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Comparing CEO Compensation Effects of Public and Private Acquisitions

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Abstract: We estimate the effect of acquisition performance and acquisition activity on CEO compensation for the full set of CEOs of large public U.S. corporations in the Execucomp database over the period 1992–2016. Most previous work has focused on publicly traded acquisition targets. We focus on the comparison between public and private targets, showing significant differences between the two. One primary finding, based on panel data regressions (using both fixed and random effects) is that the performance of private acquisitions, as measured by abnormal announcement returns, has a statistically significant positive effect of plausible economic magnitude on CEO compensation. Public acquisitions exhibit a smaller positive effect that is statistically insignificant. For both, acquisition activity (number of acquisitions) has a statistically significant positive effect on compensation. Our main results suggest that agency considerations are important for both public and private acquisitions but are more important for public acquisitions.

Keywords: acquisitions; executive compensation; CEO incentives; private equity

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1. Introduction

In recent years, CEO compensation has been a major and often controversial subject in the business media and has also been an important area of academic study. One concern is whether high levels of compensation reflect performance or whether other factors are at work (Bebchuk and Fried 2004; Cooper et al. 2016). This concern is particularly important with respect to merger and acquisition (M&A) activity in light of the relatively disappointing returns to acquisitions in recent decades (Officer et al. 2008).

Theories of executive compensation fall into two general categories. One category consists of "shareholder value" theories, which imply a close relationship between a CEO's contribution to shareholder value and subsequent compensation. The other category consists mainly of agency theory models in which CEOs are able to extract rents from shareholders, resulting in a weak link between shareholder value and CEO compensation.

In the context of acquisition activity, we define the shareholder value hypothesis as the hypothesis that CEO compensation reflects shareholder value created by acquisitions. This hypothesis is the focus of a significant empirical literature, but a careful review of this literature by Bodolica and Spraggon (2015, chp. 8) indicates no consensus. Simply asking whether the hypothesis is "true" is perhaps too simplistic. Some papers, particularly Wright et al. (2002) and Harford and Li (2007), investigate when the shareholder value hypothesis has significant explanatory power and try to assess the factors that influence the strength of the shareholder value effect on compensation.

In this paper, we have two main objectives. One objective is to assess an important but, to our knowledge, previously unstudied factor potentially affecting the significance of the shareholder value hypothesis. Specifically, we focus on the different effects of acquisition returns on CEO compensation depending on whether acquired firms are publicly traded

("public acquisitions") or privately held ("private acquisitions"). Based on the theoretical implications of agency theory as it applies to executive compensation, we suggest that agency distortions would be stronger for public acquisitions. Therefore, we hypothesize that shareholder value effects on CEO compensation would be weaker for public acquisitions than for private acquisitions.

Our second objective is to assess the effect of acquisition *activity*, as distinct from acquisition returns, on CEO compensation. A CEO may make no acquisitions, one acquisition, or multiple acquisitions. Making a large number of acquisitions that add nothing to shareholder value would not, under the shareholder value hypothesis, lead to higher CEO compensation. However, if compensation is influenced by firm size or CEO reputation or some other variable suggested by agency theory, then acquisition activity in itself could lead to increases in compensation.

In other words, agency considerations might allow CEOs to take advantage of the acquisition process to raise their compensation even when acquisitions do not raise shareholder value. Thus, acquisition activity might represent "self-dealing" by CEOs who seek to raise their own compensation, as suggested by the results of Choi et al. (2020), among others. Our innovation in this paper is to investigate whether such effects are stronger for public than for private acquisitions, consistent with our hypothesis that public acquisitions are more prone to agency effects.

We use a large sample in which the set of potential acquirers consists of nearly all S&P 1500 publicly traded U.S. firms over the period 1992–2016. When we aggregate public and private acquisitions, we find that the overall effect of acquisition performance on CEO compensation is positive. Aggregated acquisition activity also has a positive, statistically significant, and economically meaningful effect. However, to properly understand this aggregate effect, it is important to identify public and private acquisitions separately.

We find two striking differences between the effects of public acquisitions and private acquisitions. For private acquisitions, acquisition performance measured by stock market acquisition returns has a statistically significant and economically meaningful positive effect on CEO compensation. For public acquisitions, there is no significant effect in our panel data regressions. Therefore, our panel data analysis suggests that the shareholder value hypothesis applies more strongly to private than to public acquisitions. The second difference is that, in our panel data regressions, the number of public acquisitions has a strong positive effect on CEO compensation, while the number of private acquisitions has a smaller although still statistically significant positive effect.

We believe that both of these results are original contributions to the literature and are important in understanding the determinants of executive compensation. Both are consistent with the hypothesis that agency distortions are more significant for public acquisitions. Our findings also support the importance of agency considerations for both CEO compensation and as a contributing explanation to the pattern of M&A activity.

Section 2 of the paper provides a review of the relevant literature and the relationship between that literature and our primary hypotheses. Section 3 describes our data and methods, including a description of the event study methods we use to estimate abnormal returns to acquisitions. Section 4 presents our main results, and Section 5 contains concluding remarks.

2. Literature Review and Hypothesis Development

We start from the classic observation of Means (1931) that large corporations are subject to a separation of ownership (shareholders) and control (senior executives) that creates potential conflicts of interest. Given informational asymmetries between shareholders and senior executives, these conflicts give rise to agency problems. We do not review the very large theoretical literature on such issues here, but the survey by Edmans and Gabaix (2016) covers much of the relevant ground. The empirical literature on managerial agency problems identifies several ways senior managers may extract value from shareholders for their own benefit, as reviewed by Murphy (1999) and Frydman and Saks (2010).

Due to agency effects, compensation is not necessarily closely tied to performance in increasing shareholder value. In particular, CEOs may succeed in increasing their compensation even if their acquisition activities do not increase shareholder value. For example, one agency-based possibility proposed by Shleifer and Vishny (1989) is the "management entrenchment" hypothesis that CEOs pursue acquisitions (and other investments) that increase their personal value to the firm and make them harder to replace. Such CEOs might succeed in increasing their compensation even if the acquisitions that make them more important to the firm reduce firm value.

Another frequently cited agency-based reason for seemingly inefficient acquisition behavior is the size hypothesis as articulated, for example, by Jensen (1989, p. 66): "Managers have many incentives to expand company size beyond that which maximizes shareholder wealth. Compensation is one of the most important incentives". See also Hill et al. (2016).

An alternative hypothesis, studied by Avery et al. (1998) and Shi et al. (2017), is that CEOs may pursue acquisitions to enhance their social status and general reputation. In addition, several studies focus on aspects or attributes of the CEO to explain acquisitions, including age (Yim 2013), political ideology (Elnahas and Kim 2017), educational background (Wang and Yin 2018), and "individuality" Blomkvist et al. (2018).

The shareholder value hypothesis reduces to the question of how effectively contracts, monitoring, and other tools can mitigate agency problems and other factors to bring incentives of CEOs and other senior executives into alignment with the interests of shareholders. If such an alignment of interests is achieved, changes in CEO compensation would be closely related to corresponding changes in shareholder value.

Conflicts of interest between shareholders and CEOs in the acquisition process are consistent with the large empirical literature on the returns to acquisitions showing neutral or negative average returns to acquiring firms (Moeller et al. 2005; Officer et al. 2008), with most of the gains going to target firms (Betton et al. 2008). We note, however, that recent work such as Alexandridis et al. (2017) finds that returns to acquirers, while still modest overall, have improved in recent years. Brander and Egan (2017) and Draper and Paudyal (2006) investigate differences in acquisition returns for public and private acquisitions and find that private acquisitions generate better (though still surprisingly low) returns to acquirers.

Among the earliest empirical tests of the shareholder value hypothesis for acquisitions is Lambert and Larcker (1987), who consider a small sample of 35 very large acquisitions over the 1976–1980 period. They find that CEOs who made value-reducing acquisitions and those who made value-increasing acquisitions both experienced increases in cash compensation, although value-increasing acquisitions led to larger increases. The implication that CEO compensation responds positively to both value-creating and value-reducing M&A activity is consistent with much subsequent work, including Grinstein and Hribar (2004).

More broadly, some authors have found relatively strong shareholder value effects, such as Dorata (2008) and Guest (2009), while others have found weak to non-existent shareholder value effects, such as Bliss and Rosen (2001) and Harjoto et al. (2012). Notably, these mixed findings suggest that the strength of shareholder value effects depends on circumstances and leave open the question of the overall empirical importance of the shareholder value hypothesis. Wright et al. (2002) find that the shareholder value effect of acquisition performance on CEO compensation is stronger when the firm's senior executives are monitored more closely (as measured by independent board members, stock owned by institutions, and other variables). Similarly, Harford and Li (2007) find CEO compensation is more sensitive to acquisition performance when boards are stronger. Monitoring within family firms is studied by De Cesari et al. (2016).

Our paper is structurally similar to Choi et al. (2020), although the specific research question differs. Choi et al. (2020) compare the differential CEO compensation effect of domestic (U.S.) and international acquisitions, whereas we compare public and private acquisitions. The biggest difference in method is that Choi et al. (2020) focus on the effect

of acquisition activity on compensation, whereas we focus on both acquisition activity and acquisition performance as measured by stock market returns.

As for theoretical foundations for possible different effects of public and private acquisitions, we suggest that the agency-based distortions that would weaken the link between acquisition performance and CEO compensation are stronger for public acquisitions. First, public acquisitions tend to be larger than private acquisitions. Therefore, if the Jensen (1989) size hypothesis is correct, CEOs who acquire public targets would be more inclined to get sized-based compensation increases. Similarly, acquisitions of public targets more commonly involve acquiring a near equal, creating a more complex firm that would enhance the entrenchment effect. Perhaps most importantly, public acquisitions generate more attention as the target firms are normally larger and better known. Therefore, public acquisitions would increase the acquiring CEO's reputation and stature (and compensation) more than would private acquisitions, as is consistent with Shi et al. (2017).

In summary, our paper investigates the following four hypotheses.

Hypothesis 1 (H1). *(Shareholder value hypothesis): Shareholder returns to acquisitions have a strong positive effect on CEO compensation.*

Hypothesis 2 (H2). *(Agency theory hypothesis): CEO compensation is positively affected by acquisition activity (i.e., by making acquisitions) independent of shareholder returns to acquisitions.*

Hypothesis 3 (H3). *(Differential performance effects hypothesis): The effect of shareholder returns on CEO compensation is weaker for public acquisitions than for private acquisitions.*

Hypothesis 4 (H4). *(Differential acquisition activity hypothesis): The effect of acquisition activity on CEO compensation is stronger for public acquisitions than for private acquisitions.*

3. Data and Methods

We use regression analysis to assess the effect of acquisition performance on CEO compensation. We therefore need data on CEO compensation, on the number and performance of acquisitions undertaken by each CEO, and on other (control) variables that might affect compensation.

3.1. Data Sources

Our compensation data comes from the executive compensation component (Execucomp) of S&P's Compustat database. We start with all CEOs of U.S. publicly traded corporations in the Execucomp data set from its starting date in 1992 up through 2016. Execucomp included CEOs of all firms in the S&P 500 in 1992 and 1993 and expanded to the S&P 1500 subsequently.

To identify acquisitions, we use the Thomson Financial SDC Mergers and Acquisitions database. In order to match the compensation data, we restrict attention to publicly traded acquirers with a listing on the AMEX, the NASDAQ, or the NYSE. Acquisition targets may be publicly traded or privately held. We consider only 100% acquisitions, and we use only acquisitions classified as completed. This acquisition data can be linked to Execucomp compensation data using CUSIP numbers.

Once the acquisitions are identified using SDC, it is necessary to quantify acquisition performance. Various measures of acquisition performance have been used in previous work. We believe that the most direct test of the shareholder value hypothesis involves using the abnormal return to an acquisition announcement as the primary explanatory variable. We estimate this return using event study methods based on stock price data from the Center for Research on Security Prices (CRSP).

Control variables consist of variables other than acquisition returns or activity that might affect CEO compensation. We obtain control variables from Execucomp, other components of Compustat, and Thomson-Reuters' 13F institutional holdings data. As sug-

gested by the prior literature on executive compensation, potential control variables include various measures of firm size, firm profitability, and firm governance, along with CEO age, experience, and gender, and may also include industry effects or business cycle effects.

3.2. Panel Data Construction and Methods

Our data set is built around the Execucomp panel data set that tracks firm-CEO relationships through time. It is helpful to take advantage of that panel structure. As explained in, for example, Graham et al. (2012), each CEO-firm relationship has specific individual characteristics that are hard to observe but that can be at least partially captured by individual effects using fixed effects and random effects panel data estimation procedures. Furthermore, the primary explanatory or "treatment" variables relate to specific discrete events that happen relatively quickly. Therefore, basic fixed-effects and random-effects methods should be suitable for this context.

Each panel is a CEO-firm combination that is tracked through time, For each such panel, we match compensation data for year t with abnormal returns and acquisition activity for year $t-1$. The logic is that good performance would be rewarded with subsequent adjustments to compensation. Within a given year, any compensation that occurs before the acquisition takes place would be unaffected by the acquisition. Even after an acquisition takes place, CEO compensation is not immediately adjusted as various steps are required to make compensation changes, which are often made annually based on an entire year's performance. A compensation adjustment approved at the end of a year would have very little effect on that year's compensation and would show up the following year. Similarly, firm-level control variables (size, profitability, and governance) from year $t-1$ are linked to compensation in year t so that they can more reasonably be viewed as exogenous.

Some previous work has tried to isolate each specific acquisition, dropping observations for which multiple acquisitions occur in the same year. In our case, that would imply dropping more than half the acquisitions from the data. Furthermore, the performance of multiple acquirers is particularly interesting. In our primary analysis, we therefore include observations (CEO-year combinations) with more than one acquisition. There is also the question of how to treat observations for which no acquisitions occur. Most previous researchers have focused on observations for which acquisitions occur, sometimes also constructing a control group using a matching process. However, the available observations for which no acquisitions occur contain relevant information and we argue that they should be in the analysis. Our approach is as follows. Our primary measure of acquisition performance is the aggregate abnormal return from all acquisitions in a given year. For the observations for which no acquisitions occur, this number is zero. For the observations that have acquisitions, there are nearly as many negative returns as positive returns (as is consistent with the literature on the winner's curse in acquisitions). CEOs who did not make any acquisitions in a given year therefore had better acquisition performance than those who generated negative returns. We also do a robustness check using just observations with positive acquisitions.

3.3. Using Event Study Methods to Estimate Acquisition Returns

We treat an acquisition as an event that may affect an acquiring firm's stock price, creating a positive or negative return to shareholders. While there may be some information available regarding acquisitions before they are announced, uncertainty remains until the formal announcement is made. The announcement therefore transmits new information to the market. As is standard, we use a 5-day symmetric event window centered on the announcement day. Using either a 3-day symmetric window or a 7-day symmetric window makes little difference to our results.

The literature on event study methodology is large. The overview provided by MacKinlay (1997) is a standard reference and we follow the general approach described there. To identify the effect of an acquisition (or other event) we need to adjust for changes

in returns that would occur even without the event. We therefore estimate a "market model" that captures the relationship between an acquirer's return and a market index. For firm i we estimate the market model:

$$R_{it} = \alpha_i + \beta_i R_{mt} + \epsilon_{it} \quad (1)$$

where R_{it} is the return to shares in firm i at time t, α_i and β_i are firm-specific parameters of the model, ε_{it} is a random error with mean zero, and R_{mt} is the return on the overall "market" at time t. A firm's return for a given day is based on stock price changes and any dividends paid. The market return used here is constructed from the widely used value-weighted composite AMEX, NASDAQ, NYSE index available from CRSP. To obtain the parameters of the market model for each acquirer we use an estimation window of 250 trading days (about one calendar year), finishing 30 trading days before the acquisition announcement. A few acquisitions were made by firms without a 250-day estimation window. We include these acquisitions provided there is an available estimation window of at least 50 continuous trading days.

The relationship estimated between an acquiring firm's returns and market returns is used to predict the acquirer's normal returns over the event window containing the acquisition. Using asterisks for estimated values, the estimated normal return for the event window is $\alpha_i^* + \beta_i^* R_m$. The abnormal return, AR_i, is the difference between the actual return and the estimated normal return over the event window.

$$AR_i = R_i - (\alpha_i^* + \beta_i^* R_m) \quad (2)$$

This daily abnormal return is summed over the five days of the event window to yield the cumulative abnormal return (CAR), which is our measure of the "performance" of an acquisition.

Many papers have discussed problems in estimating abnormal returns. Much of the criticism amounts to observing that the model used to estimate predicted returns is an auxiliary hypothesis. If this auxiliary hypothesis is significantly inaccurate, then estimates of the abnormal return will also be inaccurate. However, for a short event window around an announcement, there is little bias from this source for any reasonable model of normal returns.

3.4. Variable Selection and Definition

In addition to acquisition abnormal returns, the other main explanatory variables are the number of public and private acquisitions made by a given CEO in a given year and are taken directly from the SDC M&A database. Most other variables come from Compustat. For most relevant economic concepts, Compustat provides several related measures. Our selections from the range of possibilities are as follows.

For CEO compensation, we use total compensation (variable TDC1 in Execucomp), which includes salary, bonuses, the value of option awards, and "other compensation" (largely stock grants and pension plan contributions). We use annual data, converting compensation over time and all other financial variables to real values (with 2016 as the base) using the CPI.

We use the natural logarithm of total compensation as the primary dependent variable. In wage or income regressions, logarithms are commonly used for at least two reasons, both of which apply in this case. First, the underlying data is highly skewed. Second, empirically, the log form is a better fit from a statistical point of view.

Firm size is commonly used as a control variable in financial economics, including in the study of executive compensation. However, as noted by Dang et al. (2018), there are several possible measures of size, and results may not be robust if the size variable is changed. The most commonly used size variables are total assets, employment, and sales revenue. We believe that sales revenue is the most suitable choice in our context as it has fewer missing observations than the other size measures and, arguably, is the best indicator

of the money available to pay senior executives. Therefore, we use revenue as the size measure in our preferred specifications. However, as a robustness check, we report one specification using assets instead of revenue as the size variable and one using employment We obtain very similar results in all cases. Both assets and sales revenue are converted to real 2016 vales using the CPI and we use natural logs of all three variables.

We use the return on assets (ROA) to measure profit. We also use two measures to reflect aspects of the board of directors—the share of shares held by institutional investors and the number of institutional investors. The importance of institutional investors is well-established in the literature as in, for example, Hartzell and Starks (2003). We control for age, which has a quadratic effect. Other things equal, CEO compensation first rises with age, peaks, and then declines, as is true in many occupations. We also include experience, using years in place (tenure). This effect is taken to be linear as, conditional on age, more experience is always better than less and the panels are short enough on average that we do not expect sharply diminishing returns to tenure. We also control for gender. In fixed effects panel regressions, gender cannot be used as it does not change over time in any panel, but it can be used in random effects regressions.

Compensation may be affected by economy-wide shocks. We control for this using fixed effects for the "dot-com" crash in 2001 and 2002 and for the financial crisis of 2007–2009. We also use the real oil price as a control measure, as oil price shocks are important for significant parts of the economy. We use the West Texas Intermediate price in 2016 real dollars. Industry fixed effects cannot be used in fixed effects regressions but we use NAICS 2-digit industry identifiers as a robustness check in the random effects formulation.

3.5. Data Description

In the Execucomp compensation data for 1992–2016, we have 43,094 CEO-firm-year observations that report both CEO total compensation and age. The dataset provides broad coverage of the U.S. economy. At the 2-digit NAICS level, by far the largest sector is manufacturing, followed by finance, and information. Those three sectors account for over 60% of the observations.

We drop five observations that report negative revenue. We also drop 275 observations in which a CEO moves from one firm to another within the same year, leaving us with 42,814 observations, of which exactly 1000 are for female CEOs. Some control variables are missing data for some observations, including revenue (439 obs.), assets (2737 obs.), employees (2654 obs.), the ownership share of institutional investors (1884 obs.), and the return on assets (3959 obs.). Therefore, the number of observations in a given regression depends on which control variables are included. Our primary regressions have about 38,000 observations. Table 1 provide descriptive statistics. Precise variable definitions are in Appendix A.

Table 1 shows that these CEOs have high incomes, with median total compensation exceeding $3 million 2016 dollars and average compensation exceeding $6 million. The comparison of means and medians shows that CEO compensation is highly skewed. Assets, employees, and revenue are also highly skewed. We use the natural logs of all these variables in our regressions.

The average return to acquisitions is negative for public acquisitions and positive for private acquisitions, but most observations report a return of zero because in any given year most CEOs do not make any acquisitions. The number of acquisitions is shown in the lower part of the table. In 36,700 observations, no private acquisitions are made. In 4320 observations, one private acquisition is made, in 1122 observations, there are two acquisitions, etc. Table 2 shows the pairwise correlation matrix for the main variables.

Table 1. Descriptive Statistics.

Variable	N	Ave	Median	St. Dev.	p10	p90
CEO Comp ($k)	42,814	6003	3362	11,876	866	12,825
CAR-all	42,814	0.024	0	3.08	0	0
CAR-pub	42,814	−0.043	0	1.37	0	0
CAR-priv	42,814	0.066	0	2.75	0	0
Assets ($b)	40,077	15.16	1.83	90.44	0.23	22.34
Employees (k)	40,160	17.95	4.64	56.51	0.47	41.07
Revenue ($b)	42,375	5.67	1.34	17.16	0.19	12.38
ROA	38,855	0.121	0.124	0.272	0.02	0.24
Inst. Share	40,930	0.568	0.679	0.366	0	1
CEO Age	42,814	56.02	56.00	7.52	47	65
CEO Tenure	42,803	4.82	4.00	3.80	1	10
Oil Price ($/bl)	42,814	57.48	46.83	31.32	26.21	95.42
Acquisitions	Private	Public			Female	Male
0	36,700	41,254		Obs	1000	41,814
1	4320	1403		Panels	196	7700
2	1122	126				
3	372	26				
4	137	3				
5 or more	106	2				

Table 2. Correlations.

	CEO-Comp	CAR-all	CAR-pub	CAR-priv	# of Public Acqs.	# of Private Acqs.	Assets	Employees	Revenue	ROA	Inst. Share	Age
CEO Comp	1											
CAR-all	0.017	1										
CAR-pub	0.009	0.447	1									
CAR-priv	0.015	0.895	0.001	1								
# of Public Acqs.	0.077	−0.083	−0.171	−0.008	1							
# of Private Acqs.	0.057	0.050	−0.009	0.061	0.127	1						
Assets	0.140	−0.004	0.006	−0.007	0.039	0.004	1					
Employees	0.160	−0.001	0.006	−0.004	0.041	0.026	0.232	1				
Revenue	0.225	−0.005	−0.002	−0.005	0.056	0.036	0.448	0.691	1			
ROA	0.026	0.006	0.008	0.003	−0.004	0.021	−0.031	0.027	0.015	1		
Inst. Share	0.018	0.012	−0.010	0.019	0.063	0.137	−0.026	−0.037	−0.024	0.071	1	
Age	0.013	−0.007	0.008	−0.012	0.003	−0.024	0.039	0.053	0.061	0.010	0.034	1
CEO Tenure	0.064	−0.004	0.004	−0.006	0.018	0.047	0.017	0.033	0.043	−0.006	0.177	0.342

Table 2 shows that the three performance variables (CAR-all, CAR-pub, and CAR-priv) all have small positive correlations with the dependent variable, CEO compensation. The CAR measures for public and private acquisitions are highly correlated with the overall CAR as they are components of it. As private acquisitions are more common, the correlation between the private and overall CAR is particularly high. The other two treatment variables, representing acquisition activity, have moderate positive correlations. The three size measures (Assets, Employees, and Revenue) have fairly high correlations with each other, particularly revenue with each of the others. These correlations are high enough to cause problems in interpreting the coefficients if more than one of these size

variables are included in the same regression. The count of institutional investors is also significantly correlated with the three size measures, as we would expect.

4. Results

4.1. Fixed Effects Regressions

Our primary results are based on panel data methods. A panel consists of a CEO-firm combination. If a CEO moves from one firm to another, a new panel is formed, although the partial years at each firm are dropped from the data. Similarly, if a firm hires a new CEO, a new panel is also formed. The underlying regression model is

$$y_{\mathrm{it}} = f(x_{it}, z_{\mathrm{it}}) + v_i + e_{it} \tag{3}$$

where y_{it} is the compensation of CEO i at time t, x_{it} and z_{it} are corresponding vectors of treatment and control variables, v_i is an unobservable individual effect associated with CEO i that applies while at a specific firm, and e_{it} is a random error. If v_i is invariant over time, this is a fixed effects model. If v_{i} varies randomly, the model is a random effects model.

Table 3 reports fixed effects regressions. In a fixed effects regression, no time invariant explanatory variables can be included as they are colinear with the individual fixed effects. Therefore, neither gender nor industry identifiers can be used. We do not include tenure, as it is collinear with the linear component of age within each panel (each increases by one each year). The results are determined entirely by within-panel variation—variation over time. In all regressions, we use robust standard errors clustered at the CEO-firm level.

Table 3 establishes the main results of the paper. The first result column does not distinguish between private and public acquisitions. It shows that both the acquisition return (CAR) and acquisition activity have significant positive effects on CEO compensation.

The second result column separates returns and activity into two components and distinguishes between public and private acquisitions. We take column 2 as our preferred specification. The return to public acquisitions loses its statistical significance, while the return to private acquisitions continues to have a statistically significant positive effect. Both public and private acquisition volumes have a significant positive effect, but the coefficient on public acquisitions is much larger.

The third result column includes only observations for which, as of year t, the CEO-firm combination has at least one acquisition prior to year t. As most CEOs do not make any acquisitions in a given year, the sample size drops dramatically. Even so, the same general pattern as in the second column applies. Result columns 4 and 5 show the effect of using different size measures (total assets and employees instead of revenue) and exhibit the same qualitative pattern of results.

In all specifications, the control variables have interesting and generally plausible effects. Age has the expected quadratic pattern with compensation rising rapidly with age initially then more slowly and eventually hitting a peak and then declining for CEOs who are old enough. The size variables have large positive effects on CEO compensation, as does the return on assets. Having a larger share of the firm owned by institutional investors is associated with higher CEO compensation. However, the number of institutional investors exhibits statistical significance in only one specification. As expected, the financial crisis of 2007–2009 had a negative effect on CEO compensation, but the "dot-com" crash of 2001–2002 did not, reflecting the fact that very few of these firms were "dot-com" firms. Oil prices are negatively associated with CEO compensation.

Table 3. Fixed Effect Panel Regressions.

	(1)	(2)	(3) (Acq > 0)	(4)	(5)
CAR-all	0.478 ***				
	(0.155)				
CAR-pub		0.490	0.379	0.488	0.492
		(0.344)	(0.365)	(0.344)	(0.361)
CAR-priv		0.499 ***	0.291 *	0.493 ***	0.404 **
		(0.172)	(0.166)	(0.170)	(0.174)
# of Acquisitions	0.0303 ***				
	(0.00720)				
# of Public Acq.		0.0577 ***	0.0412 *	0.0496 **	0.0548 **
		(0.0209)	(0.0248)	(0.0210)	(0.0222)
# of Private Acq.		0.0265 ***	0.0217 **	0.0239 ***	0.0275 ***
		(0.00730)	(0.00835)	(0.00722)	(0.00774)
Age (yrs)	0.0877 ***	0.0876***	0.0843 **	0.0704 ***	0.0876 ***
	(0.0163)	(0.0163)	(0.0313)	(0.0161)	(0.0172)
Age Squared	−0.00052 ***	−0.00052 ***	−0.00054 *	−0.00044 ***	−0.00048 ***
	(0.000144)	(0.000144)	(0.000281)	(0.000140)	(0.000151)
Log of Revenue ($b)	0.320 ***	0.320 ***	0.214 **		
	(0.0405)	(0.0405)	(0.0866)		
Log of Assets ($b)				0.190 ***	
				(0.0190)	
Log of Employees (k)					0.147 ***
					(0.0315)
ROA (%)	0.146 ***	0.146 ***	0.731 ***	0.138 ***	0.154 ***
	(0.0462)	(0.0463)	(0.170)	(0.0501)	(0.0502)
Inst. Inv. Shares (%)	0.362 ***	0.361 ***	0.428 ***	0.354 ***	0.374 ***
	(0.0287)	(0.0287)	(0.0638)	(0.0284)	(0.0309)
Financial Crisis (0/1)	−0.125 ***	−0.126 ***	−0.159 ***	−0.115 ***	−0.115 ***
	(0.0117)	(0.0117)	(0.0192)	(0.0118)	(0.0120)
Dot Com (0/1)	0.0516 ***	0.0511 ***	0.0694 **	0.0456 **	0.0613 ***
	(0.0184)	(0.0185)	(0.0317)	(0.0179)	(0.0183)
Oil Price ($)	−0.102 ***	−0.102 ***	−0.0476	−0.102 ***	−0.0890 ***
	(0.0174)	(0.0174)	(0.0267)	(0.0175)	(0.0183)
Constant	4.321 ***	4.323 ***	4.618 ***	3.970 ***	4.230 ***
	(0.461)	(0.461)	(0.904)	(0.451)	(0.484)
Observations	38,709	38,709	14,055	38,709	36,696
# of Panels	7239	7239	2791	7239	7014
R-squared w/o FE	0.217	0.217	0.214	0.254	0.108)
Adj. R-squared incl. FE	0.718	0.718	0.695	0.719	0.714
F statistic	101.1 ***	84.49 ***	23.14 ***	88.22 ***	70.08 ***

Notes: The dependent variable for each specification is logged real compensation. Panel data fixed effects methods are used. Robust standard errors clustered at the CEO-firm level are shown in parentheses. ***, **, and * represent statistical significance at the 0.01, 0.05, and 0.1 levels, respectively. Detailed variable definitions are in Appendix A.

4.2. Random Effects Regressions

Table 3 identifies the effects of acquisitions using just within-panel time series information. However, cross-sectional information is also potentially valuable. Table 4 incorporates cross-sectional as well as within-panel variation over time to obtain estimates by using random effects panel data estimation.

Computationally, random effects coefficients are a weighted average of coefficients obtained from a fixed effects regression and the corresponding purely cross-sectional "between" regression based on panel averages. As the data set contains substantial cross-sectional as well as time series variation, the random effects model provides a more complete representation of the patterns in the data.

The overall pattern for random effects regressions in Table 4 is similar to that for the fixed effects regression results even though Table 4 is based in part on cross-sectional variation. Returns to public acquisitions do not have a significant effect on compensation but returns to private acquisitions do. For public acquisitions, a CEO gains just by making the acquisition irrespective of whether the acquisition generates returns for shareholders. For private acquisitions, performance is important, although there is also a modest gain just for acquisition activity.

Table 4. Random Effects Panel Regressions.

	(1)	(2)	(3) (Acq > 0)	(4)	(5)	(6)
CAR-all	0.476 ***					
	(0.152)					
CAR-pub		0.341	0.178	0.372	0.348	0.353
		(0.333)	(0.343)	(0.333)	(0.349)	(0.334)
CAR-priv		0.541 ***	0.481 ***	0.534 ***	0.479 ***	0.547 ***
		(0.169)	(0.172)	(0.167)	(0.169)	(0.169)
# of Acquisitions	0.0291 ***					
	(0.00663)					
# of Public Acq.		0.0660 ***	0.0605 ***	0.0430 **	0.0656 ***	0.0599 ***
		(0.0199)	(0.0226)	(0.0205)	(0.0213)	(0.020)
# of Private Acq.		0.0238 ***	0.0180 **	0.0216 ***	0.0229 ***	0.0215 ***
		(0.00675)	(0.00754)	(0.00676)	(0.00714)	(0.00672)
Age (yrs)	0.0597 ***	0.0595 ***	0.0589 **	0.0447 ***	0.06034 ***	0.0623 ***
	(0.0134)	(0.0134)	(0.0248)	(0.0124)	(0.0134)	(0.0133)
Age Squared	−0.00062 ***	−0.00062 ***	−0.00063 ***	−0.00050 ***	−0.00063 ***	−0.00064 ***
	(0.000119)	(0.000119)	(0.000221)	(0.000111)	(0.000119)	(0.000118)
CEO Tenure (yrs)	0.0301 ***	0.0302 ***	0.0222 ***	0.0217 ***	0.0343 ***	0.0290 ***
	(0.00254)	(0.00254)	(0.00430)	(0.00250)	(0.00264)	(0.00253)
Female (0/1)	0.120 **	0.120 **	−0.00388	0.101 *	0.0862	0.147 ***
	(0.0494)	(0.0494)	(0.010)	(0.0550)	(0.0567)	(0.0504)
Log of Revenue ($b)	0.541 ***	0.541 ***	0.513 ***			0.564 ***
	(0.0139)	(0.0139)	(0.0214)			(0.0144)
Log of Assets ($b)				0.299 ***		
				(0.00645)		
Log of Employees (k)					0.350 ***	
					(0.0102)	
ROA (%)	0.150 ***	0.150 ***	0.509 ***	0.147 ***	0.158 ***	0.151 ***
	(0.0443)	(0.0444)	(0.141)	(0.0554)	(0.0488)	(0.0447)
Inst. Share (%)	0.414 ***	0.413 ***	0.557 ***	0.389 ***	0.413 ***	0.407 ***
	(0.0219)	(0.0219)	(0.0495)	(0.0216)	(0.0234)	(0.0219)
Financial Crisis (0/1)	−0.143 ***	−0.143 ***	−0.184 ***	−0.122 ***	−0.129 ***	−0.145 ***
	(0.0117)	(0.0116)	(0.0191)	(0.0117)	(0.0118)	(0.0116)
Dot Com (0/1)	0.0439 **	0.0434 **	0.0611 **	0.0376 **	0.0562 ***	0.0419 **
	(0.0176)	(0.0176)	(0.0290)	(0.0174)	(0.0179)	(0.0177)
Oil Price ($)	−0.0608 ***	−0.0602 ***	−0.0194	−0.0914 ***	−0.0214	−0.0634 ***
	(0.0162)	(0.0162)	(0.0291)	(0.0161)	(0.0179)	(0.0162)
Constant	5.735 ***	5.739 ***	5.742 ***	4.641 ***	5.606 ***	5.398 ***
	(0.373)	(0.373)	(0.690)	(0.344)	(0.376)	(0.403)
Industry Fixed Effects	No	No	No	No	No	Yes
Observations	38,698	38,698	14,054	38,698	36,687	38,685
# of Panels	7237	7237	2790	7237	7012	7227
R-squared	0.308	0.309	0.323	0.314	0.239	0.335
Chi-squared	3048 ***	3067 ***	1233 ***	3680 ***	2265 ***	3579 ***

Notes: The dependent variable for each specification is logged real compensation. Panel data with random effect methods are used. Robust standard errors clustered at the CEO-firm level are shown in parentheses. ***, **, and * represent statistical significance at the 0.01, 0.05, and 0.1 levels, respectively. Detailed variable definitions are in Appendix A.

As shown in Table 2, only a small fraction of CEOs are female (about 2.3%). However, that is still enough observations to estimate a meaningful effect. Looking across all the regressions on the full sample, being female had a positive effect on CEO compensation on the order of 10%. The random effects model also allows us to look at the effect of tenure (experience) as distinct from age. As expected, that effect is positive. For any given age, having longer tenure in the position has a positive effect on compensation at the rate of about 3% per year. In addition, we can consider the specific industry effects, which we do by including 2-digit NAIC codes. Including industry effects has very little impact on the treatment variable coefficients or standard errors. The other control variables exhibit plausible but interesting effects in Tables 3 and 4.

4.3. Fixed or Random Effects

Whether fixed effects or random effects specifications are preferred depends on whether the individual effect in Equation (3) is reasonably viewed as time-invariant (fixed effects) or as random and uncorrelated with other explanatory variables (random effects). As Wooldridge (2014, p. 496) points out, if the results of interest are similar using both approaches, then we are not too concerned about which approach is preferred. That statement applies here as both fixed effect and random effect models yield similar qualitative results.

However, we still do a Hausman test to shed light on which model should be emphasized. We use the preferred fixed effects specification from result column 2 in Table 3 and use the same explanatory variables for the random effects specification. The results are in Table 5, which shows that the null hypothesis that the coefficients are the same in both regressions can be rejected given the Chi-squared value of 522.5. We therefore conclude that, if the fixed effects specification is correct, the random effects estimators are not consistent and the fixed effects specification would be preferred. However, the differences in the estimated coefficients on the treatment variables are minor and do not change the qualitative nature of the results.

Table 5. Hausman Test.

	Fixed	**Random**	**Difference**
	Effects	**Effects**	**(Std. Err)**
CAR-pub	0.490 *	0.339	0.152
	(0.258)	−0.254	(0.055)
CAR-priv	0.499 ***	0.529 ***	−0.0303
	(0.125)	−0.123	(0.0261)
# of Public Acq.	0.0577 ***	0.0646 ***	−0.00689
	−0.0167	−0.0163	(0.00366)
# of Private Acq.	0.0265 ***	0.0251 ***	0.00131
	(0.00576)	−0.00558	(0.00156)
Age (yrs)	0.0876 ***	0.0716 ***	0.0160
	−0.00904	(0.00740)	(0.00527)
Age Squared	−0.000524 ***	−0.00058 ***	0.0000582
	-7.83×10^{-5}	-6.46×10^{-5}	(0.000045)
Log of Revenue ($b)	0.320 ***	0.557 ***	−0.238
	−0.0183	−0.00891	(0.0161)
ROA	0.146 ***	0.141 ***	−0.00496
	(0.0149)	−0.0145	(0.00399)
Inst. Share (%)	0.361 ***	0.467 ***	−0.106
	−0.0214	0.017	(0.0131)
Financial Crisis (0/1)	−0.126 ***	−0.163 ***	0.0371
	(0.0113)	(0.0110)	(0.00265)
Dot Com (0/1)	0.0512 ***	0.0421 ***	0.00910
	(0.0133)	−0.0131	(0.00285)
Oil Price ($/bl)	−0.102 ***	−0.0045	−0.106
	(0.0176)	(0.0161)	(0.00733)
Constant	4.323 ***	4.967 ***	
	−0.257	−0.21	
Observations	38,709	38,709	
# of Panels	7239	7239	
R-squared (w/o FE)	0.217	0.303	
F statistic	222.7 ***		
Chi-squared		6389 ***	522.5 ***

Notes: The dependent variable for each specification is logged real compensation. Robust standard errors clustered at the CEO-firm level are shown in parentheses. *** and * represent statistical significance at the 0.01 and 0.1 levels, respectively. Detailed variable definitions are in Appendix A.

4.4. Endogeneity and Instrumental Variables

Endogeneity of explanatory variables is a significant issue in empirical finance, as described in the review by Roberts and Whited (2013). However, our most important explanatory variables are the abnormal return (CAR) variables. These returns are determined by financial markets and can reasonably be taken as exogenous.

Our other treatment variables are acquisition activity variables. As noted by Choi et al. (2020), it is possible that acquisition activity might be endogenous. For example, CEOs with more aggressive personalities may be more successful in obtaining compensation increases and may also be more inclined to make acquisitions. If so, we would observe a positive correlation between acquisition activity and CEO compensation, but that correlation would be induced by an unobserved variable (aggressiveness) and would not reflect a causal effect of acquisition activity on compensation. This is endogeneity of the unobserved heterogeneity type.

Our primary effort to address this potential endogeneity uses fixed effects, as in Table 3. Unobserved individual effects (such CEO personality) can be viewed as an individual effect of the type shown in Equation (3). As noted by Wooldridge (2014, p. 513) such unobserved heterogeneity can be corrected by fixed effects if "the omitted variable does not change over time". In our case, the omitted variable is some CEO personality trait. Our panels are relatively short as the median panel is 4 years in length and 75% of the panels are seven years or less in length. CEO personalities are unlikely to change meaningfully over such time horizons. Therefore, we believe that our results from fixed effect panel data regressions are reliable.

However, as a robustness check, we investigate using two-stage least squares with instrumental variables (IV) to correct for possible endogeneity. One set of instruments are industry fixed effects at the two-digit level. The justification for using industry fixed effects is that acquisition rates vary significantly by industry on the basis of technological change and other factors that have little to do with the characteristics of individual CEOs. Therefore, industry should capture some exogenous component of acquisition activity.

Industry fixed effects are, however, purely cross-sectional. They do not change over time for any firm-CEO combination. To incorporate time series variation, we also include lagged net income on the grounds that net income provides financial resources with which to undertake acquisitions. The logic is that net income in year t affects acquisitions in year $t + 1$, which then affects CEO compensation in year $t + 2$. Furthermore, net income in year t would have little direct effect on CEO compensation in year $t + 2$ given that ROA in year $t + 1$ is an explanatory variable for compensation in year $t + 2$.

We use these instruments in a pooled time-series and cross-sectional two-stage IV regression. In the first stage, we estimate the number of public acquisitions and the number of private acquisitions. In the second stage, we take the predicted values for public and private acquisitions from the first stage and use them in the estimation explaining CEO compensation. If the first stage regressors are well-chosen, then the predicted values of acquisition activity will reflect the exogenous component of acquisition activity, eliminating (or at least reducing) the endogenous component.

As is standard, the first stage regressions include all exogenous regressors in the second stage regression. For the second stage, we use all the explanatory variables used in the random effects regression shown in column 2 of Table 4. The results are shown in Table 6.

Table 6. Two-Stage Method with Instrumental Variables.

	Stage 1	Stage 1	Stage 2
	Public Acqs.	**Priv. Acqs.**	**Total Comp.**
CAR-pub	−2.68 ***	−0.193	10.17 ***
	(0.518)	(0.538)	(2.65)
CAR-priv	−0.0635	1.323 **	1.16 ***
	(0.0730)	(0.624)	(0.421)
# oPublic Acqs.			4.07 ***
			(0.649)
# of Private Acqs.			−0.132
			(0.129)
Net income in $t-2$	3.78×10^{-6} ***	2.05×10^{-4} ***	
	(1.27×10^{-6})	(7.96×10^{-6})	
Industry Fixed Effects (2-digit NAICS)	Yes	Yes	No
Age (yrs)	0.00307 **	−0.00932	0.0670 ***
	(0.00152)	(0.00808)	(0.0214)
Age Squared	-3.07×10^{-4} **	4.49×10^{-4}	-6.43×10^{-3} ***
	(1.29×10^{-5})	(6.97×10^{-4})	(1.91×10^{-3})
CEO Tenure (yrs)	0.000335	0.00871*	0.0162 ***
	(0.00052)	(0.00470)	(0.00429)
Female	−0.0166 **	−0.0643 *	0.112
	(0.0066)	(0.0331)	(0.0760)
Log of Revenue ($b) in $t-1$	0.0196 ***	0.384 ***	0.518 ***
	(0.00235)	(0.00998)	(0.0196)
ROA in $t-1$	0.00427 **	0.0234	0.254 ***
	(0.00227)	(0.0154)	(0.0630)
Inst. Share (%)	0.0455 ***	0.259 ***	0.326 ***
	(0.00397)	(0.156)	(0.0543)
Financial Crisis (0/1)	0.0159 ***	0.448 ***	−0.225 ***
	(0.00354)	(0.0142)	(0.0238)
Dot Com (0/1)	0.00974 *	4.00×10^{-3}	−0.0036
	(0.00582)	(0.0158)	(0.033)
Oil Price ($)	−0.0527 ***	−0.0979 ***	0.296 ***
	(0.00580)	(0.0194)	(0.0449)
Constant	−0.0735	0.311	5.217 ***
	(0.0466)	(0.241)	(0.593)
Observations	31,239	31,239	31,239
F-test for Instr.	12.10 ***	12.56 ***	
Cragg-Donald F-stat			11.64 *

Notes: The dependent variable for each result column is shown in the header. Robust standard errors clustered at the CEO-firm level are shown in parentheses. ***, ** and * represent statistical significance at the 0.01 and 0.1 levels, respectively. Detailed variable definitions are in Appendix A.

The two-stage method yields a similar pattern of results for the possibly endogenous regressors as the fixed effects and random effects regressions. Specifically, the number of public acquisitions has a strongly significant positive effect on compensation, and the number of private acquisitions has no significant effect. However, in this two-stage IV regression, returns to both private and public acquisitions are positive and significant, as would be consistent with the shareholder value hypothesis for both types of acquisition. The control variables have a similar pattern to the panel data regressions.

The first stage regressions have some interesting features. First, the female fixed effect has a significant negative effect on acquisitions: Female CEOs undertake fewer acquisitions than their male counterparts. Another interesting result is that the financial crisis had a positive effect on acquisition activity even though it had a negative effect

on CEO compensation. This is not surprising as the financial crisis caused a substantial amount of corporate restructuring.

The IV regression diagnostics (F-tests and Cragg-Donald test) suggest that the instruments have only borderline significance. Therefore, while this regression has some value as a robustness check, we take the fixed effects regression results in Table 3 as providing the best estimates.

4.5. Economic Significance

The pattern of statistical significance in our results is clear. However, are the implied economic effects meaningful? To illustrate economic effects, we use specification 3 from Column 2 in Table 3, which is our preferred specification on theoretical grounds. It is also the specification best supported by the Hausman test and by regression diagnostics, although other specifications are similar.

In a regression of a logged dependent variable on a level, the coefficient multiplied by 100 shows (approximately) the percentage effect of increasing the explanatory variable by one unit. Our acquisition returns are measured in decimal form (i.e., 1% abnormal return appears as 0.01). Therefore, we estimate that if a private acquisition generates a one percentage point abnormal increase in the value of the acquiring firm's stock, CEO compensation would rise by about half (0.499) of a percentage point. The average annual total compensation for CEOs in our data is about $6 million. Half a percent of that is approximately $30,000.

A "typical" acquisition generates an abnormal return of more than 1% in absolute value. The median absolute value of the abnormal return to a private acquisition in our data is approximately 2.7% (with the actual return being negative about half the time). Therefore, a typical private acquisition is estimated to have an acquisition performance effect on compensation on the order of ($2.7 \times 60{,}000 \approx$) $162,000 (positive or negative). Standard errors are large enough that the actual effect could be much larger or much smaller. However, this effect seems of plausible magnitude, albeit small compared to the implied effect on shareholder value. For public acquisitions, abnormal returns to acquisitions do not have a statistically significant effect.

For acquisition activity, the effect of one additional private acquisition is to raise compensation by 2.65%, which is about $160,000 on average. Public acquisitions have a much larger effect, leading to an estimated increase in compensation of 5.77%, which is about $346,000 for the average CEO. These values are larger than the performance values, even for private acquisitions. Thus, acquisition activity is more economically important than acquisition performance in explaining compensation variations for both private and public acquisitions, and the difference is much greater for public acquisitions.

The estimated effects of the control variables are reasonable. The revenue variable is a logarithm, so the coefficient 0.320 is an elasticity, indicating that real compensation rises by about 1/3 of a percent when firm size as measured by revenues rises by 1%. This is a substantial size effect that is consistent with previous research.

The financial crisis had a significant effect on CEO compensation. The point estimate in Table 3 indicates that, other things equal, real CEO compensation was about 12.6% lower during the financial crisis years of 2007–2009, which is a plausible magnitude. The dot com crash of 2001–2002 is associated with small (5.1%) premium in CEO salaries, reflecting the fact that the dot com crash did not have much negative effect on compensation of S&P 1500 CEOs and this was a period of generally rising CEO compensation.

The effect of oil prices varies by firm. Some firms, particularly those in the energy sector, do well when oil prices rise and we would expect CEO compensation in those firms to rise. However, some firms do poorly, especially those in the transport sector and, to a lesser extent, in manufacturing. Therefore, it is useful to control for oil prices, but the expected aggregate effect could go either way. Looking just at time series variation (using panel fixed effects), the overall effect of oil prices is negative. We take this as our "best estimate" in light of the Hausman test reported in Table 5.

4.6. Robustness

We have reported some robustness checks in previous sections. In particular, we show that the results are robust to using different size controls (revenue, assets, or employees) and are robust to the inclusion of industry fixed effects (in the random effects model). Including fixed effects for the information technology and finance sectors does not change results. Instead of using the oil price and fixed effects to capture the "dot-com" crash and the financial crisis of 2007–2009 to capture economy-wide shocks, we also tried general year-specific fixed effects. That substitution has little effect on the results and essentially reproduces the effect of the dot-com, financial crisis, and oil price controls.

In addition to using panel data estimations, we also used a pooled regression (i.e., without using the panel structure) and first differences. Using random effects is generally better than using pooled regressions to incorporate cross-sectional variation, and using fixed effects is generally better than first differencing to capture within-panel variation over time. We do not report those results, although they do give a similar pattern.

Furthermore, we report a two-stage pooled regression using instrumental variables to control for possible endogeneity of the acquisition activity variables. The instruments are of only marginal significance, so we do not place much weight on these results. However, the results of the activity variables are strengthened by this approach as the public acquisition variable increases in magnitude and statistical significance, while the private acquisition variable loses statistical significance and the point estimate turns negative. This two-stage approach also affects the return variables, with returns to both public and private acquisitions having a statistically significant positive effect.

Our battery of reported control variables is similar to that used in other studies of CEO compensation, although it is fairly parsimonious. Other variables that are sometimes used include leverage and membership of the CEO on the compensation committee (usually as captured by the interlock variable in Compustat). Neither of those variables is significant in our regressions and neither materially affects the results.

5. Concluding Remarks

This paper estimates the effect of acquisition performance and acquisition activity on CEO compensation for the full set of large public U.S. corporations (S&P 1500) over the 25-year period 1992–2016, paying particular attention to the different effects of public and private acquisitions.

At the end of Section 2, we stated four specific hypotheses regarding the effect of acquisitions on CEO compensation. Our analysis provides evidence on those hypotheses. We find that Hypothesis 1 (the shareholder value hypothesis) is supported for private acquisitions but not for public acquisitions in our panel data regressions. Specifically, acquisition performance as measured by stock market returns has a statistically significant and economically meaningful positive effect on CEO compensation for private acquisitions, but the effect for public acquisitions is not statistically significant.

Hypothesis 2 (the agency theory hypothesis) is strongly supported for both public and private acquisitions as acquisition activity of both types increases CEO compensation independent of shareholder returns. Hypotheses 3 and 4 (differential effects hypotheses) are also supported. In our panel data regressions, the effect of shareholder returns on CEO compensation is weaker for public acquisitions than for private acquisitions, and the effect of acquisition activity on CEO compensation is stronger for public acquisitions than for private acquisitions.

We use both fixed effects and random effects panel data regressions to test these hypotheses. While there is a statistically significant difference between fixed and random effects models, as indicated by a Hausman test, the qualitative pattern of results is similar for both approaches.

In addition to standard fixed and random effects models, we also use a two-stage least squares approach, using instrumental variables in a pooled time-series and cross-sectional framework, to address the potential endogeneity of the acquisition activity variables. The

instruments are of moderate quality and we would not place too much confidence in their results. However, they are consistent with our general conclusions regarding stronger agency effects for public acquisitions.

The overall combined effects for both public and private acquisitions are also of interest given our near-comprehensive coverage of large U.S. acquirers. Overall, there is a positive but modest effect of performance on CEO compensation and a strong positive effect of acquisition activity. Therefore, while the previous literature shows that firms that monitor CEOs more closely have a tighter link between performance and compensation, our analysis shows that the overall effect of such monitoring (at least in the population of large public U.S. corporations) is of surprisingly modest economic importance.

We think our results contribute to the academic literature in identifying two previously unrecognized but important factors affecting CEO compensation—the differential effect of public and public acquisition performance, and the differential effect of acquisition activity. Our results also suggest that agency problems are more severe in public acquisitions.

In addition, our analysis also has implications for managers, investors, and company directors. Specifically, it suggests that public acquisitions should attract a higher level of scrutiny than private acquisitions from all three groups.

We recognize that this paper is just one step forward in the analysis of executive compensation and is far from definitive. First, the endogeneity problem is a major issue in empirical financial economics. Our use of an exogenous performance measure, fixed and random effects panel regressions, and a two-stage estimation to mitigate endogeneity are useful but limited responses. Further development of econometric techniques and of data sets that have a quasi-experimental character are important areas of research.

Additionally, we make only a rudimentary attempt to address how board-level governance affects CEO compensation and make no attempt to focus on how specific contractual structures affect CEO behavior and CEO compensation. These are important active research areas. Another important factor that we abstract from is behavioral economics. There is a growing literature on social and psychological determinants of CEO behavior and of board of director behavior in setting CEO compensation, and we acknowledge that such factors have significant explanatory power. These are all important areas for ongoing research.

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Appendix A. Variable Definitions

Variable	Definition (Source)
CEO-Firm	Indicator variable denoting the CEO-firm pair (Execucomp)
CUSIP	CUSIP number (Execucomp, CRSP/Compustat, SDC M&A)
R_{mt}	Value-weighted market return (CRSP: VWRETD)
R_{it}	Return for firm *i* (CRSP: RET)
AR_i	Abnormal return for firm *i* (CRSP/SDC M&A)
CAR	Cumulative Abnormal 5-day return (CRSP/SDC M&A)
CAR-All	Cumulative Abnormal Return from all acquisitions
CAR- Public	Cumulative Abnormal Return from public acquisitions
CAR- Private	Cumulative Abnormal Return from private acquisitions
CEO Comp (2016$k)	Total compensation to the CEO (Execucomp: TCD1)
CEO Tenure (yrs)	Tenure of the CEO with the firm (Execucomp)
Age (yrs)	Age of the CEO (Execucomp: AGE)
# of Acquisitions	Number of acquisitions completed (SDC M&A)
# of Public Acq.	Number of public acquisitions completed (SDC M&A)
# of Private Acq.	Number of private acquisitions completed (SDC M&A)
Revenue (2016$b)	Firm revenue (Compustat: REVT)
Employment (k)	The number of FTE employees (Compustat: EMP)
Assets (2016$b)	Total assets (Compustat: AT)
Net Income (2016$b)	Net income (Compustat: NI)
ROA	Return (i.e., EBITDA) on assets (Compustat: EBITDA, AT)
Inst. Share (%)	Fraction of common stock held by institutional investors (TR-13F)
Industry	2-digit NAICS code for the firm (SDC M&A)
Year	The year of the CEO's compensation package (Execucomp)
Financial Crisis (0/1)	Indicator for years 2007–2009
Dot Com (0/1)	Indicator for years 2001–2002
Oil Price ($/bl)	Price of West Texas Intermediate Crude Oil
CPI deflator	A deflator to convert amounts into 2016 dollars (BEA)

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