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 (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 effect on compensation. Furthermore, compensation is more sensitive to acquisition activity than to performance. Our results suggest that agency considerations are important for both public and private acquisitions but are more important for public acquisitions.

JEL codes: G34, M12

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Declarations of interest: none

1. Introduction

Theories of executive compensation can be divided 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 alternative 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.

One important class of decisions made by CEOs relates to mergers and acquisitions. 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, Ch. 8) indicates no consensus. As described more fully in our literature review, different studies have reached different conclusions regarding the effect of acquisition performance on CEO compensation. Given this range on findings, simply asking whether the hypothesis is "true" is perhaps too simplistic. A few 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, our primary 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 difference between acquisitions of *publicly traded* targets ("public acquisitions") and acquisitions of *privately held* targets ("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 than for private

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 *performance*, on CEO compensation. In any given year, a particular 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. This effect would be stronger for public than for private acquisitions if public acquisitions are more prone to agency effects.

A third objective is to assess the overall empirical relationship between acquisition performance (as measured by abnormal returns to equity) and CEO compensation as comprehensively as possible for the U.S. economy. Most previous studies use relatively small data sets, leaving open the question of the overall significance of acquisition performance effects on CEO compensation. Furthermore, most studies in the literature start by identifying a set of acquisitions, and then estimate the performance of those acquisitions and the effect of that performance on CEO compensation. We use a different approach, starting with the entire set of U.S.-based CEOs provided by the S&P Execucomp database. This database covers all U.S. companies in the S&P 1500 from 1994 through 2016 and all those in the S&P 500 for 1992 and 1993. It includes over 7,000 CEOs. We then track the acquisition record for all of those CEOs. We therefore have essentially the complete record of disclosed acquisitions made by large U.S.

publicly traded corporations between 1992 and 2016 and include the "control group" of large corporations that did not make acquisitions in any given year.

Finally, we draw attention to another important consideration that has not received much explicit emphasis in the relevant literature, which is possible endogeneity of the unobserved heterogeneity type. It is possible that both acquisition performance and CEO compensation are due in part to unobserved characteristics of the CEO. For example, CEOs with a "proactive bias" or that are "more aggressive" may be more likely to engage in low-value (or negative-value) acquisitions *and* be more likely to obtain large salary increases. The panel structure of our data allows us to assess, at least to some extent, whether such an effect is significant.

Our main findings are as follows. First, we find that there is a striking difference between public acquisitions and private acquisitions. For private acquisitions, acquisition performance has a statistically significant and economically meaningful positive effect on CEO compensation. For public acquisitions, the point estimate in our preferred specifications is positive, but is not statistically significant. Therefore, our evidence is consistent with the expectation that the shareholder value hypothesis applies more strongly to private acquisitions than to public acquisitions.

A second main finding is that the number of public acquisitions has a strong positive effect on CEO compensation. The number of private acquisitions also has a statistically significant positive effect, although smaller than for public acquisitions. This finding is also consistent with the overall hypothesis that agency distortions are more significant for public acquisitions.

When we aggregate public and private acquisitions, we find that the overall effect of acquisition performance on CEO compensation is positive and significant, as private acquisitions

dominate numerically. Aggregated acquisition activity also has a positive, statistically significant, and important effect. But, as suggested by the previous paragraph, to understand this aggregate effect properly it is important to separately identify public and private acquisitions.

As for possible endogeneity bias of the unobserved heterogeneity type, our panel data estimations suggest, based on Hausman tests, that such bias is likely present. However, these effects are of only modest economic importance and do not affect the primary qualitative conclusions of our analysis.

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

The conceptual starting point of our analysis derives 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 what we now understand as agency problems. We do not attempt to 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 in which CEOs and other senior managers may extract value from shareholders for their own benefit. Much of the early work of this type is reviewed in Murphy (1999), and a more recent overview is provided by Frydman and Saks (2010).

In the presence of agency effects, compensation is not necessarily closely tied to performance in increasing shareholder value. In this paper we focus specifically on the *acquisition* performance of CEOs. In the presence of agency effects, 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 alternative, studied by Avery et al. (1998), is that CEOs pursue acquisitions to enhance their general standing in the business community. Such CEOs might succeed in raising their compensation because their enhanced profile raises their potential for outside offers, putting

pressure on their current firms to increase their compensation. Similarly, 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."

The shareholder value hypothesis reduces to the question of how effectively contracts, monitoring, and other tools can mitigate agency problems 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. Varaiya and Ferris (1987) provide an early finding of pervasive low or negative returns to acquisitions. The title of the widely cited paper by Moeller et al. (2005) – "Wealth Destruction on a Massive Scale? …" summarizes much of the subsequent literature. Officer et al. (2008) conclude that: "Three decades of evidence on takeovers of publicly-traded targets by publicly-traded acquirers demonstrates that such takeovers are at best wealth neutral for bidding firm stockholders and potentially wealth destroying." A review article by Eckbo (2009) notes that the overall average short-run return is at best low and, in many studies, negative. Conversely, target firms seem to make large gains. Betton et al. (2008) estimate that acquirers paid an average premium of 46% relative to the prior stock market price of the target firms, generating a substantial gain for target firm shareholders. 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 (but still surprisingly low) returns to acquirers.

Among the earliest direct 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, but value-increasing acquisitions yielded larger increases. Lambert and Larker (1987) can be taken as evidence in favor of the shareholder value hypothesis. However, as even value-reducing acquisitions yielded some salary increases, the results are far from definitive, especially as there were no CEOs who did not make acquisitions to use as a control group for comparative purposes.

In another early study, Schmidt and Fowler (1990) use an explicit control group and find, using a relatively small sample of 51 acquiring firms and 41 comparable non-acquiring firms (controls), that CEOs of acquiring firms experienced greater compensation increases, irrespective of the subsequent performance of the acquiring firm. Similarly, Grinstein and Hribar (2004) show that CEO bonuses are more responsive to acquisition activity than to acquisition performance. These two papers provide evidence against the shareholder value hypothesis.

Other papers, in addition to Lambert and Larcker (1987), that support the shareholder value hypothesis, at least to some extent, include Conyon and Gregg (1994) (169 acquisitions), Dorata (2008) (323 acquisitions), Khorana and Zenner (1998) (27 acquisitions), Girma et al. (2006) (472 acquisitions), and Guest (2009) (4,528 acquisitions). However, a number of papers find evidence against the shareholder value hypothesis, including Firth (1991) (171 acquisitions), Dorata and Petra (2008) (77 acquirers), Bliss and Rosen (2001) (151 acquirers), Anderson et al.

(2004) (97 acquisitions), and Harjoto et al. (2012) (598 acquisitions). 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, the number of external analysts, and other variables). Similarly, Harford and Li (2007) find more sensitivity of CEO compensation to acquisition performance when boards are stronger. Monitoring within family firms is studied by De Cesari et al. (2016).

In this paper, we consider a possible endogeneity bias that might arise if unobserved characteristics of CEOs affect both acquisition performance and compensation, inducing a correlation between these variables that is not causal. Yim (2013) shows that CEO age (an observable CEO characteristic) has a significant effect on acquisition behavior, with younger CEOs more likely to engage in acquisitions. Such endogeneity is also consistent with the finding of Blomkvist, Felixson and Korkeamäki (2018), that CEOs with higher "individuality" scores are more inclined to make acquisitions. If such CEOs are also better at obtaining compensation increases, then we would observe a positive correlation between acquisition activity and compensation. It is also plausible that other characteristics, including unobservable characteristics, may have a similar effect.

As our primary focus is on the possible different effects of public and private acquisitions on CEO compensation, it is important to consider theoretical foundations for any such differences. 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, public targets more commonly involve acquisitions of near equals, 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 therefore his or her compensation) more than would private acquisitions.

In summary, our paper investigates the following two hypotheses.

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

Hypothesis 2 (Differential effects hypothesis): The effect of shareholder returns on CEO compensation is weaker for public acquisitions than for private acquisitions.

3. Data and Methods

3.1 Research Design and Data Requirements

Our primary approach is to use regression analysis to assess the effect of acquisition performance on CEO compensation. In its simplest representation, the associated regression equation is of the form

$$y = f(x,z) + e \qquad (1)$$

where y is a measure of CEO compensation, x is a measure acquisition performance, z is a vector of other explanatory variables, and e is a random error with expected value 0. The shareholder value hypothesis depends on the magnitude, sign, and significance of the estimated effect of x on y and the differential effect hypothesis depends on whether the results are different for public and

private acquisitions. To carry out this regression analysis, we require data on executive compensation, on the performance of acquisitions, and on other explanatory variables.

3.2 CEO Compensation Data

Our compensation data comes from the S&P Capital IQ Executive Compensation database (Execucomp) for U.S. public corporations, obtained through Wharton Research Data Services (WRDS). We start with the universe of CEOs provided in the data, which starts in 1992 and covers firms in the S&P 500 for 1992 and 1993 and in the S&P 1500 subsequently. Firms that drop out of the S&P 1500 but continue to trade on a major stock exchange are included. Our compensation data covers the period from 1992 through 2016. Various compensation measures are provided by Execucomp. We focus primarily on total compensation (variable TDC1 in Execucomp), which includes salary, bonuses, the value of option awards[1], 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 index as a deflator.

As our acquisition data covers a long period, we have the opportunity to use panel data methods. A panel in our data consists of a firm-CEO combination and each combination is observed over one or more years. The shortest panel is one year and the longest is 24 years. After a small amount of data cleaning (dropping observations for which data on important variables is missing), we are left with 7,756 panels and 41,318 observations, implying an average tenure of about 5.3 years per CEO in our data.

[1] Execucomp estimates the value of options awarded using the Black-Sholes option valuation method where possible.

Execucomp provides data on various characteristics of each CEO that can be used as explanatory variables for compensation, including age and experience. In addition, we use Compustat data (also obtained through WRDS) for data on various firm-level explanatory variables, particularly firm size measures and industry identifiers (NAICS codes at the 2-digit level).

3.3 Acquisition Data

For each year, each firm-CEO combination must be linked to the acquisitions made by the CEO in the previous year. For acquisition data, 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. Although the acquirers must be public firms, many of the targets are 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.

We match compensation data for year t with abnormal returns, acquisition activity, and firm size measures for year $t - 1$ (i.e. with lagged returns). The logic is that good performance would be rewarded when compensation is subsequently adjusted. 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 adjustments, 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.[2]

[2] Compensation data is available on a fiscal year basis. For most firms, the fiscal year coincides with the calendar year. However, some firms use a different fiscal year. In such cases, we match the acquisition data for a given

In each year, each CEO makes a non-negative number of acquisitions. For most observations (34,426), the number of acquisitions is zero. Exactly one acquisition occurs in 4,754 observations, two occur in 1,317 observations, three occur in 451 observations, and in 370 observations there are four or more acquisitions. The data set contains 10,841 acquisitions overall, of which 9,115 are acquisitions of privately held companies and 1,726 are acquisitions of publicly traded companies.

In previous work, various measures of acquisition performance have been used. 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 or "treatment" variable. We estimate this return using event study methods based on stock price data from the Center for Research on Security Prices (CRSP), also obtained through WRDS.

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. To see if using multiple acquisitions has an effect, we consider a subsample consisting only of observations with no more than a single 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

calendar year with compensation data for the fiscal year ending in the next calendar year. For example, for a firm with a fiscal year ending on June 30, we would match the acquisition data for calendar year 2014 with the compensation data for the fiscal year ending on June 30, 2015

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 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 robustness checks using just observations with positive acquisitions.

3.4 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. More specifically, we use essentially the same method as Brander and Egan (2017), subject to small adjustments necessary for the current context. 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:

$$R_{it} = \alpha_i + \beta_i R_{mt} + \varepsilon_{it} \quad (2)$$

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 in the stock 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 $\alpha_i^* + \beta_i^* Rm$. The abnormal return, AR_i, is the difference between the actual return and the estimated normal return over the event window:

$$AR_i = R_i - (\alpha_i^* + \beta_i^* R_m) \quad (3)$$

This daily abnormal return is summed over the five days of the event window to yield the cumulative abnormal return.

Many papers have discussed problems in estimating abnormal returns. Much of the criticism amounts to observing that the model is 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.

3.5 Regression Specification

At the most basic level, we measure the effect of acquisition performance on CEO compensation by regressing real CEO compensation on acquisition performance and/or acquisition activity, correcting for other factors that also affect compensation. 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.

The primary "treatment variable" is the percentage aggregate cumulative abnormal return associated with the CEO's acquisitions in a given year, which is zero if there are no acquisitions. We have this return for all private acquisitions in a given year, for all public acquisitions in a given year, and we have the combined return for both. To measure acquisition activity in a given year we can use the number of acquisitions of public companies and the number of privately held acquisitions or the total number of acquisitions.

Standard control variables for CEO compensation include firm size, industry, time, and CEO experience. For firm size, we use real revenue. Revenue is highly correlated with other possible measures of firm size such as employment or real capital or market capitalization and has the advantage that there is very little missing data for this variable. The explanatory power of revenue for compensation is also higher than for any other single measure of size. The results are very similar regardless of which size variable (or combination of variables) is used.

For industry, we use the two-digit NAICS code. As for time, it is well known (and true in our data) that real CEO compensation rose rapidly in the 1990s and more slowly subsequently. We use time fixed effects as a flexible way of correcting for secular changes in CEO compensation and other macroeconomic events (such as business cycle effects) that affect CEO compensation.

There are several possible measures of CEO experience. We use years in place as CEO in the current firm (sometimes called job tenure). In addition, 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.

It is also common in compensation regressions to control for gender. However, our sample of female CEOs is relatively small; only 944 out of 41,318 observations (2.3%) are for female CEOs, corresponding to just 189 CEO-firm combinations out of 7,833. The female indicator variable does not have a statistically significant effect on compensation and whether a female indicator variable is used or not has no discernable effect on the other coefficients, so we do not include it in the reported regressions. And of course gender cannot be used in panel data regressions of the fixed effect type as gender does not change over time for any given firm-CEO combination.

All regression analysis is done using version 15 of Stata. In all regressions, we use robust standard errors clustered at the CEO-firm level.

3.6 Descriptive Statistics

Table 1 provides descriptive statistics. Tabulations by year and industry are not shown so as to save space. The unit of observation is the CEO-firm-year. All financial variables are adjusted using the CPI index to be in real 2016 dollars.

Table 1: Descriptive Statistics

Variable	No. Obs.	Mean	Median	Std. Dev.	10th pc	90th pc
Tot. comp. ($000s)	41,318	6,030	3,320	12,213	861	12,823
Salary ($000s)	41,318	860	796	464	405	1,355
CAR to Public Acquisitions	41,318	-0.0004	0	0.014	0	0
CAR to Public Acquisitions (acq >0)	41,318	-0.012	-0.009	0.071	-0.094	0.064
CAR to Private Acquisitions	41,318	0.0007	0	0.028	0	0
CAR to Private Acquisitions (acq > 0)	41,318	0.005	0.003	0.074	-0.065	0.078
Revenue ($m)	41,318	4,969	1,138	15,825	164	10,690
CEO exp. (years)	41,318	4.74	4.0	3.71	1	10
Age (years)	41,318	55.7	56	7.50	46	65

# of Acquisitions	Private	Public	Total
0	35,407	39,789	34,426
1	4,182	1,372	4,754
2	1,074	126	1,317
3	364	26	451
4 or more	291	5	370

The upper section of Table 1 shows that these CEOs of large public companies had 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. Comparing the total compensation row with the salary row indicates that salary accounts for only a small share of total compensation. Most compensation comes from the value of stock option grants. Bonuses and other forms of compensation are also significant.

The return variable is a decimal number rather than a percentage. Therefore, a number like 0.064 (the 90th percentile for public acquisitions in the subset of firms that make at least one such acquisition) is 6.4%. If we look across all observations, the average return to acquisitions

over CEO-firm-years is -0.0004 (or -0.04%), but most observations are zero because, in any given year, most firms make no public acquisitions. If we look just at observations for which acquisitions actually occur, we see that the average return is -0.012 or -1.2%, reflecting a striking but well-known fact that the average and median abnormal announcement returns to public acquisitions are often negative in samples of this type. Private acquisitions have a small positive average and median abnormal return but it is still noteworthy that close to half of all private acquisitions have negative abnormal returns at announcement.

The lower section of the table shows private, public, and total acquisition activity. Thus, for example, 35,407 observations have no private acquisitions, 39,789 observations have no public acquisitions, and 34,426 observations have no acquisitions of either type. Private acquisitions are much more common than public acquisitions. For example, there are 4,182 observations with one private acquisition, but only 1,372 with one public acquisition. There are 4,754 observations that have exactly one acquisition in total, either public or private. We note but do not show in the table that many of the observations that have one public observation also have one or more private acquisitions (470 out of 1,372). And a significant number of the observations with one private acquisition have one or more public acquisitions (330 out of 4,182).

4. Results

Our primary results are based on panel data methods. However, before reporting the panel data results we provide some context by reporting regression results based on pooled data.

4.1 Pooled Regressions

In Table 2 we report the results for five specifications. In Specifications 1 through 4 (and subsequently in the paper), we regress (the log of) real current compensation on lagged acquisition performance, lagged acquisition activity, and (the log of) real lagged revenue. If the

specification is correct and any omitted explanatory variables are uncorrelated with acquisition performance or acquisition activity, this specification should identify whether and how much acquisition performance affects compensation. For experience and age, we rescale the variables from years to decades to scale the coefficients conveniently. Constants are not reported as they do not provide useful information given the use of industry and year fixed effects.

Table 2: Pooled Compensation Regressions

The dependent variable for each specification is logged real compensation. OLS is 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 1

	(1)	(2)	(3)	(4)	(5)
					1st difference Acq > 0
CAR to All Acquisitions	0.379**		0.354*		
	(0.19)		(0.18)		
CAR to Public Acquisitions		-0.892***		-0.499	0.368
		(0.36)		(0.37)	(0.380)
CAR to Private Acquisitions		0.695***		0.659***	0.425**
		(0.21)		(0.21)	(0.209)
Revenue	0.389***	0.389***	0.388***	0.387***	
	(0.0076)	(0.0075)	(0.0076)	(0.0076)	
Revenue 1st diff.					-0.0462
					(0.064)
Experience	0.0884***	0.0881**	0.0812**	0.0819**	-0.023
	(0.035)	(0.035)	(0.035)	(0.035)	(0.025)
Age	0.612***	0.611***	0.611***	0.606***	
	(0.014)	(0.14)	(0.14)	(0.14)	
Age Sq.	-0.0614***	-0.0613***	-0.0611***	-0.0607***	
	(0.013)	(0.013)	(0.013)	(0.013)	
Total No. of Acquistions			0.041***		
			(0.012)		
No. of Public. Acquistions				0.141***	0.0425**
				(0.024)	(0.021)
No. of Private Acq uistions				0.0268**	0.0033
				(0.012)	(0.0063)
Industry fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	41,318	41,318	41,318	41,318	5,793
Adj. R-squared	0.34	0.34	0.34	0.34	0.02

Specification 1 shows the estimated effect of combined abnormal returns from acquisitions on CEO compensation without including acquisition activity. This specification implies a positive effect of acquisition performance on compensation. Comparing specification 2 with specification 1 demonstrates the importance of distinguishing between public and private acquisition returns. In specification 2, we see that returns to private acquisitions have a strong positive effect on compensation while returns to public acquisitions apparently have a negative effect. An F-test for the equality of the coefficients on returns to public and private acquisitions strongly rejects equality. Specifications 3 and 4 add acquisition activity as explanatory variables, and show that acquisition activity has a significant positive effect on CEO compensation.

We believe that specification 4 is the most useful specification in Table 2. The performance of private acquisitions has a significant positive effect on CEO compensation, while the performance of public acquisitions does not have a significant effect. However, public acquisition activity has a strong positive effect. Private acquisition activity also has a statistically significant positive effect but it is much smaller in magnitude. Including public acquisition activity in the regression reduces the magnitude of the coefficient on public acquisition performance and makes it statistically insignificant. One reason is that public acquisitions have slightly negative returns on average (and at the median). A CEO who makes many public acquisitions will tend to generate a lower aggregate acquisition return than a CEO with no acquisitions or with fewer acquisitions. CEOs with many acquisitions also tend to have higher compensation. Specification 4 has the reasonable implication that such CEOs are being rewarded primarily for making acquisitions, not for generating low acquisition returns.

The effects of the control variables in specifications 1 through 4 are as expected. Firm size as measured by revenue has a very strong positive effect on CEO compensation, experience as a CEO has a strong positive effect, and age has a quadratic effect of plausible size (given that we measure age in decades to scale coefficients conveniently).

The industry fixed effects for all specifications (not shown) exhibit the well-known pattern that compensation is much higher in some industries than in others. The highest paying two-digit industries

include information technology (51), mining (21), real estate (53), and finance and insurance (52). The lowest paying two-digit industries include utilities (22), wholesale and retail trade (42, 44, and 45), and transportation and warehousing (48 and 49). The time fixed effects show rapid increases in real compensation in the 1990s, and modest general growth otherwise except for a retrenchment in 2002-04, coinciding with the recession of 2001-03, and a sharp decline in 2007-2009, coinciding with the financial crisis of 2007-08 and its aftermath.

Subject to standard assumptions, the level regressions in specifications 1 – 4 provide unbiased estimates of the effects of the treatment variables on compensation, provided that the explanatory variables are uncorrelated with the error term. However, unobserved individual CEO characteristics that are correlated with acquisition activity or performance and that affect compensation are possible. For example, a very aggressive or pro-active CEO might undertake more acquisitions and might be able to obtain higher compensation, even if acquisition activity does not affect compensation. One way of trying to reduce such problems is to use the first difference in compensation as the dependent variable. The "aggressiveness" trait reflected in the compensation level would, arguably, not affect the first difference in compensation, allowing for an unbiased assessment of how acquisition performance or activity affect compensation.

Specification (5) is based on first differences – regressing the year-to-year change in compensation between year t and year t + 1 on the acquisition performance and amount of acquisition activity during year t. This structure is similar to much of the early work on acquisitions and CEO compensation and, to make the parallel to that work closer, we restrict the sample to just CEOs who make at least one acquisition during a given year. The results regarding acquisition performance and acquisition activity are qualitatively similar to those in specification 4, although they are weaker.

In the first difference regression, more experience is not associated with larger percentage increases in compensation. This is reasonable, as, while compensation rises with experience, there is no reason it should rise at increasing rate. Similarly, there is no reason to expect a quadratic (or even a linear) effect of age on compensation increases. Therefore, we do not include age in the reported regression. It

also would not be reasonable to include the change in age or in experience as regressors as those changes equal one for every observation and are therefore perfectly collinear and indistinguishable from the constant term.

An alternative and generally superior method for addressing unobserved individual characteristics is to use panel data fixed effect regressions. (See, for example, Wooldridge 2013, p. 490.) Like first differencing, this method is based on CEO-specific changes, but it is based on within-panel differences from the panel average ("de-meaning") rather than on year-to year differences. Provided any unobserved characteristics of a given CEO are time invariant (as seems plausible), fixed effects panel regressions are not subject to endogeneity of the unobserved heterogeneity type for either CEOs or firms.

4.2 Panel Regressions

Table 3 reports panel data fixed effect regressions. A panel consists of a CEO-firm combination. If a CEO moves from one firm to another, a new panel is formed. Similarly, if a firm hires a new CEO, a new panel is also formed. We do not include experience, as age and experience are co-linear within each panel (each increases by one each year). Therefore, the coefficient on the linear age term captures the effects of both age and experience as both increase together, while the quadratic term captures the effect that CEO compensation ultimately declines with age. We report regressions including all CEO-firm combinations, and regressions including only CEO-firm combinations that are active in acquisition activity. To assess the determinants of compensation in year t, the active subset consists of all CEO-firm combinations that have made at least one acquisition prior to year t. Industry fixed effects cannot be used because industry is constant for each panel

Table 3: Fixed Effect Panel Regressions

The dependent variable for each specification is logged real compensation. Panel data fixed 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 1

	(1)	(2)	(3)	(4)	(5)
		Acq > 0			Acq > 0
CAR to All Acquisitions	0.424***	0.343**			
	(0.16)	(0.16)			
CAR to Public Acquisitions			0.215	0.324	0.307
			(0.35)	(0.36)	(0.384)
CAR to Private Acquisitions			0.495***	0.464***	0.367**
			(0.18)	(0.18)	(0.18)
Revenue	0.188***	0.182***	0.190***	0.188***	0.183***
	(0.021)	(0.044)	(0.021)	(0.021)	(0.45)
Age	0.420***	0.630*	0.424***	0.420***	0.627*
	(0.014)	(0.33)	(0.13)	(0.13)	(0.329)
Age Sq.	-0.0430***	-0.0430	-0.0435***	0.043***	-0.0424
	(0.014)	(0.032)	(0.014)	(0.0077)	(0.26)
Total No. of Acquisitions	0.0280***	0.0257***			
	(0.0075)	(0.0089)			
No. of Public Acquisitions				0.0455**	0.0424*
				(0.021)	(0.026)
No. of Private. Acquisitions				0.0254**	0.0234***
				(0.012)	(0.0087)
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	41,318	14,795	41,318	41,318	14,795
Adj. R-squared	0.77	0.69	0.72	0.72	0.69

Specification 1 shows that, for within-panel changes over time, better acquisition performance has a highly significant positive effect on compensation: CEOs get larger real increases in compensation when acquisitions they make generate higher announcement abnormal returns. Acquisition activity in the form of more acquisitions also has a highly significant effect on compensation. Specification 2 shows that the results are largely unaffected by whether all CEOs or only CEOs actively engaged in acquisitions are included.

Specifications 3 through 5 disaggregate acquisitions into private and public acquisitions. These regressions show that using just aggregated acquisitions or just public acquisitions misses an important part of the story. Specifically, the positive effect of acquisition performance on compensation is driven

primarily by private acquisitions, at least as far as statistical significance is concerned. Returns to public acquisitions do not have a statistically significant effect.

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.

Table 4: Random Effects Panel Regressions

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 1

	(1)	(2)	(3)	(4)
		Acq > 0		Acq > 0
CAR to All Acquisitions	0.429***	0.424***		
	(0.14)	(0.14)		
CAR to Public Acquisitions			0.169	0.0603
			(0.31)	(0.32)
CAR to Private Acquisitions			0.519***	0.545***
			(0.16)	(0.16)
Revenue	0.333***	0.369***	0.332***	0.369***
	(0.0076)	(0.012)	(0.0076)	(0.012)
Experience	0.153***	0.0869**	0.153***	0.0868**
	(0.027)	(0.043)	(0.027)	(0.043)
Age	0.482***	0.479**	0.481***	0.474**
	(0.097)	(0.12)	(0.097)	(0.22)
Age Sq.	-0.0515***	-0.0531***	-0.0514***	-0.0528***
	(0.0089)	(0.020)	(0.0089)	(0.020)
Total No. of Acquisitions	0.028***	0.0267***		
	(0.0064)	(0.0075)		
No. of Public Acquisitions			0.0576***	0.0614***
			(0.019)	(0.021)
No. of Private Acquisitions			0.0235***	0.0216***
			(0.0065)	(0.0073)
Industry fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	41,318	14,795	41,318	14,795
Adj. R-squared	0.34	0.36	0.34	0.36

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 results obtained from fixed effects and random effects models are quite similar. However, a Hausman test comparing the fixed effects and random effects results rejects the hypothesis that they are the same.[3] Under the maintained assumption of the Hausman test that the fixed effects model is correctly specified, the results of the test suggest some impact of unobserved heterogeneity in CEO characteristics in the random effects estimates. It is also possible, however, that the CEO-specific effects are in fact random variables rather than fixed effects, in which case the random effects estimator is more efficient and those estimates would be preferred. In any case, fixed and random effects estimates have a similar qualitative pattern.

4.3 Economic Significance

Acquisition returns for private acquisitions have a positive and statistically significant effect on CEO compensation in all specifications. Public acquisitions also exhibit a positive effect in our preferred specifications, though without statistical significance. But are the implied economic effects meaningful?

To illustrate economic effects, we use specification 3 in Table 4, which is our preferred specification, 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, the compensation of the CEO would

[3] To do the Hausman test, we drop experience as an explanatory variable in the random effects model to match the fixed effects model.

rise by about half (0.519) 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 $31,000.

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 effect due to the acquisition's performance on the order of (2.7)(31,000) ≈ $84,000 (positive or negative). Standard errors are large enough that the actual effect could be much larger or much smaller but 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, but the point estimate is that a 1% abnormal return would increase compensation by 0.169 %. The abnormal return to public acquisitions has a median absolute value of 3.2%. Therefore, a "typical" public acquisition is estimated to have a "performance" effect of (3.2)(0.169%)(6 million) ≈ $32,000.

These estimated effects of acquisition performance can be compared with the estimated effects of acquisition activity. The effect of one additional private acquisition is to raise compensation by 2.35%, which is about $141,000 at the average compensation level. Public acquisitions have a much larger effect, leading to estimated increase in compensation of 5.76%, which is about $346,000 for the average CEO. These values of $141,000 and $346,000 are much larger than the performance values of $84,000 and $32,000. 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 0.332 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%, which is a substantial size effect that is consistent with previous research. Experience and age are measured in decades, not years, to keep the scaling

convenient. Thus, a one-year increase in experience as a CEO increases real compensation by about 1.5%, other things equal. Using our preferred specification, the age of maximum compensation is about 47.

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 past 25 years, paying particular attention to the differences between public and private acquisitions. These estimates provide a test of the shareholder value hypothesis and what we call the differential effects hypothesis. We use several methods but place primary emphasis on fixed effect and random effect panel data regressions. While there is a statistically significant difference between fixed and random effects models, as indicated by Hausman tests, the qualitative pattern of results is similar for both approaches.

One primary finding, based on panel data regressions (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. Therefore, our analysis supports the shareholder value hypothesis for private acquisitions but not for public acquisitions. More formally, we cannot reject the alternative hypothesis of "no relationship" between acquisition returns and CEO compensation for public acquisitions. However, we can reject "no relationship" for private acquisitions.

For public acquisitions, although acquisition performance does not have an effect on CEO compensation, acquisition *activity* does have a statistically significant and economically important effect. For private acquisitions, activity is also statistically significant but less important than for public acquisitions. This pattern of results is consistent with the differential effects hypothesis: public acquisitions are more prone to agency distortions that weaken the link between performance and compensation and strengthen the link between activity and compensation.

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

previous literature shows that firms that monitor CEOs more closely have a closer 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.

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Appendix: 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_m	Value-weighted market return (CRSP: VWRETD)
R_i	Return for firm *i* (CRSP: RET)
AR_i	Abnormal return for firm *i* (CRSP/SDC M&A)
CAR_i	Cumulative Abnormal 5-day return for firm *i* (CRSP/SDC M&A)
CAR to All Acquisitions	Cumulative Abnormal Return from all acquisitions
CAR to Public Acquisitions	Cumulative Abnormal Return from public acquisitions
CAR to Private Acquisitions	Cumulative Abnormal Return from private acquisitions
Total Compensation	Total compensation to the CEO (Execucomp: TCD1)
Salary	CEO Salary (Execucomp: SALARY)
CEO Experience	Tenure of the CEO with the firm (Execucomp)
Age	Age of the CEO (Execucomp: AGE)
Total No. of Acquisitions	The number of acquisitions completed (SDC M&A)
No. of Public Acquisitions	The number of public acquisitions completed (SDC M&A)
No. of Private Acquisitions	The number of private acquisitions completed (SDC M&A)
Revenue	Firm revenue (Compustat: REVT)
Employment	The number of FTE employees (Compustat: EMP)
Real Capital	The gross total plant, property and equipment (Compustat: PPEGT)
Market Capitalization	Market capitalization (Compustat: MKTVAL)
Industry	2-digit NAICS code for the firm (SDC M&A)
Year	The year of the CEO's compensation package (Execucomp)
CPI deflator	A deflator to convert amounts into 2016 dollars (BEA)