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CEO Turnover and Relative Performance Evaluation*

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Abstract

This paper examines whether CEOs are fired after bad firm performance caused by factors beyond their control. Standard economic theory predicts that corporate boards filter out exogenous industry and market shocks to firm performance when deciding on CEO retention. Using a new hand-collected sample of 1,590 CEO turnovers from 1993 to 2001, we document that CEOs are significantly more likely to be dismissed from their jobs after bad industry and bad market performance. A decline in the industry component of firm performance from its 75th to its 25th percentile increases the probability of a forced CEO turnover by approximately 50 percent. This finding is robust to controls for firm-specific performance. The result is at odds with the prior empirical literature which showed that corporate boards filter exogenous shocks from CEO dismissal decisions in samples from the 1970s and 1980s. Our findings suggest that the standard CEO turnover model is too simple to capture the empirical relation between performance and forced CEO turnovers, and we evaluate several extensions to the standard model.

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The decision whether to retain or fire an incumbent CEO after bad stock price or accounting performance is one of the most important decisions made by corporate boards. Standard economic theory suggests that in assessing the quality of its CEO the board of directors should ignore components of firm performance which are caused by factors beyond the CEO's control. Previous studies that have examined the relation between (arguably exogenous) market or industry shocks and CEO turnover have found evidence largely consistent with this hypothesis. Using a larger data set of CEO dismissals over a more recent time period and an improved methodology, we find to the contrary that CEOs are significantly more likely to be fired after negative performance shocks to their peer group.

In a newly assembled data set of 1,206 voluntary and 384 forced CEO turnovers in 2,548 firms from 1993 to 2001, we document that low industry stock returns and low market returns significantly increase the likelihood of forced CEO turnovers. A decline in the industry component of firm performance from its 75th to its 25th percentile increases the probability of a forced CEO turnover by approximately 50 percent. There is some evidence that boards partially filter industry and market performance from their assessment of CEO quality, but the extent of this filtering is too limited to remove most of the peer performance effect. We conclude that boards fail to fully filter exogenous shocks to firm performance from their CEO retention decisions.

We document more effective filtering of more visible indicators of outside performance, such as the performance of the value-weighted market index, suggesting that boards may use some rule-of-thumb relative performance evaluation when assessing CEO quality. We also find that the effect of peer group performance on CEO dismissals is almost entirely concentrated on CEOs who underperform their peer group. CEOs who outperform their peer group are not affected by worsening industry or market performance. Finally, we document that firm-specific performance affects CEO dismissals most strongly when the peer group is not doing well. The evidence is consistent with the hypothesis that performance in recessions is more informative about CEO quality than performance in booms, and with the hypothesis that boards mistakenly credit or blame CEOs for performance caused by factors beyond their control.

Our empirical results contrast with the small prior literature on the relationship between peer group performance and CEO turnover. Warner, Watts, and Wruck (1988) examine CEO turnover from 1963 to 1978 and find that stock returns relative to the overall market are a better predictor of CEO dismissals than absolute performance. The results involving lagged returns are ambiguous, and they find no evidence that industry shocks are filtered from the CEO dismissal decision. Morck, Shleifer, and Vishny (1989) examine turnovers of entire top management teams between 1980 and 1985 and find such turnovers equally likely to occur in troubled and in healthy industries, suggesting that industry shocks are filtered from the dismissal decision. Barro and Barro (1990) find evidence similarly consistent with complete filtering of peer performance in a sample of CEO turnovers in commercial banks from 1982 to 1987. All three studies are based on small samples, with 43 forced CEO turnovers in Warner et al., 93 cases of internally precipitated turnover in Morck et al., and 51 bank CEO turnovers in Barro and Barro. Gibbons and Murphy (1990), on the other hand, examine a large sample of 1,000 CEO successions over the 1974 to 1986 period and find strong evidence that both market-wide shocks and industry shocks are filtered from stock price performance for the CEO dismissal decision. In summary, most of the evidence from previous studies supports the hypothesis that corporate boards filter industry and market shocks from firm performance when deciding whether to fire their CEO.[1]

Agency theory shows that there are benefits associated with evaluating agents on the basis of their relative performances whenever agents' performances are affected by common shocks (Holmström (1979, 1982), Diamond and Verrechia (1982)). Most of the theoretical literature modeling the CEO dismissal decision envisions a situation in which the corporate board learns from firm performance and other public and private signals about the quality of its current CEO.[2] If the board's assessment of CEO quality falls below some threshold, often equal to the expected quality of a replacement manager, then the board dismisses the CEO. Since CEO quality does not change as a function of the business cycle in these models, it follows directly that efficient boards do not force out more CEOs in down markets than in up markets. More generally, boards

[1] Warner, Watts, and Wruck (1988), Morck, Shleifer, and Vishny (1989), and Gibbons and Murphy (1990) do not formally test whether market or industry returns are completely filtered from firm performance for the management dismissal decisions. Barro and Barro (1990) do perform a formal test and find results consistent with complete filtering, with the caveat that their test imposes the assumption that firms have a stock return beta of one with respect to the benchmark return.

[2] See, for example, Hirshleifer and Thakor (1994, 1998), Hermalin and Weisbach (1998, 2003), Warther (1998), Goldman, Hazarika, and Shivdasani (2003), Adams and Ferreira (2005), and Hermalin (2005).

should filter all observable exogenous shocks from firm performance when updating their assessment of CEO quality. This prediction is strongly rejected by the empirical results we find, and we conclude that the simple framework used in much of the literature does not fully explain real-world CEO dismissal decisions.

We discuss several extensions and modifications to the basic CEO turnover model which might explain our empirical results. There is a large literature aimed at understanding the well-documented absence of relative performance evaluation in CEO compensation.[3] We first note that most of the explanations offered by this literature do not apply in the CEO turnover context.[4] We propose instead that the negative relation between the likelihood of forced CEO turnover and peer group performance could be caused (i) by CEOs' actions and skills affecting industry performance through strategic interactions in oligopolistic industries, (ii) by CEOs being dismissed for moving their firms into the wrong industries, (iii) by performance in recessions being more informative about CEO quality than performance in booms, (iv) by shareholders or boards who incorrectly blame the CEO for negative performance shocks beyond her control, and (v) by shareholders or boards whose limited attention is triggered by low absolute levels of performance.

We analyze the relation between peer group performance and forced CEO turnover in detail to assess which of the five hypotheses above are more likely to explain the empirical results. We find that the effect of industry performance on CEO turnover persists as we broaden the industry definition and as we restrict the sample to small firms. Since large industries are unlikely to be oligopolistic, and since small firms are unlikely to affect the product market equilibrium in their industries, these results speak against the hypothesis that our findings are driven by CEOs operating in oligopolistic settings. We show next that the effect of industry performance on CEO turnovers persists when we benchmark firm performance against industry competitors from several years ago, which speaks against the hypothesis that our findings are driven by CEOs being punished for moving their firms into the wrong industries. To test the hypothesis that

[3] For the empirical evidence see, among others, Murphy (1985), Coughlan and Schmidt (1985), Antle and Smith (1986), Gibbons and Murphy (1990), Janakiraman et al. (1992), Garen (1994), Aggarwal and Samwick (1999), Murphy (1999), and the review in Bebchuk, Fried, and Walker (2001).
[4] See the detailed discussion in Section I.C.

performance in recessions may be more informative about CEO quality than performance in booms, we interact firm-specific performance with indicators for low, medium, and high industry performance in the CEO turnover regressions. We find mostly confirmatory evidence showing that CEO turnover is indeed more sensitive to firm-specific performance when industry performance is low, but the result does not obtain in all specifications.

We test the hypothesis that boards mistakenly blame CEOs for exogenous performance shocks by noting that such mistakes should affect underperforming CEOs more strongly than CEOs who outperform their peer group. Even with systematic attribution errors, outperforming CEOs should only rarely be dismissed; they can always point out to their board that competitors are performing worse and induce the board to use relative performance evaluation. Underperforming CEOs, on the other hand, are more likely to be affected by attribution errors. Given that their performance is trailing the peer group, they are less able to mount a strong defense against incorrect performance attribution in recessions, but will be happy to hide behind good industry and market performance in booms. The empirical results strongly support the predicted asymmetry: the documented effect of peer performance on CEO turnover is shown to be almost entirely restricted to underperforming CEOs. This is consistent with boards committing attribution errors and failing to use relative performance evaluation unless prodded by the CEO.

Finally, we directly test the hypothesis that our results are caused by shareholders whose limited attention is triggered by low levels of performance. Under this hypothesis, efficient filtering of peer performance from the CEO dismissal decision should obtain after low absolute returns because investors have woken up and correctly assess CEO performance. It follows that the relative performance evaluation hypothesis should be confirmed for firms with very low levels of prior performance. Instead, we find that the effect of peer group performance on CEO turnover persists among firms with large negative prior returns. Furthermore, the relative performance evaluation hypothesis is also rejected for large and high-profile firms which are likely to be continuously monitored by the press and institutional investors. We conclude that limited investor attention is unlikely to be the main cause of the failure of the relative performance evaluation hypothesis.

While far from conclusive, the results of our tests are broadly consistent with the idea that performance in recessions is more revealing about CEO quality than performance in booms, and with the idea that boards mistakenly credit or blame CEOs for performance components beyond their control. We also find that boards tend to filter the performance of the value-weighted market and of the largest firms in the industry from firm performance when deciding on CEO retention. This suggests that boards may use some form of intuitive rule-of-thumb benchmarking against the most visible benchmarks, while failing to properly account for other exogenous performance components.[5] For further suggestive evidence on whether the observed turnover patterns are optimal or the symptom of a behavioral inefficiency, we examine the stock price reactions to CEO turnover announcements in our sample. We find no evidence of different stock price reactions in recessions compared to booms, and conclude that the market does not view the more frequent CEO dismissals in recessions as better or worse news than the less frequent dismissals in booms.

Independently of the underlying mechanism, the documented effect of industry and market performance on CEO turnover has interesting implications for our understanding of CEO incentives and horizons. Our findings are also important for the correct design of CEO turnover studies. The prior literature customarily assumes that CEOs are evaluated based on relative performance, and therefore regresses turnover on market- or industry-adjusted stock returns only. Given our evidence that CEO dismissals are in fact determined by both firm-specific performance and by industry and market performance, the regressions using only peer-adjusted performance suffer from omitted variable bias. It follows that future studies should include both firm-specific and peer group performance as explanatory variables.[6] An interesting question for future research is how the effect of peer group performance on CEO turnover we identify in this

[5] Fisman, Khurana, and Rhodes-Kropf (2005) propose a model in which shareholders misattribute *firm-specific* performance to the CEO rather than circumstance, and in which the board needs to decide whether to give in to shareholder demands to fire the CEO. Our results are consistent with the basic idea in Fisman et al., but suggest that shareholders may misattribute *peer group* performance to the CEO.

[6] A further frequent cause of misspecification is the implicit assumption that firm performance moves one-to-one with market or industry performance. Prior studies simply subtract index performance from firm performance, effectively imposing a beta of one on all firms. This assumption is often incorrect, as the sample betas in Panel A of Tables 2 and 5 demonstrate. The problem is more severe when industry and firm performance are measured using return-on-assets. In unreported results we found that the average firm in our sample has a return-on-assets beta of below 0.3 with its equal-weighted industry benchmark and a beta of below 0.15 with its value-weighted industry benchmark. Assuming a beta of one in these cases leads to severely biased inferences.

paper varies cross-sectionally with the determinants of the turnover-performance relationship documented in the prior literature.[7]

In contemporaneous work, Kaplan and Minton (2006) analyze both internal (board driven) and external (through takeovers and bankruptcy) CEO turnover in Fortune 500 firms from 1992 to 2005. Consistent with our results, they find internal CEO turnover to be significantly related to firm-specific performance, industry performance, and the performance of the overall market. Interestingly, they also find that external turnover is not significantly related to any of the three performance measures. Kaplan and Minton further document that the frequency of CEO turnover has increased from the first to the second half of their sample period, and that the sensitivity of internal CEO turnover to all three components of stock price performance is higher in the second half of their sample.

The next section reviews the theory behind relative performance evaluation in the CEO turnover context and derives the central testable hypothesis. Section I.B develops the empirical specification. Section I.C discusses a number of reasons why relative performance evaluation may not obtain in the CEO turnover context. Section II describes the construction of the CEO turnover sample, and Section III presents the main results on the effect of peer group performance on forced CEO turnover. Section IV examines the relationship between CEO dismissals and peer performance in more detail in an attempt to distinguish between different explanations for the observed regularities. The final section summarizes and concludes.

I. Theoretical background and hypotheses development

This section starts with an informal review of relative performance evaluation in the CEO turnover context. We use a simple model in which the board learns about CEO ability from firm performance to demonstrate that the optimal likelihood of CEO dismissals should be unrelated to industry and market performance. Section I.B restates this testable hypothesis in an instrumental variables (IV) framework in which market and industry performance act as instruments for firm

[7] The effect of firm performance on the likelihood of forced CEO turnovers has been shown to vary, among other factors, with CEO stock ownership (Salancik and Pfeffer, 1980, Denis et al., 1997), the presence of a blockholder (Denis et al. 1997), the composition of the board (Weisbach, 1988), and the availability of suitable outside candidates (Parrino, 1997).

performance. This reformulation forms the basis of the subsequent empirical analysis. Stating the relative performance evaluation hypothesis in an IV framework helps to illustrate the circumstances under which the hypothesis is likely to fail empirically. On this basis, Section I.C discusses several reasons why relative performance evaluation may not be observed in the CEO turnover context.

A. Relative performance evaluation and CEO turnover

The simple CEO turnover model sketched in this section is not meant to be an accurate description of the realities of CEO retention decisions, but illustrates the logic behind relative performance evaluation and delivers the central empirical predictions. Deviations from the simplifying assumptions of the model may render (complete) relative performance evaluation inefficient, as we discuss in Section I.C below. The derivation in this section relies heavily on Holmström (1982) and Gibbons and Murphy (1990).

Formally, let CEO *i*'s ability be given by α_i and the performance of firm *i* by y_i:

(1) $$y_i = \alpha_i + \varepsilon_i + \eta$$

Here ε_i is an idiosyncratic noise term affecting firm *i* only, and η is an unobserved shock common to all firms, of which there are *n* in the reference group. The board cannot observe CEO ability and tries to learn it from observed firm performance. The board believes that α_i is normally distributed with mean zero and variance σ_α^2. Suppose further that ε_i and η are normally distributed with mean zero and variances σ_ε^2 and σ_η^2, respectively. The common shock and the *n* CEOs' individual abilities and idiosyncratic shocks are independent.

The board of directors of firm *i* observes the performance of firm *i* and the performance of the *n-1* other firms in the reference group. Given the distributional assumptions made, the board uses the standard formula for the conditional expectation of a multivariate normal variable to calculate the optimal estimate of CEO ability:

(2) $$E[\alpha_i \mid y_1,\ldots,y_n] = \frac{\sigma_\alpha^2\left(\sigma_\alpha^2+\sigma_\varepsilon^2+(n-1)\sigma_\eta^2\right)}{\left(\sigma_\alpha^2+\sigma_\varepsilon^2\right)\left(\sigma_\alpha^2+\sigma_\varepsilon^2+n\sigma_\eta^2\right)}\left(y_i - \frac{\sigma_\eta^2}{\left(\sigma_\alpha^2+\sigma_\varepsilon^2+(n-1)\sigma_\eta^2\right)}\sum_{j\neq i}^{n-1} y_j\right)$$

The estimate of CEO *i*'s ability is positively related to the performance of firm *i*, and negatively to the average performance of the firms in the reference group. The crucial insight is that the optimal estimate of CEO ability in (2) completely purges the (noisily estimated) common shock from firm performance. To see this, note that the term in brackets is the residual from a population regression of firm *i*'s performance on the performance of all other firms in the industry:

$$(3) \qquad \begin{aligned} y_i &= \beta_0 + \left(\sum_{j \neq i}^{n} \beta_j \cdot y_j \right) + \tilde{v}_i \\ &= \left(\frac{\sigma_\eta^2}{\left(\sigma_\alpha^2 + \sigma_\varepsilon^2 + (n-1)\sigma_\eta^2\right)} \sum_{j \neq i}^{n-1} y_j \right) + \tilde{v}_i \end{aligned}$$

The performance residual $\tilde{v}_i$ in (3) is the purely firm-specific component of firm *i*'s total performance. Combining (2) and (3), the optimal estimate of CEO *i*'s ability can be rewritten as a function of this idiosyncratic performance component only:

$$(4) \qquad E[\alpha_i \mid y_1, ..., y_n] = k \cdot \left(y_i - \frac{\sigma_\eta^2}{\left(\sigma_\alpha^2 + \sigma_\varepsilon^2 + (n-1)\sigma_\eta^2\right)} \sum_{j \neq i}^{n-1} y_j \right) = k \cdot \tilde{v}_i$$

Here *k* is a constant given by the first term in (2) above. It follows directly that the optimal estimate of CEO *i*'s ability is uncorrelated with the average performance of the reference group. Said differently, the performance of the peer group has no predictive power for the ex-post assessment of CEO *i*'s ability. Good peer group performance does not make it any more or less likely that any individual CEO is assessed as having high or low ability than does bad peer group performance. Formally,

$$(5) \qquad Cov\left(E[\alpha_i \mid y_1, ..., y_n], \frac{1}{n-1} \sum_{j \neq i}^{n-1} y_j \right) = \frac{k}{n-1} Cov\left(\tilde{v}_i, \sum_{j \neq i}^{n-1} y_j \right) = 0.$$

In standard models of CEO turnover, CEOs are dismissed when the board's optimal estimate of CEO ability falls below some threshold $\underline{\alpha}$, which may be the expected ability of a replacement CEO.[8] Because the board completely filters the common shock from its assessment of CEO

[8] The ability threshold below which the current CEO is dismissed has to be adjusted for any costs of firing the CEO and for any costs of finding a suitable replacement. See, for example, Hirshleifer and Thakor (1994, 1998), Hermalin and Weisbach (1998, 2003), Warther (1998), Goldman et al. (2003), Adams and Ferreira (2005), and Hermalin (2005).

ability, it follows from (5) and the distributional assumptions made that the incidence of forced CEO turnover is uncorrelated with peer group performance:

$$(6) \qquad Cov\left(I\left[E\left[\alpha_i \mid y_1,...,y_n\right] < \underline{\alpha}\right], \frac{1}{n-1}\sum_{j\neq i}^{n-1} y_j \right) = \frac{1}{n-1} Cov\left(I\left[k \cdot \tilde{v}_i < \underline{\alpha}\right], \sum_{j\neq i}^{n-1} y_j \right) = 0.$$

Here I[.] is an indicator function that takes a value of one if the optimal estimate of CEO ability falls below the threshold level $\underline{\alpha}$. Equation (6) states that forced CEO turnover is uncorrelated with the performance of the reference group, and is the central testable implication of the simple theory of relative performance evaluation presented here. Assessing CEO competence in this model is a standard signal extraction problem, and imperfect performance filtering would imply inefficient inferences about CEO ability, and ultimately inefficient CEO dismissal decisions.

B. The empirical strategy

The main implication of the relative performance evaluation model presented in the previous section is that the agent is evaluated on the unsystematic component of her firm's performance only. Whether the reference group is booming or in a recession contains no information about CEO quality and has no predictive power for the likelihood of forced CEO turnovers. The prediction that peer group performance is *completely* filtered from the evaluation of the CEO has been termed the *strong-form relative performance evaluation hypothesis* in the prior literature.[9] The empirical strategy developed in this section to test for strong-form relative performance evaluation in CEO turnovers borrows heavily from Bertrand and Mullainathan (2001), Wolfers (2002), and Garvey and Milbourn (2004).

We estimate the sensitivity of CEO turnover to common performance factors using a two-stage regression approach: The first stage regression decomposes firm performance into a systematic component caused by peer group performance and a firm-specific component that should, in part, reflect CEO ability. In the second stage, we predict the probability of a forced CEO turnover using the peer group component and the residual component of firm performance

[9] See, for example, Janakiraman et al. (1992) and Albuquerque (2005). We discuss the corresponding *weak-form* relative performance evaluation hypothesis below.

estimated in the first stage.[10] This two-stage procedure is effectively an instrumental variables estimation, with peer group performance serving as an instrument for firm performance:

$$(7) \quad \text{(i)} \; r_{i,t-1} = \beta_0 + \beta_1 \cdot r_{peer\ group,t-1} + \nu_{i,t-1}$$

$$\text{(ii) Probability(CEO dismissal}_{i,t}) = \gamma_0 + \gamma_1 \cdot \left(\hat{\beta}_0 + \hat{\beta}_1 \cdot r_{peer\ group,t-1}\right) + \gamma_2 \cdot \hat{\nu}_{i,t-1} + \varsigma_{i,t}$$

$$= \gamma_0 + \gamma_1 \cdot \hat{r}_{i,t-1} + \gamma_2 \cdot \hat{\nu}_{i,t-1} + \varsigma_{i,t}$$

Here $\hat{r}_{i,t-1}$ is the estimated exogenous component of firm performance common to the peer group and not attributable to CEO actions or CEO quality, and $\hat{\nu}_{i,t-1}$ is the estimated firm-specific performance component. The prediction of strong-form relative performance evaluation is that the exogenous performance component does not affect CEO turnover, and hence $\gamma_1 = 0$. The interpretation of the γ_2 coefficient on firm-specific performance is more subtle since the residual variation in firm performance reflects in part CEO skill and in part unobserved shocks not related to industry or market performance.[11] Since firm-specific performance is partly driven by CEO skill, we expect that firm-specific performance is negatively related to the likelihood of CEO dismissal ($\gamma_2 < 0$).[12]

An important choice in the empirical design is whether to allow the sensitivity of firm performance to peer performance to differ across firms. Estimating firm-specific betas introduces

[10] Many variables other than firm and peer group performance affect the probability of a forced CEO turnover, with CEO tenure and board composition as obvious examples. Since none of these other determinants are likely to be correlated with peer group performance, they do not affect our test of relative performance evaluation.

[11] In Bertrand and Mullainathan's (2001) analysis of relative performance evaluation in CEO compensation the firm-specific performance residual is dropped from the second stage regression and a standard IV regression estimated. Wolfers (2002), on the other hand, includes the corresponding state-specific performance residual in his analysis of relative performance evaluation in gubernatorial election outcomes. In the linear regressions used in both papers, the inclusion or exclusion of the first stage residual has no effect on the estimated coefficient on the peer group component of performance: the residual is by construction uncorrelated with peer group performance and there is no omitted variable bias. In the binary choice models we use in our second stage regressions, the coefficient on peer group performance would be biased even if the omitted variable were uncorrelated with peer performance (Yatchew and Griliches (1985), Wooldridge (2002)), and we need to include the first stage residual in the second stage regressions.

[12] The difficulty in fully separating performance variation due to CEO skill from performance variation due to good or bad luck unrelated to industry or market shocks means that the estimated effect of firm-specific performance on CEO dismissals is a mix of the effect of skill and the effect of luck. Assuming that the true effect of luck does not exceed the true effect of skill, the coefficient on firm-specific performance is a downward biased estimate of the actual effect of CEO skill on forced CEO turnover.

estimation error into the peer performance term in the second stage regression, with two possible consequences: If the estimation error is simply noise, then the coefficient on peer performance in the CEO turnover regression is biased towards zero, making us more likely to accept the relative performance evaluation hypothesis. If, on the other hand, the estimation error introduces elements of firm-specific performance into the estimated peer performance term, then we may erroneously reject the relative performance evaluation hypothesis.[13] To avoid these problems, and to be consistent with the related literature, we estimate a common peer performance beta for all firms in the first stage regression. As a robustness check we repeat the estimations with firm-specific betas and obtain similar results.

The instrumental variables set-up clarifies the conditions under which we expect the predictions of strong-form relative performance evaluation to obtain in the data. The tests treat peer group performance as a plausibly exogenous instrument for the "luck" that has aided or hampered the CEO's running of the firm. For peer group performance to be a valid instrument it is required that (i) the instrument is exogenous and that (ii) the instrument does not have a direct effect on CEO dismissals independent from firm performance. Violations of these two assumptions correspond directly to the arguments against relative performance evaluation in CEO turnover which we discuss in the next section. Briefly, the exogeneity assumption could be violated because CEO skill or actions affect peer group performance, as may be the case in oligopolistic settings. The second assumption could be violated if times of high (or low) peer group performance were times in which boards receive more (or less) informative signals about their CEOs. For example, an industry downturn may test certain aspects of CEO skill which are otherwise unobservable by the board.

The two-stage regression approach in (7) above is not used by the prior literature which does not test for strong-form relative performance evaluation in CEO turnover. Prior studies instead test the so-called weak-form implication of the theory: the likelihood of CEO dismissals should be negatively related to firm performance, and positively to the performance of the reference group.

[13] We have simulated the two-stage estimation using actual firm stock returns and randomly generated peer group returns. The peer group returns are by construction unrelated to firm returns, which is reflected in average firm-specific betas close to zero in the first stage regressions. Using these firm-specific betas, we are able to erroneously reject the relative performance evaluation hypothesis at high levels of significance in the CEO turnover regressions if the firm-specific betas are estimated with too few data points.

Unlike strong-form relative performance evaluation, this weak-form hypothesis does not predict *complete* filtering of exogenous peer-group performance, and instead predicts only that *some* performance filtering is used by corporate boards. Following, among others, Gibbons and Murphy (1990) and Barro and Barro (1990), we test for weak-form relative performance evaluation using the following regression model:

(8) $$\text{Probability(CEO dismissal}_t) = \gamma'_0 + \gamma'_1 \cdot r_{peer\ group,t-1} + \gamma'_2 \cdot r_{i,t-1} + \nu_{i,t}$$

Weak-form relative performance evaluation predicts that CEO dismissals are negatively related to firm performance ($\gamma'_2 < 0$), holding peer performance constant, and positively related to peer performance ($\gamma'_1 > 0$), holding firm performance constant. Including both firm and peer group performance in the same single-stage regression produces coefficients which are hard to interpret. The estimated γ'_1 coefficient on peer performance is the product of the peer performance coefficient from the second stage regression 7(ii) and the sensitivity of firm performance to peer performance from the first stage regression 7(i) described above ($\gamma'_1 = \gamma_1 \cdot \beta_1$). Hence the estimated coefficient can be small either because there is no effect of peer group performance on CEO dismissals, or because firm performance is not sensitive to peer performance. The two-stage IV procedure circumvents this problem by effectively scaling the effect of "luck" on firm performance.[14]

C. Reasons for the absence of full relative performance evaluation in forced CEO turnover

The previous sections have made the case for relative performance evaluation in CEO retention decisions. In this section we discuss several reasons why peer group performance may not be, or at least not be fully, filtered from firm performance when boards decide whether to dismiss their CEOs. We review five hypotheses which posit that the simple agency model used to develop the relative performance evaluation predictions is not descriptively valid for CEOs, and which

[14] Some papers in the CEO compensation literature incorrectly test for strong-form relative performance evaluation by testing the restriction that γ'_1 and γ'_2 are of opposite sign and equal magnitude $(\gamma'_1 + \gamma'_2 = 0)$ in equation (8). The correct test for strong-form relative performance evaluation using equation (8) instead corresponds to testing a non-linear constraint of the form $\gamma'_1 / \gamma'_2 = -\beta_1$, where β_1 is the sensitivity of firm performance with respect to industry performance from equation (7(i)). See Janakiraman, Lambert, and Larcker (1992) for a paper implementing the correct test in the CEO compensation context.

instead predict that peer performance should affect the likelihood of forced CEO turnovers. The first three hypotheses interpret a peer performance effect on CEO dismissals as an efficient contracting outcome, while the last two hypotheses describe it as a behavioral inefficiency.

Hypothesis 1: CEOs in oligopolistic industries interact strategically

Linking CEO retention decisions to rival firm performance may serve shareholders by softening competition in industries subject to oligopolistic competition. Aggarwal and Samwick (1999) model optimal CEO compensation contracts in an environment with strategic interactions between imperfectly competitive firms.[15] They show that the optimal compensation contract may put positive weight on both own-firm and rival-firm performance, in contrast to the standard relative performance evaluation predictions. In the CEO turnover context, boards may dismiss CEOs for low industry performance if such performance is caused by CEO actions, for example because the CEO started a price war with competitors. We evaluate this hypothesis empirically in Section IV.A by testing whether the effect of industry performance on CEO turnover vanishes as the industry definition broadens (as broader industries are less likely to be oligopolistic) and whether the effect is weaker for small firms (which are less likely to affect the product market equilibrium in their industry).

Hypothesis 2: CEOs are fired for choosing the wrong industry

Optimal CEO evaluations may not filter peer group performance out because CEOs have at least some control over the peer group among which their firm operates. Dye (1992) has argued that relative performance evaluation motivates executives to invest in industries where they can outperform their competitors, rather than in industries that offer the highest absolute returns. The problem described by Dye can be solved by selecting the peer group benchmark for each CEO *before* any industry relocation choices are made. Practically speaking, firms should be benchmarked against their direct competitors from several years ago. Such a benchmark provides the CEO with efficient incentives: moving her firm into the industry with the highest expected returns allows her to outperform the competitors from her prior industry. We incorporate the Dye hypothesis into our empirical tests in Section IV.B by lagging industry affiliation by five years.

[15] The optimal incentive contracts for managers in oligopolistic settings have been previously analyzed by Sklivas (1987) and Fershtman and Judd (1987).

Hypothesis 3: Performance in recessions reveals more about CEO quality than performance in booms

Industry or market-wide recessions may allow boards to learn more about the quality of their CEO than booms, for example because recessions test aspects of CEO skill which are otherwise difficult to observe. On the simplest level, a recession tests whether a CEO has anticipated and properly prepared for the downturn, and such preparation is likely an important part of CEO performance. This hypothesis does not argue against relative performance evaluation, but simply posits that relative performance evaluation yields more informative signals in recessions. The testable implication of this hypothesis is that CEO turnover should be more sensitive to firm-specific performance in recessions than in booms. In Section IV.C we test this prediction by interacting firm-specific performance with indicators for low, medium, and high industry performance in the CEO turnover regressions.

Hypotheses 1 to 3 show that an effect of peer group performance on CEO dismissals may be the result of efficient and rational decision making by corporate boards. The next two hypotheses are behavioral and explain a peer performance effect on CEO turnover based on either systematic performance attribution errors or on limited investor attention.

Hypothesis 4: Corporate boards commit systematic attribution errors

Corporate boards and shareholders may make systematic mistakes in attributing performance and blame CEOs for bad performance caused by factors beyond their control. Social psychologists and economists studying attribution have found that subjects tend to take insufficient account of background and environmental factors, and as a result credit and blame individuals too much for observed outcomes.[16] Boards may therefore dismiss their CEO following bad performance even if the bad performance is (partly) caused by industry or market shocks. Bertrand and

[16] Systematic attribution errors have been documented in several contexts. Shea (1998) finds that the salaries of Major League baseball hitters (pitchers) are higher (lower) in more hitter-friendly home ballparks. Durell (2001) provides experimental evidence that employers underweight task difficulty when assessing the productivity of employees. Weber, Rottenstreich, Camerer, and Knez (2001) find that experimental subjects tend to underweight group size when assessing the ability of group leaders to inspire coordination outcomes. Wolfers (2002) shows that U.S. voters irrationally reward state governors for economic fluctuations that are likely unrelated to gubernatorial actions. For instance, governors in oil-producing states are more likely to be re-elected following a rise in oil prices, while governors in the rust-belt are more likely to be ousted.

Mullainathan (2001) offer the same argument in the CEO compensation context.[17] The effect of attribution errors on forced CEO turnovers is unlikely to affect all CEOs symmetrically. During recessions, CEOs who outperform their peer group are likely to bring that fact to the board's attention and to argue successfully against being punished for bad performance caused by outside factors. Underperforming CEOs, on the other hand, are less able to defend themselves against attribution errors in recessions, but are happy to hide behind high industry performance in booms. It follows that industry performance should have only weak effects on outperforming CEOs, but should strongly affect the likelihood of dismissal for underperforming CEOs. We test this hypothesis in Section IV.D by separately estimating the effect of peer performance on CEO turnover for under- and outperforming CEOs.

Hypothesis 5: Shareholder attention is triggered by low absolute performance

Shareholders may not be able to monitor all firms in their portfolios simultaneously, and may instead selectively direct their scrutiny to firms which have triggered their attention. Bertrand and Mullainathan (2001) propose that absolute performance measures like stock returns, accounting returns, or sales growth are easily observable and likely to function as attention triggers for otherwise passive investors. Once investors pay attention to a firm, we expect underperforming CEOs to be removed and outperforming CEOs to be retained. This hypothesis has three testable implications: First, larger and more high-profile firms which are likely to be continuously monitored by the press and institutional investors should show a smaller effect of peer performance on CEO turnovers. Second, efficient filtering of peer performance from the CEO dismissal decision should obtain after low absolute returns because investors have woken up and correctly assess CEO performance. It follows that peer group performance should not affect forced CEO turnovers for firms with very low levels of prior returns. Finally, and similar to Hypothesis 3 above, the effect of firm-specific performance on CEO turnover should be strongest when peer performance is low, as shareholders are more likely to pay attention. We test these implications in Sections IV.C and IV.E.[18]

[17] A related and observationally equivalent hypothesis is that hindsight bias leads boards to dismiss CEOs after a negative industry or market shock because boards incorrectly believe the CEO should have seen the shock coming and should have prepared for it. See Camerer and Malmendier (2004) for further discussion of this idea.

[18] Given that the board of directors of each firm monitors one firm only, it is fair to ask why corporate boards do not pay attention to relative CEO performance continuously and independently of absolute performance. A possible

To summarize, there are both rational and behavioral explanations for an effect of peer group performance on the likelihood of forced CEO turnover. Under the first three hypotheses discussed, a negative correlation between peer group performance and CEO dismissals is an efficient contracting outcome, while under the last two hypotheses it is a symptom of a behavioral inefficiency. We analyze the stock market reaction to turnover announcements for suggestive evidence of which view is closer to the truth in Section IV.F.[19]

II. Data sources, sample construction, and variable definitions

CEO turnover is observed for all firms in the Standard & Poors ExecuComp database for the time period 1993 to 2001. The ExecuComp sample contains information on the top executives of all firms in the S&P 500, S&P MidCap, and S&P SmallCap indexes. We recognize a CEO turnover for each year in which the CEO identified in ExecuComp changes. We then search the Factiva news database for the exact turnover announcement date and classify each CEO turnover according to whether the turnover was forced or voluntary.

The classification of turnovers into forced and voluntary follows Parrino (1997): all departures for which the press reports state that the CEO is fired, forced out, or retires or resigns due to policy differences or pressure, are classified as forced. All other departures for CEOs above and including age 60 are classified as not forced. All departures for CEOs below age 60 are reviewed further and classified as forced if either the article does not report the reason as death, poor health, or the acceptance of another position (including the chairmanship of the board), or the article reports that the CEO is retiring, but does not announce the retirement at least six months before the succession. Finally, the cases classified as forced can be reclassified as voluntary if the press reports convincingly explain the departure as due to previously undisclosed personal or

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explanation is that corporate boards are ineffective monitors and under CEOs' thumbs except when faced with attentive shareholders.

[19] The executive compensation literature provides a number of explanations for the absence of relative performance evaluation in CEO pay above and beyond the ones discussed in the text above. Examples are marginal products of CEO labor which rise and fall with industry fortunes (Barro and Barro (1990), Himmelberg and Hubbard (2000), Oyer (2004), and Rajgopal, Shevlin, and Zamora (2005)), the futility of indexing compensation when CEOs can trade the index (Core and Guay (2001), Jin (2002), Jenter (2002), Core, Guay and Verrecchia (2003), and Garvey and Milbourn (2003)), and the favorable accounting treatment of non-indexed options compared to indexed options and restricted stock (Core, Guay, and Larcker (2003) and Hall and Murphy (2003)). None of these explanations can explain our findings in the CEO turnover context.

business reasons that are unrelated to the firm's activities. This careful classification scheme is necessary since CEOs are rarely openly fired from their positions.[20] We separately identify CEO turnovers caused by mergers and spin-offs and exclude them from our subsequent analysis.

All accounting information comes from the Compustat Industrial Annual files, and all stock price and stock return information from the monthly CRSP tapes. Industry performance benchmarks are calculated as equal-weighted and value-weighted average stock returns for all firms available on CRSP in the same industry as the sample firm. Industries are defined using the Fama and French (1997) classification of firms into 48 industries. The corporate boards evaluating the CEOs in our sample inevitably have access to more precise measures of peer group performance than the market and industry benchmarks we employ. Using a less informative benchmark than the board in our tests biases us in favor of accepting the relative performance evaluation hypothesis. We exclude each sample firm from the construction of its specific peer group index to eliminate any artificial correlation between peer group performance and CEO turnover.

III. Empirical results

A. Descriptive statistics

Table 1 presents an overview of the new CEO turnover data set. The final sample has 2,548 firms with 15,798 firm-year observations from 1993 to 2001 and contains 1,590 CEO turnovers. Of these CEO turnovers, 1,206 are classified as voluntary and 384 are classified as forced.[21] Panel A shows the number of observations and the frequency of forced and voluntary CEO changes in the data. Panel B shows performance measures and firm characteristics by CEO retention outcome (CEO is retained, CEO leaves voluntarily, or CEO is dismissed). Firms in which the CEO is dismissed are smaller than firms with voluntary CEO turnover in terms of book assets, market value of equity, annual sales, and number of employees. Part of that difference is likely due to the fact that CEO dismissals are preceded by bad performance and associated declines in firm size. Average stock returns in the 12 months before a forced CEO

[20] Weisbach (1988), for example, analyzed 286 CEO changes over ten years and found only nine cases in which boards mentioned performance as an explicit reason why the CEO was replaced.

[21] The number of voluntary and forced CEO turnovers is larger than in any of the prior papers which hand-classify CEO turnovers into forced and voluntary. The data sets used in previous studies are usually based on the Forbes executive compensation surveys and are therefore restricted to the 800 largest firms. See Parrino (1997), Huson, Parrino, and Starks (2001), and Dezso (2005) for some of the most extensive data sets used in prior studies.

turnover are -17.90%. Interestingly, the average equal-weighted industry return is lower before forced (13.01%) than before voluntary turnovers (16.45%) or CEO retentions (19.17%). Both differences are statistically significant. This suggests that CEO dismissals are most common in industries which have performed badly and appears inconsistent with strong-form relative performance evaluation. The same pattern obtains in weaker form for value-weighted industry returns, but the differences are not statistically significant.

B. Testing for strong-form relative performance evaluation in CEO turnover

The strong-form version of the relative performance evaluation hypothesis predicts that peer group performance is completely filtered from the CEO retention decision. We estimate the sensitivity of CEO turnover to peer performance using the two-stage approach described in Section I.B, and use industry stock returns as measure of peer group performance in this section. The first stage regression partitions variation in firm performance into a predictable component caused by the industry shock and a residual firm-specific component. The second stage regresses an indicator variable for forced CEO turnovers on the predicted value (the peer performance component) and the residual (the firm-specific performance component) from the first stage regression. We include industry fixed effects in the second stage regression to account for industry differences in the likelihood of forced CEO turnover.

Table 2 presents the main result of this paper: When regressing forced CEO turnover on idiosyncratic firm performance and the component of firm performance predicted by industry performance, both idiosyncratic and predicted performance strongly predict CEO dismissals. Column (1) uses equal-weighted industry returns over the previous twelve months as instrument for firm performance. Instead of the expected coefficient of zero on predicted firm performance in the second stage regression, we find that the point estimate on predicted performance (-1.848, robust t-stat 7.78) is almost as large as the point estimate on idiosyncratic performance (-2.246, robust t-stat 9.65). This implies that bad industry performance increases the likelihood of CEO dismissals almost as much as bad firm-specific performance. Industry performance is clearly not fully filtered from the CEO retention decision, and strong-form relative performance evaluation is rejected.

Column (2) extends the performance measurement period and uses equal-weighted industry returns in year t-1 and year t-2 as instruments to predict company stock returns in two separate first stage regressions. The results from the second stage regression confirm that low stock returns caused by bad industry performance predict CEO dismissals almost as strongly as low firm-specific stock returns. The estimated coefficient on idiosyncratic and predicted stock returns for the previous year are -2.245 (t-stat 9.89) and -2.209 (t-stat 7.71) respectively, and are -0.632 (t-stat 5.23) and -1.056 (t-stat 4.56) for year t-2 respectively. Columns (3) and (4) repeat the analysis but replace equal-weighted industry returns by value-weighted industry returns. The negative coefficients on predicted performance in the second stage regression are smaller than before but remain large and statistically highly significant. The smaller effect of value-weighted industry performance on CEO turnover suggests that there may be better filtering of value-weighted than of equal-weighted peer performance from firm performance, a possibility we revisit below.

The effect of industry performance on the likelihood of CEO dismissals is economically large. Table 3 presents the implied likelihood of a forced CEO turnover calculated from the Logit models in columns (1) and (3) of Table 2 using stock returns over the previous year as the measure of performance. The average implied probability of forced CEO turnover in the base case (all independent variables left at their actual values) is 2.4% and equal to the unconditional probability of a forced turnover in the sample. The low frequency of CEO dismissals is consistent with the prior literature.[22] The average implied probability of a forced turnover increases to 3.25% (3.09%) when the component of firm performance attributable to equal-weighted (value-weighted) industry performance is set to its 25th percentile value. The average implied probability falls to 2.09% (2.13%) when the peer group component of performance is set to its 75th percentile value. Hence a decline in the peer group component of firm performance

[22] See, for example, Warner, Watts, and Wruck (1988), Parrino (1997), and Huson, Parrino, and Starks (2001) for forced turnover probabilities of a similar magnitude. Kaplan (1999) reports that the relationship between executive turnover and firm performance in Japan and Germany is similar to the one in the United States. The low implied probabilities of forced CEO turnover even for extreme bad performance may be partly due to the turnover regressions not having the right functional form. In particular, the true performance-turnover relationship is likely to be non-linear, with turnover most sensitive to performance when performance is below certain thresholds. We experiment with alternative functional forms in Section III.E and do find larger coefficients and larger implied turnover probabilities for underperforming CEOs.

from its 75th to its 25th percentile increases the average implied probability of a CEO dismissal by approximately 50%.[23]

We conclude that industry-wide shocks to stock returns are not fully filtered from firm performance in CEO retention decisions. The strong-form relative performance evaluation hypothesis appears to be rejected by the data. This still leaves the possibility that corporate boards at least partially filter industry shocks from firm performance when assessing their CEO, a hypothesis we test in the next section.

C. Testing for weak-form relative performance evaluation in CEO turnover

Most of the prior literature on CEO compensation and CEO turnovers does not test for strong-form relative performance evaluation, but tests a weaker implication of the theory: CEO dismissals should be negatively related to firm performance, holding industry performance constant, and positively related to industry performance, holding firm performance constant. Unlike strong-form relative performance evaluation, the weak form of the hypothesis does not predict complete filtering of peer performance, and instead posits only that some filtering of peer performance from firm performance is done by corporate boards.

Tests for weak-form relative performance evaluation do not use the two stage approach from the previous section, but instead simply regress forced CEO turnover on firm performance and peer performance. Table 4 provides some evidence consistent with partial filtering of industry shocks. Regressing forced CEO turnover on firm and industry performance over the previous year, firm performance comes in strongly negatively and industry performance comes in with the opposite sign, as predicted. The coefficients on industry performance are much smaller in absolute value (between 0.360 and 0.811) than the coefficients on firm performance (between -2.246 and -2.323) and have lower statistical significance, consistent with the previous result that industry shocks are far from fully filtered from CEO retention decisions.[24] The results become less

[23] An alternative method for calculating the implied probabilities is to set all independent variables to their respective means, rather than to their actual values, before varying the variable of interest. The advantage of the method used in Table 3 is that the implied probability of forced turnovers in the base case corresponds exactly to the empirical likelihood of forced CEO turnovers in the sample. Both methods deliver qualitatively similar results.

[24] For complete filtering of industry performance from firm performance in the CEO retention decision, the ratio of the coefficients on industry performance and firm performance in Table 4 has to equal the average industry beta of

consistent with weak-form relative performance evaluation when performance two years before the turnover decision is included in the regressions. The coefficient on industry performance in year t-2, predicted to be significantly positive, is insignificantly positive in the value-weighted industry specification and borderline significantly negative in the equal-weighted industry specification.

The findings in Table 4 support the notion that corporate boards do take at least some account of industry performance when assessing the performance of their CEO. Interestingly, the regressions using value-weighted industry returns as measure of peer performance (columns (3) and (4)) are more supportive of relative performance evaluation than the regressions using equal-weighted industry returns, suggesting again that boards may do a better job filtering value-weighted industry performance from firm performance. One explanation for these findings is that boards intuitively benchmark the performance of their CEOs against the largest and most visible firms in their industry, but fail to properly account for other, less salient components of industry performance.

D. Market returns as the measure of peer group performance

The previous sections show that firm performance attributable to industry performance affects the likelihood of forced CEO turnovers. We examine next whether market-wide stock returns have similar effects. We again decompose firm performance into its peer group and its firm-specific components using the two-stage regression approach described in Section I.B.

Table 5 presents the results with peer performance measured as equal-weighted (columns (1) and (2)) and value-weighted (columns (3) and (4)) stock market returns. The results using equal-weighted market returns are similar to the ones using industry returns: both the firm-specific and the market-induced performance components strongly affect the likelihood of CEO dismissals. The increase in the likelihood of a CEO dismissal caused by low market returns is of the same order of magnitude as the increase caused by low firm-specific returns. The rejection of the

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the sample firms. In the notation of equations (7) and (8), complete filtering implies that $-\gamma'_1 / \gamma'_2 = \beta_1$. Using the point estimates from column (1) in Tables 2 and 4, we have $-\gamma'_1 / \gamma'_2$ = 0.360/2.246 = 0.160 and β_1 = 0.905. Hence theory predicts five to six times more intense filtering than observed in the data.

strong-form relative performance evaluation hypothesis continues to hold when peer group performance is measured by equal-weighted market returns.

Interestingly, the same is not true when peer group performance is measured by value-weighted market returns. Value-weighted market returns have no effect on the likelihood of a forced CEO turnover, even though the first stage regressions show that value-weighted market returns significantly predict firm-level returns. It appears that corporate boards take the performance of value-weighted market indexes such as the S&P 500 into account when assessing the performance of their CEO. This hypothesis is confirmed in Table 5B which uses single-stage regressions to show that forced CEO turnover is significantly positively related to value-weighted (but not equal-weighted) market returns, holding firm stock returns constant. These findings reinforce the impression that boards may use rule-of-thumb relative performance evaluation against the most salient benchmarks, while ignoring less directly visible outside influences on firm performance.

E. Robustness tests

We have subjected the regression results from Tables 2, 4, and 5 to a variety of robustness checks. First, we have replaced the second stage Logit regressions by Multinomial Logit regressions allowing for three CEO turnover outcomes: retention, voluntary turnover, and forced turnover. Table 6 presents the results from a two-stage Multinomial Logit model using stock returns over the previous year as measure of firm performance and industry returns as measure of peer performance. The coefficient estimates for forced CEO turnovers are remarkably similar to before, with both idiosyncratic and peer performance strongly predicting CEO dismissals. Unsurprisingly, the effects of the two performance components on voluntary CEO turnover are much weaker in their economic and their statistical significance. The results including stock returns for year t-2 and the results using market returns as measure of peer performance are similar and are omitted to conserve on space.

The second robustness test re-estimates all regressions allowing for firm-specific peer performance sensitivities in the first stage regressions. Estimating firm-specific betas introduces estimation errors into the peer performance term in the CEO turnover regressions, as discussed in

detail in Section 1.B. We do not allow for firm-specific intercept terms in the first stage regressions; doing so would attribute the average firm performance over the sample period to the "luck" component of performance and bias the tests towards rejecting the relative performance evaluation hypothesis.[25] Table 7 reports summary statistics for the estimated firm-specific industry betas in Panel A and the results from the CEO turnover regressions using industries as peer group in Panel B. The peer performance effect on CEO dismissals remains economically and statistically highly significant, though smaller than in the base regressions in Table 2.

In a further robustness test, we allow for a non-linear performance-turnover relationship. The assumption that firm performance enters linearly into the turnover regressions is common to the CEO turnover literature but is likely incorrect. For example, a moderate decrease in performance is likely to have a stronger effect on the likelihood of a CEO dismissal when performance is already low than when it is high. The correct functional form for the relationship between performance and forced CEO turnover is unknown and likely varies across firms. A reasonable assumption is that the performance-turnover relationship differs between CEOs who underperform and CEOs who outperform their peer groups, effectively allowing for a break-point in the idiosyncratic performance-turnover relationship at zero. The results in Table 8 confirm that the effect of firm-specific performance on CEO dismissals is much stronger for firms with negative firm-specific performance, i.e. firms which underperform their industry.[26] More importantly for our purposes, the estimated effect of peer group performance on CEO dismissals, and hence the rejection of the relative performance evaluation hypothesis, is strengthened in Table 8.

In additional unreported robustness checks, we vary the time period over which firm and peer group performance are measured before the turnover decision. We replace the robust standard errors in the second stage regressions with bootstrapped standard errors to correct for any biases caused by the inclusion of generated regressors. We drop the industry fixed effects from the

[25] In the CEO compensation context, Garvey and Milbourn (2004) do not include firm fixed effects in the first stage regressions based on the same argument. As expected, including firm fixed effects in our regressions strengthens the rejection of the relative performance evaluation hypothesis.

[26] Goldman, Hazarika, and Shivdasani (2003) and Fisman, Khurana, and Rhodes-Kropf (2005) similarly argue for break-points in the performance-turnover relationship at zero, but define the break-point in terms of absolute levels of performance rather than relative or idiosyncratic performance. Both papers find evidence consistent with a stronger performance-turnover relationship at low levels of performance.

second stage turnover regressions, thereby allowing average industry performance to affect the peer performance term. None of these modifications changes the conclusion that both firm-specific and peer group performance strongly affect the likelihood of CEO dismissals.[27]

IV. A more detailed examination of the peer performance effect on CEO dismissals

The results so far demonstrate that peer group performance in the form of industry and market returns is not fully filtered from CEO dismissal decisions. Instead, bad industry performance increases the likelihood of a CEO dismissal by almost as much as bad firm-specific performance. We have discussed a number of potential causes for an industry performance effect on forced CEO turnovers in Section I.C. In order to shed more light on why CEOs are more likely to be laid off when their peer group is not doing well, this section examines the relationship between CEO dismissals and peer group performance in more detail.

[27] The additional robustness tests are available from the authors upon request.

A. The oligopolistic industry hypothesis

Linking CEO retention decisions to rival firm performance may serve shareholders by softening competition in oligopolistic industries. Boards may dismiss CEOs for low industry performance if such performance is caused by CEO actions, for example because the CEO started a price war with competitors. This strategic interaction hypothesis predicts that the effect of industry performance on CEO turnover should vanish as the industry definition broadens (as larger industries are less likely to be oligopolistic), and that the effect should be weaker for small firms (which are less likely to affect the product market equilibrium in their industry). The evidence presented so far speaks against the strategic interaction hypothesis: The 48 Fama and French (1997) industries used in our analysis are quite broad, with the majority of industries having more than one hundred publicly listed firms on CRSP at any point during the sample period. Furthermore, we saw in Table 5 that the peer group effect on forced CEO turnover persists when peer performance is measured as equal-weighted market returns, a finding that is hard to reconcile with the strategic interaction hypothesis.

Table 9 tests whether the effect of industry performance on CEO dismissals vanishes for firms which are small relative to their industry and therefore unlikely to affect the product market equilibrium. Small firms are identified as firms with equity market values below 1 percent of the total market value of all firms in the same industry found on CRSP (columns 1 and 2), or as firms with book assets below 1 percent of the total book assets of all firms in the same industry found on Compustat (columns 3 and 4). Independently of the exact definition of small firms, the results in Table 9 show that the industry component of firm performance continues to have a statistically and economically large effect on the likelihood of a forced CEO turnover. We conclude that the strategic interaction hypothesis is unlikely to explain the documented peer performance effects on forced CEO turnover.

B. The endogenous industry choice hypothesis

Dye (1992) argues that relative performance evaluation motivates executives to invest in industries where they can outperform their competitors, rather than in industries that offer the highest absolute returns. Efficient CEO evaluations may therefore not filter industry performance out if CEOs have (some) control over which industry their firm operates in. There is a simple fix

for the problem described by Dye: select the peer group benchmark for each CEO before any industry relocation choices are made. Practically speaking, CEOs should be benchmarked against their competitors from several years ago to provide them with efficient incentives to move their firms into the best performing industries. We test the Dye hypothesis in Table 10 by re-running the base regressions from Table 2 with the industry affiliation lagged by five years, effectively comparing the current performance of each firm to the current performance of its competitors from five years ago. The results are almost unchanged from Table 2 and show a statistically and economically large effect of peer group performance on CEO dismissals. We conclude that the endogenous industry choice hypothesis is unlikely to explain the main result of this paper.

C. Variation in the informativeness of performance between recessions and booms

Industry or market-wide recessions may allow boards to learn more from firm performance about the quality of their CEO than booms, for example because recessions test aspects of CEO skill which are otherwise difficult to observe. The testable implication of this hypothesis is that CEO turnover should be more sensitive to firm-specific performance in recessions than in booms. We test this prediction in Table 11 by interacting firm-specific performance with indicators for low, medium, and high industry performance in the CEO turnover regressions. The results are supportive of the hypothesis tested when firm and peer group performance are measured over year t-1 before the turnover decision: the effect of firm-specific performance on CEO turnover is smallest when industry performance is high, and is largest when industry performance is low. The difference in slope coefficients is statistically significant at the 10% level in the equal-weighted industry specifications and at the 1% level in the value-weighted industry specifications. The hypothesis is not supported by firm-specific performance in year t-2 which yields slope coefficients which are not significantly different from each other at different levels of industry performance. The results using equal-weighted market returns as measure of peer performance are again supportive of the hypothesis tested, and show an effect of firm-specific performance on CEO dismissals that is largest at low levels of market performance.[28]

[28] Value-weighted market returns have again no effect on CEO dismissals, as already documented in Table 5. The regression results using market returns as measure of peer performance are omitted to conserve on space and are available from the authors.

D. The systematic attribution error hypothesis

Corporate boards and shareholders may make systematic mistakes in attributing performance, and credit or blame CEOs for performance caused by factors beyond their control. Such systematic attribution errors are unlikely to affect all CEOs symmetrically. CEOs who outperform their peer group are able to bring that fact to the board's attention and are unlikely to be dismissed during a downturn. CEOs who underperform their peer group, on the other hand, are less able to defend themselves against attribution errors in downturns, but are happy to hide behind good industry performance in booms. The attribution errors hypothesis therefore predicts that industry performance should have only weak effects on outperforming CEOs, but should strongly affect the likelihood of dismissal for underperforming CEOs.

Table 12 tests this hypothesis by estimating whether the sensitivity of CEO turnovers to peer group performance depends on whether a CEO underperforms or outperforms her benchmark. We find that the effect of industry-induced performance on CEO dismissals previously documented is almost entirely restricted to firms which underperform their benchmarks. There is almost no effect of peer group performance on the likelihood of a dismissal for CEOs who outperform their industries. Similar results obtain when peer performance is measured as equal-weighted market returns (unreported). These findings are consistent with the idea that boards commit systematic attribution errors when evaluating their CEOs, and that these attribution errors mainly affect the dismissal likelihood for underperforming CEOs.[29]

E. The limited investor attention hypothesis

According to the limited investor attention hypothesis, otherwise passive shareholders scrutinize firms more closely during bad times because shareholder attention has been triggered by low stock returns or other easily observable variables. This hypothesis is consistent with the result in the previous section that the peer performance effect on CEO dismissals is strongest for CEOs who underperform at the same time. In unreported results, we repeat the base regressions from Tables 2 and 5 for firms in the S&P 500 index only. Since these firms are likely to be continuously monitored by the press and professional investors, we would expect that CEO

[29] The result is also consistent with the hypothesis from the previous section that firm-specific performance is more informative about CEO quality in recessions than in booms.

dismissals are less sensitive to peer group performance. The results are virtually unchanged from Tables 2 and 5 and from the results for small firms in Table 9, which we interpret as evidence against the limited attention hypothesis.

In this section we perform a more direct test of the limited attention hypothesis: If investors start paying attention to CEO performance after low absolute returns, then we should expect efficient filtering of peer group performance from the CEO dismissal decision after low returns. We therefore sort our sample by the level of prior returns, and test whether the industry component of firm performance continues to affect CEO turnovers even in firms with very bad prior performance. Table 13 repeats the base regressions from Table 2, but restricts the second stage CEO turnover regressions to firm-years with negative returns (columns (1) and (3)) and firm-years with returns below -20% (columns (2) and (4)). The estimated coefficients on the industry-component of firm performance are as large and significant as the ones estimated for the full sample in Table 2. We conclude that even for firms with prior stock returns below -20%, worse industry performance increases the likelihood of a forced CEO turnover. These results are further evidence against the limited attention hypothesis as an explanation for the role of peer performance in CEO dismissals.

F. Stock price reactions to forced CEO turnovers across the industry business cycle

In this section we measure the stock market reaction to the announcement of a forced CEO turnover. In particular, we are interested in whether the stock market views the more frequent CEO dismissals in industry recessions as better or worse news than the less frequent dismissals in industry booms. A crucial caveat to this analysis is that the stock price reaction measures only new information released on the announcement date, and conflates the market reaction to the dismissal with the reaction to any other news revealed about the firm at the announcement. For example, the market may react negatively to a CEO dismissal not because the market views the dismissal as a bad decision, but because the market was not fully aware of how bad the CEO's performance had been.[30]

[30] Hermalin and Weisbach (1998) argue that the stock price reaction to CEO turnovers should be negative if the CEO is fired on the basis of private information of the board, and positive if the CEO is fired on the basis of public information.

Table 14 measures the market-adjusted stock price reaction to forced CEO turnovers using three and five trading day windows around the announcement date. The average stock price reaction in this sample is negative and statistically significant. Comparing dismissals of CEOs who outperformed their industries to dismissals of underperforming CEOs, we find that the market reaction appears to be more negative for CEOs who outperform, even though the difference is not statistically significant. The more negative reaction to the dismissal of outperforming CEOs may be due to the fact that such dismissals are much less common and hence more surprising. Most interesting from our perspective, Table 14 shows that the stock market reaction to CEO dismissals does not depend on whether the CEO turnover occurs in an industry recession or an industry boom. All differences between the announcement returns across the business cycle have t-statistics below one. There is no evidence that the market views the more frequent CEO dismissals in industry recessions as different from the less frequent dismissals in industry booms.

V. Conclusion

Using a new hand-collected CEO turnover data set from 1993 to 2001, we document that low industry stock returns and low market returns significantly increase the likelihood of forced CEO turnovers. The increase in the likelihood of a forced turnover following bad peer group performance is concentrated on CEOs who underperform their peer group at the same time. We find some evidence that boards partially filter industry performance from their assessment of CEO quality, but the extent of the filtering is too limited to remove most of the peer performance effect. We conclude that boards fail to fully filter what appear to be exogenous shocks to firm performance from their CEO retention decisions.

We consider several explanations for the failure of relative performance evaluation in our CEO turnover sample. While far from conclusive, our results are consistent with the hypothesis that corporate boards commit systematic attribution errors and credit or blame CEOs for performance caused by factors beyond their control. There appears to be more filtering of more salient measures of peer performance, such as the returns to the value-weighted market index, suggesting that boards may use some imperfect rule-of-thumb relative performance evaluation when assessing CEO quality. We emphasize that our results are also consistent with the hypothesis that firm performance in recessions is more revealing about CEO skill than firm

performance in booms, leaving the possibility that the observed patterns reflect efficient learning about CEO quality by corporate boards. Independently of the underlying mechanism, the documented effect of industry and market performance on CEO turnover has interesting implications for our understanding of CEO incentives and horizons.

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Table 1
Summary statistics

This table presents an overview of the new CEO turnover data set. Panel A shows the number of observations and the frequency of forced and voluntary CEO turnovers in the sample. Panel B shows performance measures and firm characteristics by CEO retention outcome.

Panel A: Frequency of voluntary and forced CEO turnovers

Number of firm-years	Number of forced CEO turnovers	Number of voluntary CEO turnovers	Average percentage of firms with at least one CEO turnover in a year	Average percentage of firms with at least one forced CEO turnover in a year	Average percentage of firms with at least one voluntary CEO turnover in a year
15,798	384	1,206	9.47%	2.36%	7.47%

Panel B: Firm characteristics, firm performance, and industry performance by CEO turnover outcome

	CEO is retained	Voluntary CEO turnover	CEO is dismissed
	Firm characteristics		
Book assets ($m)	7,703	9,427	5,196
Market value of equity ($m)	4,745	6,550	3,318
Sales ($m)	3,345	4,478	3,268
Number of employees	16,061	20,565	16,127
	Firm and industry performance		
Stock return in the 12 months before the CEO turnover	29.56%	12.63%	-17.90%
[S.E.]	[0.65]	[1.77]	[2.30]
EW industry stock return in the 12 months before the CEO turnover [S.E.]	19.17%	16.45%	13.01%
	[0.25]	[0.89]	[1.50]
VW industry stock return in the 12 months before the CEO turnover [S.E.]	18.33%	17.50%	16.65%
	[0.20]	[0.75]	[1.37]

Table 2

Two-stage Logit regressions of forced CEO turnover on firm and industry performance

The first stage regressions use industry stock returns to predict contemporaneous company stock returns. The second stage Logit regressions predict forced CEO turnover using the predicted values and the residuals from the first stage regression as measures of the predicted (peer-group) and of the idiosyncratic component of company stock returns respectively. The industry definitions follow the Fama and French (1997) classification into 48 industries. The second stage Logit regressions include industry fixed effects. All t- and z-statistics are calculated with robust standard errors clustered at the industry level.

Panel A: First stage regressions of firm performance on industry performance

	(1)	(2)	(3)	(4)
	Firm stock return in year t-1	Firm stock return in year t-1	Firm stock return in year t-1	Firm stock return in year t-1
Constant	0.096	0.096	0.076	0.076
	[5.06]***	[5.06]***	[3.97]***	[3.97]***
EW industry stock return in year t-1	0.905	0.905		
	[11.30]***	[11.30]***		
VW industry stock return in year t-1			1.036	1.036
			[9.37]***	[9.37]***
R-squared	0.12	0.12	0.10	0.10
		Firm stock return in year t-2		Firm stock return in year t-2
Constant		0.097		0.063
		[5.78]***		[3.72]***
EW industry stock return in year t-2		0.884		
		[15.12]***		
VW industry stock return in year t-2				1.089
				[12.00]***
R-squared		0.12		0.11

Panel B: Second stage Logit regressions of CEO dismissals on peer-group induced and idiosyncratic firm performance

	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover
Constant	-19.536	-19.276	-19.637	-19.519
	[301.10]***	[232.92]***	[211.40]***	[118.82]***
Idiosyncratic stock return in year t-1	-2.246	-2.246	-2.323	-2.298
	[9.65]***	[9.90]***	[10.11]***	[9.52]***
Industry-induced stock return in year t-1	**-1.848**	**-2.210**	**-1.540**	**-1.623**
	[7.78]* **	**[7.73]*** **	**[6.44]*** **	**[6.43]*** **
Idiosyncratic stock return in year t-2		-0.633		-0.683
		[5.26]***		[5.90]***
Industry-induced stock return in year t-2		**-1.058**		**-0.495**
		[4.56]* **		**[2.37]** **
Industry fixed effects	Yes	Yes	Yes	Yes

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3
Implied probabilities of a forced CEO turnover

The implied probabilities of a forced CEO turnover are calculated using the second stage regression coefficients from columns (1) and (3) of Table 2. For the base case implied probability, all independent variables are set equal to their actual values in the data and the associated implied probabilities are averaged across all observations. This implied probability equals the observed likelihood of forced CEO turnovers in the data. The implied probability is varied by setting either the idiosyncratic or the predicted component of firm performance equal to their 25th or 75th percentile values. The other independent variables remain at their actual values.

Implied probabilities of a forced CEO turnover for different levels of the independent variables		
	Peer group performance measured as EW industry returns over the previous year	Peer group performance measured as VW industry returns over the previous year
	Implied likelihood of a forced CEO turnover	
Base Case	2.40%	2.40%
Peer-group induced stock return set to 25th percentile	**3.25%**	**3.09%**
Peer-group induced stock return set to 75th percentile	**2.09%**	**2.13%**
Idiosyncratic stock return set to 25th percentile	3.03%	2.92%
Idiosyncratic stock return set to 75th percentile	1.06%	1.02%

Table 4

Single-stage Logit regressions of forced CEO turnover on firm and industry performance

The single-stage Logit regressions regress forced CEO turnover on company and industry stock returns. The industry definitions follow the Fama and French (1997) classification into 48 industries, and the regressions include industry fixed effects. All z-statistics are calculated with robust standard errors clustered at the industry level.

Logit regressions of CEO dismissals on firm performance and industry performance				
	(1)	(2)	(3)	(4)
	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover
Constant	-19.498	-19.314	-19.577	-19.456
	[1082.46]***	[516.66]***	[240.51]***	[133.75]***
Firm stock return in year t-1	-2.246	-2.246	-2.323	-2.298
	[9.65]***	[9.90]***	[10.11]***	[9.52]***
EW industry stock return in year t-1	**0.360**	**0.033**		
	[2.30]**	**[0.18]**		
VW industry stock return in year t-1			**0.811**	**0.699**
			[4.08]***	**[3.80]*****
Firm stock return in year t-2		-0.633		-0.683
		[5.26]***		[5.90]***
EW industry stock return in year t-2		**-0.376**		
		[1.57]		
VW industry stock return in year t-2				**0.205**
				[0.87]
Industry fixed effects	Yes	Yes	Yes	Yes

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 5
Two-stage Logit regressions of forced CEO turnover on firm and stock market performance

The first stage regressions use market-wide stock returns to predict contemporaneous company stock returns. The second stage Logit regressions predict forced CEO turnover using the predicted values and the residuals from the first stage regression as measures of the predicted (peer-group) and of the idiosyncratic component of company stock returns respectively. The second stage Logit regressions include industry fixed effects using the Fama and French (1997) classification into 48 industries. All t- and z-statistics are calculated with robust standard errors clustered at the industry level.

Panel A: First stage regressions of firm performance on market performance

	(1)	(2)	(3)	(4)
	Firm stock return in year t-1	Firm stock return in year t-1	Firm stock return in year t-1	Firm stock return in year t-1
Constant	0.128	0.128	0.133	0.133
	[9.15]***	[9.15]***	[7.54]***	[7.54]***
EW market return in year t-1	0.732	0.732		
	[5.55]***	[5.55]***		
VW market return in year t-1			0.766	0.766
			[5.32]***	[5.32]***
R-squared	0.03	0.03	0.02	0.02
		Firm stock return in year t-2		Firm stock return in year t-2
Constant		0.111		0.047
		[6.48]***		[2.87]***
EW market return in year t-2		0.792		
		[8.42]***		
VW market return in year t-2				1.162
				[10.64]***
R-squared		0.04		0.04

Panel B: Second stage Logit regressions of CEO dismissals on peer-group induced and idiosyncratic firm performance

	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover
Constant	-19.593	-18.808	-20.005	-19.981
	[172.84]***	[57.94]***	[125.57]***	[101.93]***
Idiosyncratic stock return in year t-1	-2.190	-2.180	-2.299	-2.262
	[9.37]***	[9.64]***	[9.91]***	[9.45]***
Market-induced stock return in year t-1	**-1.700**	**-3.062**	**-0.214**	**-0.368**
	[4.41]* **	**[5.60]*** **	**[0.39]**	**[0.71]**
Idiosyncratic stock return in year t-2		-0.657		-0.698
		[5.89]***		[6.19]***
Market-induced stock return in year t-2		**-1.954**		**0.031**
		[3.37]* **		**[0.10]**
Industry fixed effects	Yes	Yes	Yes	Yes

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 5B
Single-stage Logit regressions of forced CEO turnover on firm and stock market performance

The single-stage Logit regressions regress forced CEO turnover on company stock returns and market-wide returns. The industry definitions follow the Fama and French (1997) classification into 48 industries, and the regressions include industry fixed effects. All z-statistics are calculated with robust standard errors clustered at the industry level.

Logit regressions of CEO dismissals on firm performance and market performance				
	(1)	(2)	(3)	(4)
	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover
Constant	-19.53	-19.065	-19.728	-19.695
	[217.53]***	[97.08]***	[244.99]***	[189.52]***
Firm stock return in year t-1	-2.190	-2.180	-2.299	-2.262
	[9.37]***	[9.64]***	[9.91]***	[9.45]***
EW market return in year t-1	**0.358**	**-0.646**		
	[1.21]	**[1.53]**		
VW market return in year t-1			**1.597**	**1.451**
			[3.51]* **	**[3.32]*** **
Firm stock return in year t-2		-0.657		-0.698
		[5.89]***		[6.19]***
EW market return in year t-2		**-1.027**		
		[2.18] **		
VW market return in year t-2				**0.847**
				[2.45] **
Industry fixed effects	Yes	Yes	Yes	Yes

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 6
Two-stage Multinomial Logit regressions of voluntary and forced CEO turnover on firm and industry performance

The first stage regressions use industry stock returns to predict contemporaneous company stock returns. The second stage Multinomial Logit regressions predict voluntary and forced CEO turnover using the predicted values and the residuals from the first stage regression as measures of the predicted (peer-group) and of the idiosyncratic component of company stock returns respectively. The industry definitions follow the Fama and French (1997) classification into 48 industries. The second stage Logit regressions include industry fixed effects. All t- and z-statistics are calculated with robust standard errors clustered at the industry level.

Panel A: First stage regressions of firm performance on industry performance

	(1)	(2)
	Firm stock return in year t-1	Firm stock return in year t-1
Constant	0.096	0.076
	[5.06]***	[3.97]***
EW industry stock return in year t-1	0.905	
	[11.30]***	
VW industry stock return in year t-1		1.036
		[9.37]***
R-squared	0.12	0.10

Panel B: Second stage Multinomial Logit regressions of voluntary and forced CEO turnover on peer-group induced and idiosyncratic firm performance

	(1)		(2)	
	Voluntary CEO turnover	Forced CEO turnover	Voluntary CEO turnover	Forced CEO turnover
Constant	-1.644	-26.433	-1.700	-26.541
	[50.59]***	[420.12]***	[33.45]***	[291.50]***
Idiosyncratic stock return in year t-1	-0.470	-2.289	-0.509	-2.370
	[4.88]***	[9.74]***	[5.19]***	[10.20]***
Industry-induced stock return in year t-1	**-0.440**	**-1.888**	**-0.217**	**-1.557**
	[2.18]**	**[7.63]*****	**[0.93]**	**[6.33]*****
Industry fixed effects	Yes	Yes	Yes	Yes

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 7
Two-stage Logit regressions of forced CEO turnover on firm and industry performance using firm-specific beta estimates

The first stage regressions use industry stock returns to predict contemporaneous company stock returns and allow for variation in the industry sensitivities of monthly firm-level log stock returns. The second stage Logit regressions predict forced CEO turnover using the cumulated predicted values and residuals from the first stage regression as measures of the predicted (peer-group) and of the idiosyncratic component of company stock returns respectively. The industry definitions follow the Fama and French (1997) classification into 48 industries. The second stage Logit regressions include industry fixed effects. All t- and z-statistics are calculated with robust standard errors clustered at the industry level.

Panel A: Firm-specific beta estimates from first stage regressions of firm performance on industry performance

	Monthly firm log stock return on EW industry performance	Monthly firm log stock return on VW industry performance
Average beta estimate	0.881	0.893
Median beta estimate	0.845	0.875
1st percentile beta estimate	0.032	0.047
99th percentile beta estimate	2.062	2.120

Panel B: Second stage Logit regressions of CEO dismissals on peer-group induced and idiosyncratic firm performance

	(1)	(2)	(3)	(4)
	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover
Constant	-19.378	-19.186	-19.342	-19.183
	[155.77]***	[130.38]***	[185.04]***	[489.92]***
Idiosyncratic stock return in year t-1	-1.723	-1.645	-1.69	-1.624
	[14.60]***	[14.72]***	[17.24]***	[16.49]***
Industry-induced stock return in year t-1	**-0.934**	**-1.122**	**-0.711**	**-0.894**
	[5.71]* **	**[7.54]*** **	**[3.24]*** **	**[3.87]*** **
Idiosyncratic stock return in year t-2		-0.714		-0.743
		[8.45]***		[9.25]***
Industry-induced stock return in year t-2		**-0.933**		**-0.672**
		[3.71]* **		**[1.86]***
Industry fixed effects	Yes	Yes	Yes	Yes

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 8
Two-stage Logit regressions of forced CEO turnover on firm and industry performance
Different turnover-performance slopes for under- and outperformers

The first stage regressions use industry stock returns to predict contemporaneous company stock returns and are reported in Panel A of Table 2. The second stage Logit regressions predict forced CEO turnover using the predicted values and the residuals from the first stage regression as measures of the predicted (peer-group) and of the idiosyncratic component of company stock returns respectively. Underperformers and outperformers are defined as firms with negative or positive firm-specific residual performance in the first stage regressions respectively. The second stage Logit regressions include industry fixed effects using the Fama and French (1997) classification into 48 industries. All t- and z-statistics are calculated with robust standard errors clustered at the industry level.

Second stage Logit regressions of CEO dismissals on peer-group induced and idiosyncratic firm performance

	Peer performance measured as EW industry stock returns		Peer performance measured as VW industry stock returns	
	(1)	(2)	(3)	(4)
	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover
Constant	-19.521	-19.27	-19.3	-19.22
	[254.99]***	[246.89]***	[556.08]***	[98.23]***
Positive idiosyncratic stock return in year t-1 (outperformer)	-0.872	-0.899	-0.779	-0.782
	[3.94]***	[3.22]***	[2.79]***	[2.57]**
Negative idiosyncratic stock return in year t-1 (underperformer)	-2.689	-2.648	-2.774	-2.712
	[12.05]***	[14.53]***	[13.32]***	[14.06]***
Industry-induced stock return in year t-1	**-2.065**	**-2.397**	**-1.739**	**-1.786**
	[8.68]* **	**[9.49]*** **	**[7.09]*** **	**[7.32]*** **
Positive idiosyncratic stock return in year t-2 (outperformer)		-0.379		-0.394
		[1.46]		[1.42]
Negative idiosyncratic stock return in year t-2 (underperformer)		-0.884		-0.932
		[3.02]***		[3.15]***
Industry-induced stock return in year t-2		**-1.191**		**-0.659**
		[4.15]* **		**[2.46]** **
Industry fixed effects	Yes	Yes	Yes	Yes

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 9
Two-stage Logit regressions of forced CEO turnover on firm and industry performance
Small firms only

The estimation is performed for firms with equity market values less than 1% of total industry market value (columns 1 and 2) and for firms with book assets less than 1% of total industry book assets (columns 3 and 4) only. The first stage regressions use industry stock returns to predict contemporaneous company stock returns. The second stage Logit regressions predict forced CEO turnover using the predicted values and the residuals from the first stage regression as measures of the predicted (peer-group) and of the idiosyncratic component of company stock returns respectively. The second stage Logit regressions include industry fixed effects using the Fama and French (1997) classification into 48 industries. All t- and z-statistics are calculated with robust standard errors clustered at the industry level.

Panel A: First stage regressions of firm performance on industry performance

	Market value less than 1% of industry market value		**Book assets less than 1% of industry books assets**	
	(1)	(2)	(3)	(4)
	Firm stock return in year t-1	Firm stock return in year t-1	Firm stock return in year t-1	Firm stock return in year t-1
Constant	0.079	0.059	0.080	0.066
	[3.64]***	[2.69]**	[3.50]***	[2.64]**
EW industry stock return in year t-1	0.970		1.016	
	[12.26]***		[8.89]***	
VW industry stock return in year t-1		1.060		1.089
		[9.16]***		[6.68]***
R-squared	0.13	0.10	0.13	0.10

Panel B: Second stage Logit regressions of CEO dismissals on peer-group induced and idiosyncratic firm performance

	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover
Constant	-4.290	-4.252	-18.9	-18.895
	[48.27]***	[54.13]***	[238.30]***	[146.07]***
Idiosyncratic stock return in year t-1	-2.211	-2.265	-2.005	-2.054
	[9.41]***	[9.76]***	[8.81]***	[9.28]***
Industry-induced stock return in year t-1	**-1.723**	**-1.450**	**-1.580**	**-1.315**
	[8.27]* **	**[6.55]*** **	**[7.14]*** **	**[5.36]*** **
Industry fixed effects	Yes	Yes	Yes	Yes

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 10
Two-stage Logit regressions of forced CEO turnover on firm and industry performance
Industry affiliation lagged by five years

The first stage regressions use industry stock returns to predict contemporaneous company stock returns. The industry affiliations are lagged by five years so that each firm is benchmarked against its competitors from five years ago. The second stage Logit regressions predict forced CEO turnover using the predicted values and the residuals from the first stage regression as measures of the predicted (peer-group) and of the idiosyncratic component of company stock returns respectively. The industry definitions follow the Fama and French (1997) classification into 48 industries. The second stage Logit regressions include industry fixed effects. All t- and z-statistics are calculated with robust standard errors clustered at the industry level.

Panel A: First stage regressions of firm performance on industry performance

	(1)	(2)	(3)	(4)
	Firm stock return in year t-1	Firm stock return in year t-1	Firm stock return in year t-1	Firm stock return in year t-1
Constant	0.050	0.050	0.043	0.043
	[3.59]***	[3.59]***	[2.81]***	[2.81]***
EW industry stock return in year t-1	0.888	0.888		
	[10.34]***	[10.34]***		
VW industry stock return in year t-1			0.872	0.872
			[7.88]***	[7.88]***
R-squared	0.14	0.14	0.10	0.10
		Firm stock return in year t-2		Firm stock return in year t-2
Constant		0.050		0.025
		[3.17]***		[1.39]
EW industry stock return in year t-2		0.859		
		[11.18]***		
VW industry stock return in year t-2				0.922
				[8.28]***
R-squared		0.13		0.11

Panel B: Second stage Logit regressions of CEO dismissals on peer-group induced and idiosyncratic firm performance

	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover
Constant	-19.540	-18.962	-19.664	-19.306
	[229.78]***	[157.59]***	[246.18]***	[107.17]***
Idiosyncratic stock return in year t-1	-2.255	-2.213	-2.337	-2.269
	[8.32]***	[9.89]***	[9.04]***	[9.53]***
Industry-induced stock return in year t-1	**-1.748**	**-2.218**	**-1.306**	**-1.450**
	[6.42]* **	**[6.79]*** **	**[3.85]*** **	**[4.04]*** **
Idiosyncratic stock return in year t-2		-0.718		-0.807
		[3.70]***		[4.05]***
Industry-induced stock return in year t-2		**-1.650**		**-0.745**
		[4.44]* **		**[1.91]***
Industry fixed effects	Yes	Yes	Yes	Yes

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 11
Two-stage Logit regressions of forced CEO turnover on firm and industry performance
Different turnover-performance slopes for different levels of industry performance

The first stage regressions use industry stock returns to predict contemporaneous company stock returns and are reported in Panel A of Table 2. The second stage Logit regressions predict forced CEO turnover using the predicted values and the residuals from the first stage regression as measures of the predicted (peer-group) and of the idiosyncratic component of company stock returns respectively. The second stage Logit regressions allow for different effects of idiosyncratic performance on CEO turnover depending on whether industry performance is in the bottom, middle, or top third of all observations. The second stage regressions include industry fixed effects using the Fama and French (1997) classification into 48 industries. All t- and z-statistics are calculated with robust standard errors clustered at the industry level.

Second stage Logit regressions of CEO dismissals on peer-group induced and idiosyncratic firm performance				
	Peer performance measured as EW industry stock returns		Peer performance measured as VW industry stock returns	
	(1)	(2)	(3)	(4)
	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover
Constant	-19.542	-19.307	-19.785	-19.657
	[181.87]***	[183.90]***	[194.75]***	[157.61]***
Idiosyncratic stock return in year t-1: high industry performance	-1.841	-1.872	-1.901	-1.883
	[4.95]***	[4.76]***	[7.28]***	[6.24]***
Idiosyncratic stock return in year t-1: medium industry performance	-2.314	-2.272	-2.298	-2.289
	[11.96]***	[11.74]***	[10.22]***	[10.35]***
Idiosyncratic stock return in year t-1: low industry performance	-2.592	-2.599	-2.986	-2.904
	[9.74]***	[10.71]***	[9.79]***	[9.55]***
Industry-induced stock return in year t-1	**-1.385**	**-1.740**	**-0.961**	**-1.058**
	[3.96]* **	**[3.97]*** **	**[3.66]*** **	**[3.28]*** **
Idiosyncratic stock return in year t-2: high industry performance		-0.714		-0.855
		[4.17]***		[5.30]***
Idiosyncratic stock return in year t-2: medium industry performance		-0.829		-0.505
		[4.16]***		[2.59]***
Idiosyncratic stock return in year t-2: low industry performance		-0.300		-0.517
		[0.73]		[1.77]*
Industry-induced stock return in year t-2		**-1.072**		**-0.551**
		[4.42]* **		**[2.08]** **
Industry fixed effects	Yes	Yes	Yes	Yes

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 12
Two-stage Logit regressions of forced CEO turnover on firm and industry performance
Different industry performance effects for under- and outperformers

The first stage regressions use industry stock returns to predict contemporaneous company stock returns and are reported in Panel A of Table 2. The second stage Logit regressions predict forced CEO turnover using the predicted values and the residuals from the first stage regression as measures of the predicted (peer-group) and of the idiosyncratic component of company stock returns respectively. Underperformers and outperformers are defined as firms with negative or positive firm-specific residual performance in the first stage regressions respectively. The second stage Logit regressions include industry fixed effects using the Fama and French (1997) classification into 48 industries. All t- and z-statistics are calculated with robust standard errors clustered at the industry level.

Second stage Logit regressions of CEO dismissals on peer-group induced and idiosyncratic firm performance

	Peer performance measured as EW industry stock returns		Peer performance measured as VW industry stock returns	
	(1)	(2)	(3)	(4)
	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover
Constant	-19.592	-19.332	-19.693	-19.560
	[515.24]***	[235.00]***	[202.58]***	[117.60]***
Idiosyncratic stock return in year t-1	-2.507	-2.522	-2.504	-2.449
	[10.35]***	[11.26]***	[10.25]***	[10.06]***
Industry-induced stock return in year t-1 for outperformers	**-0.315**	**-0.598**	**-0.469**	**-0.695**
	[0.48]	**[0.81]**	**[0.69]**	**[0.85]**
Industry-induced stock return in year t-1 for underperformers	**-2.124**	**-2.506**	**-1.754**	**-1.811**
	[7.83]* **	**[8.57]*** **	**[6.06]*** **	**[6.49]*** **
Idiosyncratic stock return in year t-2		-0.681		-0.753
		[4.18]***		[4.44]***
Industry-induced stock return in year t-2 for outperformers		**-0.863**		**-0.273**
		[2.01] **		**[0.59]**
Industry-induced stock return in year t-2 for underperformers		**-1.093**		**-0.580**
		[3.99]* **		**[2.40]** **
Industry fixed effects	Yes	Yes	Yes	Yes

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 13
Two-stage Logit regressions of forced CEO turnover on firm and industry performance
Firm-years with negative prior stock returns only

The first stage regressions use industry stock returns to predict contemporaneous company stock returns. The second stage Logit regressions predict forced CEO turnover using the predicted values and the residuals from the first stage regression as measures of the predicted (peer-group) and of the idiosyncratic component of company stock returns respectively. The second stage regressions are run for firms with negative returns (columns (1) and (3)) or firms with returns below -20 percent (columns (2) and (4)) only. The second stage Logit regressions include industry fixed effects using the Fama and French (1997) classification into 48 industries. All t- and z-statistics are calculated with robust standard errors clustered at the industry level.

Panel A: First stage regressions of firm performance on industry performance

	Firm stock return in year t-1	Firm stock return in year t-1
Constant	0.096	0.076
	[5.06]***	[3.97]***
EW industry stock return in year t-1	0.905	
	[11.30]***	
VW industry stock return in year t-1		1.036
		[9.37]***
R-squared	0.12	0.10

Panel B: Second stage Logit regressions of CEO dismissals on peer-group induced and idiosyncratic firm performance conditional on low absolute performance

	Absolute returns below 0%	**Absolute returns below -20%**	**Absolute returns below 0%**	**Absolute returns below -20%**
	(1)	(2)	(3)	(4)
	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover
Constant	-19.573	-19.432	-19.65	-19.434
	[104.91]***	[132.50]***	[102.23]***	[70.16]***
Idiosyncratic stock return in year t-1	-3.006	-2.636	-3.079	-2.73
	[8.98]***	[6.92]***	[9.73]***	[7.80]***
Industry-induced stock return in year t-1	**-2.464**	**-2.095**	**-2.272**	**-1.924**
	[7.10]* **	**[5.64]*** **	**[6.61]*** **	**[5.22]*** **
Industry fixed effects	Yes	Yes	Yes	Yes

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 14
Stock price reactions around forced CEO turnovers

This table reports 3- and 5-day market-adjusted announcement returns around forced CEO turnover announcements. Average announcement returns are calculated separately for observations with positive and negative firm-specific stock returns preceding the CEO turnover, and observations with equal-weighted industry stock returns above and below the mean industry stock return in the sample. Firm-specific stock returns are calculated as the residuals from OLS regressions of annual firm returns on equal-weighted industry stock returns (shown in column 1 of Table 2). The industry definitions follow the Fama and French (1997) classification into 48 industries.

Panel A: 3-day stock price reaction around announcements of forced CEO turnovers

	Below-average industry performance		Above-average industry performance		
	No. of observations	3-day announcement return	No. of observations	3-day announcement return	T-test for differences in means
Positive idiosyncratic stock return in year t-1 (outperformer)	36	-3.46%	20	-3.62%	0.07
Negative idiosyncratic stock return in year t-1 (underperformer)	198	-1.18%	112	-1.70%	0.35
T-test for differences in means		1.21		1.00	

Panel B: 5-day stock price reaction around announcements of forced CEO turnovers

	Below-average industry performance		Above-average industry performance		
	No. of observations	5-day announcement return	No. of observations	5-day announcement return	T-test for differences in means
Positive idiosyncratic stock return in year t-1 (outperformer)	36	-3.03%	20	-4.68%	0.58
Negative idiosyncratic stock return in year t-1 (underperformer)	198	-1.18%	112	-2.60%	0.87
T-test for differences in means		0.93		0.80	