

**Shifting the Goalposts? Analysing Changes to Performance Peer Groups Used to
Determine the Remuneration of FTSE100 CEOs**

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31 Oct, 2016

[British Journal of Management, forthcoming]

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Abstract

This paper examines year-on-year changes to the composition of performance peer groups used for Relative Performance Evaluation (RPE) in setting CEO pay in FTSE 100 companies and finds evidence of peer selection bias. We find that firms keep their peer groups weak by excluding relatively stronger performing peers. We 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 risks of peer selection bias. The results suggest that peer group modifications can be viewed, at least in part, as an expression of managerial rent-seeking.

Keywords: Corporate Governance, Executive Compensation, Performance Peer Groups

1 INTRODUCTION

Stock-Based Compensation (SBC) has become an increasingly significant component of executive reward for UK CEOs while the level of detail and complexity in the design of the stock-based element of pay has increased considerably since the mid-1980s (e.g. Buck, Bruce, Main, and Udueni, 2003; Conyon and Schwalbach, 2000; Goergen and Renneboog, 2011; Main, Bruce, and Buck, 1996; Ozkan, 2011; Renneboog and Zhao, 2011). Recent empirical research documents widespread use of Relative Performance Evaluation (RPE) in stock-based incentive contracts where CEOs and managers are evaluated on the basis of peer performance and, in particular, the use of performance peer groups in restricted share schemes, performance share plans, or long-term incentive plans (Carter, Ittner and Zechman 2009; Gong, Li, and Shin 2011). Empirical studies also document that stock-based RPE is characterised by multiple discretionary elements and find considerable variations in specific RPE characteristics and in their overall complexity across firms (Carter *et al*, 2009).

Agency theory (e.g. Holmstrom, 1979, 1982; Murphy, 1999) views performance-based pay in general and SBC in particular as a key governance mechanism that helps foster incentive alignment between the interests of executives and shareholders. Assuming that the shareholders have complete control over the process of pay design, the potential efficiency gains from using RPE in incentive contracts are well understood. Holmstrom (1979) shows that the efficiency of performance-based pay as an incentive alignment mechanism crucially depends on the amount of useful information that performance measures, such as the firm's stock price, conveys about CEO actions and about the quality of CEO decisions. Agency theory advocates the use of stock-based RPE on the grounds that it helps “de-noise” CEO

effort, insulates risk-averse CEOs from non-firm-specific risks common to the firm's peers, and makes SBC more efficient (Holmstrom, 1982; Murphy, 1999).

Empirical evidence suggests that firms vary in their ability to maintain complete control over the pay-setting process and that CEO pay outcomes are determined in part by the quality of corporate governance at the firm (e.g. Armstrong, Ittner and Larcker 2012, Bertrand and Mullainathan 2001, Garvey and Milbourn 2006). While RPE schemes may reduce pay windfalls (pay unrelated to performance) and may foster strong pay-performance links under arm's-length contracting, the relative efficiency of complex RPE schemes in creating and maintaining strong pay-performance links is less clear in those firms where key elements of RPE design, such as peer group composition, are influenced by powerful entrenched CEOs. The risk is that the quality of information that the peer group, *preferred* by the CEO, may convey about the true value of CEO actions to the firm could be compromised. If peer selection is compromised and peer groups are chosen strategically, CEO pay windfalls could be higher and value creating incentives could be lower under opaque RPE schemes than under more transparent fixed-pay arrangements.

The purpose of this paper is to investigate empirically the potential sensitivity of stock-based RPE schemes to opportunistic behaviour and rent-extraction by UK CEOs. This focus is close to that of the managerial power perspective on pay (Bebchuk and Fried, 2003, 2004, 2005) that argues that complex and opaque incentive schemes in particular are vulnerable to obfuscation by entrenched managers. As such, rather than being a part of the solution, the preference towards RPE schemes in weakly governed firms could itself be the manifestation of the problem of asymmetric information and 'hidden action' that incentive schemes are expected to solve. Whilst the managerial power perspective relaxes the assumption of arm's

length contracting over CEO pay, it is, nevertheless, a part of the broad agency view that suggests that powerful CEOs with privileged information may seek to abuse their power at the expense of shareholders.

This paper provides the first detailed examination of year-on-year changes to the composition of performance peer groups in large UK firms and reports a significant bias in the peer selection process. First, using data on FTSE 100 firms from 2005-2011, we show that a significant minority of large UK firms keep their peer groups weak by excluding relatively stronger performing firms, where (stock price) performance is measured relative to the rest of the peer group. Second, we 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 risks of peer selection bias.

The paper is structured as follows. Section 2 develops the theoretical perspectives on the role of RPE in CEO pay as, alternatively, underpinning or undermining corporate governance, and explains the institutional/regulatory landscape in the UK in relation to remuneration design and disclosure. Section 3 develops specific testable hypotheses regarding the impact of deliberate modifications of the composition of peer groups. Section 4 describes the data collection process. Section 5 details the results of the analysis and Section 6 interprets the significance of the results and offers some concluding remarks.

2 LITERATURE AND CONTEXT

2.1 Literature

A central component of the rent-extraction theory of CEO pay (Bebchuk and Fried 2003, 2005) is the notion of ‘pay camouflage’ - where powerful CEOs and weak boards disguise

the total cost of the pay package by making pay schemes more complex and opaque, in part to manage the risk of potentially damaging public ‘outrage’. Stock-based RPE components such as performance share plans and long-term incentive plans are among the most complex elements of CEO pay (Carter *et al*, 2009). The composition of bespoke performance peer groups is, in turn, one of the least standardized elements of incentive plan design. As such, where boards display ‘pay camouflage’ tendencies, they may consider deliberate modifications to performance peer groups as a potentially attractive vehicle for rent-extraction.

In an early study, using a cross-section of US-based companies, Lewellen, Park, and Ro (1996) found evidence that management overstated their firms’ relative performance by choosing industry and peer-company comparative performance benchmarks, and broader market indices, that are negatively biased. Whilst the performance benchmarks studied by Lewellen *et al* were used for disclosure purposes and did not directly affect the levels of top executive pay, in this study, by contrast, we explore relative performance bias in performance peer groups that *are* used for incentive-based pay, with a potential first order effect on pay in UK firms. This focus is closer to that of Camara (2001) who compares relative with absolute performance-based vesting hurdles, identifying a correlation between the former and increased risk and dividend payments.

The scope for abuse of peer selection bias is, arguably, greater in “bespoke” peer group plans, where group members are hand-picked by the firm, rather than in broader and more objective “index-based” peer group plans. Carter *et al* (2009) analyse a cross-section of FTSE 350 firms in 2002 and find that firms that use, alternatively, bespoke and index-based peer groups display statistically similar levels of peer-group-adjusted returns. Ferri (2009) notes that the

cross-sectional tests employed in Carter *et al*'s study result in high standard errors of the estimates that are likely caused by large between-firm variation. Ferri (2009) further recommends a panel data analysis or analysis of year-on-year changes in peer group composition as a means of offering lower standard errors and providing more powerful statistical insights, which is the methodology that we employ in this paper. Gong, Li, and Shin (2011) analyse performance peer group data disclosed by S&P 1500 firms immediately after the SEC introduced new disclosure rules in 2006. Gong *et al* report evidence that US firms select peers that better capture common risks in accordance with agency theory and the optimal contracting hypothesis. They also find evidence of peer selection bias: using analysts' forecasted annual stock returns, they show that firms with lower expected industry-adjusted stock returns are more likely to be included as peers. Gong *et al* do not investigate the extent to which the strength of peer selection bias varies across firms that are governed relatively strongly or weakly. Furthermore, the extent to which US boards have continued abusing apparent selection bias after 2006, or, have alternatively ended this practice under increased public scrutiny, enabled by better disclosure, remains underexplored. By way of comparison, we focus here on large FTSE 100 firms that have been required to publicly disclose the composition of performance peer groups since the introduction of the Directors' Remuneration Report Regulations in 2002 (Carter *et al* 2009). As FTSE 100 firms are particularly closely followed by analysts and investors, our sample selection if anything, mitigates against self-serving peer selection.

Our research embodies a number of distinctive features. First, we study *actual* subsequent stock price performance of peer group members, not analyst forecasts at the award date, as employed in Gong *et al*, (2011). This could be important if the boards of directors benefit from privileged information at the specific point when peer group decisions are made (see

Ravina and Sapienza, 2010), or, equally, where decisions are made with the benefit of hindsight. Second, we use relative performance tests of selection bias where excluded/added peers' performance is compared to the rest of the peer group, rather than the industry average as in prior research. While industry-average performance benchmarks are relevant, our methodology is relatively more robust to the presence of unobserved heterogeneity in RPE design across the firms. Third, we investigate the extent to which peer selection bias varies across firms that are governed relatively strongly or weakly.

This paper also contributes to a wider literature on a range of practices by which executives may manipulate pay structures and outcomes. As well as option backdating (see, for example, Bebchuk, Grinstein, and Peyer, 2010) these include the opportunistic (retroactive) timing of executive stock option grants, option repricing (downward adjustment of the strike price of 'under water' options), withholding of sensitive information, and 'golden parachutes'. In this context, richer understanding of the role of performance peer groups and their deliberate modification, on CEO pay, represents an important contribution at a time when CEO pay remains under intense scrutiny by shareholders and regulators.

2.2 Institutional Context

In the UK context, reward based on relative performance was endorsed by the Greenbury Committee (1995). This reflected contemporary concern that, especially in buoyant equity markets, pay arrangements which featured a significant component of "naked", rather than peer-adjusted, stock-based compensation tended to deliver excessive and unwarranted levels of individual reward. The Greenbury Committee's endorsement was instrumental in the increasing complexity of pay arrangements, as well as in the subsequent popularity of Long Term Incentive Plans (LTIPs). LTIPs involve the award of shares, cash or a combination of

the two, based on comparative performance over (usually) a three year period. Awards are invariably based on increased performance as measured by a combination of Total Shareholder Return (TSR) in real terms and/or relative to an index or performance peer group and an accounting based measure or measures (generally Earnings Per Share (EPS) growth). Our analysis of Directors' Remuneration Reports suggests that, whilst awards require parallel achievement of the market-based and accounting-based performance thresholds, accounting-based measures, with rare exceptions, are not subject to comparison with a performance peer group and typically require a specified rate of real increase in value (e.g. RPI + 2% for EPS growth targets). For instance, *all* 42 FTSE 100 boards that were identified as using bespoke peer groups in 2011 were using peers' TSR to help ascertain the proportion of vested performance shares. In comparison, in only two firms from this list, BP and Royal Dutch Shell, the proportion of vested performance shares was also linked to the reporting firm's rank against peers on other measures that were used alongside TSR – the reserves replacement ratio (BP), and hydrocarbon production, net cash from operating activities, and EPS growth (Royal Dutch Shell). This justifies our focus on peers' TSR performance in particular.

While the proportion of vested RPE shares depends on the reporting firm's relative rank, median peer performance and closely related measures, such as mean peer performance, are often used by the firms in our sample as minimum performance thresholds. In 31 of the 42 firms that were using bespoke peer groups in 2011, none of the performance shares vested unless the reporting firm's TSR was at the median of the peer group. In nine firms, the performance shares did not vest unless the reporting firm's TSR was equal to that of the index composed from peer group constituents. In two firms, Vodafone and Sage Group, the minimum vesting thresholds were linked to accounting performance measures that did not

take into account peer performance. The level of vesting was then determined by using a “multiplier” that scaled with the firms’ relative TSR performance. In Sage Group, the multiplier was equal to 0.75 for below median and was equal to 1 for median performance. In Vodafone, as the 2011 AR suggests, “There will be no multiplier until TSR performance exceeds median.” In all cases, therefore, the median peer TSR performance represents an important RPE threshold. A failure to beat the median peer significantly affects the expected RPE payoffs.

Successive versions of the UK Corporate Governance Code (‘the Code’) have required a greater degree of transparency and disclosure in reporting pay arrangements in general. In relation to the period under scrutiny in this study, the Code forms one of the two sets of recommendations or requirements which influenced the disclosure of details of remuneration policy. The Code acknowledges the principle of comparing a company’s performance with its peers in determining levels of pay, and endorses approval of such arrangements by shareholders, but it is non-prescriptive in tone and merely urges that companies ‘should use such comparisons with caution’. Ultimately, in any case, the Code is non-binding and companies engage with its recommendations on a ‘comply or explain’ basis. By contrast, the regulations for quoted companies’ remuneration reports within the Companies Act (2006) carry legal force and require publication of and explanations for the choice of performance conditions employed, albeit in the non-audited part of the report, and without specific guidance as to what constitutes an acceptable level of explanation. Prior to 2013, as part of the overall remuneration policy, these details would be subject to a *non-binding* shareholder vote. In the absence of a binding shareholder vote and with significant discretion available to firms in terms of the degree of disclosure of performance comparison arrangements, this aspect of pay design remained vulnerable to self-serving discretionary behaviour by

executives who may have been in a position to influence the remuneration committee. On the specific issue of influence, the Code observes, rather weakly: ‘where executive directors are involved in advising or supporting the remuneration committee, care should be taken to recognise and avoid conflicts of interest’.

Since 2013, the requirements have tightened significantly, with companies’ remuneration policy requiring endorsement by shareholders in a binding vote held at least every three years, together with an annual advisory vote on the implementation report, which details how policy translates into detail within a given year. It would appear, therefore, that increased disclosure, coupled with the need for shareholder approval, and stronger and more independent non-executive directors, have the potential to constrain some of the more direct means by which CEOs might be able to inflate their pay and extract rents in future.

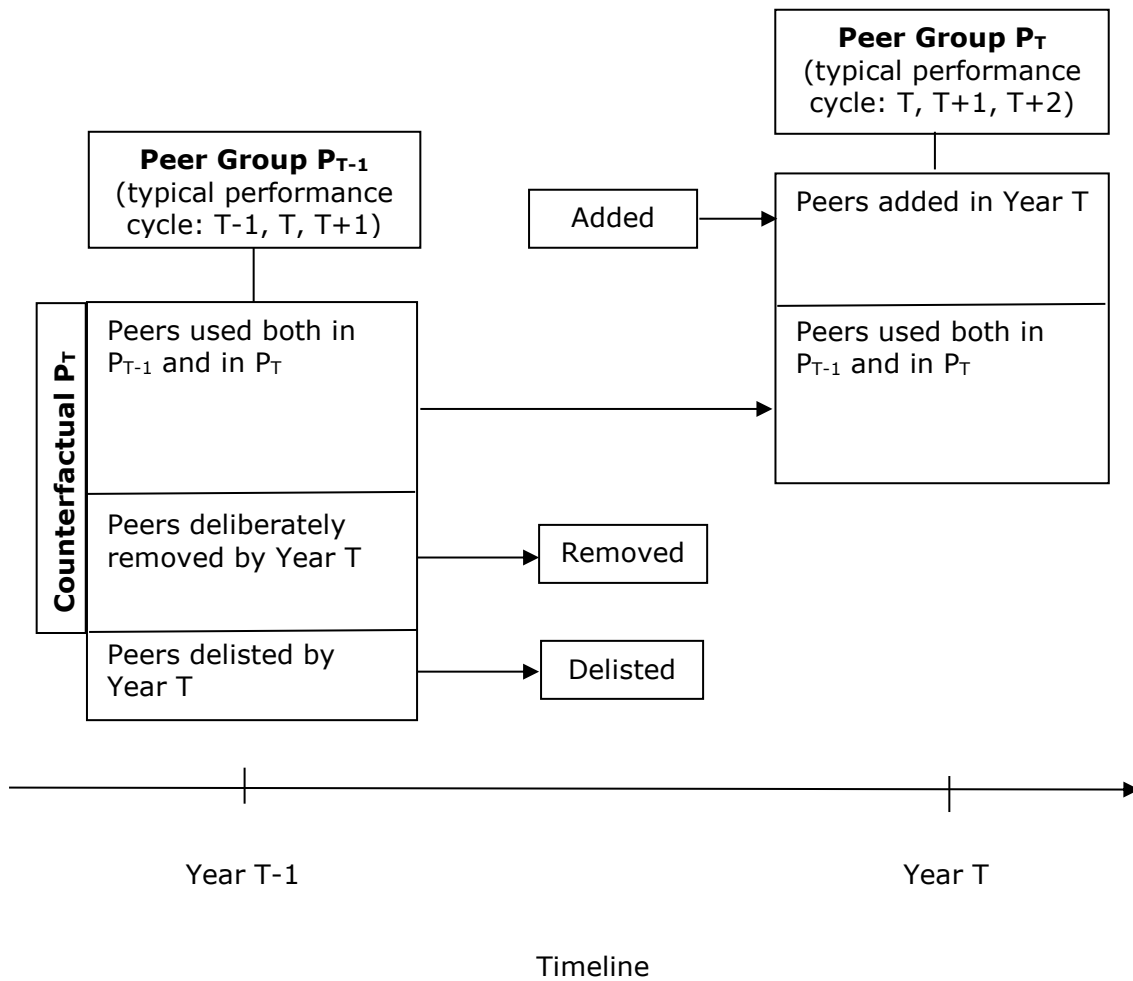
3 HYPOTHESES

To the extent that incentives to manipulate peer groups exist, how exactly could boards use their discretion opportunistically? Figure 1 illustrates this. Deliberate modifications of the composition of peer groups may include instances where weaker peers, viewed as likely to underperform, are added to the group. Alternatively, peers that are expected to outperform the group can be removed. These scenarios require a degree of insider knowledge / superior information on the part of the reporting firm’s directors (e.g. Ravina and Sapienza, 2010). Additionally, peer group modifications can be applied *ex-post* with the benefit of hindsight as reported performance periods often commence 2 to 10 months prior to the first published details of peer groups in annual reports. As such, boards can add/remove peers based on their performance in the months directly preceding the public announcement of peer group composition without any requirement for superior knowledge on the part of directors. Finally,

boards of directors may make adjustments to the peer groups *after* the public announcement of peer group composition if events such as takeovers and delistings of peer group members during the performance period warrant such changes. When a delisted firm is removed from the group entirely, group TSR is unaffected by the firm's share price prior to delisting. So instead of, say, substituting the delisted peer from the last month of trading onwards with a synthetic TSR index equal to the average TSR of the rest of the peer group, a decision could be made to completely remove a delisted peer from the peer group depending on the peer's performance prior to the delisting. This might result in weaker relative performance benchmarks and overstated relative performance. The above analysis suggests that a minority of opportunistic CEOs might have both the incentive and ability to use peer group modifications for rent-extraction. Accordingly, we propose:

Hypothesis 1: Deliberate modifications to performance peer group composition exhibit, on average, peer selection bias whereby weaker peers are added to the peer groups and stronger peers are removed.

Figure 1. Year-on-Year Changes in Peer Group Composition



The empirical literature documents that CEO pay outcomes are determined in part by the quality of corporate governance at the firm (e.g. see Armstrong, Ittner and Larcker (2012), Bebchuk *et al* (2010)), Bertrand and Mullainathan (2001), Garvey and Milbourn (2006)). A significant literature identifies institutional shareholders as a potent corporate governance mechanism that can influence CEO decisions. They have the incentive (via large ownership stakes) and the resources to monitor CEOs, and the means to influence CEO decisions (e.g.

via threats to sell their shares); see, among others, Becht *et al* (2010), Chung, Firth and Kim (2002), Ertimur *et al* (2011), Gillan and Starks (2007), Hartzell and Starks (2003).

The monitoring-enhancing effects of board independence and, in particular, the effects of the proportion of independent (non-executive) directors and a split of the roles of Chairman and CEO on pay outcomes are the subject of a substantial interdisciplinary literature (e.g. Conyon and Peck (1998), Daily, Johnson, Ellstrand and Dalton (1998), Finkelstein and Hambrick (1989)). Empirical evidence shows that weaker boards tend to reward CEOs for ‘lucky’ events outside of CEOs’ control more often than stronger boards do (Bertrand & Mullainathan, 2001; Garvey and Milbourn, 2006), while board independence is often associated with lower levels of CEO pay (e.g. Core, Holthausen, and Larcker, 1999) and with stronger pay-performance links (e.g. Rayan and Wiggins, 2004; Goh and Gupta, 2010). Board theorists warn, however, that independent boards may suffer from information problems: non-executive directors, lacking the incentives and skills to monitor CEOs, may depend on the CEOs for key information (Ezzamel and Watson, 2005). Accordingly, we propose:

Hypothesis 2: The tendency for deliberate peer group modifications to result in weaker peer groups is more pronounced in firms characterised by less independent boards and weaker investor oversight.

4 DATA AND METHODOLOGY

We study the use of performance peer groups in CEO pay arrangements in FTSE 100 companies for a seven-year period (2005-2011). The sample is formed from companies included in the FTSE 100 list in 2007 and the full list of companies is provided in Supporting Information. The hand-collected data on long-term incentive components of CEO pay,

including the composition of performance peer groups, are drawn from annual reports. Data on firm-specific corporate governance variables, ownership and financial data are sourced from Thomson Datastream, ThomsonONE, and annual reports. Table 1 summarizes and provides the definitions of all variables used in the analysis.

Table 1. Variables, Definitions and Sources

<i>Variable</i>	<i>Definition and source</i>
<i>Log (total assets)</i>	Natural log of total assets (<i>Thomson Datastream</i>)
<i>Chair is CEO or Ex-CEO</i>	Has the chairman previously held the CEO position in the company? (<i>Thomson Datastream</i>)
<i>Board Independence</i>	Percentage of independent board members as reported by the company. (<i>Thomson Datastream</i>)
<i>Director Affiliations</i>	Average number of other corporate affiliations for the board member. (<i>Thomson Datastream</i>)
<i>Board Tenure</i>	Average number of years each board member has been on the board. (<i>Thomson Datastream</i>)
<i>Institutional investor ownership concentration</i>	The percentage ownership held by the top-five institutional investors. (<i>ThomsonONE</i>)
<i>Institutional investor ownership turnover (%)</i>	Average portfolio turnover by top-five institutional investors (%), weighed by percentage ownership. (<i>ThomsonONE</i>)
<i>TSR($P_{T,i}$)</i>	Log return on the portfolio comprised from performance peer group reported by firm i in year T . TSR is measured over the relevant performance periods specified in annual reports, normally years T , $T+1$, and $T+2$. (<i>Company Annual Reports, Thomson Datastream</i>)
<i>TSR(Added$_{T,i}$)</i>	Log return on the portfolio of newly added peer group members. TSR is measured over the relevant performance periods specified in annual reports, normally years T , $T+1$, and $T+2$. (<i>Company Annual Reports, Thomson Datastream</i>)
<i>TSR(Removed$_{T,i}$)</i>	Log return on the portfolio of the companies that are deliberately removed from the peer group. TSR is measured over the relevant performance periods specified in annual reports, normally years T , $T+1$, and $T+2$. (<i>Company Annual Reports, Thomson Datastream</i>)
<i>TSR(Counterfactual $P_{T,i}$)</i>	Log return on the portfolio comprised from performance peer group reported in year $T-1$, excluding those firms that are delisted by the start of performance period T . TSR is measured over the relevant performance periods specified in annual reports, normally years T , $T+1$, and $T+2$. (<i>Company Annual Reports, Thomson Datastream</i>)
<i>Weak peers added $_{T,i}$</i>	The number of peers added that achieve TSR performance strictly lower than that of the median firm in <i>Counterfactual P</i> . (<i>authors' own calculations</i>)
<i>Strong peers added $_{T,i}$</i>	The number of peers added that outperform the median firm in <i>Counterfactual P</i> . (<i>authors' own calculations</i>)
<i>Weak peers removed $_{T,i}$</i>	The number of peers removed that underperform the median firm in <i>Counterfactual P</i> . (<i>authors' own calculations</i>)
<i>Strong peers removed $_{T,i}$</i>	The number of peers removed that outperform the median firm in <i>Counterfactual P</i> . (<i>authors' own calculations</i>)

Table 2 reports that 70 to 80 percent of FTSE 100 firms use performance peer groups to help determine CEO pay in any given year. Both *Index Peer Groups* (where performance criteria are measured *exclusively* relative to broad indices such as FTSE 100, FTSE 350, FTSE World Media etc.) and *Bespoke Peer Groups* (where performance criteria are measured, *at least in part*, relative to bespoke peer groups) are widely used. The focus of our attention is on this second group, i.e. on identifying instances of deliberate modification of peer groups in the case of companies using bespoke peer groups.

Table 2. Use of Compensation Peer Groups in Long-Term CEO Pay, FTSE 100 Firms

	Firms, count						
	2005	2006	2007	2008	2009	2010	2011
<i>Performance Peer Groups not Used:</i>							
<i>Companies without a long-term incentive component in CEO pay.</i>	7	8	7	8	9	4	5
<i>Peer groups are not used and peers' performance is not taken into account.</i>	10	9	10	15	13	17	21
<i>Index Peer Groups:</i>							
<i>Performance criteria are measured exclusively relative to broad indices (e.g. FTSE 100, FTSE 350, FTSE World Media).</i>	30	29	27	27	27	26	23
<i>Bespoke Peer Groups:</i>							
<i>Performance criteria are measured, at least in part, relative to bespoke peer groups.</i>	45	49	53	45	44	44	42
Total firms	92	95	97	95	93	91	91

4.1 Dependent variables

4.1.1 Peer Selection Bias

First, we analyse relative TSR performance of the added/removed peers to see if relatively weaker/stronger peers are added/removed. Our focus on peers' TSR performance is

motivated by the analysis of Directors' Remuneration Reports and is justified in Section 2.2.

Table 3 reports instances of deliberate modification (year-on-year changes) of the composition of bespoke peer groups, confirming that they are quite common. For instance, 20 (or 45%) of the 44 companies which used bespoke peer groups in 2009 initiated changes to their peer groups. Of these 20 companies, 15 included new peers and 10 removed some of the existing peers (excluding delisting-related changes).

Table 3. Instances of Deliberate Modification of the Composition of Bespoke Peer Groups (Year-on-Year Changes), FTSE 100 Firms

	Firms, count						Total
	2006	2007	2008	2009	2010	2011	
Companies which modified the composition of (bespoke) performance peer group either by including new peers or by removing some of the former peers (excluding changes resulting from delistings)	15	19	18	20	12	12	96
Companies which included new peers into the peer group	13	17	11	15	10	10	76
Companies which removed some of the former peers (excluding changes resulting from delistings)	9	12	14	10	6	6	57

Note: As some firms modify the composition of peer groups by both including new peers and by removing some of the old peers in the same year, the values in rows two and three do not necessarily sum to the value in row one.

With reference to Figure 1 and using notation P_T for the performance peer group reported in year T, we define the portfolio $Added_T$ as those companies that are newly included in P_T but which were not included in P_{T-1} . Portfolio $Removed_T$ is defined as those companies that were included in the peer group P_{T-1} but were then removed and so did not appear in peer group P_T . The benchmark portfolio against which we compare portfolio $Added_T$ is *Counterfactual* P_T , defined in Figure 1 as P_{T-1} net of any companies that are delisted by the start of performance

period T . The benchmark portfolio for $Removed_T$ is P_T itself. We test for peer selection bias (Hypothesis 1) by testing whether the portfolio returns of $Removed_T$ and $Added_T$ are equal to the returns on the respective benchmark portfolios P_T and *Counterfactual* P_T over the subsequent (usually three-year) performance period pertaining to P_T . By comparing TSR of $Added_T$ to that of *Counterfactual* P_T we are able to isolate and study the effect of the decision to **add** new peer group members. By comparing TSR of $Removed_T$ to that of P_T we are able to isolate and study the effect of the decision to **remove** peers.

Portfolio returns are calculated using monthly returns indices resourced from Thomson Datastream as follows: (1) In all cases we use the relevant (in most cases, three-year) performance periods specified in Annual Reports. (2) All portfolios are equally-weighted. (3) The proceeds of delisted firms are reinvested (equally) across the remaining firms in the portfolio. (4) In those cases where all firms in $Removed_T$ are delisted before the end of the relevant performance period (pertaining to P_T) portfolio returns on $Removed_T$ and P_T are compared on the last month of $Removed_T$ trading. (5) The end of the observation period is May 2013. (6) Monthly returns for foreign peers are calculated using equity prices denominated in the home currency, rather than in GBP.

4.1.2 *The Propensity to Modify the Composition of Bespoke Peer Groups*

The second step in our analysis concerns the extent to which firm-level corporate governance characteristics affect the boards' propensity to add/remove weak/strong peers (Hypothesis 2). With the view to differentiating across specific types of peer selection bias, we define *Weak peers added* as the *number* of peers added that achieve TSR performance which is strictly lower than that of the median firm in *Counterfactual* P (defined in Figure 1 as the peer group

reported in year T-1 net of firms delisted by year T). *Strong peers added* is defined as the number of peers added that outperform the median firm in *Counterfactual P*. Similarly, *Weak peers removed* is the number of removed peers that underperform the median firm in *Counterfactual P*, while *Strong peers removed* is the number of removed peers that outperform the median firm. The use of the median *Counterfactual* peer for performance benchmarking is motivated by the analysis of Directors' Remuneration Reports and is justified in Section 2.2. As before, TSR performance is measured over the relevant performance period; normally years T, T+1, and T+2.

In the Amec example, discussed in Supporting Information, our calculations suggest that seven of the fifteen firms that were deliberately removed from the peer group in 2011 performed weaker than the median firm in the counterfactual (old, 2010) peer group in the period from April 2011 (start of the performance period) to May 2013 (end of observation period), while eight removed firms outperformed the counterfactual median over the same period. In contrast, fifteen firms that were added to the peer group in 2011 performed weaker than the counterfactual median while not a single newly added peer managed to outperform the counterfactual median (TSR data, sourced through *Thomson Datastream*, was available for fifteen of the sixteen firms that were added to the peer group in 2011). Accordingly, for $i = \text{'Amec'}$ and $t = \text{'2011'}$, we write *Weak peers added* $_{i,t} = 15$, *Strong peers added* $_{i,t} = 0$, *Weak peers removed* $_{i,t} = 7$, *Strong peers removed* $_{i,t} = 8$. As a result of these changes, the performance of the new, 2011, peer group approved by the board was significantly weaker than that of the counterfactual peer group that the board used one year earlier and that could have been easily "recycled". Our estimations further suggest that if the peer group had not been changed, the value of performance shares that could be expected to vest to Amec's CEO would be lower by an amount equivalent to 65.6% of the CEO's annual salary. This

illustrates the effect that peer group selection/changes can have on a CEO's final remuneration and financial incentives. All relevant details (vesting scale, the proportion of TSR-related vs. EPS-related performance awards) are carefully taken into account and are made available in Supporting Information.

4.2 Independent variables

The percentage ownership held by the top-five institutional investors (*Institutional investor ownership concentration*) is used as a proxy for institutional ownership (Hypothesis 2).

Board independence proxies include the percentage of independent board members as reported by the company (*Board Independence*), average board tenure (*Board Tenure*) and the dummy variable *Chair is CEO or Ex-CEO*, taking the value 1 if the chairman serves as the CEO or previously held the CEO position in the company and 0 otherwise. While the vast majority of FTSE 100 boards split the roles of Chairman and CEO, a significant minority of FTSE 100 Chairmen have previously held the position of CEO at the same firm. This type of arrangement might both question the independence of the Chairman and suggest a degree of shared tolerance for the practice of deliberate peer group weakening (see Andres *et al* 2014; Evans *et al* 2010).

4.3 Control (independent) variables

Instances of CEO pay manipulation have been shown to co-vary with firm size in a number of different contexts. Heron and Lie (2009) show that option backdating is more likely to occur in smaller firms. Bizjak *et al* (2011) report that smaller firms inflate CEO pay by choosing relatively larger companies as compensation peers. Accordingly, we use the log of total assets to control for firm size. Furthermore, we use the average number of corporate affiliations of board members (*Director Affiliations*) as a proxy for board networks (Fich and

Shivdasani, 2006; Larcker, So, and Wang, 2013; Renneboog and Zhao, 2011). Finally, we control for average portfolio turnover of the top-five institutional investors (*Institutional investor ownership turnover*), weighted by their percentage ownership, as a proxy for investor heterogeneity and heterogeneous investment horizons (Gaspar, Massa, and Matos 2005).

4.4 Descriptive Statistics

Table 4 reports descriptive statistics. Panel A summarises data for 2006-2011 across all firms that employ bespoke peer groups. The regression analysis of year-on-year changes in peer group composition leaves out year 2005. Panel B reports descriptive statistics for those firms that either do not employ peer groups or use index peer groups. Together, these suggest that firms in Panel A tend to be larger than firms in Panel B. This is in accordance with prior research (Carter *et al*, 2009). Panel A is also characterised by relatively lower institutional investor ownership turnover, lower board tenure, and relatively more independent boards with better connected (busier) directors. The t-statistics for the corresponding tests across the two panels are reported and are statistically significant. An average bespoke peer group includes close to 16 peers. This result is partially influenced by an outlier, Reed Elsevier, which introduced a 125-strong peer group in 2010. If we exclude Reed Elsevier, the mean peer group size drops to 15.6. We exclude Royal Bank of Scotland from Table 4 and from the rest of the analysis on account of its majority ownership by the UK government. Including Royal Bank of Scotland tends to marginally strengthen results on peer selection bias.

Table 4. Descriptive Statistics, 2006-2011

Panel A: Firm/years that employ Bespoke Peer Groups						
	Obs	Mean	St. dev.	Min	Max	T-stat ^(a) (panels A vs. B)
<i>Log (total assets)</i>	229	16.76	1.75	13.15	21.44	4.42**

<i>Chair is CEO or Ex-CEO</i>	229	0.09	0.28	0	1	0.99
<i>Board Independence</i>	229	62.30	11.00	23.08	93.75	4.51**
<i>Director Affiliations</i>	229	2.00	1.06	0.00	6.86	3.48**
<i>Board Tenure</i>	229	5.11	1.64	0.91	12	2.90**
<i>Institutional investor ownership concentration</i>	229	0.21	0.08	0.07	0.46	1.74
<i>Institutional investor ownership turnover (%)</i>	229	24.48	6.75	11.61	63.24	4.23**
<i>Total number of peers in $P_{T,i}$</i>	229	16.11	9.77	4	125	
<i>Weak peers added T,i</i>	229	0.61	1.80	0	15	
<i>Strong peers added T,i</i>	229	0.39	1.23	0	11	
<i>Weak peers removed T,i</i>	229	0.30	1.04	0	7	
<i>Strong peers removed T,i</i>	229	0.36	1.21	0	9	

Panel B: Firm/years that do not employ Bespoke Peer Groups

	Obs	Mean	St. dev.	Min	Max
<i>Log (total assets)</i>	252	16.10	1.51	12.43	21.22
<i>Chair is CEO or Ex-CEO</i>	252	0.06	0.24	0	1
<i>Board Independence</i>	252	57.67	11.46	33.33	100
<i>Director Affiliations</i>	252	1.70	0.80	0.18	4.5
<i>Board Tenure</i>	252	5.67	2.44	0.25	12.59
<i>Institutional investor ownership concentration</i>	252	0.22	0.09	0.03	0.55
<i>Institutional investor ownership turnover (%)</i>	252	26.90	5.76	13.71	59.06

Notes:.

- a) ** - indicates significance at the 1% level; * - indicates significance at the 5% level

5 EMPIRICAL RESULTS

Statistical tests are organised as follows. We first test whether peer group modifications are characterised by self-serving peer selection (Hypothesis 1). Second, we ascertain the degree to which peer group modifications can be viewed as a product of managerial power and firm-specific governance (Hypotheses 2).

5.1 Peer Selection Bias

Table 5 reports 73 year/firm instances (T, i) where reporting firm i added new peers. These new peers form $Added_{T,i}$. There are 55 year/firm instances where firms deliberately removed peers; these make up $Removed_{T,i}$. We test if portfolio returns of $Removed_{T,i}$ and $Added_{T,i}$ are equal to returns on the benchmark portfolios $P_{T,i}$ and *Counterfactual* $P_{T,i}$ respectively. Table 5 reports summary statistics for the portfolio log total shareholder returns and the

corresponding t-tests. The mean log return of $Added_{T,i}$ peers is about 5 percentage points below the benchmark return, though this difference is not statistically significant against a two-sided alternative. The mean log return on the portfolio of removed peers is about 15.4 percentage points higher than its benchmark return. This result is both highly significant statistically and economically important. Including Royal Bank of Scotland increases the sample size to 76 added and 57 removed and yields marginally lower p-values, strengthening the results. Table 5 further reports the results of the Wilcoxon matched-pairs, signed-ranks tests on median difference, a non-parametric test that does not assume that the difference in matched pairs is normally distributed. The results of the Wilcoxon signed-ranks tests closely confirm the results of the t-tests. Accordingly, we accept *Hypothesis 1*; evidence suggests that peer group modifications display elements of peer selection bias.

Table 5. Portfolio Performance (Paired) tests

Panel A: TSR performance^(a) of newly added peers vs TSR of 'old' counterfactual peer group			
	Obs	Mean	St. dev.
TSR($Added_{T,i}$); log return on the portfolio of newly added peer group members	73	-0.076	0.604
TSR(Counterfactual $P_{T,i}$); log return on the portfolio comprised from performance peer group reported in year T-1, excluding those firms that are delisted by the start of performance period T	73	-0.024	0.452
Difference in means	73	-0.052	0.568
<i>T-test: $H_0: mean(TSR(Added_{T,i}) - TSR(Counterfactual P_{T,i}))=0$</i>			
<i>t = -0.79</i>			
<i>Pr=0.43</i>			
<i>Wilcoxon signed-rank test: $H_0: median(TSR(Added_{T,i}) - TSR(Counterfactual P_{T,i}))=0$</i>			
<i>Z = -.965</i>			
<i>Pr=0.335</i>			
Panel B: TSR performance^(a) of current peer group vs TSR of recently removed peers			
	Obs	Mean	St. dev.
TSR($Removed_{T,i}$); log return on the portfolio of the companies that are deliberately removed from the peer group	55	0.068	0.507
	55	-0.086	0.397

TSR($P_{T,i}$); log return on the portfolio comprised from performance peer group reported in year T

Difference in means	55	0.154	0.497
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T-test: $H_0: \text{mean}(\text{TSR}(\text{Removed}_{T,i}) - \text{TSR}(P_{T,i})) = 0$

t = 2.30

Pr = 0.026

Wilcoxon signed-rank test: $H_0: \text{median}(\text{TSR}(\text{Removed}_{T,i}) - \text{TSR}(P_{T,i})) = 0$

Z = 2.807

Pr = 0.005

Notes:

- a) In all cases, TSR is measured over the relevant performance periods specified in annual reports, normally years T, T+1, and T+2 (i.e. subsequent to peer changes).

5.2 The Propensity to Modify the Composition of Bespoke Peer Groups

If peer selection bias is detected, can peer group modifications be viewed, at least in part, as an expression of managerial power? Table 6 reports the results of negative binomial regressions that analyse the count variables *Weak peers added*, *Strong peers added*, *Weak peers removed*, and *Strong peers removed*. The negative binomial specification is appropriate where the count variable is characterised by overdispersion (or extra-Poisson variation). If decisions to remove firms X and Y from the peer group are made on the basis of firms' relative performance vis-a-vis the rest of the peer group, the two decisions are correlated. These correlated events can give rise to overdispersion in modification counts. Further, all models control for the lagged (log) peer group size, which is used as the offset with the coefficient constrained to one. Standard errors are clustered for 52 clusters in firms.

Results in Table 6 suggest that investor oversight (*Institutional Investor Ownership Concentration*) has a differential effect on four types of modifications. While the rates with which firms add new peers (both weak and strong) are higher in firms with stronger investor oversight, higher *Institutional Shareholder Ownership Concentration* is negatively associated with instances where relatively stronger peers are removed. In contrast, *Chair is CEO or Ex-*

CEO seems to be positively associated with all four types of modifications, and its effect on the propensity to remove stronger peers is statistically significant. The joint significance tests on the effects of co-variates across the four models are reported in the far-right column in Table 6. The effects of *Board Independence* are mixed and inconclusive as independent boards are associated both with modifications that make peer groups weaker (*Weak peers added*) and stronger (*Weak peers removed*). Overall, the empirical evidence lends partial support to Hypothesis 2. The tendency on the part of the firms to exclude relatively stronger performing peers, which we document empirically in the previous section, appears to be less pronounced in firms with high levels of *Institutional Shareholder Ownership Concentration*.

Table 6. The Propensity to Modify the Composition of Bespoke Peer Groups

FTSE 100 firms where performance criteria are measured, at least in part, relative to bespoke peer groups. Negative binomial regressions on pooled data 2006-2011. Standard errors are clustered for 52 clusters in firms. Dependent variables are defined as follows. *Weak peers added* $T_{i,t}$ is the number of peers added that achieve TSR performance strictly lower than that of the median firm in the counterfactual peer group (defined as the peer group reported in year T-1 excluding firms delisted by year T). *Strong peers added* $T_{i,t}$ is the number of peers added that outperform the median firm in the counterfactual peer group. *Weak peers removed* $T_{i,t}$ is the number of peers removed that underperform the median firm in the counterfactual peer group. *Strong peers removed* $T_{i,t}$ is the number of peers removed that outperform the median firm in the counterfactual peer group. TSR performance is measured over the relevant performance period; normally years T, T+1, and T+2.

	Outcomes Effect (Cluster-robust SE)								Wald tests of joint significance over four models, Chi2(4)
	Weak peers added		Strong peers added		Weak peers removed		Strong peers removed		
<i>Institutional Investor Ownership Concentration</i>	4.32	(3.13)	8.19**	(2.79)	.030	(3.59)	-6.14*	(2.97)	17.03** (P-value 0.002)
<i>Institutional Investor Ownership Turnover</i>	-.048	(.039)	-.026	(.036)	-.059	(.035)	-.061	(.040)	3.53 (P-value 0.47)
<i>Board Independence</i>	.043*	(.020)	.021	(.020)	.036	(.021)	.013	(.018)	5.36 (P-value 0.25)
<i>Board Tenure</i>	.008	(.093)	-.012	(.087)	-.013	(.124)	.149	(.114)	2.91 (P-value 0.57)
<i>Director Affiliations</i>	-.472*	(.231)	-.092	(.227)	-.522	(.311)	-.738**	(.250)	12.45* (P-value 0.014)
<i>Chair is CEO or Ex-CEO</i>	.462	(.447)	.476	(.377)	.654	(.409)	.965*	(.389)	7.09 (P-value 0.13)
<i>Log Assets</i>	-.074	(.154)	.082	(.099)	-.125	(.159)	-.345**	(.121)	16.04** (P-value 0.003)
Constant	-3.87	(4.31)	-7.40	(3.65)	-1.92	(4.60)	4.20	(3.33)	
<i>Log peer group size (T-1) (offset)</i>	1		1		1		1		
Ln(alpha)	1.55**	(.218)	1.66**	(.214)	1.88**	(.284)	1.77**	(.305)	
Number of obs	229		229		229		229		
Number of clusters	52		52		52		52		

Cluster-robust standard errors in parentheses; ** - indicates significance at the 1% level; * - Indicates significance at the 5% level

As far as control variables are concerned, boards with more *Director Affiliations* (a proxy for board networks and for ‘busy boards’) are found to be negatively associated with all four types of peer group modifications. The joint effect of *Director Affiliations* in the four regressions, Table 6, is statistically significant ($p < 0.05$) which suggests that busy boards generally implement fewer changes. Finally, the proxy for firm size (*Log Assets*) is negatively associated with modifications that remove peers and its effect on the propensity to remove stronger peers is statistically significant. While examining board governance, we also analysed the effect of compensation (remuneration) committee independence (e.g. Faulkender and Yang, 2010), i.e. the percentage of independent board members on the compensation committee as reported by the company. The effect of compensation committee independence was not statistically significant.

To ensure that our results are not driven by heterogeneity in capabilities and incentives across institutional monitors (Almazan, Hartzel and Starks 2005), we followed Shin and Seo (2011) and classified the top-five institutional owners into five categories - *Banks, Insurance Companies, Pension Funds, Endowments and Foundations*, and *Mutual Funds* using the investor classification available in *ThomsonONE*. This reveals that most top-five institutional owners in our data are mutual funds and the correlation coefficient between *Institutional investor ownership concentration* and share ownership held by mutual funds is 0.98. The regression results and statistical inference are robust to the use of share ownership held by mutual funds as an alternative proxy for ownership concentration.

5.3 Discussion

This study reports evidence that peer group adjustments are used by (some) CEOs to extract rents from their organisations. The empirically rather weak effects of board independence in

rent extraction regressions in Table 6 raise interesting questions. In seeking to understand these results, it is important to be clear that this study does not control for multiple alternative ways of rent extraction that could be potentially open to abuse in weak boards. The lack of evidence on the moderating effect of board independence on rent extraction via a *particular* medium does not imply increased rent extraction in aggregate, and may in fact be consistent with lower aggregate extraction, where there is substitution between different forms of rent capture. To develop this line of reasoning, it may be the case that with relatively weak, compliant boards, CEOs are able to extract rent from the organisation without having to resort to practices such as peer group adjustment, which requires investment of time and careful judgement and which cannot, in any case, be guaranteed to deliver the intended outcomes. So, for example, to the extent that CEOs with weak boards are able to extract rent more straightforwardly via increased salaries or annual bonuses, one might expect that practices such as the addition of weak peers to performance groups would be confined to cases where board vigilance was robust in relation to the more obvious rent extraction measures.

Second, the analysis of year-on-year changes to the composition of bespoke peer groups in this study does not examine the (dynamic) self-selection of firms into those that use bespoke peer groups for benchmarking and those that use “index only” peer groups. This is mostly due to the data restrictions. There are ten firm-year cases in the data where firms stopped using index peer groups and started using bespoke peer groups (there are six cases where firms started using index peer groups in lieu of bespoke peer groups), which would not constitute a sample large enough for a formal statistical investigation.

6 CONCLUSION AND POLICY IMPLICATIONS

The results suggest that peer group modifications can be viewed, at least in part, as an expression of managerial power, rather than of the desire for more robust corporate governance. This research adds weight to those studies which suggest that an important factor in the detailed design of pay arrangements for senior executives is the ability of the package to deliver significant gains to the eligible executive(s). In practical terms, these results suggest the need for stronger justification by remuneration committees and greater vigilance on the part of boards and shareholders in relation to changes in peer group composition. It is, of course, the case that the anatomy of contemporary executive pay arrangements is far more complex than at any time in the past. This reflects a response to a regulatory steer towards more individual, bespoke forms of reward and in part, perhaps, a deliberate desire to obfuscate pay structures in order to frustrate scrutiny. Either way, increased complexity has meant that the responsibility of shareholders to monitor and moderate the detailed design of pay arrangements has become significantly more onerous. Our results suggest a need for more stringent regulation regarding the composition of peer groups, and subsequent modifications to peer group membership. Boards should consider carefully whether broader, non-bespoke comparator groups might be preferable in underpinning good governance, compared with the identified governance-diluting effect of changes in bespoke group composition.

Addressing such effects via modification of governance principles and codes is, of course, not straightforward, and companies are likely to view any such moves as an unwelcome and unwarranted intrusion into corporate affairs. It is, however, difficult to argue against the need for greater uniformity and transparency in disclosure requirements relating to all aspects of peer group use.

Further avenues for investigation could include the analysis of the narratives used by companies in validating peer groups and, in the absence of prescriptive reporting requirements, the probing of differences between companies with more and less liberal approaches to detailed disclosure of remuneration arrangements. Similarly, the comparison of peer group changes which are a function of deliberate action or, alternatively, a ‘natural’ occurrence would enrich our understanding of the degree of self-serving manipulation.

In conclusion, we contend that this study contributes to a richer understanding of the design and impact of alternative remuneration arrangements. The contribution lies principally in the empirical evidence on the self-serving adjustment of performance peer groups by executives, and lends force to the managerial power perspective on board governance at the expense of the arm’s-length contracting view favoured by agency theorists.

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Supporting Information

Two cases of peer group modifications: Amec Plc and Barclays Plc.

Amec Plc: Amec Plc. states in its 2010 annual report (p.52):

“The composition of the comparator group, which is reviewed at the time of each set of awards, has up to 2010 comprised only UK-listed companies in relevant FTSE subsectors. For 2011, the group has been amended to include a number of non-UK listed companies who are direct business competitors of AMEC and to exclude UK companies where there is little or no overlap in activities...”.

Overall, fifteen firms were deliberately removed (three firms were dropped due to delisting), and sixteen firms were added, to the peer group. The new, 2011, peer group comprised 27 firms (excluding Amec). TSR data, sourced through *Thomson Datastream*, was available for 25 peers and showed, on average, virtually zero (0.7%) log total shareholder return in the period from April 2011 (start of the performance period) to May 2013 (end of observation period). In contrast, the counterfactual (old, 2010) peer group's TSR was 24% over the same period (share price data available for 25 of 29 peers excluding Amec). The firms that were added to the peer group performed particularly poorly, showing a negative TSR of -17%, while the average TSR of the firms removed from the peer group was 21.9%. Further, the median peer in the counterfactual (old, 2010) peer group outperformed 21 of the 25 peers in the new peer group, whereas the median peer in the new peer group outperformed 4 of the 25 old peers only. The annual report explains the vesting rule as follows *“...the requirement for full vesting is for AMEC to be ranked in the top quartile of the chosen comparator group. No awards will vest if AMEC's performance is below median. If AMEC's performance is at the median, 25 per cent of the award will vest.”* The vesting rule means that performance at the **median** of the old peer group would have expected to earn Amec's CEO 25% of the performance share award if the peer group had not been changed. This partial vesting would

be valued at 21.9% of the CEO’s salary. The **same level** of TSR performance **after** the peer group changes were introduced should now trigger full, 100%, vesting. The full vesting would be valued at 87.5% of annual salary. These valuations are estimated based on Amec’s policy that performance share awards have “... a value at the time of award of up to 175 per cent of base salary” and take into account the fact that 50% of the award is TSR-related (and the other 50% is EPS-based). We also assume conservatively that the market value of Amec’s shares does not change over the course of the performance period. The difference between the partial and the full vesting is therefore equivalent to $87.5 - 21.9 = 65.6\%$ of the CEO’s annual salary - clearly, a significant increase in the level of expected award - is entirely due to the fact that the peer group introduced by the board in 2011 performed significantly weaker than the counterfactual peer group that the board used one year earlier and that could have been easily “recycled”.

Barclays Plc: In another example, a banking and financial services company Barclays Plc. introduced peer group changes in 2010, where four peers-banks were removed and four new peers-banks were added to the peer group. The 2010 annual report did not comment on what seemed to be significant changes to the composition of 11-strong peer group. Peer performance over the 2010-2012 performance period is summarized as follows:

$$TSR(Added_{2010,Barclays}) = -0.47, \quad TSR(Counterfactual P_{2010,Barclays}) = -0.21,$$

$TSR(Removed_{2010,Barclays}) = 0.03, \quad TSR(P_{2010,Barclays}) = -0.39$. This shows that banks removed from the peer group in 2010 ended up outperforming the rest of the peer group by about 40 percent in the generally tough market, whereas the banks added to the peer group performed particularly poorly.

Table 1. The list of firms used in the study

3i Group	Carnival	LAND SECURITIES	SABMiller
Admiral Group	Carphone Whse. GP	LEGAL & GENERAL	SEVERN TRENT
Alliance & Leicester	Centrica	LLOYDS BANKING GROUP	SHIRE
Amec	Compass Group	London Stock Ex.Group	SSE
Anglo American	Diageo	Lonmin	Sage Group
Antofagasta	Enterprise Inns	Man Group	Sainsbury (J)
Associated Brit. Foods	Experian	Marks & Spencer Group	Schroders
Astrazeneca	First Group	Morrison(WM)SPMT KS.	Smith &Nephew
Aviva	Friends Provident Group	National Grid	Smiths Group
BAE Systems	G4S	Next	Standard Chartered
BG Group	GlaxoSmithKline	Old Mutual	Standard Life
BHP Billiton	HBOS	Pearson	Taylor Wimpey
BP	HIBU	Persimmon	Tesco
BT Group	HSBC HDG.	Prudential	Thomas Cook Group
Barclays	Hammerson	RSA Insurance	Tui Travel
British Airways	Home Retail Group	Reckitt Benckiser	Tullow Oil
British American Tobacco	ICAP	Reed Elsevier	Unilever (UK)
British Energy Group	ICTL.HTLS.GP.	Rentokil Initial	United Utilities Group
British Land	ITV	Resolution	Vedanta Resources
British Sky Broadcasting Group	Imperial Tobacco GP.	Rexam	Vodafone Group
Cable & Wireless	International Power	Rio Tinto	WPP
Cadbury	Johnson Matthey	Rolls Royce	Whitbread
Cairn Energy	KELDA GROUP	Royal Bank of Scotland	Wolseley
Capita	KINGFISHER	Royal Dutch Shell	Xstrata
Capital Shopcts.Group	Kazakhmys		