CEO PAY AND THE RISE OF RELATIVE PERFORMANCE CONTRACTS: A QUESTION OF GOVERNANCE?

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Abstract
We exploit the large rise in relative performance awards in the United Kingdom over the last two decades to investigate whether these contracts improve the alignment between CEO pay and firm performance. We first document that corporate governance appears to be stronger when institutional ownership is greater. Then, using hand-collected data from annual reports on explicit contracts, we show that (1) CEO pay still responds more to increases in the firms' stock performance than to decreases, and, importantly, this asymmetry is stronger when corporate governance is weak as measured by low institutional ownership; and (2) "pay for luck" persists as remuneration increases with random positive shocks, even when the CEO has equity awards that explicitly condition on firm performance relative to peer firms in the same sector. A major reason why relative performance contracts do not eliminate pay for luck is that CEOs who fail to meet the terms of their past performance awards are able to obtain more generous new equity rewards in the future in weakly governed firms. We show the mechanism operates both through the quantum of shares and the structure of new contracts. These findings suggest that reforms to the formal structure of CEO pay contracts are unlikely to align incentives in the absence of strong corporate governance. (JEL: J33, J31, G30)

The editor in charge of this paper was Imran Rasul.

Acknowledgments: We are grateful to the editor, Imran Rasul, three anonymous referees, Richard Belfield, Bob Gibbons, Oliver Hart, Bengt Holmstrom, Steve Kaplan, Louis Kaplow, Kai Li, Bob Pindyck, and participants in seminars at AEA, ECB, Harvard, IZA/SOLE Conference, LSE, Massachusetts Institute of Technology, NIESR, Oxford, Sheffield, RES, Towers Watson, and UBC Finance for helpful comments. The views expressed do not represent the views of the Financial Conduct Authority. This work was supported by the ESRC.

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Journal of the European Economic Association 2021 19(5):2513–2542 DOI: 10.1093/jeea/jvab005

1. Introduction

CEO pay is rarely out of the news. The remuneration of CEOs and other senior executives has risen much faster than that of ordinary workers. For S&P500 firms, average CEO pay was 31 times as high as that of the average production worker in 1970 compared to 361 times as large in 2017.[1] Although pay levels are lower in the United Kingdom, the trends are similar. In our data, CEO pay was about 110% larger in real terms in 2015 than 1999 compared to a 10% rise for the median worker.

CEO pay could have risen purely because of competitive forces such as a stronger market for superstars caused by globalization and technological change (e.g., Edmans and Gabaix 2016; Gabaix and Landier 2008; Rosen 1981). However, other commentators have attributed some of the relative increase in CEO pay to the exercise of managerial rent extraction (e.g., Bebchuk and Fried 2004; Djankov et al. 2008).

In this paper, we use UK publicly listed firms as a case study because since the late 1990s, there has been a major shift towards rewarding CEOs based on *relative* performance.[2] A typical plan is to grant executives equity conditional on improving shareholder returns relative to a peer group of large firms in the same sector (e.g., being among the top quartile of performers over a three-year period). These relative performance contracts contrast to more standard US-style stock option contracts that are based on general improvements in equity prices. Almost no equity awards are now made to UK CEOs who do not have a performance condition attached to them.[3]

Many papers have documented a link between CEO pay and firm performance.[4] Although usually interpreted through the lens of contract theory, it has long surprised economists that such relative long-term incentive plans (LTIPs) are not more common (Holmstrom, 1979,1982).[5] Relative performance plans are designed to reduce the problem that CEO remuneration can increase merely because of positive shocks to the firm unrelated to executive effort or ability. Figure 1 shows that at the start of our sample around 18% of total pay was in the form of LTIPs, rising to almost 30% by the end of the sample. Of these awards, the share that used a sector comparator

1. Conyon et al. (2011) and https://aflcio.org/paywatch

2. The move to relative performance plans arose from the recommendations of several high profile 1990s Commissions such as the 1995 Greenbury Report. http://en.wikipedia.org/wiki/Greenbury_Report. See also the 1992 Cadbury Report and 1998 Hampel Report.

3. For the sample as a whole, we have 8,695 equity awards. Of these, 94% have a performance condition attached.

4. Our paper is in the CEO pay–performance tradition (Baker, 1939; Jensen and Murphy, 1990). Surveys by Frydman and Jenter (2010) and Bertrand (2009) conclude that there is a positive, statistically significant relationship between CEO pay and shareholder returns (and accounting measures of performance such as profitability and sales growth). This link exists for most time periods and across most countries, and there appears to have been an increase in the sensitivity of pay to performance over time as CEO compensation has tilted towards a more incentive-based structure (see Hall and Liebman, 1998). The interpretation of this empirical pay–performance link is more controversial.

5. Using indirect methods relating relative firm to industry performance, Gibbons and Murphy (1990) find some evidence for their use in large US firms in the 1970s and 1980s, but Aggarwal and Samwick (1999) find little evidence for them in the 1990s.

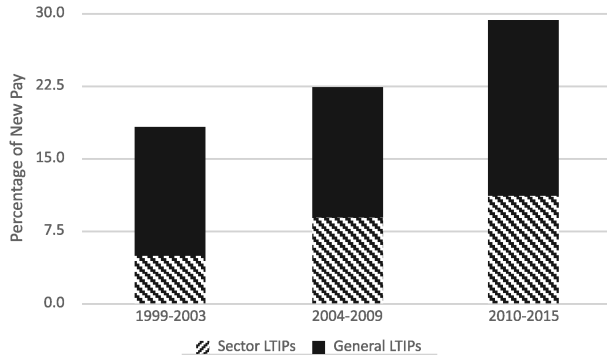


FIGURE 1. Share of CEO pay in LTIPs and share of all LTIPs that have a sector component. LTIP share is the estimated proportion of new pay in the form of LTIPs, all of which are performance conditional. Sector LTIPs show the percentage that have a sector component in the performance evaluation (i.e., are benchmarked against an industry peer average), while general LTIPs are those with a non-sector comparator. Source: Authors' calculations based on Boardex data.

rose from a quarter to two-fifths, with the remainder using a general comparator (e.g., the FTSE-100 companies). A number of US corporations have also been moving towards these plans, but at a slower rate. De Angelis and Grinstein (2016) report that although the trend is upwards, by 2007 only 30% of S&P500 firms used some form of relative performance evaluation in at least part of their CEO pay package. Gong et al. (2011) estimate the figure to be 25% for the wider group of S&P1500 firms. Bettis et al. (2018) use more recent data to show that the use of performance-vesting equity awards to top executives in large US companies has continued to rise, though by the end of their sample in 2012, they were still less common than standard time-vesting awards. The experience of UK firms can shed light on the efficacy of such plans as their spread occurred much earlier than in the United States.

We use original data on explicit CEO contracts covering just under 500 publicly listed firms accounting for almost 90% of UK stock market value between 1999 and 2015. Our paper focuses on the interaction between LTIPs and institutional ownership. Due to their scale of ownership across multiple firms and large block holdings in individual firms, institutional investors are often regarded as one of the few shareholders to have both the ability and incentive to monitor CEOs in large corporations. Institutional owners take a keen interest in the level and structure of CEO pay. In the United Kingdom, they typically use two major advisors: Institutional Voting Information Service (IVIS) and the Institutional Shareholder Services (ISS). IVIS and ISS help institutions monitor corporate governance on a wide range of issues and advise on voting at Annual General Meetings.[6] In the United Kingdom, there is "Say on Pay" legislation, where a vote has to be taken every year over CEO pay,

6. As the survey by Thomas and Van der Elst (2015) emphasised, "These advisors' recommendations for, or against, a company's pay plan may also carry significant weight with their institutional clients and can dramatically impact the outcome of a vote."

and industry analysts and the media closely watch these votes.[7] It is not just voting down a pay agreement: The threat of a negative vote helps discipline the remuneration committee. As one survey notes, "institutional investors use the threat of a negative vote to enforce compliance" (Sheehan, 2012).

We therefore begin by documenting that corporate governance appears to be stronger when institutional ownership is higher in our data. We then turn to our results on CEO pay. First, we show that there is a strong relationship between CEO pay and performance, but that it is asymmetric—pay responds more to increases in firm performance than to decreases.[8] Furthermore, this asymmetry occurs only when there is weak external control as proxied by lower institutional ownership. Second, there is substantial "pay for luck" with CEO pay increasing when the industry experiences a random positive shock even when the CEO is subject to relative *sector* LTIPs. We then turn to explanations. We find that CEOs in firms with low institutional ownership who fail to meet the terms of their existing LTIPs are able to obtain deals that are more generous on their *future* LTIPs. We show that these future LTIPs are not just more generous in the quantum of the value of shares they promise, but they are also re-designed to the benefit of the CEO, by making trigger points more generous and including multiple performance criteria that make them more likely to at least partially vest.

One efficiency-based explanation for these findings is that more generous awards are needed after LTIP failure in order to retain the CEO in the weakly governed firms. We show that CEO exit is indeed more likely following LTIP failure, but this effect is no different for firms with low versus high degrees of institutional ownership. An alternative and more plausible explanation for our findings is that CEO remuneration plans in large corporations are sufficiently complex that individual shareholders have difficulty effectively monitoring the contracts. Governance matters more than formal contract structure.

This paper is structured as follows. Section 2 discusses data, Section 3 discusses the relationship between corporate governance and institutional ownership, Section 4 reports our main results, Section 5 provides some extensions and robustness tests, while our conclusions are in Section 6.

2. Data

Our main data on pay come from Boardex, which provides annual data from the Remuneration report of all listed UK companies. This database is essentially the

7. For examples on analysts, see KPMG (https://assets.kpmg/content/dam/kpmg/uk/pdf/2018/09/review-of-the-2018-agm-season.pdf) and Deloitte (https://www2.deloitte.com/content/dam/Deloitte/uk/Documents/tax/deloitte-uk-2015-agm-season.pdf). On media, see, for example, https://www.ft.com/content/e372d9f0-8b6e-11e8-b18d-0181731a0340; https://www.constructionnews.co.uk/contractors/kier/kiers-cfo-pay-deal-source-investor-opposition-18-11-2019/; https://www.theguardian.com/business/2016/apr/14/bp-pledge-shareholder-anger-ceo-bob-dudleypay-deal; https://www.manifest.co.uk/shareholders-vote-pearsons-remuneration-report/.

8. Garvey and Milbourne (2006) find some evidence of this in US data. Daniel et al (2016) claim to find no asymmetry when they use a wider definition of pay that includes all sources of shareholder wealth.

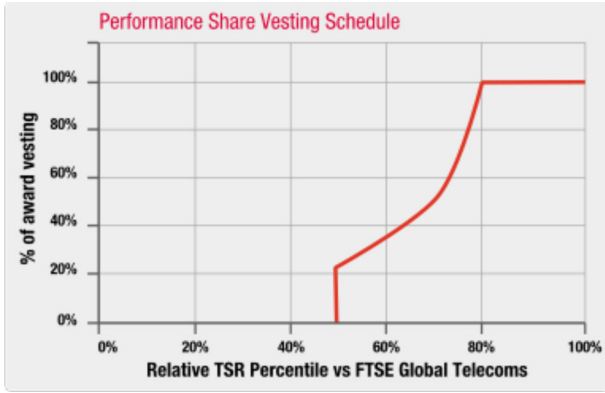


FIGURE 2. Vodafone LTIP vesting schedule. Taken from Vodafone's 2005 accounts relating to a 2004 LTIP award. The figure indicates the proportion of shares (2 million) that will be granted to CEO depending on the performance of total shareholder return relative to basket of 28 "peer" telecom companies in the 28th July 2004 to 28th July 2007 period.

UK equivalent of the US ExecuComp database. The data cover all Board executives within the firm and report base salary, cash and share bonuses, and details of all equity awards—regular stock options and LTIP awards. We supplemented these data by hand-collecting from the Annual Reports more details on each equity award (e.g., grant date, performance condition, performance comparator type), and we also track each award through to the vesting date to determine the outcome of the award. From these sources, we define two alternative measures of pay (further details in Online Appendix A):

1. **Cash pay** = salary + bonus (cash bonus + face value of unconditional deferred bonus shares)
2. **New pay** = cash pay + expected value on grant date of new equity awards (regular stock options and LTIPs)

To give an idea of a typical LTIP award and how we value it ex ante, consider the sector LTIP award made to the Vodafone CEO, Arun Sarin, on 28th July 2004 (see Figure 2). The CEO was awarded 2,016,806 shares in an LTIP share plan, with a face value of £2.4 million (share price of £1.19 on the grant date). The 2005 annual report (which recorded the 2004 award) provides details of the vesting schedule and the set of firms that made up the comparison group (29 firms in the FTSE Global Telecom index). If the total shareholder return (TSR) of Vodafone over the subsequent three years was below the median of the comparison group, no shares would vest. TSR performance in the top quintile of relative performance would result in full vesting, and a sliding (though not linear) scale operates between the median and 80th percentile. In the event, on 28th July 2007, 576,806 shares were vested and 1,440,000 were forfeit (i.e., 28.6% of the award vested) as TSR performance was in the 53rd percentile. To value the award on grant date, we take the face value of the award and adjust downwards for two effects. First, we use the history of all LTIP plans of the same broad type to determine the

average vesting percentage—this gives us an approximate estimate of what probability the CEO should assign to actually obtaining the shares on vesting date. Second, we adjust for the probability that the CEO will leave the firm during the performance evaluation period and thus lose the shares (or at best have them pro-rated). Online Appendix A discusses this in more detail, gives more examples of LTIPs, and reports on various robustness tests. For regular stock options that have no performance conditions (which are rare in our data), we value using a standard Black–Scholes formula.

There are, of course, alternative approaches to valuing such awards. Bettis et al. (2014) provide an excellent and detailed discussion of LTIPs using very granular US data. They simulate the ex-ante value of LTIPs using the full details of the award (e.g., peer group composition, vesting schedule, etc.) and show that for most awards, the simpler "fair value"[9] reported by US firms is close to their more sophisticated methods. However, they point out that there are some significant differences in the tails of the valued awards. Unfortunately, our data do not consistently provide the sufficient level of detail required for such computations across all the firms. Indeed, in the United Kingdom prior to the 2008 Large and Medium-Sized Companies and Groups Regulations, there was no formal requirement on firms to provide the level of detail needed to do such simulations, though firms could voluntarily do so. For example, National Express plc operated a TSR LTIP and simply reported that they used a "bespoke comparator group", whilst William Hill plc stated that TSR would be measured against a "selected comparator group of 16 other gaming and leisure sector companies". In Section 4.4, we use some more details of the plans for a sub-sample to consider whether our results could be biased due to not taking into account all aspects of the LTIP awards.

Carter et al. (2009) provide evidence on UK LTIPs for a single year, 2002, and conclude that there is "little evidence that differences in performance conditions are associated with differences in actual plan vesting percentages". In other words, knowing more about the structure of the contract did not significantly help in predicting the final vesting percentage. This suggests that our approach can provide a reasonable approximation to valuation in the absence of more detailed individual plan data. Carter et al. (2009) also argue that "research that fails to consider more realistic vesting rates will severely overstate the value of performance-vested equity grants", which emphasizes the importance of using actual vesting rates to adjust valuation as our method does. We have experimented with alternative assumptions about vesting probabilities such as using rolling or recursive historical outcomes or industry-specific outcomes, but this does not substantively change any of our results. Finally, it should be noted that unlike in the United States, UK firms are only required to report the face value of LTIPs on grant date, assuming 100% vesting.

Our main sample comprises the 300 largest publicly listed UK-domiciled firms in each year from 1999 to 2015, representing on average 94% of the market capitalization of the UK stock market. We keep firms in the sample regardless of whether they

9. The fair value reporting required by US accounting rules stipulates that firms must adjust the face value of the LTIP to account for the performance condition attached. However, firms do not have to conduct the sophisticated analysis presented by Bettis et al. (2014) but can instead use basic information such as relative price volatilities to estimate fair value.

TABLE 1. Summary statistics.

	Mean	Median	S.D.	25th percentile	75th percentile
Panel A: CEO and pay data					
Total compensation ("new pay")	1,638	1,039	2,866	610	1,850
Salary	539	482	285	350	671
Bonus	501	262	817	68	590
New equity	598	215	2,528	0	567
LTIP share (%)	22.6	21.9	20.8	0	35.2
Sector share of LTIPs (%)	35.2	0.0	45.9	0.0	100
Completed tenure (years)	9.3	7.0	7.6	4.4	11.7
Annual exit	0.11	0	0.32	0	0
Panel B: company data					
Market capitalization (£ million)	4,298	791	12,906	320	2,522
Total employment	19,253	4,900	47,553	1,057	15,954
Shareholder returns (%)	10.1	13.5	35.5	−9.7	33.0
Institutional ownership (%)	57.4	61.5	24.0	41.6	76.1
IVIS red top	0.07	0	0.26	0	0
IVIS score (0/1/2)	0.39	0	0.62	0	1

Notes: All pay data figures in panel (A) are in real (2014) £1,000s. Data in panel (A) are for 1,201 CEOs and data in panel (B) are for 472 firms and cover the period 1999–2015. New equity is the expected value on grant date of new equity awards (see text for discussion). Total compensation is "new pay" (the sum of salary, bonus, and new equity).

remain in the top 300 firms, so there is no attrition of firms other than those that cease to exist. This gives a total sample of 498 firms across the period as a whole, which we then match to the Boardex database to obtain 472 firms with CEO pay data. This covers 85% of total market capitalization and 1,201 CEOs.[10] For all these firms, we also have annual company account data and stock price data merged in from Thompson Datastream. From these worldwide-consolidated activities of the firm, we construct standard measures of firm performance such as shareholders' return, profitability and revenues per worker. Table 1 reports some summary statistics for CEO pay and company-level data. The average CEO total compensation ("new pay") over the sample period was £1.64 million (about $2.6 million). This average masks a strong trend over the period with average pay rising from £0.9 million to £1.9 million. For the median CEO, base salary accounts for almost half of remuneration, with bonuses and new equity accounting broadly equally for the other half.

3. Institutional Ownership and Corporate Governance

Since many of our key findings relate to interactions between CEO pay, LTIP outcomes, and institutional ownership, we begin this section with a discussion of why

10. We fully match every firm to a Boardex identifier. The 26 firms without pay data are generally those that were delisted at some point in 2001 or 2002 and appear not to have had their remuneration reports entered or archived by Boardex—see Online Appendix A for more details.

institutional ownership is likely to be important for corporate governance. We first review the literature on this link and then present new evidence for our dataset on the connection between institutional ownership and corporate governance.

3.1. Institutional Ownership and Corporate Governance: Existing Literature

Numerous studies have examined the relationship between institutional ownership and a variety of measures of corporate governance. Aghion et al. (2013) argue that institutional ownership is associated with better governance (as measured e.g., by Gompers et al. 2003, IRRC index), because activist institutions such as pension funds (e.g., CALPERS) typically have the ability and incentive to monitor CEOs more than dispersed owners. Their stronger incentives come from the fact they are often large block holders in individual companies, so they overcome the free rider problem with dispersed ownership. The stronger ability comes from their large scale that enables them to pay the fixed costs of monitoring large amounts of information.

Gillan and Starks (2000) examine 2,042 US corporate governance proposals at Annual General Meetings between 1987 and 1994. They found that proposals sponsored by institutions (as opposed to individuals) focused on problems arising from potential conflicts of interest between management and shareholders, such as pushing for increased board independence, implying that institutional owners were trying to explicitly tackle CEO agency problems. Furthermore, such proposals were also more likely to be adopted when proposed by institutional owners (even after controlling for the share of votes held by institutional owners). McCahery et al. (2016) surveyed 143 large institutional investors. They found that institutional investors engaged more often than other shareholders over concerns about a firm's corporate governance rather than about short-term issues such as equity issuance or dividend payments. Inadequate corporate governance and excessive compensation are considered "somewhat" or "very important" triggers for action by 88% of institutional respondents. Interestingly, they also show that UK institutional investors are more active than those in other countries, which is where our study focuses.

Some recent studies in this literature have tried to identify a causal link between institutional ownership and corporate governance. Crane et al. (2016) exploit new entries to the Russell index of the largest 2,000 listed US firms (the "Russell 2000") to explore plausibly exogenous changes in institutional ownership. Using the discontinuity around the market value threshold determining rank 2,000, they show that firms just to the right of the threshold not only have significantly more institutional ownership but also have two more shareholder-initiated proposals in a given year compared to the control firms. This overall difference is driven by an increase in the number of governance-related shareholder proposals. (There is no difference in the number of management proposals.) Using a similar identification strategy, Appel et al. (2016) show that an exogenous one standard deviation (S.D.) increase in institutional ownership is associated with (i) a 0.7 S.D. increase in the share of directors on a firm's board who are independent; (ii) a 0.75 S.D. decline in support for management

proposals; and (iii) a 0.5 S.D. increase in support for improved governance proposals.

The impact of corporate governance and institutional ownership on remuneration has also been explored. Numerous studies have argued that pay in the boardroom is related to measures of corporate governance such as the proportion of independent directors or the existence of an independent remuneration committee. For example, Core et al. (1999) find that the proportion of non-executives on the Board is associated with lower levels of CEO pay, whilst Ryan and Wiggins (2004) demonstrate that it is also associated with a stronger pay–performance link. Hartzell and Starks (2003) show that institutional ownership concentration is positively related to the pay–performance elasticity and negatively related to the level of pay. Bertrand and Mullainathan (2001) find that their "pay for luck" effect is substantially attenuated when there is stronger external control—as measured by the number of large shareholders of the firm. This result has also been found in Garvey and Milbourn (2006).

3.2. Institutional Ownership and Corporate Governance: New Evidence

The existing literature does suggest an important role for institutional ownership, but it is almost exclusively based on US firms. Consequently, we turn to our own data on institutional ownership from Thomson Reuters Global Ownership files for more direct UK evidence. The data we use relate to December of each year from 1997 onwards and record the percentage of outstanding shares owned by all those with a shareholding larger than 0.015%. We calculate for each year the percentage of outstanding shares held by institutional investors. Across the sample as a whole, Table 1 shows that institutional investors account for 57% of share ownership—roughly the same as observed for the United States in the 2000s (Aghion et al., 2013). There is, however, significant variation across companies. Our measure of corporate governance uses data from IVIS over the period 1998–2014. IVIS provides a detailed analysis of UK-listed companies in relation to the level of compliance with corporate governance "best practice" (see Selvaggi and Upton, 2008, for more details[11]). Its main purpose is to assist subscribers with their voting decisions at the annual general meeting (e.g., approving the accounts, dividends, elections, and remuneration of directors). IVIS draws up a list of key issues for investors to consider and highlights their seriousness using a colour-coded system. A "red top" is used to indicate the strongest concern that a proposal does not comply with best practice, an "amber top" indicates concern, and a "blue top" indicates no area of major concern.

Figure 3 presents descriptive evidence plotting the fraction of firms with a red top in decile bins of institutional ownership in the previous year. There is a clear downward sloping relationship indicating that firms with a smaller fraction of institutional owners have a much greater fraction of serious warnings of corporate governance

11. The authors also use a subset of these data to examine the link between corporate governance and shareholder returns (in the spirit of Gompers et al., 2003). They find that the shares of the well-governed firms have higher shareholder returns.

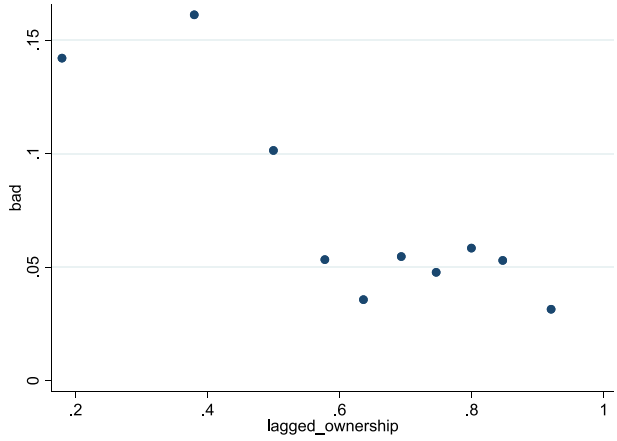


FIGURE 3. Weak corporate governance more prevalent in firms with lower instiutional ownership. The figure is a binscatter of the fraction of firms with a "red top" (signalling corporate governance problems by IVIS) in ten deciles of (lagged) institutional ownership (defined as the fraction of shares owned by institutional owners in the previous year). We control for year dummies.

concerns. The relationship looks rather non-linear: Firms in the lowest two deciles seem to have particularly bad problems (about 15% are red-top warnings), whereas those in the top seven deciles seem to have broadly similar patterns of only about 5% red tops. The top 70% correspond to about half or more of voting equity owned by institutions.

We explore the link between institutional ownership and the IVIS corporate governance scores more rigorously in Table 2. The dependent variable is either binary (=1 for red top and zero otherwise) or ordered (=2 for red top, =1 for amber top, and zero for blue top). The first four columns of Table 2 present estimates where the dependent variable is the binary variable, whilst the last four report the ordered dependent variable. Estimation is by ordinary least-squares (OLS), but marginal effects are similar using non-linear models. We begin by using the continuous measure of the one-year lagged institutional ownership percentage (*IO*) as the key right-hand side variable. In column (1), this variable has a negative and significant coefficient, implying that higher levels of institutional ownership are associated with better corporate governance. The coefficient implies that a ten-percentage point decrease in institutional ownership is associated with a 1.7 percentage point increase in the probability of a red-top warning. Since the mean of the dependent variable is 7.4%, this magnitude is economically non-trivial. Note that all the models in Table 2 contain time dummies and *ln*(market capitalization) to control for firm size.[12]

12. Unsurprisingly, the coefficient on market capitalization is negative and significant, but dropping it made little difference to the results. For example, in column (1), the coefficient (standard error) on institutional ownership was –0.166 (0.035) if we drop this measure of firm size.

TABLE 2. Institutional ownership and corporate governance.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Weak corporate governance							
Dependent variable:	1 = red top, 0 = amber top or blue top				2 = red top; 1 = amber; 0 = blue top			
Institutional Ownership	−0.172*** (0.035)				−0.338*** (0.068)			
Low_IO (lowest quartile, q1)		0.108*** (0.019)	0.034* (0.019)	0.041*** (0.015)		0.230*** (0.039)	0.092** (0.044)	0.099*** (0.036)
IO_q2 (second lowest quartile)		0.029*** (0.010)	−0.011 (0.014)			0.082*** (0.026)	−0.011 (0.033)	
IO_q3 (third lowest quartile)		0.011 (0.008)	−0.003 (0.011)			0.038* (0.023)	−0.003 (0.028)	
Firm fixed effects?	No	No	Yes	Yes	No	No	Yes	Yes
Observations	4,827	4,827	4,827	4,827	4,827	4,827	4,827	4,827
Firms	452	452	452	452	452	452	452	452

Notes: All results use Thomson Reuters ownership data and IVIS voting recommendations. Columns (1)–(4) have a binary dependent variable equal to 1 if the firm has a "red top" IVIS recommendation in that year and zero otherwise. Columns (5)–(8) have a categorical dependent variable, with values of 2 for a "red top", 1 for an "amber top", and zero otherwise. *IO* is the one-year lagged percentage of shares held by institutional investors, while *IO_qX* are dummies equal to 1 if the firm is in the *X*th quartile of institutional ownership in the year. All regressions include time dummies and the log of market capitalization. Standard errors are clustered at the firm level. *Significant at 10%; **significant at 5%; ***significant at 1%.

Motivated by the evidence of a non-linear relationship in Figure 3, column (2) of Table 2 splits up the institutional ownership variable into quartile dummies across firms evaluated separately for each year (with the top quartile as the omitted base). All the coefficients are positive in these specifications, implying that corporate governance concerns are stronger outside the firms in the top quartile of institutional ownership. It is clear, however, that the lowest quartile group has a substantially greater likelihood of having corporate governance concerns than the other groups, with the third quartile effect being insignificant and the second quartile effect being at most a quarter the size of the lowest quartile.[13] In column (3), we repeat the specification of column (2) but include a complete set of firm fixed effects. In this model, only the lowest quartile of institutional ownership is significant. (The other two quartile dummies are jointly insignificant with a p-value of the joint test of 0.71.) Therefore, column (4) presents our preferred model, which just has the lowest quartile dummy. The next four columns repeat these specifications but use the three-valued ordered outcome as the dependent variable. The results are very similar. If anything, they are slightly stronger (as might be expected) now that we are using a more finely tuned measure of corporate governance.[14] We also experimented with including the concentration of institutional ownership, measuring the share of ownership accounted for by the largest five institutional owners. This concentration measure is lower for the lowest quartile of institutional ownership (48% vs. 57%) than for the other three quartiles. When included in the models of Table 2, the coefficient on this measure was generally positive, consistent with the idea that more concentrated ownership reduces corporate governance problems. However, this ownership concentration measure became statistically insignificant in the firm fixed-effect specifications, and, importantly, our key low institutional ownership variable remained significant throughout.

Overall, the evidence in Table 2 points strongly towards a positive link between institutional ownership and corporate governance, which is driven by the firms in the lowest quartile of institutional ownership.[15]

13. As noted in the text, it does not matter that these models are estimated by OLS rather than a non-linear estimator. For example, the marginal effects on the bottom *IO* quartile dummy in a probit model of column (2) is 0.108 (0.019).

14. We also experimented with including firm fixed effects in the models of columns (1) and (5). The coefficients on institutional ownership remained negative but were insignificant. This is because, as we have shown in Figure 3 and the other columns of Table 2, institutional ownership seems to matter in a non-linear way, so columns (1) and (5) are mis-specified. It is having very low levels of institutional presence which is problematic for corporate governance.

15. In the rest of the paper, we will examine how firms with low institutional ownership behave differently from other firms. Although we will also look at heterogeneity with respect to the explicit governance indicators (e.g., Table 2's IVIS measures), we believe the structure of ownership is a preferable measure as the governing warnings are an extreme indicator of ongoing problems. For example, there may only be one explicit warning over several years, even though corporate governance problems have been persisting for many years.

4. Main Results

Given the substantial changes to the structure of CEO pay in the United Kingdom over the sample period, do the stylized facts from (the mainly US studies) carry over across the Atlantic? First, we look at asymmetric responses in the pay–performance relationship, and then in Section 4.2, we look at the evidence of "pay for luck".

4.1. Asymmetrical Response of Pay to Performance

The idea behind the examination of asymmetry is whether CEO pay increases with positive firm performance but decreases by far less when performance declines. Furthermore, we are interested in whether this asymmetry is particularly strong when corporate governance is weak. Our basic estimation equations are of the form

$$ln(\text{pay})_{ijt} = \alpha_{ij} + \beta PERF_{jt} + \tau_t + \varepsilon_{ijt}, \quad (1)$$

where $ln(\text{pay})_{ijt}$ is the total remuneration (what we label "new pay" in Section 2) of CEO i in firm j at time t, α_{ij} is a CEO-firm match-specific fixed effect (hence absorbing both the time-invariant CEO and firm effect), τ_t are time dummies, ε_{ijt} is an error term, and *PERF* is the measure of firm performance—a total shareholder return index (*TSR*) as a measure of firm value. Our baseline specification is simply the contemporaneous effect, but all our results are robust to alternative dynamic forms using lags. Standard errors are clustered at the firm level except when we use industry-level instrumental variables, where we cluster at the industry level.

First, we examine whether the estimated pay–performance link differs depending on institutional ownership. Column (1) of Table 3 presents the basic CEO pay–performance regression where we include a full set of firm-by-CEO match effects. It shows a statistically strong and positive link between pay and performance, indicating that a 10% increase in TSR is associated with a 1.6% increase in CEO pay.[16] In column (2), we switch to a first-difference specification since we will subsequently want to examine asymmetries in the pay–performance relationship depending on whether returns are positive or negative and this is more naturally specified in first-differences. The coefficient of 0.16 in the first-difference specification is very close to that in the previous column.

Column (3) of Table 3 allows for an interaction between returns and whether the firm has high or low institutional ownership.[17] We split the sample into quartiles based on average institutional ownership as in Section 3 and focus on the difference between the lowest quartile of ownership (less than 42% on average) and the other three

16. Allowing for an additional two lags in TSR results in a long-run estimate of the pay-performance link of 0.191 (0.024).

17. All regressions with institutional ownership effects also include a full set of interactions between the ownership dummies and the time dummies. The measure of institutional ownership is always lagged one period.

TABLE 3. Asymmetries in the CEO pay–performance relationship and institutional ownership.

	(1)	(2)	(3)	(4)	(5)
	Dependent variable = *ln*(new pay)				
Method:	Within groups	First differences	First differences	First differences	First differences
lnTSR	0.158***				
	(0.021)				
Δ*lnTSR*		0.163***		0.115**	
		(0.028)		(0.048)	
Δ*lnTSR*			0.230***		0.252***
**High_IO*			(0.026)		(0.035)
Δ*lnTSR*			0.025		−0.129
**Low_IO*			(0.057)		(0.091)
Δ*lnTSR*(+)				0.118	
				(0.077)	
Δ*lnTSR*(+)					−0.052
* *High_IO*					(0.069)
Δ*lnTSR*(+)					0.414***
* *Low_IO*					(0.142)
Observations	5,041	5,041	5,041	5,041	5,041
Firms	449	449	449	449	449

Notes: All results use Boardex data. Column (1) is a fixed-effect model with the *ln*(new pay) as the dependent variable. The subsequent columns report first-difference models on the same data. *Low_IO* firms are those with one-year lagged institutional investor share ownership in the bottom quartile of the distribution (and *High_IO* are all others). Column (1) includes CEO–firm match fixed effects. All regressions include time dummies and time dummies interacted with the *High_IO* dummy in columns (3) and (5). TSR is total shareholder return and Δln*TSR*(+) denotes the change in TSR is positive. Standard errors are clustered at the firm level. *Significant at 10%; **significant at 5%; ***significant at 1%.

quartiles.[18] The results presented in Section 3.2 suggest that this is the key contrast from a corporate governance perspective. The link between pay and performance is much larger (and significant) for the high institutional ownership firms (0.230) than the low institutional ownership firms (0.025). In the next column, we allow different pay–performance elasticities between positive and negative shareholder returns, by including an interaction between returns and an indicator equal to 1 if the growth was positive (Δ*lnTSR*(+)) and zero otherwise. Column (4) shows that there appears to be an economically substantial propensity to reward positive returns more favourably than negative returns are penalized looking at all firms together, but this is not significant at conventional levels.

18. Nothing hinges on using quartiles or combining the highest three quartiles into one category. Results available on request show that the remaining three quartiles have very similar estimated coefficients and one cannot reject equality. Table A.3 shows that there is little evidence of economically substantial differences between the low and high institutional ownership groups across observables such as sales, employment, market capitalisation or executive pay levels or growth rates.

Such asymmetries could indicate inefficiencies, but they may also occur as the outcome of an optimal contract for risk-averse CEOs. For example, Gopalan et al. (2010) discuss a model where the optimal contract ties CEO pay to performance to induce effort, but if CEOs are sufficiently risk-averse, they are not punished by pay cuts for negative shocks.[19] To shed light on this, we therefore focus on whether this asymmetry is a function of corporate governance. Column (5) of Table 3 generalizes the column (4) specification to allow the asymmetry of pay and performance to depend on our measures of institutional ownership. We find evidence that there is a significant asymmetry only for the more "weakly governed" firms but not the strongly governed firms. Firms with weak governance reward positive returns with a significantly higher pay (an elasticity of $0.285 = 0.414 - 0.129$) but require no pay penalty for negative returns (an insignificant -0.129). In other words, for such firms, the coefficients imply that a 10% increase in TSR is associated with a 3% higher pay, whereas a 10% decrease has no significant penalty (if anything, a 1.3% gain). By contrast, the firms with higher institutional ownership appear to reward performance symmetrically. (As the coefficient of -0.052 on the interaction between positive returns and high institutional ownership is insignificant.)

Figure 4 uses the coefficients from column (5) of Table 3 to illustrate this asymmetry for the high and low institutional investor categories separately. There appears to be a clear symmetry for panel (a) (high institutional ownership) compared to panel (b) (low institutional ownership).

All these results relate to new pay (i.e., salary plus bonus plus expected value of new equity awards). The asymmetry result is driven by the new equity awards since regressions that use cash pay (i.e., salary plus bonus) as the dependent variable do not show this asymmetry.[20] This is important since our results shown later will also point to the use of new awards to circumvent the impact of relative performance contracts.

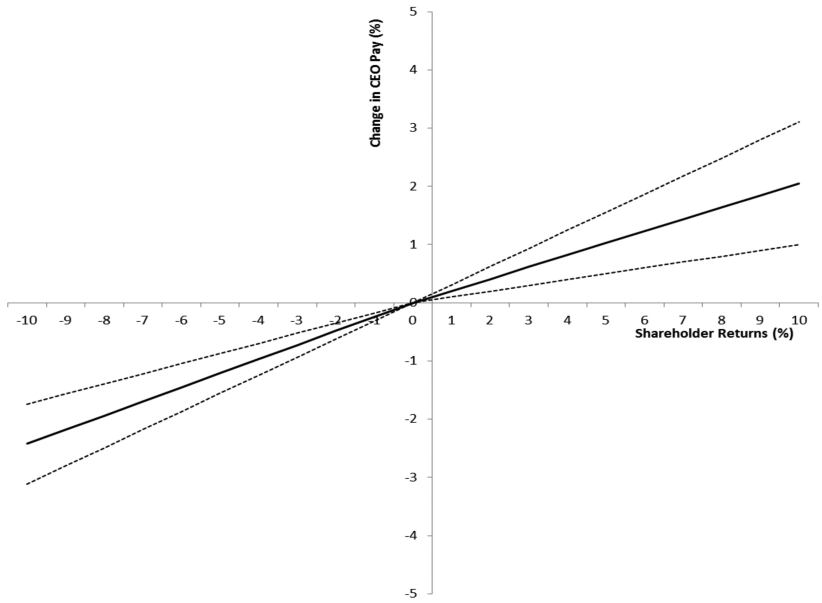
To check that these results are driven by governance and not some other correlated effect (like firm size), we repeated the analysis of Table 3 looking at interactions with other observables. For example, we constructed dummy variables based on whether the firm is in the lowest quartile or below median based on market capitalization, total employment, shareholder returns, and lagged levels of CEO pay. In no case do we observe significant evidence of asymmetry in pay with respect to performance on any of these alternative categorizations.[21]

19. Formally, this is driven by the sector-specific component of performance (the pay-for-luck issue we discuss later). The idea is that when CEOs have private information on which industries may be more profitable for the firm to focus on, shareholders may want them to be rewarded for positive shocks in those sectors to induce the right strategic choice.

20. Although they do still show a stronger link between pay and performance for higher institutional ownership firms.

21. We have also experimented with directly using the IVIS governance scores rather than institutional ownership. If we re-ran the specification in column (5) of Table 3, with firms split into either high or low governance based on their median IVIS score, we again cannot reject symmetry for well-governed firms (a coefficient of –0.093 with a standard error of 0.067), whereas we do reject it for the poorly governed firms (a coefficient of 0.249 with a standard error of 0.094).

(a): Firms with strong governance (High share of Institutional Owners)



(b) : Firms with weak governance (Low share of Institutional Owners)

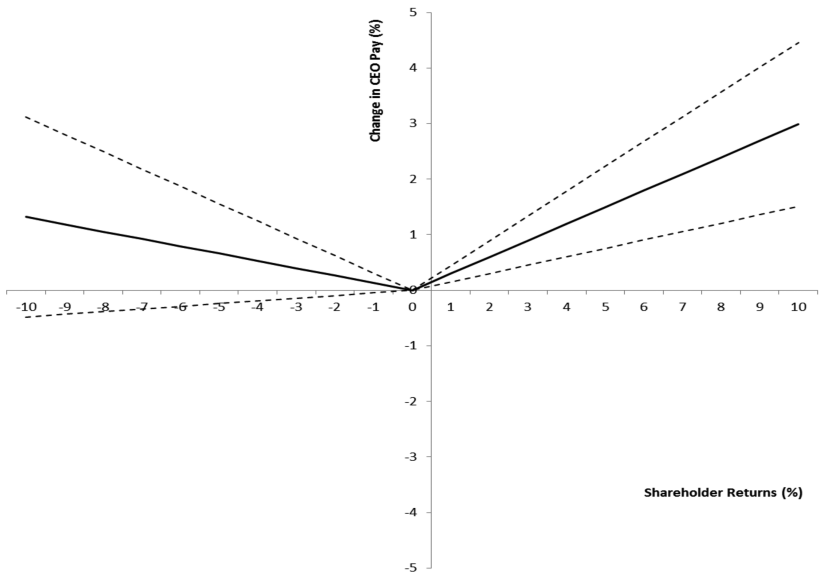


FIGURE 4. Asymmetric pay–performance estimates. Panel (a): firms with strong governance (high share of institutional owners). Panel (b): firms with weak governance (low share of institutional owners). These figures represent the implied effect of a percentage change in TSR (shareholder returns) on the percentage increase in CEO pay. The coefficients are from the specification in column (5) of Table 3. "Low share of institutional ownership" denotes firms in the bottom quartile of the (one year lagged) ownership distribution and "High share of institutional ownership" denotes all other firms. 95% confidence intervals shown.

Another concern is that our estimates implicitly assume that the executive remains with the firm and so is in a position to have their pay respond to changes in firm performance. It is plausible, however, that poor performance may lead not only to lower wages but also to an increased probability of a job separation (e.g., Huson et al. 2001; Jenter and Kanaan 2015). Thus, we may be underestimating the impact of firm performance on CEO expected returns. In Section 5.1, we will indeed show that CEO exit is more likely following poor TSR performance. This raises the issue of whether our results are biased due to this differential CEO attrition, which could be a form of dynamic selection bias. (The static selection effects are controlled for by the match specific effects.) Could the asymmetry of the coefficient on shareholder returns be attenuated, as large negative shocks to shareholder returns are followed by dismissal rather than compensation cuts?

To look at this issue, we performed several tests. First, we re-ran Table 3 excluding the last year (or alternatively the last two years) of CEO tenure. The asymmetries we identify in Table 3 continue to hold for these sub-samples (see Online Appendix Tables A.4 and A.5). Second, we allowed for an asymmetry in the CEO job-exit probability with respect to shareholder returns. However, unlike CEO pay, we found no significant difference between the impacts of positive or negative returns on job exits. Third, we allowed for both the level effect of shareholder return on CEO exit and its asymmetry with positive and negative TSR realizations to vary with our institutional ownership measures. Again, we could find no significant differences of these on job exit. We return to the issue of CEO exit in more detail in Section 5, but our initial conclusion here is that it seems unlikely that the findings on the CEO pay–performance relationships we describe are purely due to dynamic selection bias.

4.2. CEO Pay for Luck

Another way to investigate the issue of whether the pay–performance relationship is all due to market incentives is to consider the extent to which CEOs are rewarded for luck. Consider the pay of oil company CEOs. Their pay is related to their firm's shareholder returns, but this, in turn, is strongly correlated with the price of oil. Since the oil price is easily observed and outside the control of the CEO, the standard Holmstrom and Milgrom (1987) result would argue that the firm should ensure no link between pay and oil price. However, in practice, the link is strong, suggesting that CEOs are being partly rewarded for luck. Bertrand and Mullainathan (2001) show that US CEOs receive the same payoff to a "lucky dollar" of shareholder returns as they do to a general dollar of shareholder returns. Formally, this is illustrated by showing that the OLS estimate of the pay–performance elasticity is the same as the instrumental variable (IV) estimate using industry performance to instrument for firm performance.[22] Since the CEO cannot control industry performance, we can interpret the IV estimate as identifying those returns that are common to the industry, that is,

22. Many other instruments have been suggested in the literature. Blanchflower et al. (1996) focus on using lag structures, but potential external instruments have included firm-specific technological innovation (Kline et al., 2019; Van Reenen, 1996), import/export price shocks (Abowd and Lemieux, 1993; Bertrand,

luck.[23] There is substantial debate in the literature on whether pay for luck may be consistent with optimal contracting rather than rent skimming, as argued by Bertrand and Mullainathan. For example, Gopalan et al. (2010) suggest that it may be used to incentivize CEOs to optimally choose the level of sector exposure, whilst Hoffman and Pfeil (2010) show that if luck shocks are correlated with future investment opportunities for the firm, then pay for luck may be optimal in a dynamic setting.

We follow this idea by instrumenting shareholder returns with the returns in the global industry (the Datastream Industrial Sub-Sector Global-ex-UK Index) but dropping the UK firms from this index to avoid a mechanical relationship (i.e., we construct the leave-out mean). We have 92 such sub-sectors. Column (1) of Table 4 reports the OLS (in panel A) and IV (in panel B) estimates for Δln(new pay)—the same measure of CEO pay as used in Table 3. The coefficient on $\Delta lnTSR$ is significant in both specifications and we cannot reject that the IV and OLS are equal at the 5% significance level.[24] CEO "pay for luck" appears prevalent, just as in Bertrand and Mullainathan (2001).[25]

The evidence for substantial CEO pay hikes from rises in industry TSR may appear surprising. The substantial corporate reforms in the United Kingdom in the late 1990s were supposed to explicitly control for improvements in the firm position that were due to industry-wide shocks. So, is it simply that the reforms failed in this objective or is something more complex going on? An obvious starting point is to focus on those LTIPs with an explicit sector performance hurdle. At a minimum, we would expect such awards to exhibit much less sector pay for luck. Thus, for all LTIP awards, we identify those who have at *least some part of the award* that vests only on the performance of shareholder returns relative to a sector benchmark. We term these "sector LTIPs" (and their converse "non-sector" or "general LTIPs")—these are the splits shown in Figure 1.[26]

2004), and oil price shocks (Bertrand and Mullainathan, 2001). Card et al. (2014) instrument the value-added per worker of each firm (their measure of rents) by the value-added per worker of all firms in the same four-digit industry outside the region of Italy on which their analysis is conducted. The identifying assumption is then that industry demand shocks affect firm-level profitability but have no direct effect on local labour supply.

23. Subsequent work in the United States has examined whether the pay-for-luck effect is asymmetric. Garvey and Milbourn (2006) show that CEO pay rises when firm performance increases due to good luck but does not go down to the same extent when firm performance decreases due to bad luck. By contrast, Daniel et al. (2016) argue that this apparent asymmetry is a result of not controlling for firm size.

24. This equivalence between the OLS and IV estimates is also the case if we use the narrower measure of pay (cash pay) or the broadest measure (total pay).

25. We have also investigated whether the pay-for-luck effect is larger in less well governed firms. If we take the estimates in column (1) of Table 4 and instrument returns allowing for a differential effect between strong and weak governance, we do find that the pay–performance elasticities are much closer when instrumenting than in the OLS specification. This is consistent with such an interpretation, though the effect is not statistically significant.

26. Note that relative sector LTIPs account for 40% of all LTIPs by the end of the sample. The remaining 60% are still benchmarked, but against either a general index such as the FTSE 100 or some absolute performance target.

TABLE 4. Pay for luck?

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Change in new pay	Vesting percentage sector LTIP	Vesting percentage non-sector LTIP	Percentage change in sector LTIP value	Percentage change in non-sector LTIP value
Mean of dependent Variable:	0.117	0.544	0.663	0.323	0.548
Panel A: OLS estimates					
$\Delta lnTSR$	0.162***				
	(0.027)				
$\Delta_P lnTSR$		0.227***	0.159***	1.631***	1.412***
		(0.022)	(0.018)	(0.096)	(0.116)
Panel B: IV estimates					
$\Delta lnTSR$	0.173***				
	(0.047)				
$\Delta_P lnTSR$		0.078*	0.168***	1.206***	1.511***
		(0.041)	(0.039)	(0.136)	(0.162)
# firms	467	229	362	229	362
# CEOs	1,199	428	696	428	696
Observations	5,243	2,054	3,780	2,054	3,780
First stage F-stat	167	59	36	59	36

Notes: Panel (A) for each column reports results from an OLS regression, while panel (B) reports the results of an equivalent IV specification with $\Delta lnTSR$ instrumented by the ICB Industrial Sub-Sector Global ex-UK index TSR. Column (1) uses *ln*(new pay) as the dependent variable and the yearly change in TSR (total shareholder return) as the explanatory variable. Columns (2) and (3) use the vesting percentage of the relevant LTIP (long-term incentive plan) as the dependent variable, while columns (4) and (5) use the ex-post percentage change in value of LTIP as the dependent variable. Sector LTIPs are all performance-related equity plans that have at least some sector TSR comparison component, while non-sector LTIPs are all other equity plants. $\Delta_p lnTSR$ is the percentage change in TSR over the performance period p of the LTIP. All regressions include time dummies. Standard errors are clustered at the firm level for OLS and at the industry level (92 clusters) for IV. *Significant at 10%; **significant at 5%; ***significant at 1%.

We now conduct the OLS/IV comparison for two outcomes: (i) the percentage of the LTIP award that ultimately vests; and (ii) the percentage change in value of the LTIP from grant date to vest date. We are interested in the extent to which these two outcomes for CEOs successfully condition out the sector pay for luck (i.e., the OLS coefficient being significantly larger than the IV coefficient). Columns (2)–(5) of Table 4 reports the results. Columns (2) and (3) show that the percentage of the LTIP that ultimately vests is strongly correlated with firm returns, as one would expect. When we instrument firm returns with sector returns, the IV coefficient for sector LTIPs drops substantially and is no longer significant at the 5% level.[27] By contrast,

27. It would be surprising for the estimated IV coefficient to fall to zero even if the sector LTIP perfectly conditioned out sector luck. There are two key reasons for this. First, our sector instrument is not in

the non-sector LTIP IV coefficient is statistically indistinguishable from the OLS coefficient—so as we would expect given their design, these LTIPs fail to condition out sector luck. Turning to columns (4) and (5), however, when we use the percentage change in value of the LTIP from grant date to vest date as the dependent variable, the sector LTIPs continue to reward sector luck to a significant extent. Although note that even here there is attenuation of the sector luck component for sector LTIPs (column (4)), whereas this is not the case for non-sector LTIPs (column (5)).

4.3. What Happens When Relative Performance Awards Fail?

Why do sector LTIPs seem to successfully remove most pay for luck in the probability of vesting but seem to do so much less successfully for the more important issue of overall pay? One reason may be that other components of pay are used to offset any penalty for the CEO associated with LTIPs failing to meet their performance hurdles. To test this hypothesis, we focus on the LTIPs that reach their vesting date and see whether there is any reaction to these events. The actual vesting outcome is therefore fully known at the time to the firm. Suppose in a given financial year t there are a set of LTIPs, S_V, that reach their vesting date. For each of the LTIPs in this set, we can calculate the percentage of originally granted shares that actually vest. This is bounded between 0 and 1. If we have more than one LTIP in the S_V set, we can calculate a weighted percentage using the expected value on grant date as weights. We then define a dummy variable, *LTIP_Fail*, equal to 1 if this vesting percentage is less than 1 (i.e., not full vesting). We use the one-year lag of this indicator since the firm may not know for certain what will happen to the LTIPs that are vesting this year (so will not be certain of the value of *LTIP_Fail*). However, they will definitely know the previous year's outcome.

Table 5 reports the results of including such an indicator variable in the usual pay–performance regressions. We consider two of our measures of pay. First, using *ln*(new pay) as the dependent variable allows for offsetting compensation across all components of pay. Second, we use the expected value of new equity awards only to test whether firms use new LTIP awards to offset losses in the value of previously awarded LTIPs. Thus, we can compare the coefficients across the two measures of pay to determine whether *and where* any such compensation is occurring.[28]

Column (1) of Table 5 reports the coefficient estimates where the dependent variable is *ln*(new pay), showing that there is no obvious link between pay and whether LTIPs failed in the previous year. We then decompose this into cash pay (salary plus bonus) in column (2) and new equity awards in column (3). Like the first column, CEO cash does not respond to past failure of LTIPs, but in contrast, the coefficient on *Lagged_LTIP_Fail*

general the exact sector comparator group used in the evaluation. (This is because the benchmark firms are not always revealed in the company accounts.) Second, we define a sector LTIP as one with *at least some* sector–return comparison. However, such an LTIP may have other comparators as well that will not completely condition out sector luck.

28. We again report results in the Online Appendix that exclude the final one or two years of CEO tenure to ensure our results are not being driven by selection (see Online Appendix Tables A.6 and A.7).

TABLE 5. CEO pay changes after failing LTIPs.

Dependent variable:	(1) *ln* (new pay)	(2) *ln* (cash pay)	(3) New equity awards	(4) *ln* (new pay)	(5) *ln* (cash pay)	(6) New equity awards
Lagged_LTIP_Fail	−0.003	0.003	40,509	−0.022	−0.006	8,952
	(0.015)	(0.013)	(29,871)	(0.016)	(0.016)	(34,880)
Lagged_LTIP_Fail				0.093**	0.057*	135,879**
**Low IO*				(0.038)	(0.030)	(67,693)
Lagged *lnTSR*	0.185***	0.138***	117,780**	0.187***	0.144***	116,775
	(0.018)	(0.014)	(52,028)	(0.021)	(0.015)	(62,822)
Observations	5,068	5,068	5,068	5,068	5,068	5,068
# CEOs	1,049	1,049	1,049	1,049	1,049	1,049
# firms	449	449	449	449	449	449

Notes: New pay is cash pay plus the expected value of newly awarded equity and new equity awards are the expected £ value of new equity awards on grant date. Lagged_LTIP_Fail equals 1 if the LTIPs that vested in the previous financial year did not fully vest, and zero otherwise. TSR is total shareholder return and is included in all columns. *Low IO* is equal to 1 if the firm's lagged institutional ownership share is in the bottom quartile across all firms in a given year. Columns (4)–(6) also include *Low IO* interactions with both *lnTSR* and time dummies. Regressions also include time dummies, a lagged dependent variable, and match fixed effects. Standard errors are clustered at the firm level. *Significant at 10%; **significant at 5%; ***significant at 1%.

is positive, though not significant, for new equity awards. Note that all our regressions include *ln*(*TSR*) on the right-hand side, so we control for the fact that declines in the value of previous awards usually occur in years that see declines in shareholder returns. Of course, the sign of the coefficient on *Lagged_LTIP_Fail* goes the wrong way for this to be an explanation—we would expect pay to be lower in poor performing years.

Does corporate governance matter here? To assess this, we interact the "failed LTIP" indicator with our indicator of corporate governance (institutional ownership). Columns (4)–(6) replicate the previous columns but allow for this potential governance asymmetry. The key result can be seen in column (6). Echoing the results of Table 3, we find that firms with low institutional ownership provide significantly higher compensation (in the form of new equity awards) when previous LTIPs fail. This is not the case in firms with high institutional ownership. There is no pay offset for well-governed firms—implying that external control matters for CEO pay. This evidence suggests that LTIPs appear to be undermined in some firms. In firms with poor governance, when LTIPs do not pay out, CEOs are able to obtain significantly better deals for their new LTIPs to compensate them for their failure to meet the terms of their previous relative performance contracts.

4.4. How do Future LTIPs Get Changed in Response to Failure?

In this sub-section, we explore potential mechanisms for the key result obtained in Table 5. We begin by examining exactly how the rise in the expected value of new awards occurs. Column (1) of Table 6 uses the simple face value of the new awards

TABLE 6. Mechanisms.

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Face value of new equity awards	Vest percentage at minimum threshold	Minimum threshold	Positive payoff below median	No. of performance scales
Lagged_LTIP_Fail	8,887	−0.861	1.070	−0.045	0.038
	(67,939)	(1.011)	(2.243)	(0.034)	(0.071)
Lagged_LTIP_Fail	276,327**	3.699**	−4.698	0.197**	0.320**
*Low IO	(127,957)	(1.737)	(4.180)	(0.072)	(0.121)
Observations	5,068	1,513	1,513	1,513	686
# CEOs	1,049	412	412	412	179
# firms	449	263	263	263	105

Notes: Face value of new equity awards is the £ value of new equity awards on grant date unadjusted for probability of vesting, vest percentage at minimum threshold is the percentage of shares that vest when the minimum performance threshold is reached, minimum threshold is the percentile of performance at which at least some shares vest, positive payoff below median is equal to 1 if there is a positive payoff to the award below the median and zero otherwise, and no. of performance scales is the number of criteria used in evaluating the LTIP (=0 if criterion used and =1 if two or more criteria used). Lagged_LTIP_Fail equals 1 if the LTIPs that vested in the previous financial year did not fully vest, and zero otherwise. TSR is total shareholder return and is included in all columns. *Low IO* is equal to 1 if the firm's lagged institutional ownership share is in the bottom quartile across all firms in a given year. All columns include *Low IO* interactions with time dummies. Columns (1)–(4) are OLS regressions, while column (5) is a probit with marginal effects reported. Standard errors are clustered at the firm level. *Significant at 10%; **significant at 5%; ***significant at 1%.

(i.e., number of shares * grant date share price) as the dependent variable. This measure abstracts from any change in the structural design of new awards (e.g., changing the peer group, switching metrics and trigger points, etc.) and simply focuses on whether the firm awards a higher absolute quantum. There is a positive and significant coefficient on the interaction between LTIP failure and low institutional ownership, showing that increasing the amount of reward in new LTIPs is very clearly a channel through which weakly governed firms compensate CEOs when their old LTIPs are not achieved.

A complementary strategy would also be to restructure the terms of the new equity awards to make them less likely to fail, or at least to increase their expected value when performance is weak. Of course, the opposite might be the case. As discussed in Section 2, we do not have enough detailed information to construct the expected value of LTIPs in the sophisticated manner of Bettis et al. (2014). Therefore, a concern is that even if the interaction term in Table 5 reflects a more generous quantum of shares (as in column (1) of Table 6), the LTIP as a whole could have been made tougher because of other features of the LTIP contract we are not measuring in "new pay". In the remainder of this sub-section, we therefore present evidence on other structural LTIP contractual features that we have for sub-sets of the main data.

One approach to making the LTIPs *more* generous would be to alter the initial trigger point at which vesting begins. In the LTIP example for Vodafone highlighted in Figure 2, this would mean moving the kink at median performance and 25% vesting in a north-westerly direction, that is, by either increasing the percentage of shares that

vest at the minimum or reducing the minimum performance requirement (or both). To conduct this individual award-level analysis, we need to focus on comparable awards across CEOs. The single most common award is an LTIP share award with a TSR metric (e.g., the Vodafone example in Figure 2). We are able to merge in additionally collected data on the initial vesting threshold and the vesting percentage at that threshold for this sub-set of awards. Columns (2) and (3) of Table 6 show the results for these two metrics. In column (2), we can see that firms are significantly more likely to increase the percentage of shares that vest at the minimum performance threshold if the previous LTIP failed *and* they are weakly governed. No such response is observed for well-governed firms. Column (3) then shows that there is also a response in terms of reducing the minimum performance threshold, which is quantitatively large but not statistically significant. Finally, in column (4), we combine these two effects and measure whether there is a positive payoff to the award below median performance. This seeks to combine both effects in a single metric and unsurprisingly shows that poorly governed firms respond to failed LTIPs by increasing expected LTIP payouts for weaker performance. The coefficient on the interaction is positive and significant.

Over time, LTIPs across all firms have tended to become more complex. One measure of this is the number of performance scales that are used to judge success. In the Vodafone example in Section 2, there was only one performance scale (TSR), which is measured against a global peer group. In the early part of our sample, most awards had a single performance scale—usually TSR or earnings per share (EPS) growth. In the latter part of the sample, most awards have adopted at least two performance scales—generally a combination of TSR and EPS growth. Figure 5 shows the trend over time.

There are two key effects of this change. First, if we calculate the vesting percentage, that is, shares vesting as a proportion of originally granted, there is a decline (from 67% to 60%) when we switch from single to multiple performance scales. This is driven by that fact that full vesting now requires the achievement of multiple objectives, which is harder to achieve. Second, the probability of the award completely failing is reduced—from 26% to 20%. This is because there are now at least two chances to reach the minimum performance threshold. Therefore, the move towards multiple performance scales reduces the high-stakes nature of the LTIPs and has increased the likelihood that at least some payout will be achieved.

In the final column of Table 6, we explore whether the probability of switching the number of performance scales is related to the failure of prior LTIPs and to corporate governance. We can do this analysis only for the sub-set of firms that were constituents of the FTSE 100 at some point in our sample.[29] We run a probit model that has a dependent variable equal to 1 if the LTIP has multiple performance scales and zero if it has a single scale. The regressions control for corporate governance, time effects, and *ln*(*TSR*). The coefficient on *LTIP Fail* in column (5) shows no significant link between prior LTIPs failing and the decision to increase the number of performance scales used.

29. This is because we require additional detailed data on each award to determine the number and nature of each performance scale, and this information is not reported in Boardex and so was hand-collected.

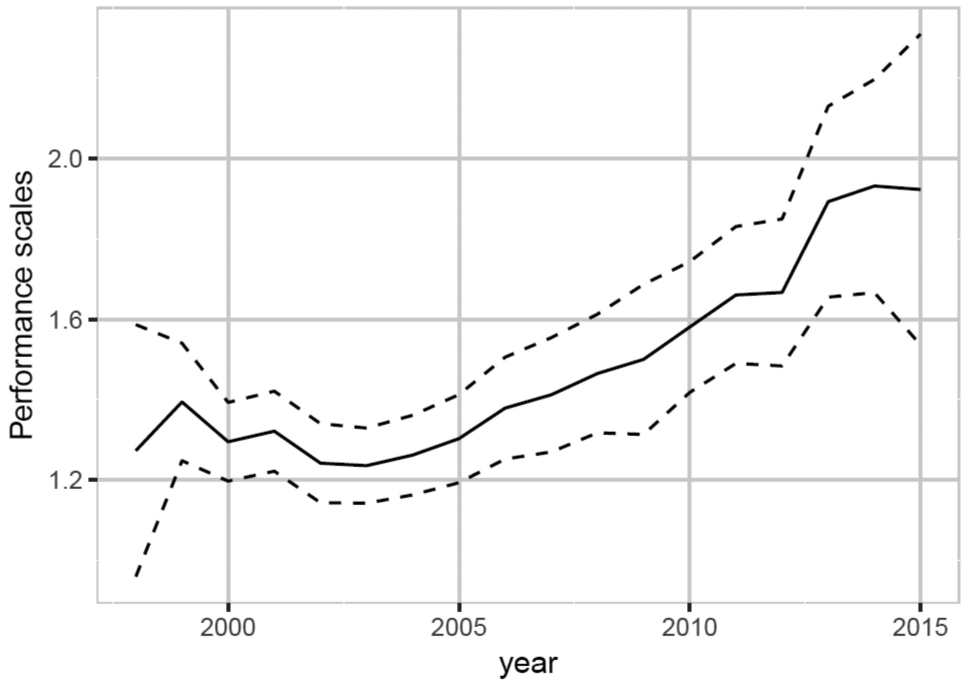


FIGURE 5. Number of performance scales per LTIP. This is the average number of performance scales per LTIP CEO contract (e.g., EPS and TSR). Dotted lines show 95% confidence intervals.

However, the interaction effect of *LTIP Fail* with our corporate governance measure shows that weakly governed firms are significantly more likely to respond to past failure by increasing the number of metrics. One might argue that if the LTIP had been poorly designed in the past and had failed more frequently than was optimal, it makes sense for the firm to revise the structure of the LTIP in response. But again it is unclear why this should only occur for firms with weak governance. Our results on the number of metrics an award uses suggest a gaming of the structure of LTIPs to compensate for past failure and complements the analysis in the earlier part of the table.[30]

One other mechanism has been explored in other work. Skovoroda and Bruce (2016) examine year-on-year changes to the composition of performance peer groups used for relative performance evaluation in setting CEO pay in FTSE 100 companies and find evidence of peer selection bias. Using confidential data on FTSE 100 firms from 2005 to 2011, the authors find that firms keep their peer groups weak by excluding relatively stronger performing peers. Of direct relevance to our results, they also show that peer selection bias is less pronounced in firms with higher institutional investor ownership, which suggests that institutional investors might be aware of the

30. We have also re-run the regressions in Table 6 conditioning on contemporaneous shareholder returns and firm size and our results are robust to the addition of these controls.

risks of peer selection bias. In a similar vein, Faulkender and Yang (2013) show that such peer groups are chosen strategically to drive up CEO pay in the United States, especially in weakly governed firms. By contrast, Bizjak et al. (2019) do not find much evidence of strategically chosen custom peers (what we term sector LTIPs), but they do find a switch towards opportunistic selection of general indexes (which give greater rewards) than custom peers.[31] Ma et al (2019) find that the selection of more noisy benchmarks is driven by compensation consultant preferences, and weakly governed firms are more likely to choose such consultants.

In summary, the evidence in Tables 5 and 6 suggests that when LTIPs fail, CEOs achieve compensation for this in the form of new more generous LTIPs in weakly governed firms. The increased generosity comes in a mixture of a greater quantum of shares plus changes to the structure of LTIPs that effectively make them easier to achieve.

5. Extensions and Robustness

We have considered a number of alternative explanations for our results some of which we detail in Online Appendix B. In this section, we consider two important extensions relating to CEO attrition and the quantitative importance of LTIPs.

5.1. Alternative Explanations for Compensation of Failing LTIPs: CEO Attrition

One alternative explanation is that the failed LTIP was poorly designed and firms respond to this mistake by correcting the design of the LTIP that we then tend to value more highly ex ante, making it look as if there is compensation for failure. It is not obvious why this occurs only for low institutional ownership firms since we find no such effect for those with higher levels of institutional ownership. But perhaps this is because more attention is paid to the good design of LTIPs when there is a larger share of institutional investors. However, we also show in Table 6 that firms increase the quantum of the award, which is hard to explain because of optimal restructuring.

An alternative explanation builds on Oyer (2004). Firms want to retain their CEOs and so need to offset the negative shock of failing LTIPs with more generosity, that is, they need to ensure the CEO remains on the participation constraint. It may just be that low institutional ownership firms are for some reason more exposed to this effect. To test this hypothesis, we estimate CEO job-exit probabilities. We define a job exit as occurring subsequent to the last observed pay year for a CEO with a particular firm, provided we observe the same firm in the following year but without the same

31. Following this idea, we examined in our data whether following an LTIP failure, weakly governed firms were more likely to switch from a sector LTIP to a non-sector LTIP (e.g., from a group of peers in the industry to a more general group of peers such as the FTSE 100). Using this as an alternative dependent variable in Table 6, we found that sector LTIP failure had no significant effect on switching into a non-sector LTIP, and although the coefficient on the interaction was in the expected direction, it was statistically insignificant.

TABLE 7. Job-exit marginal probabilities.

	(1)	(2)	(3)	(4)
$\Delta lnTSR$	−0.055***	−0.056***	−0.055***	−0.063***
	(0.009)	(0.009)	(0.009)	(0.011)
Lagged_LTIP_Fail		0.041***	0.037***	0.040***
		(0.011)	(0.012)	(0.012)
Lagged_LTIP_Fail			−0.001	−0.013
*Low IO			(0.024)	(0.024)
Observations	5,581	5,581	5,350	5,350
# firms	470	470	451	451
# CEOs	1,114	1,114	1,078	1,078

Notes: The coefficient is the marginal effect from a probit model of job exit with time dummies. All variables are as defined in Table 5. All columns include time dummies, column (3) also includes an indicator for *Low IO*, and column (4) further includes *ln*(market capitalization) and interactions of *Low IO* with the time dummies, change in TSR, and *ln*(market capitalization). Standard errors are clustered at the firm level. *Significant at 10%; **significant at 5%; ***significant at 1%.

CEO employed. We can then estimate an equation of the form

$$Exit_{it} = \alpha(LTIP\_Fail)_{it-1} + \beta(LTIP\_Fail{*}Low\_IO)_{it-1} + \gamma\Delta TSR_{it} + \delta(Low\_IO)_{it-1} + \tau_t + \varepsilon_{it}, \quad (2)$$

where $Exit_{it}$ equals 1 if the CEO exits the firm in that year and zero otherwise. In equation (2), we would expect α to be positive—when LTIPs fail, it is more likely the CEO will leave. According to a retention-based story, we would expect β to be negative because the reason that low IO firms are observed to award LTIPs that are more generous after failure is in order to retain the executive. All else equal, we should therefore see lower exit among such CEOs (compared to the average given the failed LTIPs).

Table 7 reports the results of estimating equation (2). In column (1), we find that when TSR growth is strong, CEOs are less likely to exit as we might expect. Column (2), additionally, shows that CEO exit is indeed more likely when their LTIPs fail. However, column (3) shows that this LTIP failure effect does not differ by institutional ownership, that is, $\hat{\beta}$ is almost exactly zero. In the final column, we also included lagged market capitalization (as a measure of size) and interactions of *Low IO* with the time dummies, market capitalization, and ΔTSR_{it} and the result remains robust.

We conclude that the alternative story of differential retention is unlikely to explain our results.

5.2. Are LTIPs Large Enough in Magnitude?

Another alternative argument is simply that the share of LTIPs in the pay that CEOs receive each year is not yet substantial enough to align CEO and shareholder incentives. It is true that even by the end of our sample only around one-third of pay is accounted for by LTIPs, with the remainder reasonably evenly divided between salary and annual

bonus. A key question is therefore whether our results that point to governance failures that allow LTIPs to be undermined are really just reflecting their relative unimportance. To examine this hypothesis, we take two approaches. First, we exploit the fact that the share of LTIPs in pay has risen over the sample period (Figure 1). If the problems we identify were really one of magnitude, we would expect those problems to be stronger in the first half of the sample when LTIPs were less prevalent. Therefore, we can re-estimate the results allowing for interactions with a dummy for the second half of the sample and test whether there are significant changes in the coefficients. Second, we can exploit the fact that firms differ in their use of LTIPs. We obviously do not want to claim that this is exogenous, but we can divide our sample of firms into "High LTIP" and "Low LTIP" based on their average share of pay accounted for by LTIPs. Again, if we re-estimate with interactions for the "High LTIP" firms, we can test whether the same governance problems occur in both sets of firms. For our sample, the average LTIP share is 12% in the "Low LTIP" firms and 29% in the "High LTIP" firms.

Online Appendix Table A.8 reports both experiments. The first column again shows the key result from Table 5—poorly governed firms compensate for failed LTIPs with more generous new awards. In column (2), we see that there is no evidence to suggest that this effect is removed as the share of LTIPs in total pay grows—in fact, the coefficient is the wrong sign, though statistically insignificant. Similarly, this effect of poor governance has not weakened over time (column (3)).

6. Conclusions

We examine the pay–performance relationship for CEOs over the last two decades using explicit measures of performance contracts. The United Kingdom has moved much more rapidly and aggressively than the United States to eliminate all equity awards to executives who do not have performance conditions attached to them (LTIPs). Our analysis suggests that not all of the CEO pay–performance relationship is likely to be rationalized by efficiency considerations. First, CEO pay rises much more when the firm does well than it falls when the firm does badly, and this asymmetry occurs only for firms with weaker governance. Our primary measure of weak governance is low institutional ownership (below about 42% of equity): We show that in the United Kingdom (like the United States), institutional owners like pension funds tend to be more activist in demanding stronger corporate governance. Second, there remains substantial pay for luck with pay responding to industry-wide improvements in performance. Third, even when CEO pay is explicitly tied to performance relative to sector averages, it seems to have little effect on reducing pay for luck. A major reason for this is that when CEOs fail their relative performance contracts, they are compensated by even more generous incentive pay deals, in terms of both the ex-ante value of new awards and the structure of such awards. Again, these rewards are concentrated in those firms that have weaker governance. We examined efficiency-based explanations for these findings, such as the need to retain CEO talent in the face of negative shocks in low institutional ownership firms but did not find strong evidence for them.

In our view, the fundamental problem is that many CEO pay packages are so complex that it is hard for individual shareholders to gauge their true structure and generosity unless they are unusually assiduous and strongly motivated. Greater mandated transparency is unlikely to resolve this (e.g., Hermalin and Weisbach 2012; Mas 2016). Institutional owners, because they have greater resources and larger block holdings, are more likely to have the ability and incentive to be active monitors. In the absence of such agents "stepping up to the plate", it is likely that calls for cruder and more direct intervention in CEO pay will become ever stronger over time.

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Supplementary Data

Supplementary data are available at *JEEA* online.