreases	.278	--
CEO pay decreases and average pay increases	.202	--
CEO pay decreases and average pay decreases	.162	--
Sample size	717	895

Standard deviations are reported in parentheses. All dollar values have been adjusted to 1991 levels using the consumer price index for urban areas. Levels represent thousands of dollars, and growth measures represent real changes. Sample sizes are smaller in the CEO column because observations for the first, incomplete year of executive tenure are omitted. The minimum (maximum) value for CEO pay is 162.4 (12513.4), while for average employee pay it is 10.95 (98.97). The minimum (maximum) value for CEO pay growth is -.528 (11.272), while for average employee pay it is -.530 (.491).

Table 2.3: Summary Statistics, Means (Standard Deviations)

	Levels			Growth		
	Mean	Min	Max	Mean	Min	Max
Log(*sales/L*)	5.054 (.523)	3.760	6.963	.010 (.082)	-.261	.336
Sample size	767			689		
Log(*value added/L*)	4.476 (.413)	3.445	5.784	.009 (.086)	-.299	.250
Log(*K/L*)	4.022 (.892)	1.793	6.527	.036 (.091)	-.228	.359
Log(*L*)	3.095 (1.284)	-.127	6.776	-.011 (.085)	-.458	.243
Log(*K/L*)×log(*K/L*)	16.973 (7.775)	3.216	42.599	.293 (.745)	-2.623	2.918
Log(*L*)×log(*L*)	11.226 (8.219)	.000	45.918	-.090 (.558)	-3.848	1.970
Log(*K/L*)×log(*L*)	12.381 (5.737)	-.604	30.080	.077 (.379)	-1.245	1.996
Log(*CEO/LAB*)	2.999 (.553)	1.403	4.778	.035 (.237)	-.788	1.636
Industry pay variables:						
Log(MED_{wc})	2.860 (.109)	2.520	3.064	-.004 (.046)	-.261	.238
Log(COV_{wc})	-.965 (.142)	-1.293	-.568	-.003 (.135)	-.525	.321
Log(MED_{bc})	2.468 (.185)	1.760	2.873	-.010 (.029)	-.107	.089
Log(COV_{bc})	-1.012 (.123)	-1.135	-.676	.008 (.097)	-.487	.252
Log(MED_{pc})	2.376 (.131)	2.012	2.645	-.002 (.069)	-.266	.289
Log(COV_{pc})	-.988 (.133)	-1.279	-.530	.002 (.169)	-.550	.450
Sample size	743			674		

Table 2.4: The Effect of Pay Differences on Output

	Log(*value added/L*)			Log(*sale/L*)		
	OLS	FE	Dif	OLS	FE	Dif
Log(*CEO/LAB*)	-.049	.037	.037	-.041	.038	.034
	(.028)	(.025)	(.017)	(.030)	(.023)	(.014)
Log(COV_{exec})	.065	.015	-.029	.029	.014	-.013
	(.032)	(.029)	(.017)	(.035)	(.028)	(.015)
Log(*K/L*)	.223	.112	.397	.217	.288	.191
	(.165)	(.230)	(.222)	(.155)	(.192)	(.198)
Log(*L*)	-.178	-.308	-.216	-.249	-.478	-.396
	(.143)	(.107)	(.150)	(.048)	(.100)	(.136)
Log(*K/L*)×Log(*K/L*)	-.006	.002	-.040	.008	-.031	-.028
	(.020)	(.026)	(.024)	(.019)	(.021)	(.022)
Log(*L*)×Log(*L*)	.007	-.016	-.005	.019	-.021	-.004
	(.003)	(.011)	(.016)	(.005)	(.011)	(.014)
Log(*K/L*)×Log(*L*)	.030	.057	-.011	.035	.091	.028
	(.090)	(.022)	(.025)	(.009)	(.018)	(.023)
Sample size	738	738	669	762	762	684

Robust standard errors are reported in parentheses. OLS and difference estimates also include industry dummy variables and all specifications include year effects, and industry-level pay variables.

Table 2.5: The Effect of Pay Differences on Output, Alternative Lag Structures

		Log(*value added/L*)			Log(*sale/L*)		
		OLS	FE	Dif	OLS	FE	Dif
A.	Log(*CEO/LAB*)$_{t-1}$	-.033	.015	.033	-.058	.005	.015
		(.029)	(.024)	(.016)	(.032)	(.023)	(.014)
	Sample size	603	603	552	640	640	584
B.	Log(*CEO/LAB*)$_{t-2}$	-.041	-.044	-.025	-.076	-.028	-.007
		(.027)	(.029)	(.013)	(.028)	(.021)	(.014)
	Sample size	533	533	448	588	588	488
C.	Log(*CEO/LAB*)$_{t}$	.021	-.014	.055	.055	.032	.051
		(.055)	(.042)	(.024)	(.058)	(.033)	(.021)
	Log(*CEO/LAB*)$_{t-1}$	.055	.014	.055	.016	-.014	.036
		(.059)	(.029)	(.023)	(.064)	(.027)	(.017)
	Log(*CEO/LAB*)$_{t-2}$	-.115	-.022	-.008	-.140	-.016	.007
		(.047)	(.027)	(.020)	(.055)	(.024)	(.016)
	Sample size	378	378	341	384	384	347

Robust standard errors are reported in parentheses. Estimates are from augmented translog production functions.

Table 2.6: The Effect of Pay Differences on Output, Instrumental Variables

	Value Added			Sales		
	FE	FE:IV_1	FE:IV_2	FE	FE:IV_1	FE:IV_2
Log(*CEO/LAB*)	.039	.120	-.195	.038	.505	-.055
	(.026)	(.301)	(.153)	(.024)	(.375)	(.126)
Log(COV_{exec})	.014	-.027	.130	.010	-.222	.057
	(.029)	(.152)	(.080)	(.028)	(.189)	(.066)
Log(*K/L*)	.137	.212	-.081	.287	.729	.199
	(.242)	(.387)	(.280)	(.195)	(.446)	(.228)
Log(*L*)	-.391	-.330	-.180	-.470	-.677	-.429
	(.109)	(.183)	(.151)	(.099)	(.221)	(.120)
Log(*K/L*)×Log(*K/L*)	.004	-.003	.024	-.028	-.069	-.020
	(.027)	(.034)	(.029)	(.023)	(.037)	(.024)
Log(*L*)×Log(*L*)	-.014	-.012	-.020	-.020	-.013	-.022
	(.012)	(.012)	(.014)	(.011)	(.017)	(.011)
Log(*K/L*)×Log(*L*)	.051	.050	.052	.090	.087	.091
	(.023)	(.023)	(.024)	(.018)	(.023)	(.019)

Sample size for value added is 709 and for sales is 734. Robust standard errors are reported in parentheses. A continuous measure of board ownership is used as an instrument in columns IV_1, while categorical measures are used in IV_2.

Table 2.7: The Effects of Pay Differences on Output, Difference Estimates Using 2-Year Lagged Relative Pay Developments

	Log(*value added/L*)	Log(*sales/L*)
CEO pay increased & ave pay decreased	-.025 (.013)	-.000 (.010)
CEO pay decreased & ave pay increased	-.028 (.013)	-.016 (.011)
Both CEO pay & ave pay increased	-.013 (.012)	.002 (.010)
Sample size	448	488

Robust standard errors are reported in parentheses. Relative pay developments are between years t-2 and t-3.

APPENDIX A

This appendix presents the results from the estimation of alternative specifications of production functions. It does so by estimating and comparing the results from three different functional forms: Cobb-Douglas, constant elasticity of substitution (CES), and translog production functions.[68] The purpose of this appendix is threefold: first, it briefly reviews the literature on production functions and some of the issues in productivity analysis; second, it serves as an exploratory exercise to determine if any of the three functional forms introduced above fits the data best; and third, it checks for the robustness of the results presented in the text, to determine if the results are sensitive to the functional form used.

The estimation of production functions stems from the theory of production in neoclassical economics. The production function illustrates the technical relationship of how inputs are transformed into outputs:

$$Q = \mathrm{f}(K, L), \tag{A1}$$

where Q is output, K is capital stock, L is effective hours of labor input (measured here as the total number of employees). It is assumed that $f_i>0$ and $f_{ii}<0$ for $i=K,L$. In terms of the familiar Cobb-Douglas production function, equation (A1) can be expressed,

$$Q = A\, K^{\theta} L^{\eta}, \tag{A2}$$

where A represents a scale technology parameter. Dividing through by labor gives,

$$Q/L = A\, (K/L)^{\theta} L^{\eta+\theta-1}\,. \tag{A3}$$

Taking the log of both sides and adding an error term gives a convenient estimating equation:

[68] Caves and Barton (1990) present the specific forms used here. Bairam (1994) presents

$$ln(Q/L) = \alpha_0 + \alpha_1\, ln(K/L) + \alpha_2\, ln(L) + \zeta, \qquad \text{(A3a)}$$

where $\alpha_0 = \log A$, $\alpha_1 = \theta$, and $\alpha_2 = \eta+\theta-1$.

A variety of other possible specifications for production functions have been explored in the literature. One such equation is a Taylor expansion of a CES equation:

$$ln(Q/L) = \beta_0 + \beta_1\, ln(K/L) + \beta_2\, ln(L) + \beta_3\, ln(K/L)^2 + \varepsilon, \qquad \text{(A3b)}$$

A third possible form is the translog production function:

$$ln(Q/L) = \gamma_0 + \gamma_1\, ln(K/L) + \gamma_2\, ln(L) + \gamma_3\, ln(K/L)^2 +$$

$$\gamma_4\, ln(L)^2 + \gamma_5\, ln(K/L)\times \ln(L) + \psi, \qquad \text{(A3c)}$$

The translog form is a "consistent second order approximation to any production function that takes the form $lnQ = F(lnK, lnL)$ (Caves and Barton, 1990, p.21)."

CES and translog functions are more flexible representations of the production process in that they relax assumptions implicit in the Cobb-Douglas form about the homogeneity of production and the elasticity of substitution between inputs. Tests of the parameters on second order inputs in (A3b) or (A3c) indicate whether Cobb-Douglas is a sufficient specification of the production relationship.[69]

A noteworthy source of bias associated with the estimations of production functions stems from the likelihood that firms simultaneously choose capital and labor with their levels of output. Caves and Barton note that the estimation of single-equation production functions implies the quite strong assumption that capital and labor inputs are chosen, *ex ante,* and are uncorrelated with the error:

> the production function is made stochastic with a disturbance representing purely random elements, on the assumption that the entrepreneur cannot know the effect

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a survey of various production functions.

[69] See Greene (1990), Christensen, Jorgenson, and Lau (1973), and Bairam (1994) on how to recover the elasticity estimates from regression parameters.

of the disturbance until after the quantities of the inputs have been preselected. The procedure further assumes that the prices of output and inputs either are known with certainty or are statistically independent of the production function disturbance and that entrepreneurs maximize the mathematical expectation of profit (p.25).

A possibly richer estimation of the effects of increasing inequality that would circumvent the above assumption includes estimating a system of factor cost equations which relate the cost of inputs to their share of production (Caves and Barton 1990, and Norsworthy and Zabala 1990). Such an exercise is not carried out here due in part to the implied endogeneity of wages in the model tested here. Economic theory assumes that firms are price takers both in product and input markets. This assumption leads to duality between profit maximization and cost minimization for the firm, and it provides the rationale for cost equation estimation. The implied endogeneity of wages complicates the use of such a procedure here.[70]

On this point, Bairam (1994) argues that the cost minimization assumption may be dubious for such reasons, so that the cost equation methods of estimating technical efficiency in production are also suspect. He argues, consequently, that estimation of output equations is superior to cost equation estimation. The relative validity of either argument is left unresolved here, except insofar as this exercise is consistent with the latter.

As is common among analyses of firm production, the specifications here are augmented with other variables. Specifically, all specifications include year and 2 digit industry controls, and table A.3, as in the text, includes relative compensation variables.

Before turning to the results in this section, a final note on the use of "augmented"

[70] See Agell and Lundberg (1992) for an analysis of general equilibrium conditions when firms face "fair wage-effort" constraints.

production functions is in order. The estimating framework implies that widening pay gaps affect production in a "factor neutral" manner. That is, the functional form will not help determine if wage inequality affects output through "indirect" avenues, such as by shifting the relative use of inputs. To illustrate this, consider the following model,

$$Y = f(K, L_1(w_1, w_2), L_2(w_2)), \tag{A4}$$

where K is capital, L_1 can be thought of as effective effort for low-wage labor, L_2 as effort for high-wage labor, w_1 is the wage for L_1, and w_2 is the wage for L_2.[71] Changes in relative wages may impact the technical efficiency of producing Q both through its direct impact and through its effects on the use of the other inputs, such as the relative use of L_1 and L_2. Hamermesh, for example, argues that the demand for high wage workers can increase with increases in w_2 if low wage workers retract effort sufficiently in response to their lower relative wage.[72] The effects of relative wage changes on factor use or substitution elasticities are not explored in this analysis. That is, by including them as additive terms to the production function, the interactions are implicitly set to zero.[73]

Table A.1 presents results from regressing output on the various specifications for capital and labor. Three procedures were used: OLS, firm fixed effects, and first difference. The first three columns give results for the Cobb-Douglass specifications, the third through sixth columns give specifications for a constant elasticity of substitution (CES) specification, and the last three columns presents results from translog regressions.

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[71] This model is essentially the same as Hamermesh (1975). See table 1 for examples of production functions which reflect the model outlined here.

[72] This is because the change in effort level changes the slope of the marginal rate of substitution between high and low wage workers. Hamermesh's model, however, assumes that firms are wage takers. While the discussion is instructive, this assumption, as discussed in the text, is not likely to be tenable.

[73] See Norsworthy and Zabala (1982, 1985, 1990) for similar analysis that specifies production such that the interactive effects are identified.

Other control variables in table A.1 include year and 2-digit industry fixed effects. Sample sizes in these regressions are much larger than those presented in the text because of the substantial portion of missing values in the compensation variables, especially for the data available from Compustat. Similar tables are presented below for the text sample.

Results reject the Cobb-Douglas production function in most cases. F-statistics for the translog production function relative to the Cobb-Douglas imply rejection of the hypothesis that $\gamma_3 = \gamma_4 = \gamma_5 = 0$ in 4 of 6 cases (2 of 3 cases for value added and 2 of 3 for sales) For OLS regressions, the F-statistic exceeds 7 for both output measures. For fixed effects estimates, the F-statistic is above 4. In all cases, p values are less than .01. For difference estimates, the F-statistics do not reject the Cobb-Douglas specification in any case. Similarly the F-statistics imply that the translog function is preferred to the CES function in 4 of 6 cases. CES is only preferred to Cobb-Douglass in one case.

Table A.2 gives results that correspond to the sample used in the text but that exclude the all compensation variables. The point estimates are similar to the results in table A.1, as is the preference for tranlsog functions. In this case, both Cobb-Douglas and CES functions are rejected in favor of the translog specification in the same 4 of 6 cases. F-statistics for difference equations are moderately larger, though they still do not imply rejection of the Cobb-Douglas specification.

Given the generally positive results for the translog function, it will be used for regressions presented in the text. The regressions do tend to exhibit inordinately strong decreasing returns to scale, however, with the estimates for fixed effects and differenced regressions being very large. Point estimates on the first-differenced translog equations compare favorably to those presented in Kruse (1993).

Table A.3 gives estimates that include the relative compensation variable. It is clear that estimates on relative pay are not sensitive to which specification is used. Neither is there any indication of omitted variable bias attributable to the compensation variable in estimates for the capital and labor variables.

Table A.1, Production Function Estimates: Full Sample

	Cobb-Douglas			CES			Translog		
	OLS	FE	Dif	OLS	FE	Dif	OLS	FE	Dif
A.) Log(*value added/L*)									
Log(*K/L*)	.281	.326	.071	.293	.515	.150	.139	.293	.123
	(.015)	(.027)	(.029)	(.105)	(.112)	(.134)	(.114)	(.138)	(.178)
Log(*L*)	-.026	-.117	-.230	-.026	-.127	-.232	-.165	-.183	-.294
	(.006)	(.019)	(.031)	(.006)	(.022)	(.031)	(.035)	(.073)	(.124)
Log(*K/L*)×Log(*K/L*)	--	--	--	-.002	-.023	-.010	.006	.010	-.009
				(.013)	(.013)	(.017)	(.013)	(.014)	(.019)
Log(*L*)×Log(*L*)	--	--	--	--	--	--	.003	-.015	.007
							(.003)	(.008)	(.013)
Log(*K/L*)×Log(*L*)	--	--	--	--	--	--	.030	.037	.006
							(.007)	(.014)	(.021)
Sample size	1345	1345	1345	1345	1345	1345	1345	1345	1345
B.) Log(*sales/L*)									
Log(*K/L*)	.366	.282	.113	.283	.257	.242	.286	.238	.248
	(.011)	(.015)	(.017)	(.046)	(.051)	(.060)	(.046)	(.055)	(.064)
Log(*L*)	-.022	-.099	-.229	-.021	-.098	-.229	-.083	-.138	-.213
	(.004)	(.015)	(.019)	(.004)	(.015)	(.019)	(.017)	(.042)	(.050)
Log(*K/L*)×Log(*K/L*)	--	--	--	.011	.004	-.018	.008	-.003	-.020
				(.006)	(.006)	(.008)	(.006)	(.007)	(.008)
Log(*L*)×Log(*L*)	--	--	--	--	--	--	.008	-.017	-.005
							(.002)	(.005)	(.006)
Log(*K/L*)×Log(*L*)	--	--	--	--	--	--	.008	.029	.001
							(.005)	(.008)	(.010)
Sample size	4063	4063	4063	4063	4063	4063	4063	4063	4063

Table A.2: Production Function Estimates, Text Sample without Relative Compensation Variables

	Cobb-Douglas			CES			Translog		
	OLS	FE	Dif	OLS	FE	Dif	OLS	FE	Dif
A.) Log(*value added/L*)									
Log(*K/L*)	.281	.286	.065	.406	.360	.353	.251	.073	.403
	(.021)	(.053)	(.040)	(.153)	(.290)	(.185)	(.162)	(.235)	(.225)
Log(*L*)	-.028	-.165	-.267	-.029	-.168	-.278	-.193	-.345	-.200
	(.007)	(.037)	(.047)	(.007)	(.042)	(.048)	(.044)	(.108)	(.151)
Log(*K/L*)×Log(*K/L*)	--	--	--	-.016	-.009	-.036	-.008	.003	-.039
				(.019)	(.025)	(.024)	(.020)	(.026)	(.025)
Log(*L*)×Log(*L*)	--	--	--	--	--	--	.007	-.012	-.007
							(.003)	(.011)	(.016)
Log(*K/L*)×Log(*L*)	--	--	--	--	--	--	.031	.064	-.009
							(.009)	(.022)	(.026)
Sample size	743	743	674	743	743	674	743	743	674
B.) Log(*sales/L*)									
Log(*K/L*)	.404	.286	.062	.425	.750	.347	.241	.329	.217
	(.020)	(.043)	(.038)	(.150)	(.189)	(.174)	(.154)	(.197)	(.202)
Log(*L*)	-.008	-.215	-.293	-.008	-.239	-.305	-.265	-.483	-.370
	(.009)	(.034)	(.044)	(.009)	(.035)	(.045)	(.049)	(.099)	(.137)
Log(*K/L*)×Log(*K/L*)	--	--	--	-.003	-.056	-.036	.006	-.039	-.029
				(.019)	(.023)	(.022)	(.019)	(.022)	(.023)
Log(*L*)×Log(*L*)	--	--	--	--	--	--	.020	-.019	-.005
							(.004)	(.011)	(.015)
Log(*K/L*)×Log(*L*)	--	--	--	--	--	--	.035	.093	.025
							(.010)	(.018)	(.024)
Sample size	767	767	689	767	767	689	767	767	689

Table A3: Production Function Estimates, Text Sample with Relative Compensation Variables

	Cobb-Douglas			CES			Translog		
	OLS	FE	Dif	OLS	FE	Dif	OLS	FE	Dif
A.) Log(*value added/L*)									
Log(*K/L*)	.274	.303	.053	.379	.383	.347	.225	.123	.387
	(.022)	(.054)	(.039)	(.153)	(.213)	(.180)	(.162)	(.231)	(.222)
Log(*L*)	-.023	-.191	-.266	-.024	-.195	-.277	-.182	-.325	-.215
	(.010)	(.035)	(.047)	(.009)	(.040)	(.047)	(.043)	(.105)	(.150)
Log(*K/L*)×Log(*K/L*)	--	--	--	-.013	-.010	-.037	-.006	.001	-.039
				(.020)	(.025)	(.023)	(.020)	(.026)	(.024)
Log(*L*)×Log(*L*)	--	--	--	--	--	--	.006	-.015	-.006
							(.003)	(.011)	(.016)
Log(*K/L*)×Log(*L*)	--	--	--	--	--	--	.031	.059	-.007
							(.009)	(.021)	(.025)
Log(*CEO/LAB*)	-.023	.051	.030	-.021	.052	.030	-.025	.047	.030
	(.026)	(.021)	(.015)	(.025)	(.021)	(.015)	(.025)	(.020)	(.015)
Industry pay variables									
Log(MED_{wc})	.429	.236	.102	.425	.232	.102	.374	.221	.102
	(.240)	(.146)	(.080)	(.240)	(.145)	(.080)	(.238)	(.142)	(.080)
Log(COV_{wc})	.253	.185	.036	.249	.180	.036	.224	.155	.036
	(.158)	(.081)	(.029)	(.158)	(.082)	(.029)	(.161)	(.082)	(.029)
Log(MED_{bc})	.213	-.005	-.024	.202	-.002	-.009	.156	-.121	-.008
	(.281)	(.188)	(.116)	(.282)	(.187)	(.117)	(.281)	(.188)	(.117)
Log(COV_{bc})	-.001	.114	.068	.005	.121	.069	-.012	.105	.069
	(.141)	(.084)	(.030)	(.145)	(.084)	(.030)	(.142)	(.082)	(.030)
Log(MED_{pc})	.097	.051	.107	.097	.055	.110	.139	.070	.109
	(.180)	(.116)	(.050)	(.182)	(.118)	(.050)	(.180)	(.114)	(.056)
Log(COV_{pc})	.160	.129	-.034	.159	.127	-.032	.158	.130	-.032
	(.076)	(.042)	(.018)	(.076)	(.043)	(.017)	(.077)	(.044)	(.018)
Sample size	743	743	674	743	743	674	743	743	674

Table A.3: Production Function Estimates, Text Sample with Relative Compensation Variables (continued)

	Cobb-Douglas			CES			Translog		
	OLS	FE	Dif	OLS	FE	Dif	OLS	FE	Dif
B.) Log(*sales/L*)									
Log(*K/L*)	.397	.304	.057	.399	.721	.336	.215	.296	.180
	(.020)	(.044)	(.036)	(.151)	(.190)	(.170)	(.153)	(.192)	(.197)
Log(*L*)	-.001	-.244	-.294	-.001	-.266	-.305	-.254	-.491	-.397
	(.011)	(.034)	(.044)	(.011)	(.035)	(.045)	(.049)	(.097)	(.136)
Log(*K/L*)×Log(*K/L*)	--	--	--	-.0003	-.050	-.036	.009	-.032	-.027
				(.019)	(.023)	(.022)	(.019)	(.021)	(.022)
Log(*L*)×Log(*L*)	--	--	--	--	--	--	.019	-.020	-.004
							(.005)	(.011)	(.014)
Log(*K/L*)×Log(*L*)	--	--	--	--	--	--	.035	.093	.030
							(.010)	(.017)	(.023)
Log(*CEO/LAB*)	-.029	.050	.032	-.029	.055	.032	-.031	.046	.032
	(.028)	(.021)	(.013)	(.027)	(.020)	(.013)	(.027)	(.019)	(.013)
Industry pay variables									
Log(MED_{wc})	.473	.091	.038	.473	.081	.042	.403	.046	.043
	(.297)	(.140)	(.081)	(.298)	(.138)	(.081)	(.296)	(.132)	(.081)
Log(COV_{wc})	.220	.168	.015	.220	.145	.014	.179	.110	.015
	(.146)	(.067)	(.024)	(.146)	(.068)	(.024)	(.143)	(.065)	(.023)
Log(MED_{bc})	-.069	.160	-.177	-.069	.171	-.166	-127	-.001	-.170
	(.327)	(.171)	(.101)	(.328)	(.167)	(.101)	(.325)	(.169)	(.102)
Log(COV_{bc})	-.251	-.174	.011	-.251	-.143	.011	-.249	-.170	.008
	(.151)	(.087)	(.028)	(.152)	(.085)	(.028)	(.149)	(.081)	(.028)
Log(MED_{pc})	-.208	-.110	.102	-.208	-.093	.104	-.139	-.067	.107
	(.212)	(.114)	(.048)	(.213)	(.111)	(.047)	(.202)	(.104)	(.047)
Log(COV_{pc})	.085	.103	-.033	.085	.094	-.030	.072	.095	-.031
	(.081)	(.038)	(.015)	(.084)	(.038)	(.015)	(.081)	(.037)	(.015)
Sample size	767	767	689	767	767	689	767	767	689