DYNAMICS IN EXECUTIVE LABOR MARKETS:
CEO EFFECTS, EXECUTIVE-FIRM MATCHING, AND RENT SHARING

DISSERTATION

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* * * * *

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ABSTRACT

Previous empirical efforts to examine the link between CEOs and firm performance using variance decomposition suffer from methodological problems that systematically understate the relative impact of CEOs on firm performance compared to industry and firm effects. The percentage of the variance in firm performance explained by heterogeneity in CEOs is re-examined with methodological refinements addressing this prior literature. The re-estimated “CEO effect” on corporate-parent performance is found to be substantially more important than that of industry and firm effects, but only moderately more important than industry and firm effects on business-segment performance.

Despite the importance of CEOs in influencing firm performance, much debate surrounds the observed heterogeneity in executive compensation practices across firms and industries. Two broad explanations of this heterogeneity are explored: differences between firms—“where you work” and differences between executives—“who you are”. Results suggest that “where you work” is more important than “who you are” in determining executive compensation differentials. Why some firms would systematically pay above market wages while other firms would systemically pay below market wages is furthered explored. Both labor market “sorting” and rent-sharing are found to account for firm effects on wage differentials among executives.
Dedicated to my husband, Tyson Brighton Mackey,

and to my daughter, Brooke Nicole Mackey.
Completing this dissertation has made me acutely aware of how much I need others as well as how much my friends, family, and academic advisors want me to succeed. I wish to thank my husband, Tyson Mackey, for intellectual support as well as emotional encouragement as we both labored away at our respective dissertations. And, I thank my daughter, Brooke Mackey, for bringing a joy into my life that has kept my spirits up even in the darkest times of this dissertation.

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CHAPTER 1

INTRODUCTION

The extent to which CEOs influence firm performance is fundamental to scholarly understanding of how organizations work; yet, this linkage is poorly understood. Previous empirical efforts to examine the link between CEOs and firm performance using variance decomposition, suffer from methodological limitations which systematically understate the relative impact of CEOs on firm performance compared to industry and firm effects. In the second chapter of this dissertation, these methodological problems are identified and corrected and then the percentage of the variance in firm performance explained by heterogeneity in CEOs is re-examined. When these refinements are made, the “CEO effect” on corporate-parent performance is substantially more important than that of industry and firm effects, but only moderately more important than industry and firm effects on business-segment performance.

Despite the importance of executives in influencing firm outcomes, executive labor markets are poorly understood in organizational studies. For example, much debate surrounds the observed heterogeneity in executive compensation practices across firms and industries. Chapter 3 explores two broad explanations of this heterogeneity in executive compensation. First, this heterogeneity might be explained by differences between firms—“where you work”. Second, this heterogeneity might be explained by differences between
executives—“who you are” (intangible human capital). This question is also explored for various groups of executives (e.g. founders versus non-founders, CEOs versus non-CEOs, males versus females, MBAs versus non-MBAs).

A matched-longitudinal firm-executive dataset is used in Chapter 2, combining information on over 1110 executives from S&P 1500 firms with measures for compensation, career and educational background, and biographical information. “Where you work” is found to be more important than “who you are” in determining executive compensation. Implications for this result are explored.

Since executive wage differentials are substantially impacted by firm differences, it is important to understand why some firms would systematically pay above market wages while other firms would systemically pay below market wages. Chapter 4 explores labor market “sorting” as one possible explanation to account for firm effects on wage differentials. Assignment or ‘matching’ models have been used by economists to describe how workers and firms “sort” or “match up” in labor markets and, consequently, how wages are determined (Koopmans and Beckmann, 1957; Shapley and Shubik, 1972; Becker, 1973; Shimer, 2001). Exploring whether or not the predictions of these models hold in executive labor markets provides the opportunity to answer some fundamental questions about how executives and firms “match up” in the executive labor market.

For example, do highly talented executives work in firms that are performing well or in firms that are performing poorly? Where do executives realize their “highest and best use”? On the other side of the market, if a firm is performing well, are they likely to want to attract a high quality—but probably expensive—executive or might they actually prefer a less talented executive? Conversely, are firms with low levels of performance likely to prefer
paying high premiums in effort to attract high quality executives, in hopes of improving firm performance (Leonard, 1990)? Will these high premiums induce executives to join firms with poor prospects?

Answering these questions and others about how executives and firms “match up” in labor markets requires disentangling the effects of decisions made by firms and decisions made by individuals in the labor market. Chapter 4 provides empirical insight into the dynamics of executive-firm matching in labor markets—answering whether or not compensation differentials arising from firm differences can be attributed to sorting in executive labor markets.

Rent-sharing between executives and shareholders is explored in Chapter 5 as another possible explanation for firm effects on compensation differentials. Additionally, the question is asked whether or not the practice of rent-sharing creates value for corporations. Addressing this latter question provides insight into recent discussions regarding whether some executives are “overpaid”. Insights from how executives and firms “match-up” in the labor market are used to study how rent is split between executives and firms. That is, how is the surplus generated from synergies in the executive-firm match divided between executives and corporations? Recent theory in strategic management suggests that powerful organizational members with critical knowledge may be more able to appropriate rent from shareholders. Executives are likely to be such organizational members with key knowledge about firm resources and capabilities. Yet, this rent sharing may be beneficial to firms if it serves maximize the value of the firm. Using a compensation bargaining model, rent sharing is related to the total quasi-rent of the firm. Results suggest that for the large majority of firms rent sharing by executives serves to maximize the value of the firm.
CHAPTER 2

HOW MUCH DO CEOS INFLUENCE FIRM PERFORMANCE—REALLY?

Do CEOs have an impact on firm performance? This question has captured the attention of organizational scholars, business practitioners, and government policy makers for well over a century (Bass, 1990; Yukl, 2002). On the one hand, some theorists (Barnard, 1938) and many practitioners (Drucker 1954; Collins, 2001) have argued that leadership—especially in a firm’s senior positions—has an important impact on firm performance and survival. For example, Barnard (1938) argued that top leaders formulate a collective purpose that morally binds participants in an organization; Selznick (1957) described how top leaders infuse an organization with values; Schein (1992) argued that top leaders help create an organization’s culture. More recently, Tichy and Cohen (1997) argued for the crucial role of top leaders in deciding an organization’s course of action—especially in the face of technical and environmental change (Woodward, 1965; Lawrence and Lorsch, 1967; Thompson, 1967). All these effects of leadership are thought to be leveraged throughout an organization (Rosen, 1990), resulting in a substantial impact on a firm’s performance.

On the other hand, empirical work is generally inconsistent with these expectations (e.g. Lieberson and O’Connor, 1972). Research on the percentage of variance in firm performance explained by a firm’s CEO ranges from a low of 3.9% (Thomas, 1988) to a high of 14.7% (Wasserman, Nohria, and Anand, 2001). The size of this “CEO effect” is
much smaller than the impact of industry and other firm attributes on a firm’s performance. Other scholars have also documented this relatively modest impact that leaders have on organizational performance in a variety of empirical settings (Bass, 1990; Hambrick and Finkelstein, 1996).

At least four explanations for the limited effect of CEOs on firm performance have been suggested in the literature. First, population ecologists and institutional theorists argue that managerial influence on firm outcomes is limited by environmental, organizational, and legitimacy constraints, which restrict executive choice (e.g., Hannan and Freeman, 1977; Pfeffer and Salancik, 1978; DiMaggio and Powell, 1983). Without choice, CEOs can do little to influence firm outcomes (Hambrick & Finkelstein, 1987).

Second, some organizational theorists have shown that, as a group, CEOs are homogeneous with respect to personal characteristics, socialization, and training (March and March, 1977; Whitehill, 1991). According to this view, since CEOs are more or less “interchangeable” in their positions, it is unlikely that CEOs, on average, would have a significant impact on firm performance.

Third, another group of scholars suggest that CEOs play more of a symbolic than substantive role in organizations (Pfeffer, 1981). In this view, performance outcomes are attributed to CEOs as a way to make sense out of complex organizational outcomes (Calder, 1977; Pfeffer, 1977; Meindl, Ehrlich, and Dukerich, 1985; Meindl and Ehrlich, 1987).

Finally, another group of scholars has suggested that the CEO is not the right unit of analysis for understanding the managerial determinants of firm performance and should be replaced by the top management team as the unit of analysis (Hambrick and Mason, 1984; Murray, 1989; Halebian and Finkelstein, 1993). These scholars argue that even though the
CEO is part of the larger top management team at a firm and can play a role in creating and directing this team, dynamic relations among members of the team exert a much greater influence on firm performance than the CEO as an individual.

This paper proposes a fifth possible explanation of the limited impact of CEOs on firm performance: The methodology used in the previous research has limitations that actually make it difficult to test the impact of CEOs on firm performance. It turns out that methodological limitations of the previous research will systematically understate the relative impact of CEOs on firm performance compared to industry and firm factors. Thus, the purpose of this paper is to propose methodological refinements to the prior empirical work on executive leadership and then demonstrate that the CEO effect on firm performance, when estimated with these refinements, is substantially larger than the size of industry and firm effects. This demonstration of the influence of leaders on firm outcomes has important theoretical implications for how scholars think about organizations as outlined in the concluding sections of the paper.

2.1 Variance Decomposition of CEO Effects

Perhaps the most influential work on the impact of CEOs on firm performance applies variance decomposition methods, which methods enable researchers to estimate the extent to which a given variable influences the variance in performance across firms. The first of these papers was published in 1972 (Lieberson and O’Conner, 1972). This paper has been replicated and extended several times over the years (e.g. Weiner, 1978; Thomas, 1988; Wasserman, Anand, and Nohria, 2001). The strengths and weaknesses of each of these papers are discussed below.
2.1.1 Lieberson and O’Connor (1972)

Sociologists Stanley Lieberson and James O’Connor (1972) conducted the first empirical study of the relationship between CEOs and firm performance using variance decomposition. This study was based on a sample of 167 firms in 13 different industries over a 20 year time period (1946-1965) and utilized sales, earnings, and profit margins as performance metrics. Using sequential ANOVA, Lieberson and O’Connor’s central conclusion is that industry and firm effects are far more important than leadership effects. Specifically, the incremental impact of leadership on firm performance ranged from 6.5% when sales was used as the dependent variable to 14.5% when profit margin was used as the performance proxy, while firm effects ranged from 22.6% to 67.7% and industry effects ranged from 18.6% to 28.5%. These results, as well as the results of other studies reviewed in this section, are summarized in Table 1.

2.1.2 Early Critiques of Lieberson and O’Connor (1972)

Lieberson and O’Connor’s study generated intense criticism from organizational scholars (e.g. Hambrick and Mason, 1984; Romanelli and Tushman, 1988) and multiple empirical critiques and replications (Weiner, 1978; Weiner and Mahoney, 1981; Thomas, 1988; Wasserman, Nohria, and Anand, 2001). Weiner (1978) published the first follow-up study disputing Lieberson and O’Connor’s (1972) finding that top leaders exert minimal influence on firm performance. Using 193 manufacturing firms, Weiner (1978) replicates Lieberson and O’Connor’s (1972) results using sequential ANOVA but finds radically different results by reversing the sequence of the decomposition (e.g. decomposing CEO
effects before industry or firm effects). Weiner (1978) concludes that Lieberson and O’Connor’s (1972) findings are simply statistical artifacts of the variance decomposition method and no theoretical implications can be drawn from their work.

2.1.3 Recent Critiques of Lieberson and O’Connor (1972)

More recently, Thomas (1988) studied the CEO-performance relationship in twelve firms in one industry in the United Kingdom. Unable to estimate an industry effect, Thomas (1988) finds that firm effects accounted for 72.7% to 89.6% of the variance in firm performance while CEO effects accounted for 3.9 to 7% of the variance in firm performance. Thomas (1988) interprets these results as strong support for the importance of leadership in determining firm performance because of the substantial amount of unexplained variance explained by the CEO effect after the year and firm effects are decomposed.

More recently Wasserman, Nohria, and Anand (2001) replicated previous empirical efforts as well as examined the contingencies under which leaders might matter. Using a sample of 531 companies across 42 industries with sequential ANOVA techniques, they find that leader influences account for 14.7% of the variance in firm profitability when return on assets is the dependent variable (13.5% when Tobin’s q is used as the performance measure)—still, relatively less explanatory power than industry and company effects.
2.2 Limitations of Previous Empirical Studies

While each of these papers is suggestive as well as significant to our evolving understanding of leadership within organizations, there are important methodological limitations in these papers that make it difficult to interpret their findings. In particular, these limitations have the effect of systematically reducing the reported level of the CEO effect on firm performance. The major limitations of each of these studies are summarized in Table 2.

While the methodology of some of the previous research (e.g. Lieberson and O’Connor, 1972) has been criticized at length (e.g. Hambrick and Mason, 1984; Pfeffer and Davis-Blake, 1986; Day and Lord, 1988; Romanelli and Tushman, 1988), the flaws identified in this paper are common not only to the early studies in this literature (e.g. Lieberson and O’Connor, 1972) but also to subsequent replications and extensions (Weiner, 1978; Thomas, 1988; Wasserman, Nohria, and Anand, 2001).

2.2.1 Perfectly Nested Samples

Prior variance decomposition studies of CEO effects use samples in which executives are perfectly nested within industry and corporate effects—that is, no individual in the dataset was CEO for more than one corporation or worked in more than one industry (See Figure 1 for an illustration of perfectly nested samples). When a variable such as the CEO effect is perfectly nested within another variable (e.g. industry or corporate), almost all of the variance in the dependent variable (i.e. firm performance) from leadership influences will be common to the industry or to the corporation. In other words, if none of the leaders
in a dataset ever switches industries (or corporations), only a portion of the impact of the leadership influence on firm performance can be statistically detected since, for example, the corporate effect could be expressed as a linear combination of the CEOs that have worked for the corporation (See Figure 1).

When sequential ANOVA techniques are used for variance decomposition, common variance between correlated variables is attributed to the variable that enters first in the decomposition. In perfectly nested samples, there is more common variance between the leader, firm, and industry—hence, the majority of the variance will be attributed to whichever variable is decomposed first and the significance of latter variables will be understated.

Reversing the order of the decomposition (e.g. leader, firm, then industry) further exacerbates the problems of perfectly nested samples (e.g. Weiner, 1978). Weiner (1978) found leadership influences as large as 96 percent of the variance in firm profitability when decomposed first in perfectly nested samples and as low as 8.7 percent when decomposed last in these samples. Again, in perfectly nested samples, the majority of the variance in the dependent variable cannot be distinguished between variables—thus, all the common variance is attributed to the variable that enters first in the decomposition. Thus, perfectly nested samples flaw the sequential ANOVA technique because results are statistical phenomena based on the order of decomposition.
2.2.2 Sequential ANOVA

Without exception, the previous studies in the leadership effects literature have used sequential (nested) ANOVA techniques (analogous to hierarchical OLS) for the variance decomposition. There are two limitations to the sequential ANOVA technique specific to the context of the leadership effects studies. One limitation in particular is that the sequential ANOVA technique assumes that each effect is independent and thus no covariance between effects is modeled. Thus, for example, since industry and corporate effects have been shown to be highly correlated (McGahan and Porter, 1997), sequential ANOVA methods would be inappropriate. An additional limitation, as stated in the previous section, is that the common variance between correlated variables is attributed to the variable that enters first in the decomposition when sequential ANOVA is used. Thus previous results in the leadership studies should be cautiously interpreted as an artifact of decomposition order.\(^1\)

2.2.3 Firms without Turnover Events

A third limitation of many of these studies also concerns the sample upon which they were based. When a firm does not experience a turnover event during the time period of the sample, the leader and firm effects are indistinguishable. (See Figure 2 for an illustration of how firms without turnover events appear in the data.) Failure to remove firms from the sample in which no turnover event occurs causes more common variance between leader and firm effects. When sequential ANOVA techniques are used, having

\(^1\) Brush and Bromiley (1997) criticize the interpretation of variance estimates created with components of variance (COV) methods (e.g. Schmalensee, 1985; Rumelt, 1991). However, their criticisms do not apply to sequential ANOVA methods applied here.
more common variance will result in attributing more variance to the variable that enters first in the decomposition (i.e. biasing upwards the effect decomposed first).

None of the prior studies in this literature address whether or not firms were excluded if no turnover event occurred. Given the low turnover rate of CEOs during the sample years of the prior empirical work, it is probable that some firms in the sample may have never experienced a succession event. Since these studies used sequential ANOVA techniques, if such firms were included in the sample, then the leader influence was consequently understated in these studies.

2.2.4 Exclusion of Diversified Firms

Previous empirical work is also limited by the exclusion of diversified firms from the analysis (e.g. Lieberson and O’Connor, 1972; Thomas, 1988). CEO effects are likely to be understated in sample without diversified firms as leadership influence is likely to be significant at the corporate level. Both Lieberson and O’Connor (1972) and Thomas (1988) explicitly excluded from their sample firms pursuing corporate-level strategies in which the influence of leadership executive is likely to be profound (i.e. diversification, M&A). This decision likely contributes to the relative insignificance of the CEO effect in these studies.

2.2.5 Level of Analysis

A fifth limitation of the prior empirical work is the choice of the level of analysis. Without exception the chosen level of analysis in the prior work is corporate-parent—that is,
each observation in the sample is a corporation and not, for example, a business-segment. This creates a problem for estimating accurate industry effects when diversified (multi-segment) firms are included in the sample. Three of the prior studies in this literature (Weiner, 1978; Weiner and Mahoney, 1981; Wasserman, Nohria, and Anand, 2001) do not provide any information as to whether or not diversified firms were included in the sample. If diversified firms were included in the sample but the level of analysis was the corporate-parent, the resulting estimates would be less meaningful since industry estimates are inaccurate when one industry categorization is used to represent multiple business-segments operating in many different industries. While inaccurate industry effects do not automatically understate the CEO effect, such a bias in the results may possibly impact the reported CEO effect.

2.3 Methodology

The variance decomposition literature in strategic management has grown rapidly over the last several years (Rumelt, 1991; McGahan and Porter, 1997, 1999, 2002, 2003). Most of this work has focused on the percentage of firm performance explained by industry, corporate, and firm effects (e.g. Schmalensee, 1985; Rumelt, 1991; Roquebert, Phillips, and Westfall, 1996; McGahan and Porter, 1997, 1999, 2002, 2003). On average, the percentage of firm performance explained by industry effects is 14%, by corporate effects is 9%, and by firm/business unit effects is 36.8%. With the exception of those papers cited here (i.e., Lieberson and O'Connor, 1972; Weiner, 1978; Thomas, 1988; Wasserman, Nohria, and Anand, 2001), none of this recent work has examined the percentage of variance in firm
performance explained by CEO effects. Despite this different theoretical focus, methodological conventions and advances in that literature suggest possible solutions to the limitations of the previous studies examining leader effects.

2.3.1 Model

The performance of diversified firms can be measured as corporate-parent or business-segment performance; hence, the extent to which a CEO influences the variance in corporate-parent performance as well as the variance in business-segment performance should be estimated to obtain a more complete picture of the influence of CEOs on firm outcomes. A CEO’s influence is likely to be different on corporate-parent performance than on business-segment performance as the CEO’s role differs accordingly. For example, CEOs impact corporate-parent performance through the choice of the portfolio of businesses the corporation will be in; CEOs impact business-segment performance by the selection of the business-level strategies used in their portfolio of businesses, the management team that will run the business-segments, and the accounting practices for how assets and profits are allocated across segments. CEO influence may not be equivalent on corporate-parent and business-segment performance. This study investigates the CEO effect on both.
Consider the following model of the CEO’s influence on the variance in corporate-parent performance across firms:

\[ R_{i,j,k,t} = \alpha_i + \beta_j + \delta_k + \gamma_t + \epsilon_{i,j,k,t} \quad (2.1) \]

In equation 2.1, \( R_{i,j,k,t} \) is the corporate-parent accounting profit (ROA) in year \( t \) of corporate-parent \( j \)'s business in industry \( i \), which corporate-parent is led by CEO \( k \) industry \( i \) of corporate-parent \( j \)'s business segment led by CEO \( k \). The other variables in the model are \( \alpha_i \), the industry effect; \( \beta_j \), the corporate effect; \( \delta_k \), the CEO effect; \( \gamma_t \), the year effect, and \( \epsilon_{i,j,k,t} \), the residual. Note that the observations in the data are at the business-segment level of analysis (for estimating accurate industry effects), but the dependent variable is corporate-parent performance—that is, each observation is a unique business-segment instead of a corporate-parent but the dependent variable is corporate ROA.

The specification for assessing the CEO’s influence on the variance in business-segment performance across firms is as follows:

\[ r_{i,j,k,t} = \alpha_i + \beta_j + \delta_k + \gamma_t + \phi_{i,j} + \epsilon_{i,j,k,t} \quad (2.2) \]

In equation 2.2, \( r_{i,j,k,t} \) is the business-segment accounting profit (ROA) in year \( t \) of corporate-parent \( j \)'s business in industry \( i \), which corporate-parent is led by CEO \( k \). The other variables in the model are \( \alpha_i \), the industry effect; \( \beta_j \), the corporate effect; \( \delta_k \), the CEO effect; \( \gamma_t \), the year effect; \( \phi_{i,j} \), the segment effect, and \( \epsilon_{i,j,k,t} \), the residual. Although some prior work in variance decomposition in strategic management has specified models with interaction effects (e.g. Rumelt, 1991), other work elects not to do so because of

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2 Note that business-segment effects are excluded from this model since the dependent variable is corporate-parent performance. We would not expect business-segments to significantly impact the variance in corporate ROA.
overspecification from including all interaction effects instead of just one set of interaction effects (see, McGahan & Porter, 1997: 18 for further discussion). Regardless, there are not sufficient degrees of freedom to include transient industry effects (industry-year interaction) or corporate-executive effects in this study.

Further, consistent with traditional variance decomposition work in the strategic management literature, an accounting-based measure of firm performance (ROA) is used for the dependent variable. The weaknesses of using accounting measures of firm performance are well known (e.g. Fisher and McGowan, 1983). However, this measure is adopted for two reasons. First, to make this research directly comparable to previous work, it is helpful to adopt the same dependent variable as previous work. Second, to estimate segment effects and industry effects for multi-divisional firms, information on segment performance is required. Since segments of firms are not publicly traded, market-based measures of segment performance are not available.

Both models are estimated using simultaneous ANOVA (e.g. McGahan and Porter, 2002) unlike previous work that has relied on sequential ANOVA (e.g. Lieberson and O’Connor, 1972; Weiner, 1978; Thomas, 1988). Simultaneous ANOVA method allows for covariance effects unlike sequential ANOVA, which imparts covariance effects to the variable which enters first in the decomposition. Since common variance is captured by covariance effects, estimates from simultaneous ANOVA are usually more conservative (i.e. smaller) than estimates from sequential ANOVA.

There are certainly limitations to the types of questions that variance decomposition studies can answer. For example, while variance decomposition can answer whether CEOs on average impact firm performance, this estimation technique cannot provide insight into
why one factor might be more important than another factor on firm performance nor does it suggest the source of the effect. Additionally, only average effects for each factor are estimated. It is likely that the distribution of industry, firm, and CEO effects on firm performance might reveal information as to when environmental, internal, and individual influences are likely to substantially impact firm performance.

2.3.2 Data and Sample

Business-segments are identified from the Compustat Business-Segment reports, which include all companies that are publicly traded in the United States, since 1983. This dataset numbers each segment that belongs to a particular corporate parent, and hence by combining the segment number and the corporate identifier, business segments are uniquely identified.

Criteria for inclusion in the sample were based on the conventions in the strategic management literature (e.g. McGahan and Porter, 1997). Namely, segments in financial institutions were deleted as returns are not comparable to other industries (SIC in 6000’s), segments in government or unclassifiable industries were deleted as firms in these segments are not direct competitors (SIC greater than 9000), and segments with less than 20 million in assets were deleted—all according to convention in the prior literature. These deletions do not preclude an entire corporation from being in the dataset but rather just the segments in problematic industries (e.g. financial institutions, government, unclassifiable, or monopolistic industries). This data was merged with the Compustat Executive Compensation database, which reports 100 compensation and financial variables for executives and their respective
corporations in the S&P 500, S&P MidCap 400, and S&P SmallCap 600. The available years were 1992-2002. Since the sample is comprised of the largest firms in the US economy, this analysis represents a conservative test of the CEO effect, since it is likely that the CEO effect would be more substantial in smaller firms.

For this particular study, leaders are defined as CEOs and not other members of the executive team or members of the board. CEO effects cannot be separated from corporate effects if the same CEO is running a corporation for the entirety of the data range (CEO effects and corporate effects for a given observation will be perfectly collinear). Thus, if companies were included in the dataset that did not experience a turnover event, then all of the firm performance variance due to the CEO would be allocated to the corporate effect. Hence firms were omitted from the sample if there were no changes in the CEO position—avoiding a common limitation of prior empirical work in this area. Dropping these companies without a leadership event resulted in 496 firms and 5028 segment year observations being deleted from the sample. The remaining sample is comprised of 520 firms and 8522 segment year observations.3

Lastly, to avoid the problem of perfectly nested samples, only firms that had a CEO who worked (as CEO) for more than one company in the dataset were included in the sample. All observations for these firms are included in the sample—that is, the CEOs that worked at the firm before or after the “mobile” CEOs were also included in the sample so that all years of observations for these firms will be in the dataset (required for accurate firm effects). In the end, this sample contains 801 segment year observations—92 CEOs and 51

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3 Potential sample selection bias from the exclusion of these firms is discussed and results from a Heckman 2-step procedure correcting for sample selection bias are detailed in the results section.
firms across ten years. This sample represents the activities of 181 distinct business segments in a total of 98 industries, which are designated by their four-digit SIC codes. The average business segment reports 4.43 years of data. The majority (83%) of the observations are associated with diversified firms. Table 3 reports descriptive statistics for this sample and for the sample without the mobility restriction imposed.

In sum, the analysis in this paper incorporates solutions to all the limitations in the previous empirical studies examining CEO effects. Specifically, the sample of leaders is not perfectly nested within the industry and firm effects, statistical analysis is conducted with simultaneous ANOVA, firms without CEO turnover events are excluded from the sample, both diversified and non-diversified firms are included in the sample, and business-segment level data is used in order to estimate accurate industry effects.

2.4 Results

This paper asserts that the methodology used in the previous research has limitations that actually make it difficult to test the impact of CEOs on firm performance. Specifically, this paper asserts that the methodologies used in these prior studies have systemically understated the impact of CEOs on firm performance. Thus, to demonstrate that the methodological issues identified in this paper are in fact impacting the size of the CEO effect, the results in this paper are first reported without correcting for these methodological limitations. The results for this model are presented in Table 4.

This model (referred to as “current replication with limitations”) represents analysis conducted with sequential ANOVA for a perfectly nested sample of executives—including
firms without CEO turnover and with corporate level observations for multi-segment firms (biases industry effect) for the Execucomp data as described previously. The results are consistent with prior variance decomposition studies within the leadership literature—namely, industry (18.0%) and firm influences (29.5%) are significantly higher than CEO effects (12.9%) on corporate performance.

Table 5 presents the main results of the paper—with all the methodological corrections identified in the previous section. In this model, CEO effects on corporate performance are found to be quite substantial (29.2%)—almost four times larger than the corporate effect (7.9%) and almost five times larger than the industry effect (6.2%). CEO influence on the variance in business-segment performance is, not surprisingly, smaller (12.7%) than on corporate performance.

Corporate effects are consistent with prior empirical work that has found, on average, a nine percent corporate effect (Schmalensee, 1985; Rumelt, 1991; Roquebert, Phillips, and Westfall, 1996; McGahan and Porter, 1997, 1999, 2002, 2003). Industry effects, though smaller than early work in variance decomposition (Rumelt, 1991; Roquebert, Phillips, and Westfall, 1996; McGahan and Porter, 1997, 2002) are consistent with recent empirical work that suggests that the industry effect declined in the 1990s (McNamara, Vaaler, and Devers, 2003; Mackey, Kiousis, and Barney, 2005). Since the data used in this paper is from the years 1992-2002 and early work in variance decomposition utilizes data for years in which the industry effect was more substantial (e.g. Rumelt, 1991; Roquebert, Phillips, and Westfall, 1996; McGahan and Porter, 1997, 2002), it is not surprising to find small industry effects. Additionally, due to the small sample size, many of the firms in the sample are the only representatives of their respective industries (48% of the observations).
This creates a problem for estimating accurate industry effects; however, the size of the industry effect is only of secondary interest in this paper.

Business-segment effects are not estimated for the model in which corporate performance is the dependent variable because including these effects in a model with corporate effects mathematically changes the analysis from simultaneous ANOVA to sequential ANOVA (See McGahan and Porter, 2002 for a more technical explanation) and, more importantly, because the business-segment effect on corporate ROA would be understandably insignificant.

### 2.5 Robustness Checks

As stated in the section on the data and sample construction, two sample restrictions were imposed on the data—only firms with CEO turnover during the time of the study and only firms with CEOs that had worked for more than one firm in the dataset were selected for the final sample. Such choices may create sample selection bias and thus alter the results of the analysis. This and other methodological choices are evaluated in this section for any potential influence on the results.

#### 2.5.1 Sample Selection Bias

Skepticism about the CEO effect reported in this paper might center on the potential selection biases created from restrictions placed on the original sample. For example, since prior empirical work examining CEO effects did not explicitly exclude firms without CEO turnover (like was done in this study), it could be argued that the more
substantial CEO effect reported in this paper is due systematic differences between firms with CEO turnover and those without turnover. Rather, this paper argues that the difference in reported effects is due to the statistical impact of including firms without CEO turnover in the samples of the previous work and not a sample selection bias.

The Heckman 2-step procedure (1979) to correct for sample selection bias is used to ascertain whether the results presented in this paper are an artifact of sample selection bias. The first step of this procedure predicts what variables impact whether or not a firm has any turnover during the years it is in the sample. Identifying appropriate instruments to predict CEO turnover is based on Finkelstein and Hambrick’s (1996) theoretical work on the determinants of executive turnover. Turnover is thus modeled as a function of firm performance (corporate ROA relative to the segment’s median competitor), firm size (corporate sales), firm structure (divisional structure), and environmental conditions (number of firms in a segment’s industry as well as the percent of a segment’s competitors in an industry that have had executive turnover during their time in the sample). The probit estimates for this step are shown in Table 6.

The second step of the Heckman procedure revealed no significant bias in the estimated effects reported in this paper. This can be seen in Table 7 which reports the ANOVA estimates for a sample excluding firms without CEO turnover—correcting for the sample selection bias of selecting only firms with CEO turnover for the sample. The Mills ratio (lambda coefficient) is significant at a 90% confidence level for both the model in which corporate performance is the dependent variable and the model in which business-segment performance is the dependent variable. However, even though there is some statistical significance for the lambda coefficient which indicates the presence of a sample
selection bias, this bias is not practically significant as the reported effects (in the corrected model) do not change from models in which the selection bias is not corrected (also reported in Table 7).

There is another selection restriction further imposed on the data—that is, the restriction that only firms with CEOs working for more than one company in the sample can be included in the final sample. The same specification from the probit in the previous sample selection model was used for the first step of the Heckman procedure (results in Table 8). The Mills ratio is significant \((p<0.04)\) for the model in which corporate performance is the dependent variable, but not for the model in which business-segment performance is the dependent variable \((p<0.08)\). As expected, correcting for sample selection does not impact the reported size of the CEO effect on business-segment performance (see Table 8 as well). And, even though the Mills ratio is significant in the model for corporate performance, the impact on the reported CEO effect is minimal (a decrease of 1.6% to the CEO effect). Thus, the models run with the Heckman procedure correcting for sample selection bias do not significantly impact the outcome of the estimation; therefore, the results in Table 5 are appropriate estimations of the impact of the CEO effect on firm heterogeneity.

2.5.2 Expanded Sample

The initial model (Table 5) was estimated from a sample with a two selection restrictions—only firms with CEO turnover and with CEOs that worked (as CEO) at more than one firm in the sample could be in the included in the sample. Although the resulting sample is smaller than ideal \((n=848)\), the sample size is sufficiently large for estimation;
nevertheless, the restriction on executive mobility is relaxed to examine the impact on the results. Relaxing this restriction increases the sample size substantially—8522 firm years (1176 CEOS in 520 corporations across ten years). (Refer to Table 4 for more descriptive statistics on both samples.) However, the limitation of this “expanded” sample is the nesting of CEOs within corporate and industry effects—effectively understating the CEO effect on performance.

The results from re-estimating the original model with only one sample selection restriction have already been reported in Table 7. The largest influence on corporate performance is now the corporate effect which has increased from 7.5% (Table 5) to 24.4%4 (Table 7). CEO effects, as expected, have decreased to 23.8% (Table 7)—down from 29.2% in the initial model (Table 5) because of the introduction of CEO observations that are perfectly nested within corporate and industry data. Since some of the observations of CEOs are not perfectly nested and since firms without turnover are still excluded from this sample, CEO effects on corporate performance are nevertheless larger than in the prior literature.

A similar impact to the CEO effect from introducing perfectly nested observations is seen for the model predicting business-segment performance. As the variance decomposition work in strategic management would predict, business-segment effects are the most important influence over business-segment performance (34.4%, Table 7) and CEO effects have decreased from 12.7% (Table 5) to 7.6% (Table 7). Nevertheless, because of the vast majority of the observations that are perfectly nested, results from this

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4 Corporate effects of 24.4 percent are only slightly higher than those reported by McGahan and Porter (2002) who also used simultaneous ANOVA (corporate effects in their study ranged from 12.0 to 23.7 percent).
“expanded” sample and previous studies that have used samples of CEOs that only worked in one industry and for one firm in the sample should be interpreted with caution.

Thus, this robustness check confirms that it is important to select only those firms with CEOs that have worked (as CEO) for more than one company even though it does restrict the sample size. Not doing so will mathematically impact the size of the CEO effect on firm performance (refer to Figure 2).

2.5.3 Business-Segment Effects

In accordance with convention in the variance decomposition work in strategic management, business-segment effects are estimated for the model in which business-segment performance is the dependent variable (Table 5). The size of the impact is estimated at 17.81%. Scholars familiar with the variance decomposition work in strategy will notice that this segment effect is smaller than that reported in other studies. For example, McGahan and Porter (1997) report segment effects of 35.1 percent and Rumelt (1991) reports slightly lower business-unit effects of 33.9 percent. However, the business-segment effect reported for the “expanded” sample (34.4%, Table 7) is very similar to the prior literature.

A potential reason that the business-segment effect might be larger in the “expanded” sample compared to the sample in which executives are not perfectly nested within industry and corporate effects is two-fold. As previously stated, when corporate and business-segment effects are included in variance decomposition, the simultaneous ANOVA reverts to sequential ANOVA. The impact of this estimation change on the business-segment effect is that this effect essentially becomes a residual of the effects decomposed
first. Thus industry, corporate, and CEO effects are all “taking away” from the businesssegment effect. In the “expanded” sample, the impact of CEOs is systematically understated because of its nesting within industry and corporate effects. Consequently, the CEO effect takes less away from the business-segment effect in this scenario making business-segment effects higher than in a sample in which CEO effects are not understated and can take away more from the business-segment effect.

2.6 Discussion

In short, the empirical analysis conducted in this paper suggests that firm leaders, operationalized as CEOs, account for 29.2% of the variance in corporate profitability while only accounting for 12.7% of the variance in business-segment profitability. These results suggest that CEOs significantly influence corporate-level performance through corporate strategies such as diversification, mergers, and acquisitions, but also exert some influence on business-segments through control exerted over product market strategies (i.e. vertical integration, cost leadership, product differentiation) of the segments as well as the management team that will run the business-segments and the accounting practices for how assets and profits are allocated across segments.

CEO effects of close to 30 per cent of the variance in firm profitability are significantly different than most of the prior empirical studies of leadership effects in firms (Lieberson and O’Connor, 1972; Thomas, 1988; Wasserman, Nohria, and Anand, 2001), government (Salancik and Pfeffer, 1977), religious organizations (Smith, Carson, and Alexander, 1984), and in sports teams (e.g. Gamson and Scotch, 1964; Brown, 1982). This prior work has only demonstrated a relatively modest leadership impact (Bass, 1990;
Finkelstein and Hambrick, 1996). For example, in line with CEO effects ranging from a low of 3.9% (Thomas, 1988) to a high of 14.7% (Wasserman, Nohria, and Anand, 2001), mayoral effects range from a low of five percent to a high of fifteen percent of the variance in city expenditures (Salancik and Pfeffer, 1977). Likewise, sports teams studies of succession effects suggest that leaders do not impact team performance (Gamson and Scotch, 1964; Eitzen and Yetman, 1972; Allen, Panian, and Lotz, 1979; Brown, 1982) while others have suggested that this effect exists although significantly smaller than effects such as prior team performance and changes in player personnel (Pfeffer and Davis-Blake, 1986).

2.6.1 Other Methodologies for Examining CEO-Firm Performance Linkage

Variance decomposition is not the only methodology for examining the linkage between CEO leadership and organizational outcomes. Structural models using theoretical variables to capture the specific industry, corporate, or individual leadership effects on performance have also been used (e.g. Weiner and Mahoney, 1981; Pfeffer and Davis-Blake, 1985). Empirical studies of the CEO-performance linkage with structural methodologies are limited by their measures (e.g. Weiner and Mahoney, 1981), samples (e.g. Gamson and Scotch, 1964; Eitzen and Yetman, 1972; Allen, Panian, and Lotz, 1979; Brown, 1982), and some of the same methodological limitations identified in this paper (e.g. perfectly nested samples). Nevertheless, more structural studies of leadership effects are needed to explain the source of the CEO effect documented in this paper. Structural methodologies will be instrumental in explaining the phenomenon identified in this paper—namely, that leaders can have substantial impact on firm outcomes. Additionally, because of the significance of
the CEO effect identified in this paper, scholars should be cautious to accept empirical studies with structural methodologies not finding leadership effects.

2.6.2 Methodological Extensions to Other Literatures

Perfectly-nested samples are common to empirical studies in many other literatures within executive leadership such as the executive compensation, succession, and top management team literatures. These samples confound individual, corporate, and industry heterogeneity. Using a sample with executive mobility to estimate the determinants of executive compensation would, for example, decompose the unobserved component of wages (estimated as at least 50% in Tosi, Werner, and Katz, 2000) into person and firm effects (Rosen, 1986; Willis, 1986; Abowd, Finer, Kramarz, 1999), such that the portion of the variance in wages due to executive ability (unobservable executive heterogeneity) and the portion of the wages due to unobservable firm heterogeneity can be explicitly estimated (see Chapter 3).

Within the succession literature, some empirical work has drawn on samples with individual mobility and has found significant succession effects on performance—although other effects are more substantial on performance (e.g. Smith, Carson, and Alexander, 1984). However, the majority of the work in this area fails to track individuals in different organizational settings (e.g. Brown, 1982; Pfeffer and Davis-Blake, 1986).

2.6.3 Heterogeneity in Executive Ability

The results presented in this paper suggest that, on average, CEOs can substantially impact firm performance. Future research should focus on why some CEOs might matter
more in influencing firm outcomes. Variance decomposition is a crude method for controlling for the backgrounds and abilities of leaders (Smith, Carson, and Alexander, 1984; Pfeffer and Davis-Blake, 1986; Day and Lord, 1988). Previous empirical work suggests that “effective” leaders are more able to influence firm performance than “ineffective” leaders and hence, the proportion of “effective” versus “ineffective” leaders in a sample will influence the degree of the CEO effect (Day and Lord, 1988: 457). However, while “ineffective” leaders might have no material impact on firm performance, it seems likely that they some will destroy firm value, and hence, the presence of “ineffective” leaders in a sample with “effective” leaders will create more variance in the type of impact on firm performance (i.e. positive or negative).

Additionally, it is incorrect to suggest that the previous research using variance decomposition has not controlled for executive heterogeneity. Since these models include a fixed effect for each individual in the sample they implicitly control for individual effects (e.g. background differences, varying levels of ability). While continuous variables typically offer more information than dichotomous variables, establishing construct validity for continuous variables measuring something like managerial talent is extremely problematic. Yet, even if a good measure of managerial ability were available, this measure would have to vary over time to avoid being collinear with the fixed effect for each executive.

2.6.4 Executives and Rent Appropriation

Intuitively, significant CEO effects may mean that executives behave differently and make different policy decisions even within the same or similar external environment (Bertrand and Schoar, 2003). Significant CEO effects also may suggest that managers have
their own unique styles and preferences for how to influence the resource-base of the firm (e.g. aggressive versus conservative strategies, internal growth versus growth through acquisition, etc). These unique styles are manifest as CEOs decide whether to acquire or divest business units, layoff or hire employees, financially or organizationally restructure the firm, introduce new products or enter new markets, change executive level personnel, and form alliances with other firms (Hansen, Perry, and Reese, 2004). All of these decisions are likely to be based on heterogeneous preferences among executives (Bertrand and Schoar, 2003).

Since part of the CEO effect is manifest in how a firm chooses its competitive position, including which industries it competes in and how the firm competes in those industries (industry and corporate effects, respectively), then, in line with the resource-based view of the firm (Barney, 1986a), corporate strategies and the executives who design and implement them may be important resources for the firm’s ability to generate competitive advantage, create value, and ultimately appropriate economic rents.

However, since powerful organizational members with critical knowledge may be able to appropriate rent from shareholders, firm performance metrics might not always reflect the true value created by a strategic choice. For example, if the value created is appropriated by an individual, such as a CEO, and not the shareholders, such rent appropriation might mask leadership effects on performance. For example, if CEOs really make a large positive financial impact on the performance of the firm yet also appropriate a portion of that profit, the variance in profitability between firms in the sample will be reduced. However, unless CEOs from either the high or low performing firms in the sample are able to disproportionately appropriate rents, the overall variance in firm profitability in
the sample will not be altered. It is accordingly critical for organizational scholars—specifically, those focused on strategic firm issues—to understand the conditions in which executives will be able to wield their bargaining power to appropriate economic rents. Rent appropriation by executives certainly deserves further study.

2.6.5 Top Management Team Effects

Currently empirical work on top management teams argues and provides supporting empirical evidence to suggest that top management teams are better predictors of firm outcomes than CEOs alone (e.g. Hage and Dewar, 1973; Finkelstein, 1988; Ancona, 1990; Haleblian and Finkelstein, 1993). This paper suggests, however, that the study of individual leaders and groups of leaders are both important to organizational scholars. Even though this paper examines only changes in the position of CEO and not in the top management team, it is likely that the CEO effect in this paper is capturing part of the top management team influence on firm performance (Hambrick and Mason, 1984) since when a CEO succession event occurs, changes are usually made in the top management team as well. Future research could disentangle how much of the leadership effect on firm performance is from the CEO and how much is from the entire top management team effect to better understand the relative importance of the CEO to the entire top management team for influencing organizational outcomes and vice versa.
2.7 Conclusion

Not just practitioners and scholars think that CEOs matter in determining firm outcomes—CEOs themselves certainly think they matter and they are certainly paid as though they significantly influence firm outcomes. Yet, beyond anecdotal evidence of leadership influences on firm outcomes, these viewpoints of top leadership and the compensation of CEOs have been incongruent with large-sample archival research that suggests other influences are far more important than leadership influences on firm performance. In contrast, this paper suggests that CEO effects on the variance in firm performance may be as high as 29.2%.

This study also provides a better answer than was previously available to organizational scholars to the fundamental question of whether or not CEOs can significantly impact firm outcomes, and thus, alters how scholars think about organizations. Many scholars have expressed skepticism concerning the reality and importance of leadership—at any level of the organization—because of the lack of empirical work supporting the leadership hypothesis (Miner, 1975; Bass, 1990). These results re-affirm the many theoretical perspectives in organizational studies that assume significant leadership effects.

These results also have important implications for theoretical perspectives in organizational studies that assume small CEO effects. For example, much of the executive compensation literature assumes that compensation is not commensurate with executive influence on firm outcomes; yet, if executives can exert as much influence as this study suggests, then CEO compensation packages may be warranted (Mackey, 2005), and much of the foundation of corporate governance theory which suggests that firm differences arise
due to heterogeneous governance practices in monitoring executives and not executive effects (Bertrand and Schoar, 2003) may be questionable. These and many other assumptions organizational scholars have about organizations based on small leadership effects might be revisited in light of these new empirical results.
CHAPTER 3

HETEROGENEITY IN EXECUTIVE COMPENSATION: WHERE YOU WORK VERSUS WHO YOU ARE

Robert Lane, CEO at John Deere, received total compensation in 2003 in excess of $7.8 million. James Owens, CEO at Caterpillar—a larger and more profitable competitor to John Deere—received just over $2.5 million in total compensation in 2003. This compensation differential between these two men is stable across their tenure in these firms. How much of this heterogeneity in compensation is due to differences between Robert Lane and James Owens, and how much is due to differences between John Deere and Caterpillar?

Heterogeneity in executive compensation practices persists widely both within and between industries (Murphy, 1999). Two broad explanations of the heterogeneity in executive compensation can be identified. First, this heterogeneity might be explained by differences between firms—“where you work”. For example, firms may differ in their ability to find “good” matches in the executive labor market. Better alignment between management styles and career background with the organizational structure or strategy may increase firm performance (Bertrand and Schoar, 2003) and, therefore, may result in higher compensation for executives. Firms may also differ in their need of or preference for individuals with high external wage rates. Certainly compensation design, incentive systems, and opportunities for bargaining are also likely to differ across firms.
Second, this heterogeneity might be explained by differences between executives—"who you are". Executives may differ in their endowment or accumulation of human capital (Becker, 1964). Thus, individuals with similar career, educational, or demographic backgrounds might have markedly different compensation because of heterogeneous endowments of ability and/or learning processes resulting in different levels of managerial talent (Harris and Helfat, 1997).

Thus, the purpose of this paper is to estimate the extent to which heterogeneity in executive compensation is due to “where you work” versus “who you are”. This question is also explored for various groups of executives (e.g. founders versus non-founders, CEOs versus non-CEOs, males versus females, MBAs versus non-MBAs). A matched-longitudinal firm-executive dataset is used for this paper combining information on over 1110 executives from S&P 1500 firms with measures for compensation, career and educational background, and biographical information. Consistent with theories of human capital (Becker, 1964), the results suggest that executives with greater firm-specific, industry-specific and position-specific experience receive wage premiums in the labor market. The source of the gender-wage gap among corporate executives is explored. “Where you work” is found to be more important than ”who you are” in determining executive compensation. Implications for this result are discussed.
3.1 Theories of Compensation Determination

Traditional equilibrium models of the labor market suggest that wages reflect the opportunity cost of an employee’s time. In this context, observed wage heterogeneity can be explained by unmeasured differences among individuals and not differences among firms. In short, traditional equilibrium models of the labor market suggest that “where you work” does not impact compensation.

In contrast, other models of the labor market consider rent-sharing (e.g. Leontief, 1946; MacDonald and Solow, 1981; Manning, 1987), imperfect information (efficiency wage and agency models) (e.g. Shapiro and Stiglitz, 1984; Hart and Holmstrom, 1987; Sappington, 1991), or sorting (Becker, 1973; Bulow and Summers, 1976; Jovanovic, 1979) to explain why firm-level differences might explain wage heterogeneity. In this context, the observed wage heterogeneity may be due to permanent, unmeasured differences among employers or the industry they operate in, as well as unmeasured differences among individuals. In short, these models of the labor market suggest that “where you work”, as well as “who you are”, is instrumental in determining compensation.

Specifically, rent-sharing models argue that wages are affected by product market competition as well as the supply and demand forces in the labor market. These models view wages as the sum of an opportunity wage and a rent component. “These rents are the product of workers’ bargaining power, an outcome of labor market institutions…, and of the firms’ quasi-rent, an outcome of product market competition. (Abowd et al, 2005: 15).

Efficiency wage and agency models predict that firms pay wages above market prices in order to increase employee productivity or “efficiency”. Firms may pay efficiency wages to curtail shirking by increasing the cost to the individual of losing the job—particularly in
instances in which monitoring is difficult such as in larger firms. Such wages can provide incentives for executives to maximize the value of the firm. By closely tying the payoffs to executives and shareholders together (i.e. stock and stock options), the firm can easily and inexpensively monitor executive behavior (Sappington, 1991). Paying above market wages to the firm’s top executives may also motivate lower-level managers to make firm-specific investments in hopes of a future promotion to these lucrative positions within the firm (Lazear and Rosen, 1981).

Assignment or ‘matching’ models have been used to describe how workers and firms “match up” in labor markets and, consequently, how wages are determined (Koopmans and Beckmann, 1957; Shapley and Shubik, 1972; Becker, 1973; Shimer, 2001). For example, firms paying above market wages may be selecting executives with high external wage rates or with skill sets best matched to the firm’s current strategic needs (Bertrand and Schoar, 2003) resulting in sorting executives into firms with differential compensation policies (Abowd, Kramarz, and Margolis, 1999; Bulow and Summers, 1976; Jovanovic, 1979).

Despite the overwhelming focus on firm-level variables in explaining executive compensation, previous research examining wage heterogeneity among corporate executives has also examined how measurable aspects of human capital (e.g. management experience, education, and tenure) impact compensation. For example, much empirical work has been devoted to looking at whether internal or external successors, who differ with respect to the specificity of their human capital, will receive higher compensation (e.g. Gilson and Vetsuypens, 1992; Joskow, Rose, and Shephard, 1993; Hambrick and Finkelstein, 1995; Harris and Helfat, 1997). Empirical findings generally suggest that external successors earn
wages considerably higher than the CEOs they are replacing (Hambrick and Finkelstein, 1996; Harris and Helfat, 1997).

While examining how measurable executive characteristics impact compensation is important as well as interesting, there are limitations to this approach. For example, unobservable personal traits are likely to impact the rate of return on investments in human capital. Accumulation of similar years of general, firm-, and industry-specific experience will yield different levels of managerial skill for different individuals because endowed ability levels and learning processes are heterogeneous across individuals. Similarly, the same level, type, and quality of education will yield different levels of managerial skill for individuals with different endowments of ability and/or learning processes. Thus, tangible measures of human capital may not be good indicators of executive ability and therefore may not fully explain how “who you are” impacts wage differentials. Unobservable person heterogeneity is likely to capture differences in compensation rates above and beyond that which observed person heterogeneity can capture.

Examining unmeasured person heterogeneity may be particularly fruitful to the study of executive compensation since the prior empirical work suggests that measured executive characteristics—even with detailed information on prior experience, education, power, and demographics—are unlikely to explain much of the variance in executive wages (Finkelstein and Hambrick, 1996; Tosi, et al 2000). Unfortunately there is very little existing theory to guide empirical work in this area. For example, previous studies have found that founders receive less compensation than non-founders even after controlling for measurable human capital (Wasserman, 2004). However, it is not known to what extent “who you are” (intangible human capital) and “where you work” accounts for the remaining unexplained
heterogeneity between founder and non-founder compensation. Do founders receive less
than non-founders (after controlling for measurable human capital) because of less “talent”
or because of differences in firm-specific compensation policies?

These same questions can be asked of many different categorizations of executives.
For example, does “who you are” matter more or less than “where you work” in explaining
why male executives tend to receive higher compensation than female executives? Further,
does “who you are” matter more or less than “where you work” in explaining why there is a
wage differential, after controlling for measured human capital, between executives with and
without MBAs? The following section outlines a methodology for empirically examining
these types of questions about executive compensation.

3.2 Methodology

Labor market outcomes (such as compensation contracts) result jointly from
individual and firm decisions—a firm selects an executive to hire, but the executive must
also choose to join the firm. Estimating the extent to which executive wages are based on
“where you work” versus “who you are” requires simultaneous study of both sides of the
labor market (Rosen, 1986; Willis, 1986; Abowd, Finer, Kramarz, 1999). Abowd, Kramarz,
and Margolis (1999) offer the first extensive statistical analysis of simultaneous individual-
and firm-level heterogeneity in wage determination in a study of persistent inter-industry
wage differentials in general (non-executive) labor markets. The methodology used in this
paper draws heavily on their work, with necessary adaptations for the study of executive
labor markets.
3.2.1 Model

The most common specification used in the labor literature (see Abowd et al. 1999) to estimate wage differentials based on a simultaneous analysis of individual- and firm-level heterogeneity is the following:

\[ y_{it} = \mu_y + (x_{it} - \mu_x) \beta + \mu_i \eta + \theta_i + \psi_j + \epsilon_{it} \] 

for \( i = 1, \ldots, N \) executives and \( j = 1, \ldots, J \) firms where \( y_{it} \) represents the logarithm of annual compensation for executive \( i \) in time \( t \), \( \mu_y \) is the grand mean of \( y_{it} \), \( x_{it} \) is a vector of observable, time-varying executive characteristics, \( \mu_x \) is the grand mean of \( x_{it} \), and \( \mu_i \) is a vector of time-invariant observable individual characteristics. Permanent unmeasured differences between individuals, referred to as person effects, (“who you are”) are represented by \( \theta_i \) and permanent unmeasured differences between firms, referred to hereafter as firm effects, (“where you work”) are represented by \( \psi_j \).

3.2.2 Data and Sample

Implementing equation 3.1 so that simultaneous analysis of individual- and firm-level heterogeneity is possible requires the use of matched-longitudinal firm-executive data which connects groups of executives and firms based on the mobility of executives throughout their career. Such a dataset contains observations for multiple executives within the same firm as well as observations for these executives in the other firms they have worked at during their careers. Additionally, other executives that have spent time with these same employers during their career are included in the sample and grouped in with these initial executives. Various sets of these “connected” groups of firms and executives produce the
needed variation to disentangle person and firm effects on compensation. For example, within these “connected” groups of firms and executives, data is contained on the compensation for a particular executive in more than one firm context as well as what those firms paid other executives.

Constructing the sample required a match between two databases. The first is Compustat’s Execucomp database, which contains the top five executives for firms in the S&P 500, MidCap 400, Small Cap 600 from 1992 to 2004. This database contains all the relevant compensation information for the executives in this sample. The second database is the Marquis’ Who’s Who in Corporate Executives biographical file. Marquis collects educational, professional, and personal information about executives from various public and private firms—specifically targeting Russell 1000 firms which are highly correlated with the set of firms in the Execucomp database. A name and firm match was done between the two databases, arriving at 1127 executives in 628 firms in both databases. These executives are the sample for the paper. It should be noted that because of increases in the breadth of coverage of executives in the S&P 1500 over time within Compustat’s Execucomp, the number of executives in the sample for which compensation data is available steadily increases as well from 1992-2004. While this does affect the number of years of compensation data available for each executives, it does not impact which executives “matched” between the two databases.

Of the executives in the sample, 403 individuals hold the position of CEO and 518 individuals are directors at their firm at one time during 1992-2004 (years for which compensation data is available). There are 38 founders in the sample. Consistent with the overall portion of females in Execucomp, 72 executives in the sample are female. As would
be expected, the majority of the executives in the sample are in the same age cohort—approximately 44% of executive are between the ages of 50-59. For the 400 executives for which nationality is known, about 100 individuals in the sample were born outside of the United States and about 300 individuals were born inside the United States.

A broad representation of industries is contained in this sample: manufacturing (290 firms), finance/insurance (85 firms), transportation (79 firms), retail trade (73 firms), services (59 firms), agriculture, construction, mining, public administration, and wholesale trade industries. The distribution of executives is similar to this distribution of firms in industries with the majority of the executives in the sample being in the manufacturing industry (567 executives).

3.2.3 Dependent Variable

Executive pay is typically composed of a base salary, an annual bonus tied to accounting performance, stock options, and long-term incentive plans (e.g. restricted stock and multi-year accounting based plans) (Murphy, 1999). Two methods, both common in the executive compensation literature (Tosi et al, 2000), are used in this paper for grouping these components of compensation to calculate the level of pay. The first method uses the total cash pay which is the total current compensation for the executive comprised of salary and bonus.

The second method uses the total pay which is the total compensation for the executive comprised of salary, bonus, other annual compensation (e.g. perquisites, tax reimbursements, etc), the total value of restricted stock grants, the total value of stock options granted (using Black-Scholes), long-term incentive payouts, and all other total
compensation (e.g. signing bonuses, 401K contributions, debt forgiveness, life insurance premiums, etc). Compensation is calculated for option grants instead of exercised options because the timing for exercising options is arbitrarily determined by the executive whereas the timing of option grants is determined contractually between the executive and the firm.

**Total cash pay** was prorated for instances in which an executive switches jobs in the middle of a year. For example, if an executive worked at a firm from October through December of the year, the salary reported in Execucomp would be substantially lower than if an entire year had been worked. Using this figure for compensation instead of a prorated amount would generate inaccurate estimates of the model. **Total pay** was not prorated as it appeared to not be substantially lower for an executive in the first year with a firm. This is most likely because total pay includes stock grants which are likely to be substantial in the initial year of employment.

3.2.4 Independent Variables

Time-variant variables for observable executive characteristics include proxies for **experience** (e.g. years of position-, firm-, and industry- specific experience as well as an adjusted industry-specific experience measure that only captures years of industry experience acquired prior to joining the current firm) and **power** (e.g. director, CEO, and founder status). Time-invariant variables for observable executive characteristics include variables such as **education** (time invariant since it is most likely completed and thereby fixed for executives) and **demographics** (gender).
All of these variables are constructed from the Marquis’ Who’s Who biographical entry for each executive in the sample. Industry-specific experience is calculated at the 2-digit level. Position-specific experience is calculated based on the following five position levels: Level 1: Chairman, Vice-Chairman, President, CEO; Level 2: C-level Executives (CFO, COO, and other chiefs); Level 3: Vice-President; Level 4: Functional Vice-President; Level 5: General Executive Positions. These levels were used to condense over 300 position classifications (generated by the author) based on the listing of positions held by each executive in his or her respective biographical entry in Marquis’ Who’s Who. There appears to be no significant differences in the average level of position-specific experience across industries. For example, the spread in position-specific experience across the sample is between 5-8 years. There is however some differences in which industries tend to have higher levels of industry-specific experience among its executives. For example, the average executive in the construction industry has 20 plus years of industry specific experience; whereas, the average executive in the agriculture/forestry industry has less than five years of industry-specific experience.

Rankings of education degrees were based on the current U.S. News and World Report rankings for each respective degree program. Interestingly, of those executives in the sample with MBAs, 46% of these individuals have MBAs from top ten institutions while almost as many (34%) executives have MBAs from unranked programs.
3.2.5 Estimation

Equation 3.1 is estimated with a two-way error components model with crossed random effects (e.g. Abowd et al, 2005; Woodcock, 2005). This approach, sometimes referred to as mixed effects, has two important features essential for estimating the model presented in this paper. First, with the mixed effects estimator the regression coefficients are estimated with fixed effects assumptions and the person and firm effects are estimated with random effects assumptions without assuming that the person and firm effects are uncorrelated with the explanatory variables. This is important since unmeasured person and firm heterogeneity is likely correlated with measured heterogeneity.

Secondly, since the sample contains crossed factors instead of nested factors, the mixed effects estimator is particularly important since it can be estimated with crossed random effects. The difference between crossed factors and nested factors in a sample is important to understand why analysis with crossed effects is fundamentally different from hierarchical linear modeling (HLM) with nested effects. Figure 3, adapted from Rabe-Hesketh and Skrondal (2005), illustrates the difference between crossed factors and nested factors.

When executives are nested within firms, an executive working at two different firms would statistically be treated as two different executives. Hence the information about the executive’s compensation in multiple firm contexts would not be incorporated into the estimation of the person effect on wage differentials. Likewise, when firms are nested within executives, the firm effect on compensation of working at firm 2, for example, would be different for executive A than for executive B. In contrast, with crossed effects the

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estimation of the firm effect on compensation of working at firm 2 would be the same for all three executives who worked at firm 2. Crossed factor models assume that there are specific things about a firm that would influence the compensation of all executives working at that firm in a similar way while nested factor models cannot account for such an assumption. Thus, using hierarchical linear modeling with nested effects to the estimate heterogeneity in executive compensation would only incorporate information within each executive-firm nesting instead of information across the factors. Hence, estimating crossed effects is essential to examining variation in individual compensation rates holding firm effects constant while simultaneously examining variation in firm compensation rates holding person effects constant; thus, providing empirical insight into the effects of permanent unmeasured differences among executives (person effects) and permanent unmeasured differences among firms (firm effects) on executive compensation.

3.3 Results

The central question raised in this paper is what accounts for heterogeneity in executive compensation—“who you are” (person effects) versus “where you work” (firm effects). To answer this question, equation 3.1 was estimated with a two-way error components model with crossed effects. Table 10 presents results for this estimation with both dependent variables—total cash pay and total pay. Recall that both dependent variables are demeaned to represent the wage differential (e.g. Log (total cash Pay) – Ave [Log (total cash pay)]). Coefficient estimates for measured human capital are consistent with prior empirical work. For example, consistent with theories of human capital (Becker, 1964), individuals with more firm-, industry-, and position-specific experience receive higher levels
of compensation. A founder discount is found in the model with total pay (measure that includes equity-based pay) but not with the model with cash pay. This result is counter-intuitive since founders tend to take the majority of their compensation in equity ownership of the firm and not in salary. Further investigation of this apparent “founder-gap” when total pay is used requires looking at changes in total wealth and not just changes in total compensation. A gender-wage gap is also found—female executives make 32% less than male executives in terms of total pay (Model 1, Table 10).

The estimates of interest (person and firm effects) are not actually reported in Table 10 since a person and firm effect is estimated for every executive and firm in our sample, respectively. Estimates of the standard deviation of these effects are reported in Table 10. Figure 4 overlays a histogram of the distribution of firm effects on the distribution of person effects. This figure illustrates that more of the variance in wages is explained by firm effects than person effects. Another approach for looking at the distribution of firm and person effects is presented in Table 11. In this table, a percentile breakdown of the importance of firm and person effects on wages is reported. In both models, firm heterogeneity appears to be more significant than unmeasured person heterogeneity in determining compensation levels.

Whether compensation differentials for various sub-samples of executives are impacted more by “where you work” or “who you are” are also explored. Tables 12 and 13 represent a breakdown of the impact of firm and person effects on compensation differentials for various sub-samples of executives. Specifically, Table 12 compares different wage differentials for CEOs based on sample splits of various executive characteristics, while Table 13 does the same for non-CEOs.
3.3.1 CEOs versus Non-CEO Executives

Figure 5 presents an overlay of the distribution of firm effects on compensation differentials with the distribution of person effects on compensation differentials for the sub-sample of CEOs. Contrasting Figure 5 with the next figure (Figure 6) which presents an overlay of the distribution of firm effects on compensation differentials with the distribution of person effects on compensation differentials for the sub-sample of non-CEO executives, shows that firm effects are much more important in explaining wage differentials among non-CEO executives than person effects but that the relative impact of these effects is quite similar for the sub-sample of CEOs. This suggests that wage differentials among the general population of executives is more due to firm-effects (e.g. rent sharing, efficiency wages, sorting) than person effects (intangible talent), but that, surprisingly, firm and person effects are of similar importance in explaining CEO wage differentials.

3.3.2 Gender Gap in Executive Wages

As shown in Table 10, gender does appear to significantly explain wage differentials across executives. Female executives tend to earn 32% less total pay than the average executive and 13% less cash pay (Table 10, Model 1 and 2, respectively). To further explore this gender-wage gap, the results reported in Tables 12 and 13 are used. From Table 12 we see that female CEOs appear to have lower average measured person effects compared to male CEOs (0.704 for females versus 1.096 for males, difference is statistically significant, $p<.001$). This suggests that female CEOs have, on average, lower observed human capital compared to men (e.g. experience). The unobserved person effects for males and females are not statistically different (difference is -0.011, male person effects less female person effects,
Table 12) and neither are the firm effects on wage differentials. This suggests that the gender-wage gap between male and female CEOs is might be purely due to differences in measured human capital and not to differences in firm-specific compensation policies or “talent”.

This is not the case for the general population of executives. Turning to Table 13, differences between male and female executives are compared again, but for non-CEO level executives. Again, wage differentials between these two groups are statistically significant with men earning higher wages than female executives. Male executives again appear to have higher measured human capital (e.g. experience), but firm and person effects appear to be significantly impacting the wage differential. Female executives appear to have larger person effects (0.003 for females compared to -0.064 for males, Table 13)—meaning that females CEOs seemingly receive a greater premium over market wages from person effects than male CEOs. In short, one interpretation of these results is that female executives may make less than male CEOs, but female CEOs also receive a greater person adjustment to what the market price for their human capital would be than male CEOs receive. Further, when comparing the average firm effects for male executives and female executives, one interpretation of the results is that female executives work for firms who systematically pay more than firms employing male executives. Thus, in comparing compensation differentials between female and male executives, it is interesting to note that the gender-wage gap may be more pronounced because of differences in amounts of tangible human capital between male and female executives since the results seem to imply that female executives work for firms who systematically pay higher executive wages and that they receive a great person-specific adjustment to the market price for their human capital than male executives.
These results are preliminary and require more systematic study to confirm what has been reported in this analysis. Sample sizes of female CEOs as well as the general population of females executives are obviously are small and statistical inference from the techniques used in this paper should be cautiously interpreted.

3.3.3 Founders versus Non-Founders

One interpretation of the estimates in Table 10, is that founders earn less total pay than non-founders (Model 1, Table 10). This interpretation is preliminary since an examination of total wealth differentials between founders and non-founders has not been explored in this paper.

Splitting out the founders into sub-samples of CEO founders versus non-CEO founders, might reveal some differences between these individuals. For example, the compensation gap between founder and non-founder CEOs does not appear to be significantly impacted by firm or person effects but rather it appears to be impacted by tangible human capital (e.g. non-founder CEOs have more experience). This is not the case for the founder/non-founders compensation gap among general executives (see Table 13). First of all the wage differential in this group is not significant. What is significant, and particularly interesting, is that founder wages compared to non-founder wage are on average discounted for person effects (e.g. “talent”). In other words, one interpretation of these results is that among the general class of executives, founders make less than their observed human capital would suggest. This is counter-balanced by higher levels of observed human capital for founders compared to non-founders—resulting in a net effect that appears to
suggest that no compensation differentials exists between founders and non-founders in the general executive population.

Again, these results are preliminary and require more systematic study to confirm what has been reported in this analysis. This study has looked at wage differentials among founders and non-founders when an examination of total wealth differentials may be more informative.

3.3.4 Education and Wage Differentials

Despite much criticism of the value of the MBA degree in the popular press, the real winners among CEOs in terms of wage differentials are individuals with MBAs (see Table 12). CEOs with MBAs earn substantial premiums over CEOs without MBAs. Measured human capital does not explain this wage differential. In fact, it just makes the differential larger since non-MBA CEOs actually have higher levels of measured human capital (e.g. more years of experience) than MBA CEOs. So what does explain the wage differential—“where you work” or “who you are”? A small portion can be attributed to MBA CEOs having higher person effects (e.g. “talent”), but the majority of the differential is due to firm effects. It appears that CEOs with MBAs work for firms that systematically pay above what firms with CEOs without MBAs pay. The results are very similar for the general population of executives (Table 13); however, the wage differential between MBAs and non-MBAs is not as pronounced in this sub-sample.
3.4 Discussion

At the outset, compensation levels for two CEOs in the same industry were compared. Recall that Robert Lane, CEO at John Deere, received total compensation in 2003 in excess of $7.8 million. James Owens, CEO at Caterpillar—a larger and more profitable firm in the same industry as John Deere—received just over $2.5 million in total compensation in 2003. The question was asked, “Is this heterogeneity in compensation due to differences between Robert Lane and James Owens or to differences between John Deere and Caterpillar?”

The answer, in this particular example, appears to be that both where these men work and who they are impacts the total compensation they receive, but that consistent with the main results of this paper, the compensation differential is more impacted by where these men work than who they are. Caterpillar does have a lower firm effect on wages than John Deere, suggesting that Caterpillar systemically pays executives less than John Deere. Additionally, James Owens does have a lower person effect on wages than Robert Lane, suggesting that James Owens systemically makes less for his observable human capital than Robert Lane earns. These differences reveal themselves in the observed $5.3 million wage differential between these two men.

Overall, the results of the paper suggest that compensation differentials among executives are more a function of firm effects (e.g. rent sharing, efficiency wages, sorting) than of person effects (e.g. intangible ability). This result conflicts with similar empirical studies of general labor markets which find that wage differentials are mostly explained by person effects (Abowd et al, 1999). These conflicting results should be expected since it is likely that executives will earn compensation above and beyond their opportunity cost—
perhaps in the form of rent-sharing. Future research should examine the source of the firm effects on compensation differentials. Are these significant firm effects on compensation differentials evidence of rent sharing, sorting, or efficiency wages?
CHAPTER 4

DO “GOOD” EXECUTIVES WORK FOR “GOOD” FIRMS?

Interest in CEOs and their compensation continues to be high, both among academics and the broader business community. Despite this interest, some fundamental questions remain concerning why vast heterogeneity in executive compensation practices persists both within and between industries. In Chapter 3, two broad explanations of the heterogeneity in executive compensation are explored: differences between firms—“where you work”—and differences between executives—“who you are”. Across executives, “where you work” is found to explain more of the variance in compensation levels than “who you are”. In other words, executive wages are impacted more so by firm-specific compensation policies than “intangible” managerial ability.

Since executive wage differentials are substantially impacted by firm differences, it is important to understand why some firms would systematically pay above market wages while other firms would systemically pay below market wages. For example, it may be that firms differ in their ability to find “good” matches in the executive labor market. Better alignment between management styles or career background with the organizational structure or strategy may increase firm performance (Bertrand and Schoar, 2003) and, therefore, may
result in higher compensation for executives. Firms may also differ in their need of or preference for individuals with high external wage rates.\(^7\)

These explanations for compensation differentials due to firm differences would be indicative of labor market “sorting” of executives into firms with differential compensation policies (Abowd, Kramarz, and Margolis, 1999; Bulow and Summers, 1976; Jovanovic, 1979). Assignment or ‘matching’ models have been used by economists to describe how workers and firms “sort” or “match up” in labor markets and, consequently, how wages are determined (Koopmans and Beckmann, 1957; Shapley and Shubik, 1972; Becker, 1973; Shimer, 2001). Exploring whether or not the predictions of these models hold in executive labor markets provides the opportunity to answer some fundamental questions about how executives and firms “match up” in the executive labor market.

For example, do highly talented executives work in firms that are performing well or in firms that are performing poorly? Where do executives realize their “highest and best use”? On the other side of the market, if a firm is performing well, are they likely to want to attract a high quality—but probably expensive—executive or might they actually prefer a less talented executive? Conversely, are firms with low levels of performance likely to prefer paying high premiums in effort to attract high quality executives, in hopes of improving firm performance (Leonard, 1990)? Will these high premiums induce executives to join firms with poor prospects?

Answering these questions and others about how executives and firms “match up” in labor markets requires disentangling the effects of decisions made by firms and decisions

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\(^7\) This chapter does not directly address whether efficiency wages to address agency problems and/or rent-sharing between executives and shareholders also account for firm differences in executive compensation schemes. Chapter 5 addresses these issues further.
made by individuals in the labor market. Hence, the purpose of this paper is to provide empirical insight into the dynamics of executive-firm matching in labor markets—answering whether or not compensation differentials arising from firm differences can be attributed to sorting in executive labor markets.

4.1 How Do Executives and Firms Match in Labor Markets?

Labor economists interested in how types of workers and firms “sort” or “match-up” in labor markets (Abowd et al 1999) have drawn on the concept of “assortative matching” described by Becker (1973), who examines how individuals sort in marriage markets. “Assortative matching” refers to whether types in a market exchange tend to be complements or substitutes. For example, in marriage markets, individuals might choose their mates based on education levels. If highly educated individuals tend to mate or “match up” with other highly educated individuals, then it would be said that the matching process for types in marriage markets involves positive assortative matching on education—that is, levels of education for spouses are positively correlated (Becker, 1973).

In the context of general labor markets, the logic of Becker (1973) suggests that under competitive conditions, identical workers should receive the same compensation, even if they work in different corporations; and identical corporations should receive the same level of profits, even if they hire different workers (Koopmans and Beckmann, 1957; Shapley and Shubik, 1972; Shimer, 2001). Moreover, individuals of similar talent should be guided to work together in corporations when there are complementarities in production (Becker,
Another way of saying this is that “good” workers work for “good” firms (Abowd et al 2004). Empirical tests of Becker’s model and its prediction of assortative matching in general labor markets have found conflicting results concerning the relation between person and firm quality; although, most results suggest that worker and firm quality is negatively correlated. (Abowd et al, 1999; Abowd et al, 2004; Andrews, Upward, and Schank, 2004).

Empirical investigations of Becker’s logic for assortative matching in executive labor markets are notably absent from the literature despite the likelihood that executive-firm sorting explains in part why some firms are more successful than other firms (Mackey, 2005; Bertrand and Schoar, 2003). The following sections outline why we might expect firm and person quality to be positively or negatively correlated or not correlated at all in executive labor markets.

4.1.1 Positive Assortative Matching in Executive Labor Markets

As illustrated in Figure 9, positive assortative matching on quality implies that highly talented executives would tend to sort into high performing firms (quadrant 2) and that less talented executives would tend to sort into low performing firms (quadrant 4). To explain why positive assortative matching might occur in executive labor markets, we explore both quadrants in turn.

First, why would we expect that high performing firms and highly talented executives would match up in labor markets (quadrant 2, Figure 9)? From the firm’s perspective, it

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8 Becker (1973, 1974) shows that matching is positively assortative if types (here, workers and firms) are complements, meaning that the joint production function is supermodular. With complementarities in types, the marginal product of one’s partner rises in one’s type. Thus in a core allocation, and thus, in any competitive equilibrium, matching is positively assortative. In other words, individuals sort themselves into matches with identical partners.
might be the case that managers in highly performing firms are likely to attribute firm success to managerial talent as opposed to firm capabilities and routines. Or there may be a great deal of casual ambiguity associated with a firm’s high performance and it is therefore unclear whether the advantages the firm is enjoying are due to firm-level capabilities or managerial capabilities. In either case, it is conceivable that these high performing firms would want to hire the most talented executives possible. From the executive’s perspective, high performing firms might be the most appealing jobs to executives because these firms might be in the best position to pay for the best executive talent (Rosen, 1990). In addition these jobs might maximize future earnings for executives as being associated with high performing firms is likely to increase an executive’s external wage rate (Fama, 1980; Khurana, 2004).

Second, why would we expect that poorly performing firms and less talented managers would match up in executive labor markets (quadrant 4)? One simple explanation, from both the firm’s perspective and the executive’s perspective, would be that this is the best outcome either party can achieve. Poor future prospects for a firm might deter highly talented executives from accepting employment—particularly if the performance of the firm influences the executive’s future employment and compensation outcomes. Even though accepting employment at a poorly performing firm may have negative ramifications for an executive’s external wage rate, this may be a better alternative for an executive than having an employment gap. Taken together, these arguments suggest the following hypothesis:

Hypothesis 1: Firm quality and executive quality will be positively correlated.
4.1.2 Negative Assortative Matching in Executive Labor Markets

As illustrated in Figure 9, negative assortative matching on quality implies that highly talented executives would tend to sort into low performing firms (quadrant 1) and that less talented executives would tend to sort into high performing firms (quadrant 3). To explain why negative assortative matching might occur in executive labor markets, we explore both quadrants in turn.

First, why would less talented executives tend to sort into high performing firms (quadrant 3)? From the firm’s perspective, it might be the case that superior financial performance is derived from firm-level capabilities and routines; consequently, acquiring managerial talent—which might be very costly—is less to important to these high performing firms. From the executive’s perspective, being associated with a high performing firm is likely to create a halo for the executive on the external labor market (Fama, 1980; Khurana, 2004). For this reason, it is easy to see why a less talented executive would be interested in working for a high performing firm if that opportunity were available.

Secondly, why would highly talented executives tend to sort into low performing firms (quadrant 1)? Poorly performing firms may not have valuable firm capabilities and talented executives could be instrumental in developing or acquiring these needed capabilities. Hence, it may be critical for poorly performing firms to attract highly talented executives.

However, highly talented executive must be willing to work for low performing firms. Earlier arguments suggest that the performance of the firm is used as a signal to the external labor market of the talent level of an executive (Khurana, 2004). Hence, it would seem to be extremely risky for highly talented executives to work for low performing firms.
However, despite this risk, it might actually be appealing for talented executives to work for poorly performing firms. These “turnaround situations” might allow for the executive to be earn compensation premiums for bearing such risk to future job prospects, but also, these situations allow for executives to showcase their talent for developing firm-level capabilities in struggling firms. Recent empirical work suggests that extremely narcissistic CEOs prefer to work for low-performing firms because of the opportunity for extreme and volatile levels of performance (Chatterjee & Hambrick, 2006). Taken together, these arguments suggest the following hypothesis:

_Hypothesis 2: Firm quality and executive quality will be negatively correlated._

4.1.3 No sorting in Executive Labor Markets

It might be that sorting does not occur and hence there is no significant correlation between firm and executive quality. Such a result might occur if, on average, the market is not particularly efficient at identifying which executives are the most talented. Or it may be that some highly performing firms are succeeding because of managerial capabilities and hence will tend to try to attract talented managers and some highly performing firms are succeeding because of firm routines and hence will not necessarily try and attract talented managers. From the executive side of the market we might see the same situation—namely, some talented executives might not be interested in working for low performing firms while other really talented executives have great idiosyncratic reasons for their interest in these firms. Taken together these arguments suggest the third hypothesis:

_Hypothesis 3: Firm quality and executive quality will not be correlated._
4.2 Methodology

Labor market outcomes (such as compensation contracts) result jointly from individual and firm decisions—a firm selects an executive to hire, but the executive must also choose to join the firm. Estimating the relationship between person and firm effects on compensation requires simultaneous study of both sides of the labor market (Abowd, Kramarz, and Margolis, 1999; Rosen, 1986; Willis, 1986). Abowd, Kramarz, and Margolis (1999) offer the first extensive statistical analysis of simultaneous individual-and firm-level heterogeneity in wage determination in a study of persistent inter-industry wage differentials in general (non-executive) labor markets. The methodology used in this paper draws heavily on their work, with necessary adaptations for the study of executive labor markets.

4.2.1 Model

The most common specification used in the labor literature (see Abowd et al. 1999) to estimate wage differentials based on a simultaneous analysis of individual- and firm-level heterogeneity is the following:

\[ y_{it} = \mu_y + (x_{it} - \mu_x)\beta + \mu_i + \theta_i + \psi_j + \epsilon_{it} \quad (4.1) \]

for \( i = 1, \ldots, N \) executives and \( j = 1, \ldots, J \) firms where \( y_{it} \) represents the logarithm of annual compensation for executive \( i \) in time \( t \), \( \mu_y \) is the grand mean of \( y_{it} \), \( X_{it} \) is a vector of observable, time-varying executive characteristics, \( \mu_x \) is the grand mean of \( X_{it} \), \( \mu_i \) is a vector of time-invariant observable individual characteristics. Permanent unmeasured differences between individuals (“person effects”) are represented by \( \theta_i \) and permanent
unmeasured differences between firms (“firm effects”) are represented by $\psi_j$. The estimation of these effects is of primary interest in this chapter.

4.2.2 Data and Sample

Implementing equation 4.1 so that simultaneous analysis of individual- and firm-level heterogeneity is possible requires the use of matched-longitudinal firm-executive data which connects groups of executives and firms based on the mobility of executives throughout their career. Such a dataset contains observations for multiple executives within the same firm as well as observations for these executives in the other firms they have worked at during their careers. Additionally, other executives that have spent time with these same employers during their career are included in the sample and grouped in with these initial executives. Various sets of these “connected” groups of firms and executives produce the needed variation to disentangle person and firm effects on compensation. For example, within these “connected” groups of firms and executives, data is contained on the compensation for a particular executive in more than one firm context as well as what those firms paid other executives.

Constructing the sample required a match between two databases. The first is Compustat’s Execucomp database, which contains the top five executives for firms in the S&P 500, MidCap 400, Small Cap 600 since 1992. This database contains all the relevant compensation information for the executives in this sample. The second database is the Marquis’ Who’s Who in Corporate Executives biographical file. Marquis collects educational, professional, and personal information about executives from various public and private
firms—specifically targeting Russell 1000 firms which are highly correlated with the set of
firms in the Execucomp database. A name and firm match was done between the two
databases, arriving at 1121 executives in both databases. These executives are the sample for
the paper.

4.2.3 Dependent Variable

Executive pay is typically composed of a base salary, an annual bonus tied to
accounting performance, stock options, and long-term incentive plans (e.g. restricted stock
and multi-year accounting based plans) (Murphy, 1999). Two methods, both common in the
executive compensation literature (Tosi et al, 2000), are used in this paper for grouping these
components of compensation to calculate the level of pay. The first method uses the total
cash pay which is the total current compensation for the executive comprised of salary and
bonus.

The second method uses the total pay which is the total compensation for the
executive comprised of salary, bonus, other annual compensation (e.g. perquisites, tax
reimbursements, etc), the total value of restricted stock grants, the total value of stock
options granted (using Black-Scholes), long-term incentive payouts, and all other total
compensation (e.g. signing bonuses, 401K contributions, debt forgiveness, life insurance
premiums, etc). Compensation is calculated for option grants instead of exercised options
because the timing for exercising options is arbitrarily determined by the executive whereas
the timing of option grants is determined contractually between the executive and the firm.

Total cash pay was prorated for instances in which an executive switches jobs in
the middle of a year. For example, if an executive worked at a firm from October through
December of the year, the salary reported in Execucomp would be substantially lower than if an entire year had been worked. Using this figure for compensation instead of a prorated amount would generate inaccurate estimates of the model. Total pay was not prorated as it appeared to not be substantially lower for an executive in the first year with a firm. This is most likely because total pay includes stock grants which are likely to be substantial in the initial year of employment.

4.2.4 Independent Variables

Time-variant variables for observable executive characteristics include proxies for experience (e.g. years of position-, firm-, and industry-specific experience as well as an adjusted industry-specific experience measure that only captures years of industry experience acquired prior to joining the current firm) and power (e.g. director, CEO, and founder status). Time-invariant variables for observable executive characteristics include variables such as education (time invariant since it is most likely completed and thereby fixed for executives) and demographics (gender).

All of these variables are constructed from the Marquis’ Who’s Who biographical entry for each executive in the sample. Industry-specific experience is calculated at the 2-digit level. Position-specific experience is calculated based on the following five position levels: Level 1: Chairman, Vice-Chairman, President, CEO; Level 2: C-level Executives (CFO, COO, and other chiefs); Level 3: Vice-President; Level 4: Functional Vice-President; Level 5: General Executive Positions. These levels were used to condense over 300 position classifications (generated by the author) based on the listing of positions held by each executive in his or her respective biographical entry in Marquis’ Who’s Who.
Rankings of education degrees were based on the current U.S. News and World Report rankings for each respective degree program.

4.2.5 Estimation

Equation 4.1 is estimated with a two-way error components model with crossed random effects. This approach, sometimes referred to as mixed effects, has two important features essential for estimating the model presented in this paper. First, with the mixed effects estimator the regression coefficients are estimated with fixed effects assumptions and the person and firm effects are estimated with random effects assumptions without assuming that the person and firm effects are uncorrelated with the explanatory variables. This is important since unmeasured person and firm heterogeneity is likely correlated with measured heterogeneity.

Secondly, since the sample contains crossed factors instead of nested factors, the mixed effects estimator is particularly important since it can be estimated with crossed random effects. The difference between crossed factors and nested factors in a sample is important to understand why analysis with crossed effects is fundamentally different from hierarchical linear modeling (HLM) with nested effects. Figure 3, adapted from Rabe-Hesketh and Skrondal (2005), illustrates the difference between crossed factors and nested factors.

When executives are nested within firms, an executive working at two different firms would statistically be treated as two different executives. Hence the information about the executive’s compensation in multiple firm contexts would not be incorporated into the

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estimation of the person effect on wage differentials. Likewise, when firms are nested within executives, the firm effect on compensation of working at firm 2, for example, would be different for executive A than for executive B. In contrast, with crossed effects the estimation of the firm effect on compensation of working at firm 2 would be the same for all three executives who worked at firm 2. Crossed factor models assume that there are specific things about a firm that would influence the compensation of all executives working at that firm in a similar way while nested factor models cannot account for such an assumption. Thus, using hierarchical linear modeling with nested effects to the estimate heterogeneity in executive compensation would only incorporate information within each executive-firm nesting instead of information across the factors. Hence, estimating crossed effects is essential to examining variation in individual compensation rates holding firm effects constant while simultaneously examining variation in firm compensation rates holding person effects constant; thus, providing empirical insight into the effects of permanent unmeasured differences among executives (person effects) and permanent unmeasured differences among firms (firm effects) on executive compensation.

4.3 Results

The central question raised in this paper is how the sorting process between executives and firm occurs in executive labor markets. To answer this question, equation 1 was estimated with a two-way error components model with crossed effects. Table 14 presents results for this estimation with both dependent variables—total cash pay and total pay. Recall that both dependent variables are demeaned to represent the wage differential
(e.g. Log (total cash Pay) – Ave [Log (total cash pay)]. Results are very stable across the different models.

The important estimates from this process, the correlation between firm and person effects on compensation, are also reported in Table 14. Consistent across both models in Table 14, is that a positive correlation is found between firm and person effects on compensation. Thus, high wage executives and high wage firms go together. In other words, this suggests that executives with higher than average person effects on compensation tend to be hired by firms with higher than average firm effects on compensation.

One interpretation of high person effects is higher levels of unmeasured human capital. In other words, “high wage” executives could be interpreted as more “talented” executives compared to “low wage” executives since these individuals across jobs in their career command wage differentials in excess of what their measurable human capital would predict.

There is some evidence to suggest that high wage firms are, in fact, more profitable than low wage firms. In Table 15, a quintile breakdown of firms based on Tobin’s q is presented. When using firm effects estimated from Model 1 in which compensation is measured as total pay instead of just cash pay, firms with the highest q in the sample also have the highest average firm effects. This is not the case for the model when firm effects are estimated based on cash pay differentials.

Ordinary least squares regressions are presented in Table 16 to demonstrate how predictive the firm effect is of firm performance as measured by Tobin’s q as well as five additional performance measures. With the exception of one performance measure (return on assets), high wage firms (in terms of total pay differentials) have higher levels of
performance than low wage firms. The results are not as definitive when looking at cash compensation differentials.

Arguably, models with total pay differentials are more interesting than those with just cash pay differentials since the majority of executive pay and the variance in pay is earned through stock grants and options. Using the results from Model 1 then, the analysis suggests some support for hypothesis 1 over the other hypotheses in this paper—that executives with higher than average person effects on wage tend to work for firms that more profitable. Firm quality and person quality appear to be positively correlated.

4.4 Discussion

Positive assortative matching on quality implies that firm-level capabilities and managerial capabilities are complements in generating superior firm performance. In other words, positive assortative matching on quality implies that high performing firms and more talented executives match up in labor markets and that the least talented executives would tend towards the least productive firms. The results of this paper support this conceptualization of the executive labor market.

Highly performing firms seek out and are able to hire the most talented executives in the labor market. This behavior on the part of firms is consistent with the idea that managers in highly performing firms are likely to attribute firm success to managerial talent instead of firm capabilities and routines. Or if there is a great deal of casual ambiguity associated with a firm’s high performance and it is therefore unclear whether the advantages the firm is enjoying are due to firm-level capabilities or managerial capabilities, managers in highly
performing firms are still likely to attribute firm success to managerial talent instead of firm level capabilities and routines.

That talented executives seem to be more interested in working for highly performing firms instead of poorly performing firms seems to support the idea that since high performing firms might be the most appealing jobs to executives because these firms might be in the best position to pay for the best executive talent and that these jobs might maximize future earnings for executives since being associated with a high performing firm is likely to increase an executive’s external wage rate (Fama, 1980; Khurana, 2004).
CHAPTER 5

SPLITTING THE PIE AT THE TOP:
EXECUTIVE COMPENSATION, VALUE CREATION, AND
VALUE DISTRIBUTION

During the bull market of the 1990s, corporate boards sought to align managerial and shareholder interests by tying executive compensation more closely to firm performance (Jensen, Murphy, & Wruck, 2004; Murphy, 1999). As a result, “median total realized [CEO] compensation (including gains from exercising stock options)...nearly quadrupled” (Murphy, 1999: 1). Perhaps the only thing rising faster than executive compensation during this period was the outrage over the level of executive compensation. Yet, scholars have argued that it is not how much executives make, but how executives are paid (e.g. independent of performance), that should generate outrage (Jensen & Murphy, 1990). Increasing levels of managerial power are hypothesized to account for inappropriate compensation schemes resulting in windfalls of compensation (Bebchuk, Fried, & Walker, 2002). Yet, in an era of plummeting executive tenure and sky-rocketing turnover, managerial power may not be as significant as it once was (Jenkins, 2002; Business Week, 2005).

Even still, “high” executive compensation does not necessarily mean that firm value is suffering because of executive compensation contracts. Implicit in agency theory is the assumption that compensation contracts can be constructed to align executive and shareholder interests such that executives are given incentive to adjust strategic behaviors to
maximize firm performance (Alchian and Demsetz, 1972; Fama and Jensen, 1983). Hence, a
great deal of theoretical (Holmstrom, 1982; Holmstrom and Milgrom, 1982) and empirical
(Brickley, Bhagat, and Lease, 1985; Larcker, 1983; Tehranian and Waegelein, 1985; Leonard,
1990; Lambert and Larcker, 1987) interest has been directed towards understanding whether
rent-sharing between shareholders and executives may actually generate value for at least
some corporations.\footnote{The theory laid out in this paper does not require that compensation contracts create incentive alignment between executives and shareholders in order to increase the value of the firm. It is possible that some executives are motivated by factors other than monetary remuneration (Donaldson and Lorsch, 1983) and that for some executives monetary remuneration does not cause them to work harder (Finkelstein and Hambrick, 1988; Barkema, 1993). What is of primary interest is whether or not compensation contracts have the effect of increasing the value of the firm \textit{regardless of} how individual executives are motivated by financial incentives.}

Empirically testing whether rent-sharing appropriately or inappropriately aligns
executive and shareholder interests requires carefully decomposing executive compensation
into the \textit{portable} part of the executive’s compensation that reflects the value of general human
capital in the external labor market (opportunity wages) and the \textit{non-portable} part of
compensation that reflects the portion of a firm’s quasi-rents the executive appropriates.
This study follows in the spirit of Abowd, Lengermann, and McKinney (2003) and Abowd,
Kramarz, Lengermann, and Roux (2005) who study rent-sharing as a potential explanation
for persistent inter-industry wage differentials in general (non-executive) labor markets.
Their approach emphasizes the role of labor and product market competition in jointly
determining wages:

Apart from labor market institutions, the intensity of competition prevailing on the
product market should affect wages…because wages are the sum of an opportunity
wage and a rent component. These rents are the product of workers’ bargaining
power, an outcome of labor market institutions…, and of the firms’ quasi-rent, an outcome of product market competition. (Abowd et al, 2005: 12).

While such analyses provide insight into how shareholders might share rent with a firm’s general labor pool, they do not tell us much about rent-sharing at different levels of the corporate hierarchy—for corporate executives, in particular. This paper provides a first step towards addressing this issue, by studying whether the practice of rent-sharing with corporate executives is associated with higher or lower levels of firm value.

To this end, this paper examines whether rent sharing is associated with higher or lower levels of firm value by estimating the relationship between the portion of an executive’s compensation associated with rent appropriation and the total quasi-rent generated by the firm. This is done by implementing a variant of the empirical methodology presented by Abowd and colleagues (2005) to decompose executive compensation into the portable (opportunity wage) and non-portable (rent sharing) components and then by relating rents appropriated by executives to total quasi-rents generated by firms.

The analysis conducted in this paper suggests that for a large majority of firms, rent-sharing aligns shareholder and executive interests and thereby maximizes corporate value rather than pitting the interests of executives and shareholders against each other. In particular, findings suggest that for approximately 81% of the corporations in this sample, any executive gains in compensation are associated with gains to the corporation’s total quasi-rent. Reasons why some firms are able to generate gains from rent sharing and other firms are not are also explored.
5.1 Incentive Alignment, Managerial Influence, and Firm Outcomes

In the context of executive compensation, agency theory assumes that compensation contracts can be constructed to align executive and shareholder interests such that executives are given incentives to maximize firm performance (Alchian and Demsetz, 1972; Fama and Jensen, 1983). Much empirical work has been devoted to demonstrating that such contracts actually result in executive behavior consistent with shareholder interests (e.g. Lambert, 1986; Larcker, 1984; Lambert and Larcker, 1984; Waegelein, 1983; Hoskission, Hitt, and Hill, 1993; Rapport, 1978). However, this work does not directly test whether or not creating an incentive system to solve the principal-agent problem actually results both in greater incentives for executives to behave according to shareholder interests and in greater profits for the firm (Leonard, 1990).

Implicit in this previous work is the assumption that greater incentives for executives to maximize firm performance will actually result in greater profits for the firm. This assumption depends upon executive discretion over firm outcomes. In short, executives need to be able to significantly influence firm outcomes for incentive alignment to even matter. Empirical work has generally been inconsistent with the notion of significant executive effects on firm performance (e.g. Lieberson and O’Connor, 1972). Research on the percentage of variance in firm performance explained by a firm’s CEO ranges from a low of 3.9% (Thomas, 1988) to a high of 14.7% (Wasserman, Nohria, and Anand, 2001). The size of this “CEO effect” is much smaller than the impact of industry and other firm attributes on a firm’s performance. Other scholars have also documented this relatively modest impact that leaders have on organizational performance in a variety of empirical settings (Bass, 1990; Hambrick and Finkelstein, 1996).
Recent empirical work argues that methodological limitations of the previous research have systematically understated the relative impact of CEOs on firm performance compared to industry and firm factors. That in fact, top executives can exert significant influence over firm outcomes—for both the good and the bad (Mackey, 2005). This influence varies across industries and within firms, but on average, executive effects may account for as much as 27% percent of the variance in firm performance (Mackey, 2005). Incentive alignment in the context of such significant managerial influence is likely to be of critical importance to shareholders.

5.2 Heterogeneity in Executive Compensation

As noted in Chapter 2, empirical work in the executive compensation literature indicates that executive compensation practices are extremely heterogeneous across firms and industries (Murphy, 1999). Some firms seemingly pay more than the “going wage” for an executive and other firms are able to pay less. One potential explanation for this is rent-sharing (e.g. Abowd, Kramarz, Lengermann and Roux, 2005).

For example, wages can be thought of the combination of the (1) market price for given observable executive characteristics (e.g. prior experience, education), (2) an adjustment based on the executive’s intangible talent or lack thereof (“person effects”), and (3) the firm specific compensation policies of the firm that is offering the wage (“firm effects”). The last two components, person and firm effects, reflect adjustments to what the market price for a given executive. Person effects, for example, capture systematic premiums or discounts to the market wage that given executives earn throughout their career due to the intangible nature of an human capital. In other words, above average person effects on
wages suggest that an executive earns more than the market would predict for someone with the same observable characteristics.

Firm effects, the firm-specific adjustment to the wage, account for factors about a firm that influence the systematic over or underpayment of executives with the same observable characteristics. For example, suppose a firm is large or poorly governed and therefore pays more than the market would bear for given executive characteristics. The wage would be adjusted accordingly through the firm effect. The firm-specific adjustment to the wage could also reflect the additional compensation given to an executive because of a particularly good person-firm match capable of creating value firm synergies. Likewise, the firm-specific adjustment of the wage could also reflect less compensation given to an executive because of a particularly poor person-firm match. One interpretation of the firm effect is rent sharing since it reflects compensation above and beyond the executive’s opportunity wage.

These person- and firm-specific “adjustments” to the wage provide insight into the central question of the paper. For example, are these person and firm effects on wage (“adjustments” above and below what the market price for an executive is) related to the total quasi-rents generated by the firm? In other words, does being a “high wage” firm (i.e. as a firm you systematically pay more than the market is willing to bear for executives) increase the value of the quasi-rents generated?
5.3 Methodology

The methodology used in this paper has two components. The first component is a “compensation differential model.” Here, the model estimates coefficients for several observable executive-specific variables and, simultaneously, estimates unobserved person and firm effects on wage differentials. The second component is a ‘compensation bargaining model,’ which estimates the relationship between these unobserved person and firm effects and the firm’s quasi-rent. The following section explains the estimation, data, sample, and measures for the model for the “compensation differential model.” The ‘compensation bargaining model,’ is then introduced in a similar fashion.

5.3.1 Compensation Differential Model

As introduced in Chapter Three, the most common specification used in the labor literature (see Abowd et al. 1999) to estimate wages, based on a simultaneous analysis of individual- and firm-level heterogeneity, is the following:

\[
y_{it} = \mu_y + (x_{it} - \mu_x) \beta + \mu_i \eta + \theta_i + \psi_j + \epsilon_{it} \tag{5.1}
\]

for \( i = 1, \ldots, N \) executives and \( j = 1, \ldots, J \) firms where \( y_{it} \) represents the logarithm of annual compensation for executive \( i \) in time \( t \), \( \mu_y \) is the grand mean of \( y_{it} \), \( x_{it} \) is a vector of observable, time-varying executive characteristics, \( \mu_x \) is the grand mean of \( x_{it} \), and \( \mu_i \) is a vector of time-invariant observable individual characteristics. The permanent unmeasured differences between individuals (labeled the person effect) is represented by \( \theta_i \) and the permanent unmeasured differences between firms (labeled the firm effect) is represented by \( \psi_j \).
In such a specification, individuals with a greater than average person effect, $\theta_i$, are considered (and described in the literature as) as “high-wage workers” since these are individuals who are paid more than expected based on their observable characteristics (i.e. the time varying and time invariant individual characteristics in the model) (Abowd et al, 1999). Likewise, firms with compensation higher than expected given these same observable executive characteristics are referred to as “high-wage firms”. These firms are identified as those with greater than average firm effects, $\psi_j$ (Abowd et al, 1999; Abowd et al, 2004).

Equation 5.1 is estimated with a two-way error components model with crossed random effects. This approach, sometimes referred to as mixed effects, has two important features essential for estimating the model presented in this paper. First, with the mixed effects estimator the regression coefficients are estimated with fixed effects assumptions and the person and firm effects are estimated with random effects assumptions without assuming that the person and firm effects are uncorrelated with the explanatory variables. This is important since unmeasured person and firm heterogeneity is likely correlated with measured heterogeneity.

Second, since this sample of corporate executives contains crossed factors instead of nested factors, the mixed effects estimator is particularly helpful since it can be estimated with crossed random effects. There are important differences between samples containing crossed factors and nested factors. These differences underscore why analysis with crossed effects is necessary in lieu of traditional approaches such as hierarchical linear modeling (HLM) with nested effects.

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As shown in Figure 3, adapted from Rabe-Hesketh and Skrondal (2005), in a nested factors model, estimating the compensation equation yields two different firm effects for executives A and B working at firm 2. In contrast, a crossed factors model yields an estimated firm effect for firm 2 that is the same for all three executives who worked at firm 2. Thus, crossed factors models have the advantage of considering firm factors that influence the compensation of all executives working at a particular firm in a similar way, while nested factors models assume a differential impact of firm effects on each individual. Thus, a model using hierarchical linear modeling with nested effects to the estimate heterogeneity in executive compensation would only incorporate information for each executive-firm combination instead of information across the factors. Hence, it is essential to use a crossed effects approach so that the effects of individual and firm heterogeneity on compensation can be appropriately disentangled.

5.3.1.1 Data and Sample

Implementing equation 1 so that simultaneous analysis of individual- and firm-level heterogeneity is possible requires the use of matched-longitudinal firm-executive data which connects groups of executives and firms based on the mobility of executives throughout their career. Such a dataset contains observations for multiple executives within the same firm as well as observations for these executives in the other firms they have worked at during their careers. Additionally, other executives that have spent time with these same employers during their career are included in the sample and grouped in with these initial executives. Various sets of these “connected” groups of firms and executives produce the needed variation to disentangle person and firm effects on compensation. For example,
within these “connected” groups of firms and executives, data is contained on the compensation for a particular executive in more than one firm context as well as what those firms paid other executives.

Constructing the sample required a match between two databases. The first is Compustat’s Execucomp database, which contains the top five executives for firms in the S&P 500, MidCap 400, Small Cap 600 since 1992. This database contains all the relevant compensation information for the executives in this sample. The second database is the Marquis’ Who’s Who in Corporate Executives biographical file. Marquis collects educational, professional, and personal information about executives from various public and private firms—specifically targeting Russell 1000 firms which are highly correlated with the set of firms in the Execucomp database. A name and firm match was done between the two databases, arriving at 1121 executives in both databases. These executives are the sample for the paper.

5.3.1.2 Dependent Variable

Executive pay is typically composed of a base salary, an annual bonus tied to accounting performance, stock options, and long-term incentive plans (e.g. restricted stock and multi-year accounting based plans) (Murphy, 1999). Two methods, both common in the executive compensation literature (Tosi et al, 2000), are used in this paper for grouping these components of compensation to calculate the level of pay. The first method uses the total cash pay which is the total current compensation for the executive comprised of salary and bonus.
The second method uses the **total pay** which is the total compensation for the executive comprised of salary, bonus, other annual compensation (e.g. perquisites, tax reimbursements, etc), the total value of restricted stock grants, the total value of stock options granted (using Black-Scholes), long-term incentive payouts, and all other total compensation (e.g. signing bonuses, 401K contributions, debt forgiveness, life insurance premiums, etc). Compensation is calculated for option grants instead of exercised options because the timing for exercising options is arbitrarily determined by the executive whereas the timing of option grants is determined contractually between the executive and the firm.

**Total cash pay** was prorated for instances in which an executive switches jobs in the middle of a year. For example, if an executive worked at a firm from October through December of the year, the salary reported in Execucomp would be substantially lower than if an entire year had been worked. Using this figure for compensation instead of a prorated amount would generate inaccurate estimates of the model. **Total pay** was not prorated as it appeared to not be substantially lower for an executive in the first year with a firm. This is most likely because total pay includes stock grants which are likely to be substantial in the initial year of employment.

### 5.3.1.3 Independent Variables

Time-variant variables for observable executive characteristics include proxies for **experience** (e.g. years of position-, firm-, and industry- specific experience as well as an adjusted industry-specific experience measure that only captures years of industry experience acquired prior to joining the current firm) and **power** (e.g. director, CEO, and founder status). Time-invariant variables for observable executive characteristics include variables
such as education (time invariant since it is most likely completed and thereby fixed for executives) and demographics (gender).

All of these variables are constructed from the Marquis’ Who’s Who biographical entry for each executive in the sample. Industry-specific experience is calculated at the 2-digit level. Position-specific experience is calculated based on the following five position levels: Level 1: Chairman, Vice-Chairman, President, CEO; Level 2: C-level Executives (CFO, COO, and other chiefs); Level 3: Vice-President; Level 4: Functional Vice-President; Level 5: General Executive Positions. These levels were used to condense over 300 position classifications (generated by the author) based on the listing of positions held by each executive in his or her respective biographical entry in Marquis’ Who’s Who. Rankings of education degrees were based on the current U.S. News and World Report rankings for each respective degree program.

5.3.2 Compensation Bargaining Model

As the central issue in this paper is to understand whether rent sharing is associated with higher or lower levels of firm value, the second aspect of our analysis estimates the relationship between the portion of an executive’s compensation associated with rent appropriation and the total quasi-rent generated by the firm. This is accomplished by adopting a model of compensation bargaining that uses estimates from the wage determination model. A variant of the empirical methodology presented by Abowd et al (2005) is implemented to decompose executive compensation into its portable (opportunity wage) and non-portable (rent sharing) components. This enables us to then understand the
relationship between the rents appropriated by executives to total quasi-rents generated by firms.

Abowd et al (2005) present a simple bargaining model of wage determination, where wages, \( w_i \), are composed of an opportunity wage, \( x_i \), and the individual’s portion of the firm’s quasi-rent (bargaining power, \( \gamma \), multiplied by the quasi-rent, \( QR_{j(i)} \)) is introduced. Time subscripts are omitted for simplicity:

\[
 w_i = x_i + \gamma (QR_{j(i)}) \tag{5.2}
\]

The expectation of equation 5.2 is taken:

\[
 E[w_i] = \mu_w = \mu_x + \gamma \mu_{QR} \tag{5.3}
\]

Then the opportunity wage and the firm’s quasi-rent are rewritten according to equations 5.4 and 5.5, respectively:

\[
 x_i = \mu_x + \xi_i \tag{5.4}
\]

\[
 QR_{j(i)} = \mu_{QR} + \rho_{j(i)} \tag{5.5}
\]

Collecting terms in equations 5.6-5.8, Abowd et al (2005) show that

\[
 w_i = \mu_w + x_i - \mu_x + \gamma (QR_{j(i)} - \mu_{QR}) \tag{5.6}
\]

\[
 w_i = \mu_w \left( 1 + \frac{x_i - \mu_x}{\mu_w} + \gamma \frac{QR_{j(i)} - \mu_{QR}}{\mu_w} \right) \tag{5.7}
\]

\[
 w_i = \mu_w \left( 1 + \theta_i + \gamma \frac{\rho_{j(i)}}{\mu_w} \right) \tag{5.8}
\]
results in a first-order approximation of the wage bargaining equation in a log-separable format (equation 5.9):

\[
\ln w = \ln \mu_w + \theta_i + \psi_{j(i)} \quad (5.9)
\]

where the person effects, \( \theta_i \equiv \frac{\xi_i}{\mu_w} \), are identified as opportunity wages and the firm effects, \( \psi_{j(i)} = \gamma \frac{\rho_{j(i)}}{\mu_w} \), are identified as real measures of the share of the quasi-rents that executives appropriate. It is worth noting that in the measure of the share of the quasi-rents going to executives there are two variables at play—the bargaining power of the executive, \( \gamma \), and the quasi-rent, \( QR_{j(i)} - \mu_{QR} \), which is related to product market competition.

To implement the compensation equation described in equation 8, we follow Abowd et al (2005) and Abowd, Kramarz and Margolis (1999), using a simple linear statistical model described in equation 5.1. To keep notation consistent with the above and adding time subscripts, equation 5.1 is re-written as:

\[
w_{it} = \mu_y + (x_{it} - \mu_x) \beta + \mu_\eta + \theta_i + \psi_{j(i)} + \epsilon_{it} \quad (5.10)
\]

for \( i = 1, \ldots, N \) executives and \( j = 1, \ldots, J \) firms, where \( w_{it} \) represents the logarithm of annual compensation for executive \( i \) in time \( t \), \( \mu_y \) is the grand mean of \( y_{it} \), \( x_{it} \) is a vector of observable, time-varying executive characteristics, \( \mu_x \) is the grand mean of \( X_{it} \), and \( \mu_\eta \) is a vector of time-invariant observable individual characteristics. The permanent unmeasured differences between individuals (the person effect) is represented by \( \theta_i \) and the permanent
unmeasured differences between firms (the firm effect) is represented by $\psi_{j(i,t)}$. As is standard, the statistical error term is represented by $\varepsilon_{it}$.

Person and firm effects estimated in the compensation equation (equation 5.10) are then related to the total firm quasi-rent. Specifically, the following equations are estimated:

$$\theta_i = (QR_{j(i,t)})\gamma + \varepsilon_{it} \quad (5.11)$$
$$\psi_{j(i,t)} = (QR_{j(i,t)})\gamma + \varepsilon_{it} \quad (5.12)$$

where $\theta_i$ is the estimated vector of person-effects obtained from equation 9; $\psi_{j(i)}$ is the estimated vector of firm-effects obtained from equation 5.10; and $QR_{j(i,t)}$ is a vector of the total quasi-rents. This quasi-rent includes the portion which went to the shareholders of the firm as well as the portion which was appropriated by the executive, the calculation of which is described below. $\gamma$ is a vector of coefficients to be estimated and $\varepsilon_{it}$ is the statistical error term.

Two controls, which might help explain the respective person and firm effects, are also introduced into the estimation: governance and firm size. This yields the following equations to be estimated:

$$\theta_i = (QR_{j(i,t)})\gamma + g_j \beta_1 + s_j \beta_2 + \varepsilon_{it} \quad (5.13)$$
$$\psi_{j(i,t)} = (QR_{j(i,t)})\gamma + g_j \beta_1 + s_j \beta_1 + \varepsilon_{it} \quad (5.14)$$

where $g_j$ is a measure of how well governed the firm is, $s_j$ is measure of firm size. The measures for these variables are also described below.
5.3.2.1 Dependent Variables

Following Abowd et al (2005), the second stage of the analysis examines whether or not the total firm quasi-rent explains more of the variance in person effects than in firm effects. Doing so requires using two dependent variables estimated in the first stage of our analysis—namely, our estimates for the person and firm effects. The compensation differential equation considered several observed executive characteristics described earlier: experience (position-specific, firm-specific, and industry-specific experience captures industry experience acquired prior to joining the current firm), power (director, CEO, and founder status), and demographics (gender). This yields (i) a person effect which reflects any unobserved person-specific characteristics producing compensation differences among executives and (ii) a firm effect which reflects any firm-specific characteristics producing compensation differences among executives. Our firm effect is “pure” in the sense that observed firm-characteristics are not included in the compensation decomposition.

5.3.2.2 Independent Variables

The measure for the firm’s total quasi-rent must include both the portion of the firm’s quasi-rent that went to a corporation’s shareholders as well as the portion of the quasi-rent that went to the executive. Net Income, obtained from Compustat’s Industrial File, is used to measure the portion of a firm’s quasi-rent that went to the corporation’s shareholders. (Hence, this measure does not include quasi-rents already appropriated by non-executive employees).
The portion of the quasi-rent that went to the executives is less straightforward. Estimated in natural logs and then exponentiated, the person effect from the compensation equation is the opportunity cost of time (not including the time varying aspect), whereas the firm effect from the compensation equation reflects the quasi-rent appropriated by the individual executive (see Abowd, et al, 2005 for a discussion). Using this insight, we must then put this in levels comparable to our measure of the shareholders portion of the quasi-rent (net income). To do so, for each executive we must obtain a dollar value for the non-portable portion of their compensation. For this, we take an executive’s total compensation and subtract from it the portable portion of their compensation, where the portable portion (omitting time subscripts) is \( \frac{\ln w_i}{\psi_{j(i)}} \). Thus, the total quasi-rent for the corporation is measured as (omitting time subscripts):

\[
QR_{Total} = \ln(NI) + \left[ \ln w_i - \frac{\ln w_i}{e^{\psi_{j(i)}}} \right] 
\]  

(5.15)

For this analysis, two additional variables are considered to get at the following questions: Does the size of an executive’s employer account for the firm-level heterogeneity producing compensation differentials? And similarly, Does poor corporate governance account for the firm-level heterogeneity producing compensation differentials? For firm size, we take the natural log of a corporation’s sales, obtained from Compustat’s Industrial File. To measure how well-governed a corporation, we use the Gompers, Metrick, and Ishii Index, obtained from Compustat’s IRRC file.

\[\text{Notice that we are taking out the portable part of an executive’s compensation from their total compensation using the estimated firm effect.}\]
5.4 Results

First, the *portable* and *non-portable* (rent sharing) part of an executive’s compensation is estimated, using equation 5.10. These results are presented in Table 17. The *portable* part consists of measured and unmeasured person effects on compensation, while the *non-portable* part consists of unmeasured firm effects on compensation. The second stage of analysis uses the estimates of the non-portable part of an executive’s compensation to estimate whether or not rent sharing is associated with higher or lower total rents for the firm.

Table 18 provides estimates for equations 5.11—5.14. First, notice that the coefficient on the total quasi-rent, $\gamma$ is positive in all models, including those with controls. While it is significant in the person-effect regression, the size of $\gamma$ is small (around .067). The size of $\gamma$ in the firm-effect regression is also significant, positive, and much larger (around 0.213). The positive $\gamma$ in this firm-effect regression suggests that any gains to the compensation system are associated with gains to the corporation’s total quasi-rent.

Looking more deeply, the distribution of coefficient estimates from equation 5.12 is provided in Figure 10. This histogram reports the estimated $\gamma$ for each firm in the sample. If increases in rent sharing are associated with increases in total quasi-rents for the firm, the value on the x-axis, $\gamma$, will be positive. Approximately 81% of the firms in the sample have estimated $\gamma$ coefficients which are positive. One interpretation of these results is that 81% of the firms in the sample are reaping gains from the compensation contracting process. Hence, for the large majority of firms, rent-sharing with executives actually increases the firm’s total quasi-rent, suggesting that executives maximize corporate value.
Of immediate interest is why some firms are able to reap gains from rent sharing and other firms are not able to do so. In other words, why do executive compensation contracts maximize the value of some firms while pitting the interests of shareholders against those of executives in other firms? To explore these questions, equations 5.13 and 5.14 are estimated in the next two columns in Table 18, where a governance index and a measure of firm size are added as controls.

Does poor corporate governance account for any firm-level heterogeneity producing compensation differentials? First, notice that the coefficient for corporate governance is not significant\(^\text{14}\) and does not meaningfully change the coefficient on the quasi-rent (\(\gamma\)) in either regression. This means that governance (how well or poorly governed a firm is) does not explain firm-level heterogeneity producing compensation differentials. To investigate this further, the sample is split into firms with positive and negative estimated \(\gamma\) coefficients and the respective governance index estimates are compared (See Table 19). Notice that this index does not differ much between these groups. Hence, governance, as measured by takeover defenses in the Gompers, Metrick, and Ishii Index, does not appear to impact whether (or not) the executive creates value for the firm. Future investigation into the role of governance in whether rent-sharing will be positively or negatively associated with the firm’s total quasi-rent need to be expanded with additional proxies for governance.

Does the size of an executive’s employer account for whether (or not) the compensation contract maximizes the value of the firms or pits the interests of shareholders against those the executives? Table 19 shows the large difference in size between firms that

---

\(^{14}\) To clarify, if it were significant, the negative value on this coefficient would imply that firms which are better governed explain more of the firm-effect.
are reaping gains from rent-sharing and those that are not able to do so. Larger firms tend to not reap gains from rent sharing while smaller firms do (average firm size in terms of sales is 16.577 billion for those not making gains and 8.403 billion for those making gains).

5.5 Discussion

Ideally, executive compensation contracts are designed to maximize the value of the firm. Expressions of outrage over executive compensation seem to imply that reality is far from this ideal—that in fact, academics and commentators from the business community alike have suggested that executive compensation contracts only serve to pit the interests of shareholders against those of executives, instead of serving to maximize the value of the firm.

However, one interpretation of the results of this analysis is that for most firms, executive compensation practices are working. High-rent sharing with executives is associated with higher levels of total firm quasi-rents for about 81% of the firms in the sample. Further empirical inquiry is needed to test the robustness of these results to other measures of compensation and to other measures of quasi-rents.

It does not appear that differences in governance, profitability (ROA or Tobin’s q) explain when firms are able to see gains from the contracting process and when they are not. It does appear that firms with losses from rent sharing pay their executives about 12% more than firms with gains from rent sharing. This wage differential is deceptive for the following reason. As noted in the results section, firm size does appear to explain some of the differences in firms that are able to see gains from the contracting process and those that are
not. Since firms with losses from rent sharing are twice as large (in terms of sales) as those with gains from rent-sharing, wage differentials would be expected to be much larger than 12% more because of the established firm-size effect on executives wages. Factoring in size differences, firms making gains from rent sharing appear to be paying premiums to attract executive talent. Based on growth rates between these sets of firms, it seems likely that executives in these smaller firms have more opportunity to create value than executives in larger firms. In sum, executive compensation and subsequent rent sharing benefits shareholders when the executive has opportunity for value creation.

If these results hold to further empirical scrutiny, it would seem to suggest that most of the outrage over executive pay should be centered on situations which little discretion exists for executives to create value. It is in these situations that executive rent sharing will not be likely to maximize the value of the firm.

This paper does not directly address what level of salary for executives is “fair.” It may be that in some instances the rent sharing for the executive exceeds the value he or she is personally responsible for creating while in other cases the rent sharing for the executive falls short of the value created. Answering such a question requires knowing exactly what portion of the firm’s total quasi-rent the executive was directly or indirectly responsible for creating and then assessing what the firm’s “opportunity executive” would cost. What has been suggested in this paper is that even if executives bargain excessive amount of the rent from shareholders, the majority of firms are still seeing the total value of the firm maximized through the compensation contracting process.

In conclusion, efforts to align managerial and shareholder interests by tying executive compensation more closely to firm performance may have resulted in skyrocketing levels of
salary for executives (Murphy, 1999), but for the far majority of firms, this process may have done what it set out to do—namely, maximize the value of the firm.
LIST OF REFERENCES


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Mackey, T. B., Kiousis, P. K., & Barney, J. B. 2005. *Is the industry effect constant over time?* Working Paper, The Ohio State University Fisher College of Business, Columbus, OH.


APPENDIX A

TABLES AND FIGURES
Table 1: Prior Empirical Results on the Decomposition of Variance in Organizational Performance

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<td>-</td>
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Table 1: Prior Empirical Results on the Decomposition of Variance in Organizational Performance
Table 2: Limitations of Prior Empirical Work

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Table 3: Descriptive Statistics

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Table 4: Replication of Prior Empirical Studies
Table 5: How Much Do CEOs Firm Performance? A Comparison of CEO Impact on Corporate Versus Business-Segment Performance

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| N | 848 | 848 |

Table 5: How Much Do CEOs Firm Performance? A Comparison of CEO Impact on Corporate Versus Business-Segment Performance
<table>
<thead>
<tr>
<th></th>
<th>Coefficient</th>
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<tbody>
<tr>
<td>Corporate Sales</td>
<td>0.00**</td>
<td>(0.00)</td>
</tr>
<tr>
<td>Corporate Diversification</td>
<td>0.082**</td>
<td>(0.027)</td>
</tr>
<tr>
<td>Industry Competitors</td>
<td>-0.002**</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Competitors with CEO Turnover</td>
<td>1.94***</td>
<td>(0.055)</td>
</tr>
<tr>
<td>Relative Corporate ROA</td>
<td>0.25</td>
<td>(0.128)</td>
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<tr>
<td>Constant</td>
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<td>(0.029)</td>
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<td>N</td>
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<tr>
<td>Log Likelihood</td>
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<tr>
<td>Likelihood Ratio ~ χ²(5)</td>
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<td>Prob &gt; χ²</td>
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*** p<.001, ** p<.01, * p<.05

Table 6: Probit Estimates for Table 2.7
<table>
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<th>Models Corrected for Sample Selection Bias</th>
<th>Models Not Corrected for Sample Selection Bias</th>
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<tbody>
<tr>
<td>DV: Corporate ROA</td>
<td>DV: Segment ROA</td>
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<tr>
<td><strong>ANOVA estimate</strong></td>
<td><strong>% of total</strong></td>
</tr>
<tr>
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<tr>
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<td>Corporate Effect</td>
<td>83613.4</td>
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<tr>
<td>CEO Effect</td>
<td>81644.7</td>
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<tr>
<td>Segment Effect</td>
<td>--</td>
</tr>
<tr>
<td>Lambda</td>
<td>53.4†</td>
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<tr>
<td>Residual</td>
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<tr>
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</tr>
<tr>
<td>N</td>
<td>8522</td>
</tr>
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<table>
<thead>
<tr>
<th><strong>Year Effect</strong></th>
<th><strong>Industry Effect</strong></th>
<th><strong>Corporate Effect</strong></th>
<th><strong>CEO Effect</strong></th>
<th><strong>Segment Effect</strong></th>
<th><strong>Lambda</strong></th>
<th><strong>Residual</strong></th>
<th><strong>Total SS</strong></th>
<th><strong>N</strong></th>
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<td>2738.3</td>
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<td>5461.2</td>
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<td>90.92</td>
<td>4.6</td>
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<td>154.45</td>
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<tr>
<td>CEO Effect</td>
<td>81655.2</td>
<td>23.8</td>
<td>150.21</td>
<td>7.6</td>
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<tr>
<td>Segment Effect</td>
<td>--</td>
<td>--</td>
<td>685</td>
<td>34.4</td>
<td></td>
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<td>Residual</td>
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<td>N</td>
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*p <.05, †p <.1

Table 7: Do Sample Restrictions Create Sample Selection Bias? A Comparison of ANOVA Estimates with and without Correcting for Selection Bias From Restricting Sample to only with Firms with CEO Turnover
<table>
<thead>
<tr>
<th>Coefficient</th>
<th>Coefficient</th>
<th>Std Errors in Parentheses</th>
</tr>
</thead>
<tbody>
<tr>
<td>Corporate Sales</td>
<td>4.74 X 10^-6</td>
<td>(1.22 X 10^-4)</td>
</tr>
<tr>
<td>Corporate Diversification</td>
<td>0.206***</td>
<td>(0.044)</td>
</tr>
<tr>
<td>Industry Competitors</td>
<td>0.005***</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Competitors with CEO</td>
<td>0.228**</td>
<td>(0.072)</td>
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<tr>
<td>Turnover</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Relative Corporate ROA</td>
<td>-0.252</td>
<td>(0.194)</td>
</tr>
<tr>
<td>Constant</td>
<td>-1.86***</td>
<td>(0.49)</td>
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N 13550
Log Likelihood -3135.37
Likelihood Ratio ~ χ²(5) 71.10
Prob > χ² 0.000

*** p<.001, ** p<.01, * p<.05

Table 8: Probit Estimates for Table 9
### Models Corrected for Sample Selection Bias

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<tr>
<th></th>
<th>DV: Corporate ROA</th>
<th></th>
<th>DV: Segment ROA</th>
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<tr>
<td></td>
<td>ANOVA estimate</td>
<td>% of total</td>
<td>ANOVA estimate</td>
<td>% of total</td>
</tr>
<tr>
<td>Year Effect</td>
<td>179.9</td>
<td>0.7%</td>
<td>0.454</td>
<td>1.3%</td>
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<tr>
<td>Industry Effect</td>
<td>1728.1</td>
<td>6.3%</td>
<td>2.63</td>
<td>7.5%</td>
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<tr>
<td>Corporate Effect</td>
<td>2035.2</td>
<td>7.4%</td>
<td>2.43</td>
<td>6.9%</td>
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<tr>
<td>CEO Effect</td>
<td>7623.7</td>
<td>27.6%</td>
<td>4.53</td>
<td>12.9%</td>
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<td>Segment Effect</td>
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<td>6.21</td>
<td>17.6%</td>
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<td>Lambda</td>
<td>38.97*</td>
<td>0.1%</td>
<td>0.064†</td>
<td>0.2%</td>
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<td>5991.1</td>
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<td>12.0</td>
<td>34.1%</td>
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### Models Not Corrected for Sample Selection Bias

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<th>DV: Segment ROA</th>
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</thead>
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<tr>
<td></td>
<td>ANOVA estimate</td>
<td>% of total</td>
<td>ANOVA estimate</td>
<td>% of total</td>
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<td>.405</td>
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<td>35.2</td>
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* p < .05, † p < .1

Table 9: Do Sample Restrictions Create Sample Selection Bias? A Comparison of ANOVA Estimates with and without Correcting for Selection Bias From Restricting Sample to only Firms with Mobile CEOs
Figure 1: CEO Observations Perfectly Nested Within Corporate Observations

<table>
<thead>
<tr>
<th>Corporation_1</th>
<th>Corporation_2</th>
<th>CEO_1</th>
<th>CEO_2</th>
<th>CEO_3</th>
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Figure 2: Observations Without CEO Turnover

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Table 10: Estimates from Two-Way Random Parameters Model with Crossed Factors

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<td>Firm-specific (yrs)</td>
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<td>0.012***</td>
<td>0.027***</td>
<td>0.011***</td>
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<td></td>
<td>(0.002)</td>
<td>(0.002)</td>
<td>(0.002)</td>
<td>(0.002)</td>
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<tr>
<td>Industry-specific (yrs outside firm in industry)</td>
<td>0.027***</td>
<td>0.011***</td>
<td>0.026***</td>
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<td>Position-specific (yrs)</td>
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<td>0.009***</td>
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<td>0.299***</td>
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<td>(0.031)</td>
<td>(0.034)</td>
<td>(0.031)</td>
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<td>(0.107)</td>
<td>(0.089)</td>
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<td>Female Dummy</td>
<td>-0.318***</td>
<td>-0.127*</td>
<td>-0.174*</td>
<td>-0.033</td>
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<td>-0.219***</td>
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<td>(0.046)</td>
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<td>Elite MBA degree (Rank 1-10)</td>
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<td>0.097</td>
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<td>(0.095)</td>
<td>(0.07)</td>
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<td>Person Effect (estimate of std deviation)</td>
<td>0.55</td>
<td>0.36</td>
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<td>Firm Effect (estimate of std deviation)</td>
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<td>Correlation (Person, Firm)</td>
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<td>1123</td>
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<td><strong>No. of firms</strong></td>
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p<.01***, p<.05**, p<.10*
<table>
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<th>Model 1 DV: Total Pay</th>
<th>Model 2 DV: Cash Pay</th>
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<td>Centile</td>
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<td>50</td>
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<td>90</td>
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Table 11: Are Person or Firm Effects More Important in Determining Wage Differentials?
<table>
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<tr>
<th></th>
<th>Raw Wage Differential</th>
<th>Average Measured Person Effects</th>
<th>Average Firm Effect</th>
<th>Average Person Effect</th>
<th>Observations</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Male Executives</strong></td>
<td>0.615</td>
<td>1.096</td>
<td>-0.374</td>
<td>-0.108</td>
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<td></td>
<td>(1.117)</td>
<td>(0.445)</td>
<td>(0.700)</td>
<td>(0.407)</td>
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<tr>
<td><strong>Female Executives</strong></td>
<td>0.162</td>
<td>0.704</td>
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<td>(0.683)</td>
<td>(0.919)</td>
<td>(0.443)</td>
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<td><strong>Male - Female</strong></td>
<td><strong>0.453</strong>*</td>
<td><strong>0.392</strong>*</td>
<td><strong>0.047</strong></td>
<td><strong>-0.011</strong></td>
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<td>(0.374)</td>
<td>(0.644)</td>
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<td>(1.130)</td>
<td>(0.430)</td>
<td>(0.702)</td>
<td>(0.411)</td>
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<td><strong>Directors- Non-Directors</strong></td>
<td><strong>0.051</strong></td>
<td><strong>0.753</strong>*</td>
<td><strong>-0.466</strong>*</td>
<td><strong>-0.103</strong>*</td>
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<tr>
<td><strong>Non-Founders</strong></td>
<td>0.616</td>
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<td>-0.377</td>
<td>-0.104</td>
<td>1864</td>
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<td>(1.124)</td>
<td>(0.457)</td>
<td>(0.711)</td>
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<td><strong>Founders</strong></td>
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<td>(1.064)</td>
<td>(0.444)</td>
<td>(0.664)</td>
<td>(0.344)</td>
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<td><strong>-0.196</strong></td>
<td><strong>-0.182</strong>*</td>
<td><strong>0.018</strong></td>
<td><strong>-0.049</strong></td>
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<td><strong>No UG Degree</strong></td>
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<td>1316</td>
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<td>(1.127)</td>
<td>(0.479)</td>
<td>(0.739)</td>
<td>(0.430)</td>
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<tr>
<td><strong>UG Degree- No UG Degree</strong></td>
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<td><strong>-0.119</strong>*</td>
<td><strong>0.219</strong></td>
<td><strong>0.146</strong>*</td>
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<td>(1.116)</td>
<td>(0.494)</td>
<td>(0.740)</td>
<td>(0.427)</td>
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<td><strong>MBAs</strong></td>
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<td>(1.131)</td>
<td>(0.427)</td>
<td>(0.695)</td>
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<td><strong>MBAs- No MBA</strong></td>
<td><strong>0.307</strong>*</td>
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<td><strong>0.238</strong>*</td>
<td><strong>0.050</strong>*</td>
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<tr>
<td><strong>MBA from Top Ten School</strong></td>
<td>0.664</td>
<td>1.121</td>
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<td>210</td>
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<td>(1.156)</td>
<td>(0.440)</td>
<td>(0.780)</td>
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<tr>
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<td>0.609</td>
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<td>(0.744)</td>
<td>(0.419)</td>
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<td>(0.395)</td>
<td>(0.679)</td>
<td>(0.473)</td>
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<tr>
<td><strong>JD Degree – No JD Degree</strong></td>
<td><strong>0.008</strong></td>
<td><strong>0.051</strong></td>
<td><strong>-0.036</strong></td>
<td><strong>0.006</strong></td>
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</tr>
</tbody>
</table>

Table 12: Person Versus Firm Effects in Determining CEO Wage Differentials. Reported significance of the differences between groups is taken from 2-tailed t-tests.
<table>
<thead>
<tr>
<th></th>
<th>Raw Wage Differential</th>
<th>Average Measured Person Effects</th>
<th>Average Firm Effect</th>
<th>Average Person Effect</th>
<th>Observations</th>
</tr>
</thead>
<tbody>
<tr>
<td>Male Executives</td>
<td>-0.221</td>
<td>0.083</td>
<td>-0.212</td>
<td>-0.064</td>
<td>4886</td>
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<td>Female Executives</td>
<td>-0.418</td>
<td>-0.484</td>
<td>0.058</td>
<td>0.003</td>
<td>296</td>
</tr>
<tr>
<td><strong>Male - Female</strong></td>
<td><strong>0.196</strong>*</td>
<td><strong>0.567</strong>*</td>
<td><strong>0.270</strong>*</td>
<td><strong>0.067</strong>*</td>
<td></td>
</tr>
<tr>
<td>Non-directors</td>
<td>-0.347</td>
<td>-0.152</td>
<td>-0.121</td>
<td>-0.035</td>
<td>3831</td>
</tr>
<tr>
<td>Directors</td>
<td>0.089</td>
<td>0.616</td>
<td>-0.407</td>
<td>-0.130</td>
<td>1371</td>
</tr>
<tr>
<td><strong>Directors - Non-Directors</strong></td>
<td><strong>0.436</strong>*</td>
<td><strong>0.768</strong>*</td>
<td><strong>0.286</strong>*</td>
<td><strong>0.095</strong>*</td>
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</tr>
<tr>
<td>Non-Founders</td>
<td>-0.233</td>
<td>0.047</td>
<td>-0.198</td>
<td>-0.057</td>
<td>5115</td>
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<td>Founders</td>
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<td>0.246</td>
<td>-0.128</td>
<td>-0.256</td>
<td>87</td>
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<tr>
<td><strong>Founders - Non-Founders</strong></td>
<td><strong>0.069</strong></td>
<td><strong>0.198</strong>*</td>
<td><strong>0.070</strong></td>
<td><strong>-0.199</strong>*</td>
<td></td>
</tr>
<tr>
<td>No UG Degree</td>
<td>-0.418</td>
<td>0.905</td>
<td>-0.925</td>
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<td>3121</td>
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<td><strong>-0.844</strong>*</td>
<td><strong>0.713</strong>*</td>
<td><strong>0.299</strong>*</td>
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<tr>
<td>No MBA</td>
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<td>0.096</td>
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<td>MBAs</td>
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<td>-0.034</td>
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<tr>
<td><strong>MBAs - No MBA</strong></td>
<td><strong>0.146</strong>*</td>
<td><strong>-0.076</strong>*</td>
<td><strong>0.144</strong>*</td>
<td><strong>0.072</strong>*</td>
<td></td>
</tr>
<tr>
<td>MBA from Top Ten School</td>
<td>0.003</td>
<td>0.067</td>
<td>-0.096</td>
<td>0.008</td>
<td>444</td>
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<td>MBA from School Ranked 11-25</td>
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<td><strong>JD Degree – No JD Degree</strong></td>
<td><strong>-0.134</strong>*</td>
<td><strong>-0.228</strong>*</td>
<td><strong>0.084</strong>*</td>
<td><strong>0.012</strong></td>
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</table>

Table 13: Person versus firm effects in determining non-CEO executive wage differentials. Reported significance of the differences between groups is taken from 2-tailed t-tests.
Figure 3: An illustrative example of the difference in nested and crossed factors.
Figure 4: An overlay of the distribution of firm effects on compensation differentials with the distribution of person effects on compensation differentials.
Figure 5: An overlay of the distribution of firm effects on compensation differentials with the distribution of person effects on compensation differentials for sub-sample of CEOs.
Figure 6: An overlay of the distribution of firm effects on compensation differentials with the distribution of person effects on compensation differentials for sub-sample of non-CEOs.
Figure 7: An overlay of the distribution of firm effects on compensation differentials with the distribution of person effects on compensation differentials for sub-sample of male executives.
Figure 8: An overlay of the distribution of firm effects on compensation differentials with the distribution of person effects on compensation differentials for sub-sample of female executives.
<table>
<thead>
<tr>
<th></th>
<th>Model 1 DV: Total Pay</th>
<th>Model 2 DV: Cash Pay</th>
<th>Estimate (Std Error)</th>
<th>Estimate (Std Error)</th>
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<tr>
<td>Experience</td>
<td></td>
<td></td>
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<td></td>
</tr>
<tr>
<td>Firm-specific (yrs)</td>
<td>0.031*** (0.002)</td>
<td>0.012*** (0.002)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Industry-specific (yrs outside firm in industry)</td>
<td>0.027*** (0.005)</td>
<td>0.011*** (0.003)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Position-specific (yrs)</td>
<td>0.008*** (0.003)</td>
<td>0.006*** (0.002)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Power</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>CEO Dummy</td>
<td>0.472*** (0.034)</td>
<td>0.285*** (0.031)</td>
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<td></td>
</tr>
<tr>
<td>Director Dummy</td>
<td>0.517*** (0.036)</td>
<td>0.24*** (0.030)</td>
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<td>Founder Dummy</td>
<td>-0.381*** (0.109)</td>
<td>-0.107 (0.90)</td>
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<td>Demographics</td>
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<tr>
<td>Female Dummy</td>
<td>-0.318*** (0.103)</td>
<td>-0.127* (0.074)</td>
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<tr>
<td>Random Effect Parameters</td>
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</tr>
<tr>
<td>Person Effect (estimate of std deviation)</td>
<td>0.55</td>
<td>0.36</td>
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<tr>
<td>Firm Effect (estimate of std deviation)</td>
<td>0.88</td>
<td>0.53</td>
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<tr>
<td>Correlation (Person, Firm)</td>
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<td>No. of observations</td>
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<td>7174</td>
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<td>No. of executives</td>
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<td>No. of firms</td>
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p<.01***, p<.05**, p<.10*

Table 14: Explaining Wage Differentials among Executives
Table 15: Are High Wage Firms High Performing Firms?

<table>
<thead>
<tr>
<th>Model</th>
<th>Percentile</th>
<th>Corr (Person, Firm)</th>
<th>Average Person Effect</th>
<th>Average Firm Effect</th>
<th>No. of Obs.</th>
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<td>DV: Total Pay</td>
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<tr>
<td>DV: Cash Pay</td>
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<td>0.5945</td>
<td>-.0560</td>
<td>-.1492</td>
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<td>-.0707</td>
<td>-.1750</td>
<td>1229</td>
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<td>DV</td>
<td>Coefficient Estimate on Firm Effect (as estimated in Model 1, DV: Total Pay) (Std Error)</td>
<td>R-square</td>
<td>Coefficient Estimate on Firm Effect (as estimated in Model 2, DV: Cash Pay) (Std Error)</td>
<td>R-square</td>
<td>Number of Observations</td>
</tr>
<tr>
<td>----------------------------------------------</td>
<td>------------------------------------------------------------------------------------------</td>
<td>----------</td>
<td>----------------------------------------------------------------------------------------</td>
<td>----------</td>
<td>------------------------</td>
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<td>Tobin’s q</td>
<td>0.41***</td>
<td>0.022</td>
<td>-0.2897***</td>
<td>0.004</td>
<td>6165</td>
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<td>5 yr Average Total Market Value</td>
<td>13414.58****</td>
<td>0.101</td>
<td>20716.63***</td>
<td>0.094</td>
<td>6489</td>
</tr>
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<td>5 yr Average Percent Change in Market Value</td>
<td>18.82***</td>
<td>0.003</td>
<td>43.34***</td>
<td>0.007</td>
<td>5761</td>
</tr>
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<td>Industry Adjusted 5 Year Total Return to Shareholders</td>
<td>0.01***</td>
<td>0.006</td>
<td>0.006**</td>
<td>0.001</td>
<td>6552</td>
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<tr>
<td>Industry Adjusted 3 Year Total Return to Shareholders</td>
<td>0.01***</td>
<td>0.003</td>
<td>0.002**</td>
<td>0.000</td>
<td>6838</td>
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<td>Return on Assets</td>
<td>-0.37**</td>
<td>0.001</td>
<td>-0.966***</td>
<td>0.003</td>
<td>7174</td>
</tr>
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</table>

Table 16: Are High Wage Firms High Performing Firms? How Predictive is the Firm Effect on Wages of Tobin’s q? (OLS Estimates)
Figure 9: An illustration of positive and negative assortative matching

<table>
<thead>
<tr>
<th>Executives</th>
<th>Firms</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>High Performers</td>
</tr>
<tr>
<td>High</td>
<td>Positive Assortative Matching</td>
</tr>
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<td>Performers</td>
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</tr>
<tr>
<td>Low</td>
<td>Negative Assortative Matching</td>
</tr>
<tr>
<td>Performers</td>
<td></td>
</tr>
<tr>
<td>Experience</td>
<td>Model 1 DV: Total Pay</td>
</tr>
<tr>
<td>-------------------------------------</td>
<td>-----------------------</td>
</tr>
<tr>
<td>Firm-specific (yrs)</td>
<td>0.031*** (0.002)</td>
</tr>
<tr>
<td>Industry-specific (yrs outside firm in industry)</td>
<td>0.027*** (0.005)</td>
</tr>
<tr>
<td>Position-specific (yrs)</td>
<td>0.008*** (0.003)</td>
</tr>
<tr>
<td>Power</td>
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</tr>
<tr>
<td>CEO Dummy</td>
<td>0.472*** (0.034)</td>
</tr>
<tr>
<td>Director Dummy</td>
<td>0.517*** (0.036)</td>
</tr>
<tr>
<td>Founder Dummy</td>
<td>-0.381*** (0.109)</td>
</tr>
<tr>
<td>Demographics</td>
<td></td>
</tr>
<tr>
<td>Female Dummy</td>
<td>-0.318*** (0.103)</td>
</tr>
<tr>
<td>Random Effect Parameters</td>
<td></td>
</tr>
<tr>
<td>Person Effect (estimate of std deviation)</td>
<td>0.55</td>
</tr>
<tr>
<td>Firm Effect (estimate of std deviation)</td>
<td>0.88</td>
</tr>
<tr>
<td>Correlation (Person, Firm)</td>
<td>0.45</td>
</tr>
<tr>
<td>No. of observations</td>
<td>7174</td>
</tr>
<tr>
<td>No. of executives</td>
<td>1123</td>
</tr>
<tr>
<td>No. of firms</td>
<td>626</td>
</tr>
</tbody>
</table>

*p<.01***, p<.05**, p<.10*

Table 17: Explaining Wage Differentials among Executives
<table>
<thead>
<tr>
<th></th>
<th>Person Effect</th>
<th>Firm Effect</th>
<th>Person Effect</th>
<th>Firm Effect</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Estimate</td>
<td>Estimate</td>
<td>Estimate</td>
<td>Estimate</td>
</tr>
<tr>
<td></td>
<td>(Std Error)</td>
<td>(Std Error)</td>
<td>(Std Error)</td>
<td>(Std Error)</td>
</tr>
<tr>
<td>Quasi-rent</td>
<td>.0669***</td>
<td>.2125***</td>
<td>.0694***</td>
<td>.2382***</td>
</tr>
<tr>
<td></td>
<td>(.0045)</td>
<td>(.0140)</td>
<td>(.0046)</td>
<td>(.0118)</td>
</tr>
<tr>
<td>Governance Index</td>
<td>.0022</td>
<td>-.0020</td>
<td>(.0026)</td>
<td>(.0029)</td>
</tr>
<tr>
<td>Firm Size</td>
<td>-.0103***</td>
<td>-.0249***</td>
<td>(.0031)</td>
<td>(.0036)</td>
</tr>
<tr>
<td>(LN_sales)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>6128</td>
<td>6128</td>
<td>2510</td>
<td>2510</td>
</tr>
</tbody>
</table>

Table 18: Regressions of Person and Firm Effects on Quasi-rents

<table>
<thead>
<tr>
<th>Average Wage Differential</th>
<th>Average Tobin's q</th>
<th>Average Industry Adjusted Total Stock Return (3yr)</th>
<th>Average Firm Effects</th>
<th>Average Person Effects</th>
<th>Average Governance Index</th>
<th>Average Firm Size (Sales)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Firms with Gains from Rent Sharing</td>
<td>.0045</td>
<td>2.233</td>
<td>1.015</td>
<td>-.2431</td>
<td>.0848</td>
<td>9.529</td>
</tr>
<tr>
<td>Firms with Losses from Rent Sharing</td>
<td>.1272</td>
<td>2.200</td>
<td>1.018</td>
<td>-.1973</td>
<td>-.0249</td>
<td>9.231</td>
</tr>
</tbody>
</table>

Table 19: What is Different about Firms with Gains from Rent Sharing and Firms with Losses from Rent Sharing?
Figure 10: Distribution of Bargaining Power for Quasi-Rents
APPENDIX B

DESCRIPTION OF DATA COLLECTION
AND VARIABLE CONSTRUCTION
Marquis’ Who’s Who Publications Database

Marquis’ Who’s Who (referred to hereafter as “Marquis”) solicits and collects biographical information on a range of individuals (e.g. celebrities, politicians, academics, executives, etc) throughout the world. Some individuals opt to provide their biographical information to be included in the publications of Marquis, while other individuals are included because of the public access to their biographical information. For example, Marquis specifically targets executives in the largest publicly traded companies in America for inclusion in their database. Many of the executives opt to provide the information, but with others, the requisite information can be obtained through public sources such as annual reports, proxy statement, press releases, and corporate websites.

The biographical data that Marquis seeks to collect is comprised of the following list:
- Name
- Gender
- Principal/Secondary Occupation
- Birth Information (Date, Location, Nationality, Immigration Date)
- Family Information (Parents, Current and Past Spouse, Children, Dates of Changes in Marital Status--Marriage, Divorce, Widowed, Remarried)
- Education (Earned & honorary degrees, Schools, Locations, Dates)
- Professional Certifications
- Career History (Position, Organization, Location, Dates)
- Career-related Activities (e.g. consulting, corporate/advisory boards)
- Creative Works (e.g. books, journals, patents, awards)
- Civic and Political Activities (Role, Organization, Location, Dates)
- Awards, Honors, and Grants
- Professional, Club, and Lodge Memberships
- Achievements (e.g. patents, notable findings, etc)
- Military Record (Highest Rank, Branch of Service, Location, Dates)
- Affiliations (Political Party, Religious Denomination)
- Avocations (e.g. hobbies, recreational activities)
- Mailing contact information

Compustat’s Executive Compensation Database

Compustat’s Executive Compensation Database reports more than 100 compensation and financial items for over 2,513 firms and 23,171 executives in the Standard and Poor’s 1500 Index. This database has been updated annually since 1992.

Combining Marquis’ Who’s Who and Compustat’s Execucomp Databases

Selection of Sample. Initially I provided Marquis with the listing of all executives in Execucomp (approx 25K executives), which was merged against Marquis’s entire database of biographies. From this process, I was given a listing of 8307 potential matches based on exact spelling of first and last name (this match was not based on company name). Of these 8307 matches,
only 1523 could ever potentially result in correct matches because many of the 8307 potential matches were multiple observations of name matches. In other words, John Adams of RSA Security listed in Execucomp matched with 108 individuals in the Marquis database named John Adams. Obviously, at most 1 of these 108 individuals in the Marquis database could potentially be the “real” John Adams.

Confirming which of the John Adams was the correct one (as well as all the other name matches with duplicates) and confirming that names such as Larry Bossidy which did not come with duplicate matches were true matches required a confirmation that the individual’s career history as reported in Marquis matched with the company information provided in the Execucomp database. Since Marquis does not report company name information in a standard fashion (e.g. American Greetings might be listed for one person as “Am. Greetings”, as “Am Greetings”, or “American Greetings”), the first pass was attempting to match ticker symbols between the two databases. For a small set of the company names listed in the Marquis database, a company code is assigned by Marquis. These company codes correspond to the company’s ticker symbol. From this, 622 individuals were confirmed as matches. Visual inspection of the remaining individuals was conducted, resulting in 29 additional matches.

There were additional issues even with name and company match being successful. For example, family firms in which executives are father and son with the same first and last name would erroneously match. In these instances, age and education data were used to insure that the right individual was matched. Another example is the few instances in which the same individual had more than one entry in Marquis (meaning, more than one unique identifier in Marquis). Once the information in each entry was confirmed to be the same, the duplicate entry was dropped.

After all these changes, the final sample was from the 8307 list from Marquis was 649 executives. Since the first pass with matching the Marquis and Execucomp databases was not extremely successful, another approach was taken. This time a listing of the S&P 500 firms was given to Marquis. This list of firms was very similar to the Russell 1000 Index, which are the firms for which Marquis specifically targets executives for inclusion in the database. The listing of executives I was given this time was all individuals working for these firms. From these 5200 individuals, I was able to identify an additional 706 matches between the Execucomp and Marquis databases. This results in 1355 matches between the database.

The STATA programming for this process is given below:

/*Data plan is to match on last name and first initial, as well as common alias--check by looking at company names*/

use "C:\DATA\680 plus 195 matches.dta", clear
sort personid
merge personid using "C:\DATA\11 matches.dta", _merge(_merge56)
codebook _merge56
drop _merge _merge56
sort personid
save "C:\DATA\680 195 11 matches.dta"
clear

use "C:\DATA\S&P raw 121305B.dta", clear
sort personid
merge personid using "C:\DATA\680 195 11 matches.dta", _merge(_merge45)
codebook _merge45
unique personid if _merge45==3/*confirms that 762 people matched*/
drop if _merge45==3

foreach v of varlist last_name first_name middle_name {
    replace `v' = upper(`v')
    replace `v' = substr(`v',"/&",1)
    replace `v' = substr(`v',",",1)
    replace `v' = substr(`v',"&",1)
    replace `v' = substr(`v',"",1)
    replace `v' = substr(`v',")",1)
    replace `v' = substr(`v',"",1)
    replace `v' = substr(`v',":",1)
    replace `v' = substr(`v',"",1)
    replace `v' = substr(`v'," ",10)
}
gen firstname_1 = substr(first_name,1,1) /*returns the substring of s starting at n1 for a length of n2*/
gen firstname_2 = substr(first_name,1,2) /*returns the substring of s starting at n1 for a length of n2*/

/* Eliminates 1st space and punctuation */
    foreach v of varlist company {
        replace `v' = upper(`v')
        replace `v' = substr(`v',"/&",1)
        replace `v' = substr(`v',",",1)
        replace `v' = substr(`v',"&",1)
        replace `v' = substr(`v',"",1)
        replace `v' = substr(`v',")",1)
        replace `v' = substr(`v',"",1)
        replace `v' = substr(`v',":",1)
        replace `v' = substr(`v',"",1)
        replace `v' = substr(`v'," ",10)
    }
gen company_3 = substr(company,1,3) /*returns the substring of s starting at n1 for a length of n2*/
gen company_4 = substr(company,1,4) /*returns the substring of s starting at n1 for a length of n2*/
gen company_5 = substr(company,1,5) /*returns the substring of s starting at n1 for a length of n2*/
gen company_2 = substr(company,1,2) /*returns the substring of s starting at n1 for a length of n2*/

sort last_name firstname_1
save "C:\DATA\121905A.dta"
clear
use "C:\Documents and Settings\alison\My Documents\Backup of Jump Drive\Dissertation\111005A.dta",
clear
keep co_per_r execid coname ticker exec_lna exec_fna exec_mna
rename exec_lna last_name

foreach v of varlist last_name exec_fna exec_mna {
    replace `v' = upper(`v')
    replace `v' = subinstr(`v','/','' ,1)
    replace `v' = subinstr(`v','&','' ,1)
    replace `v' = subinstr(`v','-','' ,1)
    replace `v' = subinstr(`v','('','' ,1)
    replace `v' = subinstr(`v',')','' ,1)
    replace `v' = subinstr(`v','"','' ,1)
    replace `v' = subinstr(`v','.' ,1)
    replace `v' = subinstr(`v',' ' ,10)
}
gen exec_fna1 = substr(exec_fna,1,1) /*returns the substring of s starting at n1 for a length of n2*/
gen exec_fna2 = substr(exec_fna,1,2) /*returns the substring of s starting at n1 for a length of n2*/
gen exec_fna3 = substr(exec_fna,1,3) /*returns the substring of s starting at n1 for a length of n2*/
rename exec_fna1 firstname_1
drop if execid==.
sort last_name firstname_1
merge last_name firstname_1 using "C:\DATA\121905A.dta", _merge(_merge9)
codebook _merge9
drop if _merge9==1 /*compustat non-matches*/
unique personid if _merge9==3/*Number of unique values of personid is 1321 Number of records is 14776 */
gen fna1_match=1 if _merge9==3
gen fna2_match=1 if  exec_fna2==firstname_2
unique personid if fna2_match==1 /*Number of unique values of personid is 803 Number of records is 6472*/

/*match on executive names and then clean the company names and use these as a check*/

/*****************************/
Match on Company
/*****************************/
/*Clean coname, company*/
/*Eliminates 1st space and punctuation */
foreach v of varlist coname company {
    replace `v' = upper(`v')
    replace `v' = subinstr(`v','/','' ,1)
    replace `v' = subinstr(`v','&','' ,1)
    replace `v' = subinstr(`v','-','' ,1)
    replace `v' = subinstr(`v','('','' ,1)
    replace `v' = subinstr(`v',')','' ,1)
    replace `v' = subinstr(`v','"','' ,1)
    replace `v' = subinstr(`v','.' ,1)
    replace `v' = subinstr(`v',' ' ,10)
replace `v' = subinstr(`v',"","",10) 
}
gen coname_3 = substr(coname,1,3) /*returns the substring of s starting at n1 for a length of n2*/
gen coname_4 = substr(coname,1,4) /*returns the substring of s starting at n1 for a length of n2*/
gen coname_5 = substr(coname,1,5) /*returns the substring of s starting at n1 for a length of n2*/
gen coname_2 = substr(coname,1,2) /*returns the substring of s starting at n1 for a length of n2*/
gen coname_1 = substr(coname,1,1)
gen company_1=substr(company, 1,1)
format company %-20s
gen coname1_match=1 if coname==company & fna1_match==1
gen coname2_match=1 if coname==company & fna2_match==1
gen coname3_match=1 if coname_2==company_2 & fna1_match==1
gen coname4_match=1 if coname_2==company_2 & fna2_match==1
gen coname5_match=1 if coname_1==company_1 & fna1_match==1
drop if File_match==0
unique personid if coname1_match==1
unique personid if coname2_match==1
unique personid if coname3_match==1
unique personid if coname4_match==1
gen File_match=1 if coname3_match==1
sort coname3_match personid
unique personid if File_match==1
drop if File_match==0
drop coname4_match
drop coname1_match coname2_match coname3_match /*dropping all the zeros in File_match effectively deleted all the observations from these variables*/
sort fna2_match personid
save "C:\DATA\121905C.dta"

inout using "C:\DATA\import of JD 120505D.csv", comma

drop if File_match==0
foreach v of varlist coname marcompany {
    replace `v' = upper(`v')
    replace `v' = subinstr(`v',"","",1)
    replace `v' = subinstr(`v',"","",1)
    replace `v' = subinstr(`v',"","",1)
    replace `v' = subinstr(`v',"","",1)
    replace `v' = subinstr(`v',"","",1)
    replace `v' = subinstr(`v',"","",1)
    replace `v' = subinstr(`v',"","",1)
    replace `v' = subinstr(`v',"","",1)
    replace `v' = subinstr(`v',"","",10)
}
gen company_3 = substr(marcompany,1,3) /*returns the substring of s starting at n1 for a length of n2*/
gen company_4 = substr(marcompany,1,4) /*returns the substring of s starting at n1 for a length of n2*/
gen company_5 = substr(marcompany,1,5) /*returns the substring of s starting at n1 for a length of n2*/
gen company_2 = substr(marcompany,1,2) /*returns the substring of s starting at n1 for a length of n2*/
gen coname_3 = substr(coname,1,3) /*returns the substring of s starting at n1 for a length of n2*/
gen coname_4 = substr(coname,1,4) /*returns the substring of s starting at n1 for a length of n2*/
gen coname_5 = substr(coname,1,5) /*returns the substring of s starting at n1 for a length of n2*/
gen coname_2 = substr(coname,1,2) /*returns the substring of s starting at n1 for a length of n2*/
gen coname_1 = substr(coname,1,1)
drop company_1
gen company_1=substr(marcompany, 1,1)
gen coname_match=1 if coname==marcompany
gen coname2_match=1 if coname_2==company_2
drop coname1_match
gen coname1_match=1 if coname_1==company_1
codebook coname_match coname2_match coname1_match
drop company_1
format marcompany %-20s
save "C:\DATA\20 matches.dta /*20 matches from the JD 120505D file*/

/*merge with compustat ticker file which is only compustat observations that match the list of companies from Marquis*/
/*do a ticker, last name merge; once merge is complete check first names to confirm the match*/
use "C:\DATA\S&P raw 121305B.dta", clear
sort ticker last_name
drop TLN_match==1 /*dropped the matches for going back to compustat and trying to rematch*/
drop if TLN_match==1/*dropped the people who match on last_name and ticker but not first name--this is approximately 28 people who are going to need to be looked at again*/
drop exec_fna exec_mna co_per_r allothpdp epsex5ls reason execid coname cusip spcode pgender p_age_2
becamee joined_c rejoin lefttof leftco gykey /*drop all the execucomp variables*/
/*clean executive names*/
foreach v of varlist last_name first_name middle_name {
    replace `v' = upper(`v')
    replace `v' = subinstr(`v',/"",","",1)
    replace `v' = subinstr(`v',"&","",1)
    replace `v' = subinstr(`v',"-","",1)
    replace `v' = subinstr(`v',"(()","",1)
    replace `v' = subinstr(`v',")","",1)
    replace `v' = subinstr(`v',"'","",1)
    replace `v' = subinstr(`v',".","",1)
    replace `v' = subinstr(`v',".",","",1)
    replace `v' = subinstr(`v'," ","",10)
}

codebook last_name first_name middle_name

gen firstname_1 = substr(first_name,1,1) /*returns the substring of s starting at n1 for a length of n2*/
gen firstname_2 = substr(first_name,1,2) /*returns the substring of s starting at n1 for a length of n2*/
gen firstname_3 = substr(first_name,1,3) /*returns the substring of s starting at n1 for a length of n2*/

codebook firstname_1 firstname_2 firstname_3
sort last_name firstname_3
save "C:\DATA\3initialsusing.dta"
clear

use "C:\Documents and Settings\alison\My Documents\Backup of Jump Drive\Dissertation\111005A.dta",
clear
keep co_per_r execid coname ticker exec_lna exec_fna exec_mna
rename exec_lna last_name

foreach v of varlist last_name exec_fna exec_mna {
    replace `v' = upper(`v')
    replace `v' = subinstr(`v',/"",","",1)
    replace `v' = subinstr(`v',"&","",1)
    replace `v' = subinstr(`v',"-","",1)
    replace `v' = subinstr(`v',"((","",1)
    replace `v' = subinstr(`v',")","",1)
    replace `v' = subinstr(`v',"'","",1)
    replace `v' = subinstr(`v',".","",1)
    replace `v' = subinstr(`v',".",","",1)
    replace `v' = subinstr(`v'," ","",10)
}
gen exec_fna1 = substr(exec_fna,1,1) /*returns the substring of s starting at n1 for a length of n2*/
gen exec_fna2 = substr(exec_fna,1,2) /*returns the substring of s starting at n1 for a length of n2*/
gen exec_fna3 = substr(exec_fna,1,3) /*returns the substring of s starting at n1 for a length of n2*/
rename exec_fna3 firstname_3
drop if execid==.
sort last_name firstname_3
merge last_name firstname_3 using "C:\DATA\3initialsusing.dta", _merge(_merge56)
codebook _merge56
drop if _merge56==1 /*compustat non-matches*/
unique personid if _merge56==3 /*Number of unique values of personid is 1248; Number of records is 10808*/
gen match2=1 if _merge56==3

/*match on executive names and then clean the company names and use these as a check*/

*****************
Match on Company
*****************

/*Clean coname, company*/

drop match3
  /* Eliminates 1st space and punctuation */
foreach v of varlist coname company {
  replace `v' = upper(`v')
  replace `v' = subinstr(`v','/',' ',1)
  replace `v' = subinstr(`v',' ',' ',1)
  replace `v' = subinstr(`v','&',' ',1)
  replace `v' = subinstr(`v','-',' ',1)
  replace `v' = subinstr(`v','(',' ',1)
  replace `v' = subinstr(`v',')',' ',1)
  replace `v' = subinstr(`v','''',' ',1)
  replace `v' = subinstr(`v','.'',' ',1)
  replace `v' = subinstr(`v',' ',' ',10)
}

gen match3=1 if match2==1 & coname==company
unique personid if match3==1 /*match3 means that it matched on last name, first three initials, and the whole company name without space and punctuation*/
  /*Number of unique values of personid is 46; Number of records is 134*/
sort match3
describe company
format company %-20s

/*do a match with first 3,4,5 letters of the company name*/
gen coname_3 = substr(coname,1,3) /*returns the substring of s starting at n1 for a length of n2*/
gen coname_4 = substr(coname,1,4) /*returns the substring of s starting at n1 for a length of n2*/
gen coname_5 = substr(coname,1,5) /*returns the substring of s starting at n1 for a length of n2*/
gen company_3 = substr(company,1,3) /*returns the substring of s starting at n1 for a length of n2*/
gen company_4 = substr(company,1,4) /*returns the substring of s starting at n1 for a length of n2*/
gen company_5 = substr(company,1,5) /*returns the substring of s starting at n1 for a length of n2*/
gen company_2 = substr(company,1,2) /*returns the substring of s starting at n1 for a length of n2*/

gen match4=1 if match2==1 & company_5==coname_5
unique personid if match4==1
sort match4 personid /*Number of unique values of personid is 171; Number of records is 691*/
gen match5=1 if match2==1 & company_4==coname_4
unique personid if match5==1

gen match6=1 if match2==1 & company_3==coname_3
unique personid if match6==1 /*Number of unique values of personid is 195; Number of records is 794*/
sort match6 match4 personid
/*match6 works good--only a few mistakes were generated--they were fixed by hand*/
gen match7=1 if match2==1 & company_2==coname_2
unique personid if match7==1
sort match7 match6 personid/*upon visual inspection, match7 only generates one more real match--so for right now, match7 is the variable*/

/*so match3 (46 people) is a subset of match7 (195 people) which means the last name first three letter matched generated 195 people*/
/*Next step is to try aliases for more matches and two letters of first name and one letter of first name*/

drop match4 match5 match6
gen match8=1 if match3==1 | match7==1
drop match3
rename match8 match3
unique personid if match8==1
rename match8 final_match_file
unique personid if final_match_file==1
drop match3 match7
save "C:\DATA\3initials195matches.dta" /*this will be the second file to combine with the first 680 matches*/
clear

use "C:\DATA\3initials195matches.dta", clear /*drop the matches????*/

/*DEC 16 2005*/
/*turns out that SP 1210305 C didn't have all observations for matches labeled so about 120 of the matches found in the 195 were already matched*/

MATCH ON NAMES WITH ALIASES AND LAST NAME ONE INITIAL

="/***********

/* code to signify previous matches from the first round of data given by Marquis in August 2005--used the BYU data*/
use "C:\DATA\BYUdata.dta", clear
sort personid
save, replace
clear
use "C:\DATA\S&P raw 121305B.dta", clear
sort personid
merge personid using "C:\DATA\BYUdata.dta", _merge(_merge78)
codebook _merge78
unique personid if _merge78==3 /*shows that 85 personids are in the BYU data and the S&P raw 121305B file*/
/*drop superfluous variables from the BYU data*/

/* code to signify previous matches from the first round of data given by Marquis in August 2005--used the JD 120505A data*/
use "

cd c:\projects05\exec\data\S&P500firms

use sp500raw1, clear
unique personid
unique execid

bysort personid: egen temp = mean(match) /*These were Alison's first round matches*/
drop if temp==1
drop temp
unique personid
unique execid

/********************
Match on Company
********************
/*Clean coname, company, name */

keep coname name company execid personid last_name exec_fna exec_mna

    gen temp1=coname
    gen temp2=company
    gen temp3=name

    /* Eliminates 1st space and punctuation */
    foreach v of varlist temp1 temp2 temp3 {
        replace `v' = upper(`v')
        replace `v' = substr(`v',"/","","",1)
        replace `v' = substr(`v',"",","",1)
        replace `v' = substr(`v',"",","",1)
        replace `v' = substr(`v',"",","",1)
        replace `v' = substr(`v',"","","",1)
        replace `v' = substr(`v',"","","",1)
        replace `v' = substr(`v',"",","",10)
    }

    rename temp1 conamek1
    rename temp2 companyk1
    rename temp3 namek1

/********************
FINAL Match on Company, Coname and/or Name
********************

/* 1)Match between conamek1, namek1, companyk1 */

gen temp1 = substr(conamek1,1,7) /*returns the substring of s starting at n1 for a length of n2*/
gen temp2 = substr(namek1,1,7)
Once the list of executives was determined, relevant variables from the Marquis database had to be coded so that empirical analysis would be possible. The following sections detail this process.

**Career Histories**

The Marquis database provides a detailed look at an executive’s entire career history. The career history typically contains position titles, organization, and years. For example, an executive who has spent her entire career at one organization is likely to have reported not just the years at the organization but also the years at each position held. This enables us to code the progression of the executive’s experience throughout their career. We are also able to differentiate years in various industries and years in various firms.

The Marquis does not have a numerical identifier for each organization in the career histories of executives; hence, alternate approaches had to be used to assign organizations to a ticker and industry classification. Obviously, many of the organizations in these career histories are privately held or no longer in operation. These organizations presented a unique challenge in the coding process. Firms in which a name change has occurred also presented an unique challenge for coding.

Many of the firms in the Russell 1000 have been given a unique identifier within the Marquis database. These firms were relatively easy to match with Compustat’s unique identifier “gvkey”. This enables the assignment of financial data and industry classification to these. This captured the publicly traded firms in the career histories. However, subsidiaries, foreign companies, private firms, firms with name changes, and firms no longer in operation remained to code. Identification of most of these firms was done through a manual search of the Thompson Gale Company database which allows searches on all of these elements.

**Job Titles**

Both the Marquis and Execucomp databases contain data for various job titles held by the individuals. Neither database has a numerical identifier for each job title; hence, a coding protocol for doing so had to be created. This protocol is described below.
Founder (11)
   Founder (11)
   Co-founder (1101)

Owner (12)
   Owner (12)
   Co-owner (1201)
   Partner (general & managing) (1202)
   Sr Partner (1203)
   Principal (1204)
   Pvt. Practice (1205)
   Executive Producer (1206)
   Publisher (1207)

Chairman (Board of Directors) (13)
   Corporate (1301)
   Subsidiary (1302)
   Business-Unit (1303)
   Region (1304)
      Co-chairman (13##01)
      Chairman Emeritus (13##02)
      Founding (13##03)
      Non-executive Chairman (13##04)
      Deputy Chairman (13##05)

Vice Chairman (14)
   Corporate (1401)
   Subsidiary (1402)
   Business-Unit (1403)
   Functional-Unit (1408)

Committees (15)
   Board of Directors (1501)
      Emeritus (150101)
      Region/Business-unit (150102)
      Advisor to BOD (150103)
      Secretary to BOD (150104)
   Executive Committee (1502)
      Chairman (150201)
      Vice-chairman (150202)
      Member (150203)

President (16)
Corporate (1601)
Subsidiary (1602)
Business-Unit (1603)
Group (1604)
Head (1605)
Division (1606)
Region (1607)
  Acting (#####01)
  Co-president (#####02)
  Former President (#####03)

Chief Executive Officer (17)
  Corporate (1701)
  Subsidiary (1702)
  Business-Unit (1703)
  Group (1704)
  Managing Director (1705/2311)
  Editor-in-Chief (1706)
  Regional CEO (1707)
    Co-CEO (17##01)
    Former CEO (17##02)
    Acting CEO (17##03)
    Interim CEO (17##04)

Chief Financial Officer (18)
  Corporate (1801)
  Subsidiary (1802)
  Business-Unit (1803)
  Group (1804)
  Region (1805)
    Acting CFO (#####01)

Chief Operating Officer (19)
  Corporate (1901)
  Subsidiary (1902)
  Business-Unit (1903)
  Group (1904)
    Co-COO (19##01)
    Acting COO (19##02)
    Former COO (19##03)
    Assistant COO (19##04)
Other Chief Officers (20)

Corporate (2001)
Subsidiary (2002)
Business-Unit (2003)
Group (2004)
  Chief Administrative Officer (20##01)
  Chief Engineer (20##02)
  Chief Information Officer (20##03)
  Chief Investment officer (20##04)
  Chief Legal Officer (20##05)
  Chief Marketing Officer (20##06)
  Chief Scientist (20##07)
  Chief Technical Officer (20##08)
  Executive Director (20##09)
  Chief Accounting Officer (20##10)
  Chief Misc Officer (20##11)
  Chief Development Officer (20##12)
  Chief Medical Officer (20##13)
  Chief Economist (20##14)
  Co-Chief (20#####01)

Vice President (21)

Corporate (2101)
Subsidiary (2102)
Business-Unit (2103)
Group (2104)
Former (2105)
Regional (2106)
Staff (2107)
  Senior (#####01)
  EVP (#####02)
  Assistant (#####03)
  Sr Exec (#####04)

Functional Vice President (22)

Corporate (2201)
Subsidiary (2202)
Business-Unit (2203)
Group (2204)
  Business Development (22##01)
Corporate Control (22##02)
Corporate services (22##07)
Development (22##08)
General Merchandise (22##09)
Internal Audit (22##10)
Technology (22##11)
Administrative Services (22##12)
Advertising (22##13)
Corporate Development (22##14)
Corporate Strategy (22##15)
Customer Relations/Service (22##16)
Engineering (22##17)
Finance/Financial Services (22##18)
Human Resources (22##19)
Manufacturing (22##20)
Marketing (22##21)
Misc. (22##22)
Operations (22##23)
Planning (22##24)
Research and Development (22##25)
Sales (22##26)
Strategic Programs (22##27)
Transportation (22##28)
Sales and Marketing (22##29)
Accounting (22##30)
Legal Affairs (22##31)
Senior (22####01)
EVP (22####02)

Other Executive Positions (23)
Company Secretary (2301)
Company Treasurer (2302)
General Counsel (2303)
Controller/Comptroller (2304)
"Executive"/Corporate Officer (2307)
Medical Director (2308)
Former Executive (2309)
Corporate Banking Officer (2310)
Director (not BOD) (2311/1705)
Group Executive (2312)
Advisor to CEO (2313)
   Assistant (23##01)
   Associate (23##02)

**Managerial Positions (24)**
Manager (2401)
General Manager (2402)
Regional Managers (2403)
Plant Manager (2404)
District Manager (2405)
Merchandise Manager (2406)
Director of Marketing (2407)
Product Manager (2408)
Project Manager (2409)
Brand Manager (2410)
Distribution Manager (2411)
Division Manager (2412)
Various Management Positions (2413)
Engineering Manager (2414)
Marketing Manager (2415)
Production Manager (2416)
Sales Manager (2417)
Audit Manager (2418)
General Foreman (2419)
Operations Manager (2420)
Store Manager (2421)
Country Manager (2422)
Administrative Manager (2423)
Advertising Manager (2424)
Asset Manager (2425)
Loan Manager (2426)
Cash Manager (2427)
   Senior (24##01)
   Assistant (24##02)
   District (24##03)
   Subsidiary (24##04)
   Business Unit (24##05)

**Individual Contributor (non-managerial) Business (25)**
Attorney (2501)
Accountant (2502)
Associate (2503)
Consultant (2504)
Financial Analyst (2505)
Account Executive/Supervisor (2506)
Auditor (2507)
Reporter (2508)
Sales Rep (2509)
Actuary (2510)
Management Staff (2511)
Staff (2512)
Trader (2513)
Various Finance Positions (2514)
Buyer (2515)
Geologist (2516)
Investment Banker (2517)
Marketing Rep (2518)
National Bank Examiner (2519)
Various HR Positions (2520)
Various Marketing Positions (2521)
Various Sales Positions (2522)
Analyst (2523)
Copywriter (2524)
Credit Investigator (2525)
Police Officer (2526)
Underwriter (2527)
TV Announcer (2528)
Economist (2529)
Senior (25##01)

**Individual Contributor (non-managerial) Technical (26)**

Engineer (2601)
Research Position (2602)
Member (2603)
Project Engineer (2604)
Various (2605)
Sales Engineer (2606)
Design Team (2607)
Chemist (2608)
Trainee (2609)
Senior (26##01)

**Entry Level (27)**
Administrative Support (2701)
Executive Assistant (2702)
Assistant to President (2703)
Intern (2704)
Management Trainee (2705)
Executive Assistant to Chairman (2706)
Sales Trainee (2707)

**Commander (28)**
1st Lt (2801)
2nd Lt (2802)
Captain (2803)
Colonel (2804)
Various Positions (2805)

**Generic Positions (29)**
Member (2901)
With/various positions/joined (2902)
Other (2903)
From x to y position (2904)
Political Position (2905)
Retired (2906)
Editor (2907)
formerly with (2908)
Administration (2909)
Nurse (2910)
Health Care (2911)
Assistant (29##01)
Associate (29##02)

**Academic Position (30)**
Instructor (3001)
Assistant professor (3002)
Post-doctoral fellow (3003)
Assoc. professor (3004)
Fellow (3005)
Asst dean (3006)
Dean (3007)
Chair (3008)

Director of (31)
Director of Marketing (3101)
Director of Corporate/Strategic Planning (3102)
Director of Development (3103)
Director of Engineering (3104)
Director of Finance/Financial Planning (3105)
Director of Operations (3106)
Director of Sales (3107)
Director of Advertising/Creative Director (3108)
Director of R & D (3109)
Director of Manufacturing (3110)
Director of Research (3111)
Director of Administration (3112)
Director of Bus Dev./Strategy (3113)
Director of Human Resources (3114)
Director of XYZ (3115)

Athletic Team (32)
Player 3201

Government (33)
Ambassador (3301)
Fellow (Whitehouse) 3302
Research Associate (3303)
AIDE (3304)
Admin/Sec (3305)
Chief of Staff (3306)
Division Head (3307)
Astronaut (3308)

Independent/Entrepreneurial (34)

Executive Level/Director (35)
Managerial Level (36)
Individual Contributor (37)
Entry Level (38)
Groupings

Since over 300 classifications were made using the coding protocol, groupings were a practical necessity for statistical analysis. These groupings are listed below as well as the variables that were computed from this information.

Position Levels. Five position levels were created to represent promotions in the organizational hierarchy for the executive.

- Level 1: Chairman, Vice-Chairman, President, CEO (Codes: 13, 14, 16, 17)
- Level 2: C-level Executives (CFO, COO, and other chiefs) (Codes: 18, 19, 20)
- Level 3: Vice-President (Codes: 21)
- Level 4: Functional Vice-President (Codes: 22)
- Level 5: General Executive Positions (Codes: 23, 31, 35)

These hierarchies of positions were used in the calculation of years of position-specific experience.

Functional Background. The Marquis database provides a detailed look at an executive’s functional background—throughout the entire career history. Therefore, we can identify not only the functional background of the executive but also the number of years spent in these functions.

Educational Experience

Within the Marquis database, the educational backgrounds of executives are reported. This information typically includes the earned (or honorary) degree, granting institution, location, and years. This information, for the most part, is straightforward for coding; however, there were some idiosyncratic elements to how the information was reported. Ultimately, the information in the individual’s educational background was coded into school type (e.g. undergraduate or graduate, as well as type of degree) and ranking of degree.

School (Degree) Type.

For the information on degree type (i.e. BA, BS, MBA, JD, etc) there were 343 unique entries. Thus, for commonly repeated entries such as “student”, “Postgrad.”, “PhD”, “MS”, “MBA”, etc the coding to assign a numerical value to the degree entry was straightforward. For the remaining entries which did not occur frequently enough to allow for programming, manual inspection was used to place the degree entry into the categories listed below:

- High School (0)
- National Undergraduate Institution (1)
- Liberal Arts Institution (2)
- MBA Program (3)
- Law School (4)
Other Graduate Program (5)
Medical School (6)
Divinity (7)
Junior/Community/Technical College (8)
Drop out (9)
Executive Education Program (10)

Some of the entries for degree type were difficult to interpret. For example, for degree entries labeled “student”, the classification chosen was “drop out”. It was still recorded what school the individual was attending but did not graduate from.

**Rankings.**

Schools were ranked according to the current US News and World Report (referred to as “US News” hereafter) ranking classifications. Differential rankings of schools were made based on the type of degree the individual received. For example, an undergraduate degree from Ohio State would receive a different ranking than a MBA from Ohio State since the rankings for these degrees differ in the US News rankings.

MBA and JD graduate degrees were ranked according to the corresponding US News ranking; however, other graduate degrees (e.g. PhD, MA) were ranked according to the institution’s undergraduate ranking in US News.

Medical degrees were not ranked because there are multiple rankings for schools based on teaching, research, and areas of specialization and it is not clear which ranking would apply for a given executive. Degrees from divinity schools were not ranked because US News does not provide rankings for these institutions.

Attempts were made to rank premiere institutions outside of the United States. For example, top European and Canadian schools (e.g. Oxford and Toronto) were ranked similarly to top tier American schools. (5/10)

For the construction of variables based on school type and rankings, tiers were used in lieu of a continuous variable. For example, all undergraduate degrees were classified into the following five tiers:

- UG ranked 1-10
- UG ranked 11-25
- UG ranked 26-75
- UG ranked 76-120
- UG 121+ (“unranked”)
Other degrees followed a similar pattern:

MBA ranked 1-10
MBA ranked 11-25
MBA ranked 26-50
MBA 51+ (“unranked”)

Law Degree ranked 1-10
Law Degree ranked 11-25
Law Degree ranked 26-50
Law Degree 51+ (“unranked”)

Graduate Program ranked 1-10\(^{16}\)
Graduate Program ranked 11-25
Graduate Program ranked 26-75
Graduate Program ranked 76-120
Graduate Program 121+ (“unranked”)

\(^{16}\) As previously noted, the graduate programs outside of MBA and Law were ranked according to the undergraduate institution’s national ranking.