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CEO Turnover and Relative Performance Evaluation

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ABSTRACT

This paper shows that 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 from firm performance before deciding on CEO retention. Using a hand-collected sample of 3,365 CEO turnovers from 1993 to 2009, we document that CEOs are significantly more likely to be dismissed from their jobs after bad industry and, to a lesser extent, after bad market performance. A decline in industry performance from the 90th to the 10th percentile doubles the probability of a forced CEO turnover.

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

Specifically, using a new data set of 2,490 voluntary and 875 forced CEO turnovers in 3,042 firms from 1993 to 2009, we document that low industry stock returns and (to a lesser extent) low market returns increase the frequency of forced CEO turnovers. A decrease in the industry component of firm performance from its 90th to its 10th percentile doubles the probability of a forced CEO turnover. There is some evidence that boards partially filter industry and especially market performance from their assessments of CEO quality, but the filtering is much too weak to remove all of the peer performance effect. We conclude that boards allow exogenous shocks to firm performance to affect their CEO retention decisions.

Standard agency theory shows that there are benefits to evaluating agents on the basis of their relative performance when agents are affected by common shocks (Holmström (1979, 1982), Diamond and Verrecchia (1982)). In most of the theoretical literature on CEO dismissals, a corporate board learns the quality of its CEO from firm performance and other signals. If the board's assessment of CEO quality falls below some threshold, often the expected quality of a replacement, then the board dismisses the CEO.[1] Since CEO and CEO-firm match quality are not functions of the business cycle in these models, it follows that efficient boards do not force out

more CEOs in bad times than in good times. More generally, these models hold that boards should filter all observable exogenous shocks from firm performance before updating their assessments of CEO quality. This prediction is strongly rejected by our empirical results, and we conclude that the simple framework used in much of the literature does not fully explain real-world CEO dismissals.

There are several possible explanations for why more CEOs are fired when their peer group is not doing well, almost all of which fall into one of three categories: First, CEOs may optimally be rewarded or punished for peer group performance if CEOs' actions affect peer performance, as would be the case in an oligopolistic industry. Second, peer group performance may affect the optimal frequency of CEO dismissals if boards receive more (or more important) information about their CEOs in times of bad peer performance. This may be the case because, for example, downturns change the skills required of CEOs or downturns test skills that are otherwise unobservable.[2] Finally, peer group performance may affect CEO turnover because boards do not behave optimally and misattribute exogenous performance components to the CEO.

To shed light on why CEOs are fired less frequently when their peer group is doing well, we examine the relationship between CEO dismissals and peer performance in more detail. The main result of these analyses is that peer performance has only small effects on outperforming CEOs, but large effects on underperforming CEOs. Better peer group performance substantially reduces the probability that an underperformer is dismissed, which implies that many fewer underperformers are fired in good times than in bad times. This may be optimal if underperformance in good times is less revealing about deficiencies in CEO skill, but may also suggest that boards fail to fulfill their monitoring role when times are good.

Consistent with the idea that performance in recessions is more informative about the quality of the CEO (or the CEO-firm match), we find some evidence that firm-specific performance affects CEO turnover more in recessions than in booms, even though this result does not obtain in all specifications. However, there is no evidence that the effect of peer performance on CEO turnover is smaller for CEOs with longer tenure. This finding is surprising if one believes that recessions reveal deficiencies in CEO skill. CEOs with long tenure should have already proven their ability in both good and bad times, and hence the effects of recessions on CEO turnover should be larger for new CEOs, contrary to what we find in the data.

An alternative explanation for the peer performance effect on CEO turnover is that boards mistakenly credit or blame CEOs for exogenous performance shocks. The result that underperforming CEOs are more frequently dismissed in recessions than in booms, while outperforming CEOs are almost unaffected by peer performance, is consistent with this idea. Even with systematic attribution errors, outperforming CEOs should only rarely be dismissed, as they can always point out that competitors are performing worse and induce the board to use relative performance evaluation. Underperforming CEOs, in contrast, 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. This asymmetry between out- and underperformers is exactly what we find in the data.

Systematic attribution errors are also consistent with the observation that boards do a better job filtering out the performance of both their value-weighted industry and the market than of their equal-weighted industry when deciding on CEO retention. This suggests that boards may use relative performance evaluation against the most salient benchmarks, such as the largest firms in

their industry, while failing to properly adjust for other, less obvious exogenous performance components.

Finally, we find no support for the idea that our results are driven by CEOs interacting in oligopolistic industries. 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 challenge the idea that CEOs are dismissed in downturns because their actions have affected industry performance.

We also examine whether CEO power affects the relationship between peer performance and CEO turnover. More powerful CEOs should be better able to defend themselves against "unfair" dismissals in bad times, weakening the effect of peer performance on forced turnovers. Alternatively, more powerful CEOs may be better able to hide behind peer performance in good times, strengthening the effect of peer performance on turnovers. We therefore test whether CEOs who are founders, CEOs with large equity stakes, CEOs with less independent boards, and CEOs with more excess compensation are differently affected by peer group performance than other CEOs. We find no consistent effects of CEO power on firms' propensity to use relative performance evaluation in their CEO turnover decisions.

More research is needed to identify the root cause of the peer performance effect on CEO turnover. Our results are broadly consistent with boards mistakenly crediting and blaming CEOs for performance beyond their control, with the skills required of CEOs changing in recessions, and with performance in recessions revealing more about CEO quality than performance in booms. Under all of these hypotheses, however, it is surprising that the failure of relative performance evaluation does not vary systematically with CEO tenure or CEO power.[3]

Independent of the underlying mechanism, the effects of industry and market performance on CEO turnover have important implications for our understanding of CEO incentives and horizons. Our results show that CEOs have to worry much more about underperforming their peer groups in industry recessions than in booms. This should bias CEOs towards strategies that enhance performance in recessions, for example, strategies that come with abandonment options, over strategies that boost performance in booms. Moreover, CEO horizons are bound to shrink at the onset of a recession, especially for CEOs who are already struggling, causing CEOs to shift to strategies that boost short-run performance.

Our findings also have implications for the correct design of CEO turnover studies. Prior studies customarily assume that CEOs are evaluated based on relative performance, and thus regress turnover on market- or industry-adjusted stock returns only. Given that CEO dismissals are determined jointly by firm-specific, industry, and market performance, these regressions might suffer from an omitted variable bias. A correctly specified CEO turnover regression needs to include both firm-specific and peer group performance as explanatory variables.

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 turnovers from 1963 to 1978 and find that market-adjusted stock returns are a better predictor of CEO dismissals than absolute performance. The results using lagged returns are ambiguous, and they find no evidence that industry shocks are filtered from CEO dismissal decisions. Morck, Shleifer, and Vishny (1989) examine turnovers of entire top management teams from 1980 to 1985 and find them to be equally likely in troubled and healthy industries, suggesting that industry performance is filtered from dismissal decisions. Barro and Barro (1990) find evidence for complete filtering of peer performance in a sample of bank CEO turnovers from 1982 to 1987. Finally, Gibbons and

Murphy (1990) examine a large sample of CEO successions from 1974 to 1986 and find that both market and industry shocks are filtered out. In summary, most prior evidence supports the hypothesis that boards filter industry and market shocks from firm performance before deciding whether to fire their CEOs.[4]

What might have caused this change in board behavior? There are several possibilities. First, there is considerable evidence that corporate governance has improved since the 1970s.[5] If more dismissals of underperforming CEOs in downturns are efficient, for example, because downturns change the skills required of CEOs, then improved governance may explain the change in board behavior. Second, increased pressure from shareholders may compel boards to act against CEOs when stock prices are down, even if the bad performance is not the CEOs' fault (Fisman, Khurana, and Rhodes-Kropf (2014)). Thus, stricter governance may be associated with not just more performance pressure but also more performance misattribution. Finally, if, as argued by Bebchuk and Fried (2004), corporate governance has actually worsened since the 1970s, then the change in turnover patterns may be caused by more underperforming CEOs hiding behind good industry performance in the 1990s and 2000s than in the 1970s.

A large literature aims to explain the well-documented absence of relative performance evaluation in CEO *compensation*.[6] This literature proposes several explanations for the lack of relative performance evaluation in CEO pay, including marginal products of CEO labor that vary with industry fortunes, the futility of indexing compensation when CEOs can trade the index, and the favorable accounting treatment of non-indexed options.[7] None of these explanations are good candidates to explain why fewer (underperforming) CEOs are dismissed in good times.

An interesting theoretical point is that most of the CEO turnover literature does not interpret firings as an extreme case of pay-for-performance. Instead, CEO selection is viewed as driven by

the desire to have the "right" person in charge of the firm, and CEO turnover as caused by learning about CEO or CEO-firm match quality.[8] Assuming the prior literature's interpretation is correct, then CEO compensation and CEO turnover are driven by different mechanisms. Thus, knowing that peer performance affects compensation does not imply that it should also affect CEO turnover, and showing that it in fact does is interesting.

Finally, the CEO turnover results in this paper may shed some light on the ongoing discussion of why CEO compensation is not properly indexed. Several studies document that "pay-for-luck" is asymmetric, with pay increasing more with positive exogenous shocks than it declines with negative shocks (Bertrand and Mullainathan (2001), Garvey and Milbourn (2006)). In reaction, the literature has proposed that pay-for-luck is a form of rent extraction caused by powerful managers who control the pay-setting process. Our finding that more managers are fired after bad luck raises doubts about the power of CEOs in bad times. Also, mechanically, our results point to a sample selection effect as one driver of the asymmetry in pay-for-luck. Compensation studies drop dismissed CEOs from their samples, and our results show that the selection pressure against underperforming CEOs is stronger in bad times than in good times. As a result, surviving CEOs may appear relatively well paid in bad times at least in part because underperformers have been fired.

Section I of this paper reviews the theory behind relative performance evaluation, develops the hypotheses, and describes the empirical strategy. Section II describes the data sources, sample selection, and variable definitions. Section III presents the main empirical results. Section IV describes and tests several explanations for the failure of relative performance evaluation. Section V summarizes and concludes.

I. Theoretical Background and Hypothesis Development

This section starts with an informal review of the theory behind relative performance evaluation in CEO turnover. The main prediction is that CEO dismissals should be unrelated to exogenous shocks to firm performance. Section I.B restates this testable hypothesis in an instrumental variables framework in which market and industry performance act as instruments for firm performance. This reformulation forms the basis for the subsequent empirical analysis.

A. Relative Performance Evaluation and CEO Turnover

The standard CEO turnover model envisions a board of directors that learns about CEO ability from firm performance and other signals. To conduct relative performance evaluation, the board also observes a group of peer firms subject to similar industry and market shocks. CEOs are dismissed when the board's estimate of CEO ability falls below some threshold, usually the expected ability of a replacement CEO, adjusted for any costs of replacing the CEO. The model assumes that CEO ability (and the quality of the CEO-firm match) is not a function of the business cycle or other exogenous shocks to firm performance. Also, the speed with which the board learns about CEO ability is the same in good times and bad times. Finally, the CEO's ability and actions have no effect on the performance of her peer group.

Given these assumptions, the model's central prediction is that CEOs should be evaluated based on the firm-specific component of firm performance only. Whether the reference group is booming or in a recession contains no information about CEO quality (or about the speed with which CEO ability is being learned). As a result, the performance of the peer group should have no predictive power for the likelihood of forced CEO turnovers.[9]

It is obvious that this model does not capture all the complexities of real-world CEO retention decisions. Deviations from the model's simplifying assumptions may render (complete) relative

performance evaluation inefficient. We discuss several such extensions in Section IV, after we present the main empirical results.

B. Empirical Strategy

The central testable prediction of the relative performance evaluation model is that industry and market performance should have no predictive power for the likelihood of forced CEO turnovers. The prediction that peer performance is *completely* filtered from the evaluation of the CEO is called the *strong-form* relative performance evaluation hypothesis.[10] Our empirical strategy to test for such strong-form relative performance evaluation borrows from Bertrand and Mullainathan (2001), Wolfers (2002), and Garvey and Milbourn (2006).

We estimate the sensitivity of CEO turnover to peer performance using a two-stage regression approach. The first stage decomposes firm performance into a systematic component caused by peer group performance and a firm-specific component that should reflect, among other things, CEO ability. In the second stage, we predict the probability of a forced CEO turnover using both the estimated peer group component and the estimated residual component of firm performance. This two-stage procedure is effectively an instrumental variables estimation, with peer group performance serving as instrument for firm performance:

(i) First stage: $$r_{i,t-1} = \beta_0 + \beta_1 \cdot r_{peer\ group,t-1} + \nu_{i,t-1}$$

(ii) Second stage: $$\text{Probability(CEO dismissal}_{i,t}) = \gamma_0 + \gamma_1 \cdot \hat{r}_{i,t-1} + \gamma_2 \cdot \hat{\nu}_{i,t-1} + \varsigma_{i,t}, \qquad (1)$$

where $\hat{r}_{i,t-1} = \hat{\beta}_0 + \hat{\beta}_1 \cdot r_{peer\ group,t-1}$.

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 skill, 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 CEO skill as well as other shocks not related to peer performance. However, given that firm-specific performance is driven in part by CEO skill, we expect it to be negatively related to the probability of a CEO dismissal ($\gamma_2 < 0$).

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 additional estimation error into the peer performance term in the second-stage regression. To avoid this problem, and to be consistent with the literature, we estimate a common peer performance beta for all firms in the first-stage regression. As a robustness check, we repeat the estimation with industry- and firm-specific betas and obtain similar results.

The instrumental variables interpretation clarifies the conditions under which we expect the predictions of strong-form relative performance evaluation to obtain in the data. The tests treat peer 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, (i) peer performance should be exogenous and (ii) peer performance itself should not have an effect on CEO dismissals. Violations of these two assumptions correspond directly to the arguments against relative performance evaluation in CEO turnover that we discuss in Section IV below.

The two-stage regression approach in (1) is not used by prior literature. The prior literature instead tests for *weak-form* relative performance evaluation, which holds that the likelihood of CEO dismissals should be negatively related to firm performance and positively related to the performance of the reference group. The weak-form hypothesis does not predict *complete* filtering

of peer performance, but instead predicts only that *some* filtering is done by corporate boards. Following Gibbons and Murphy (1990) and Barro and Barro (1990), we test for weak-form relative performance evaluation using a simple one-stage regression:

$$\text{Probability(CEO dismissal}_{i,t}) = \gamma'_0 + \gamma'_1 \cdot r_{peer\ group,t-1} + \gamma'_2 \cdot r_{i,t-1} + \varepsilon_{i,t}. \quad (2)$$

Weak-form relative performance evaluation implies 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 performance in the same single-stage regression produces coefficients that are hard to interpret. The γ'_1 coefficient on peer performance is the product of the sensitivity of firm performance to peer performance from the first-stage regression (1.i) and the difference between the peer and the firm-specific performance coefficients from the second-stage regression (1.ii): $\gamma'_1 = \beta_1 \cdot (\gamma_1 - \gamma_2)$. Hence, the estimated coefficient can be small either because firm performance is not sensitive to peer performance or because boards do not distinguish between peer and firm-specific performance when evaluating CEOs.

II. Data Sources, Sample Construction, and Variable Definitions

CEO turnover is observed for all firms in the S&P *ExecuComp* database for the time period 1993 to 2009. 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 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 voluntary. Departures for CEOs below age 60 are reviewed further and classified as forced if either the press does not report the reason as death, poor health, or the acceptance of another position (including chairmanship of the board) or the press reports that the CEO is retiring but does not announce the retirement at least six months before the succession. Finally, cases classified as forced can be reclassified if the reports convincingly explain the departure as due to reasons unrelated to the firm's activities. This careful classification scheme is necessary since CEOs are rarely openly fired from their positions. We exclude CEO turnovers associated with mergers and spin-offs from the analysis.

All accounting information comes from the *Compustat* Industrial Annual files, and all 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 on *CRSP* from the same industry as the sample firm. Industries are defined using the Fama and French (1997) classification of firms into 48 industries, with all firms in the "Other" industry dropped from the analysis. We exclude each sample firm from its own industry benchmark.[11]

III. Empirical Results

A. Descriptive Statistics

Table I presents an overview of the CEO turnover data set. Panel A reports the frequencies of forced and voluntary turnovers. The final sample has 3,042 firms with 31,185 firm-year observations from 1993 to 2009 and contains 3,365 CEO turnovers. Of these, 2,490 are classified as voluntary and 875 are classified as forced. Panel B shows firm performance and characteristics

by CEO retention outcome (CEO is retained, CEO leaves voluntarily, 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, and sales. Part of this difference is likely due to the fact that CEO dismissals are preceded by bad performance and associated declines in firm size. The average stock return in the 12 months before a forced CEO turnover is -19.39%.

[Table I]

Notably, the average equal-weighted *industry* return is lower before forced turnovers (10.78%) than before voluntary turnovers (13.96%) and CEO retentions (15.73%). Both differences are statistically significant at the 1% level. This suggests that CEO dismissals are more common in industries that have performed badly. Panel C, where we report CEO dismissal frequencies by industry performance quintile, confirms this result. Using equal-weighted industry returns, forced CEO turnovers are almost twice as likely in the lowest industry performance quintile compared to the highest. The same pattern obtains in slightly weaker form for value-weighted industry returns, and the differences are again both statistically and economically significant. These results are prima facie inconsistent with strong-form relative performance evaluation, which predicts no relation between peer group performance and CEO turnover. However, the analysis in Table I fails to control for systematic differences across CEOs and firms, and thus we turn to regression analyses next.

B. Testing for Strong-Form Relative Performance Evaluation in CEO Turnover

In this section, we estimate the sensitivity of CEO turnover to peer group performance using the two-stage approach described in Section I.B, with industry stock returns as the measure of peer performance. The first-stage regression partitions variation in firm performance into a predictable component caused by industry performance and a residual firm-specific component. The second

stage regresses an indicator for forced CEO turnover on the predicted value (the peer performance component) and the residual (the firm-specific component) from the first-stage regression. Strong-form relative performance evaluation predicts that peer performance should not affect the CEO retention decision.

We estimate the second-stage CEO turnover regression using the Cox (1972) proportional hazard model. The Cox model flexibly accommodates the fact that each CEO's hazard rate, that is, (approximately) the probability that a currently employed CEO is dismissed over the next month, is a function of the CEO's tenure as well as other CEO characteristics and control variables. We treat voluntary turnovers as right-censored observations in the estimation. The regressions include a dummy variable for CEOs between ages 63 and 66 to account for likely retirements, and a second dummy variable for CEOs who own more than 5% of their firm's equity to control for CEOs who may be difficult to dislodge. Only CEOs who have been in office at least 24 months are included in the analysis.

Table II 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 affect CEO dismissals. Column (1) uses equal-weighted industry returns over the previous two years as instruments for firm performance. Instead of the expected coefficient of zero on predicted performance in the second stage CEO turnover regression, we find that the point estimate on predicted performance for year *t*-1 (-1.602, robust *z*-stat. of 8.66) is of almost the same magnitude as the point estimate on idiosyncratic performance (-2.542, robust *z*-stat. of 11.71). Adding calendar year fixed effects in column (2) to account for time trends and economy-wide shocks strengthens this result and makes the predicted performance from *t*-2 significant as well. Columns (3) and (4) replace equal-

weighted by value-weighted industry returns. The negative coefficients on predicted performance in the CEO turnover regressions remain large and statistically highly significant.

The results in Table II show that bad industry performance increases the likelihood of a CEO dismissal by 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.

[Table II]

Instead of Cox hazard regressions, the prior literature uses logit regressions to measure the effect of performance on CEO turnover. To show that our results are not an artifact of using the (arguably more suitable) hazard model, we repeat the second-stage turnover analysis using logit regressions in Table III. Since the logit model by itself does not account for the effect of tenure on the frequency of CEO dismissals, we include CEO tenure as an explicit control in all specifications. The logit regressions in Table III show an equally impressive rejection of strong-form relative performance evaluation as the results in Table II.

[Table III]

The effect of industry performance on the frequency of CEO dismissals is economically large. Table IV presents implied probabilities of forced CEO turnover calculated from the logit models in columns (1) and (3) of Table III. The average implied probability of a forced CEO turnover in the base case (all independent variables left at their actual values) is 2.86% and equal to the unconditional probability of a forced turnover in the sample. The average probability of a forced turnover increases to 4.14% (4.05%) when year *t*-1 equal-weighted (value-weighted) industry performance is at its 10^{th} percentile value. The turnover probability falls to 2.05% (2.12%) when peer performance is at its 90^{th} percentile. Hence, a decrease in peer performance from the 90^{th} to the 10^{th} percentile doubles the implied probability of a CEO dismissal.

[Table IV]

C. Testing for Weak-Form Relative Performance Evaluation in CEO Turnover

Most prior literature does not test for strong-form relative performance evaluation but instead 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. Weak-form relative performance evaluation does not predict complete filtering of peer performance. Rather, it posits that only some filtering of peer performance from firm performance is done by corporate boards.

Tests of weak-form relative performance evaluation regress forced CEO turnover on both firm and peer performance in a single-stage regression, as shown in Table V. The results are consistent with partial filtering of industry shocks: firm performance enters strongly negatively, and industry performance comes in with the opposite sign, as predicted. However, the coefficients on industry performance are much smaller in absolute value (between 0.646 and 1.405 for year *t*-1) than the coefficients on firm performance (between -2.542 and -2.780 for year *t*-1), confirming the result that industry shocks are not fully filtered from CEO retention decisions.[12] The coefficients on industry performance in year *t*-2, while positive, are not significantly different from zero in three out of four regressions.

[Table V]

The results in Table V indicate that corporate boards do take some account of industry performance when assessing the performance of their CEOs. Interestingly, the point estimates suggest more relative performance evaluation against value-weighted than against equal-weighted industry returns in *t*-1. One explanation 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 Peer Group Performance

The previous sections show that industry performance affects the frequency of forced CEO turnovers. This section examines whether market-wide performance has similar effects. We again decompose firm performance into its peer group component and its firm-specific component using the two-stage regression approach described in Section I.B.

Table VI presents CEO turnover regressions using peer performance measured as equal-weighted (column (1)) or value-weighted (column (2)) stock market returns. Strong-form relative performance evaluation is again rejected, but the results are considerably weaker than with industry returns. Both equal- and value-weighted market returns in year *t*-1 have a significant negative effect on CEO dismissals, suggesting more CEO dismissals after bad market performance. However, the point estimates are much smaller than those for industry returns in Table II. Market performance in year *t*-2 has a negative effect on CEO dismissals if value-weighted, but a positive effect if equal-weighted. The latter result is unexpected and surprising, suggesting more CEO dismissals two years after equal-weighed market returns have been high.

[Table VI]

There are at least two explanations for why the estimated effect of market performance on CEO turnover is weaker than the effect of industry performance. First, there is less variation in market returns than in industry returns over the sample period, which makes it more difficult to estimate the relationship between market returns and turnover from the data. One or two years with unusual return and CEO turnover patterns can have large effects on the estimated coefficients. Second, it is possible that boards use relative performance evaluation against more salient

benchmarks, such as the widely reported S&P 500 index, while ignoring less directly visible outside influences on firm performance.

E. Robustness Tests

In this section, we subject the results presented so far to a variety of robustness tests, all of which are available in the Internet Appendix. First, we reestimate the second-stage regressions allowing for *three* CEO turnover outcomes: retention, voluntary turnover, and forced turnover. We continue to use Cox hazard regressions and apply the method of Lunn and McNeil (1995) to estimate differential effects of the explanatory variables on voluntary and forced turnover. The coefficient estimates for forced CEO turnover are similar to those in Table II, with both idiosyncratic and peer performance strongly predicting CEO dismissals. Unsurprisingly, the effects of the two performance components on voluntary turnover are much weaker.

The second robustness test allows for *industry*-specific peer performance sensitivities in the first-stage regressions. The results using industry-specific betas are very similar to those using the same peer performance beta for all firms. Next, we allow for *firm*-specific betas in the first-stage regressions. The effect of peer performance on CEO dismissals is slightly stronger than when using the same beta for all firms.

Finally, we examine whether the peer performance effect on CEO turnovers is also present when performance is measured by accounting returns. Using two-year changes in operating return on assets as the performance measure, we find that both the firm-specific and the industry component of operating performance determine the likelihood of CEO dismissals. The statistical significance of the peer-performance effect is smaller than in Table II but remains above the 1% level in all regressions.

IV. Explaining the Lack of Relative Performance Evaluation

The results so far demonstrate that peer performance is not fully filtered from CEO dismissal decisions. Instead, bad industry performance increases the likelihood of a forced CEO turnover by almost as much as bad firm-specific performance. This pattern can be consistent with models in which CEOs are fired efficiently, but may also suggest that boards behave suboptimally.

There are several possible explanations for why more CEOs are fired when their peer group is not doing well, almost all of which fall into one of three categories. First, CEOs may optimally be rewarded or punished for peer performance if CEOs' actions affect the peer group, as would be the case in oligopolistic industries. Second, peer performance may affect the optimal frequency of CEO dismissals directly, without working through firm performance. This can be the case, for example, if bad industry performance correlates with changing skill requirements for CEOs or with more informative signals about CEO ability. Finally, peer performance may affect CEO turnover because boards do not behave optimally. Boards may misattribute performance and credit or blame CEOs for performance caused by factors beyond their control.

To shed light on why CEOs are more frequently fired in bad times, this section examines the relationship between CEO dismissals and peer performance in more detail. We start by testing whether peer group performance may be endogenous to CEOs' actions or abilities.

A. The Oligopolistic Industry Hypothesis

Linking CEO retention decisions to rival firm performance may serve shareholders by softening competition in oligopolistic industries.[13] Boards may dismiss CEOs for low industry performance if such performance is caused by suboptimal CEO actions, for example, because the CEO started a price war. We evaluate this hypothesis empirically by examining whether the effect of industry performance on CEO turnover vanishes as the industry definition broadens (as larger

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).

The evidence speaks against the oligopoly hypothesis. The industries used in our analysis are broad, with most Fama-French industries having more than 100 firms on *CRSP* at any point during the sample period. In addition, the peer group effect on forced CEO turnover persists (in weaker form) when peer performance is measured as *market* returns, which is hard to reconcile with the idea that peer performance is caused by any particular CEO.

Finally, we test whether the effect of industry performance on CEO dismissals vanishes for firms that are small relative to their industry and therefore unlikely to affect the industry equilibrium. Independently of the exact definition of small firms, we find that industry performance has a statistically and economically large effect on the likelihood of forced CEO turnovers in small firms.[14] We conclude that strategic interactions in oligopolistic industries are unlikely to explain the observed peer performance effects on CEO turnover.

B. More Informative Signals about CEO Ability in Recessions

Industry or market-wide recessions may allow boards to learn more about the quality of their CEO than booms because, for example, recessions test aspects of CEO skill that are otherwise difficult to observe. On the simplest level, a recession tests whether a CEO has anticipated and prepared for the downturn, which is arguably an important element of CEO performance. Alternatively, the skills that firms require of their CEOs may change in recessions, causing more firms to replace their CEOs.[15] Note that these arguments do not weaken the case for relative performance evaluation; rather, they simply suggest that relative performance evaluation yields more informative or more relevant signals in recessions.

The hypothesis that performance in bad times is more informative about CEO ability (or the quality of the CEO-firm match) yields three testable predictions. First, any increase in CEO dismissals in recessions should be concentrated on underperforming CEOs. Underperformance in bad times sends an especially bad signal about CEO quality and should lead to forced turnovers. Second, and closely related, CEO turnover should be more sensitive to firm-specific performance in recessions than in booms. Finally, if recessions reveal deficiencies in CEO skill to boards, then the effects of recessions on CEO turnover should be largest for new CEOs. Long-tenured CEOs, in contrast, should have already proven their skills in both good and bad times.

The first prediction is that any increase in CEO dismissals in bad times should concentrate among underperforming CEOs. Table VII tests this idea by estimating whether the sensitivity of CEO turnover to peer performance depends on whether a CEO underperforms or outperforms her peer group. To ease interpretation, Panel B reports the marginal effects of peer performance on CEO dismissals separately for under- and outperformers. We find that the previously documented effect of industry performance on CEO dismissals is almost entirely restricted to CEOs who underperform their industry. There is almost no effect of peer performance on CEOs who outperform.[16] We conclude that the peer performance effect on CEO turnovers is driven by boards removing many more underperforming (but not outperforming) CEOs in bad times than in good times.

[Table VII]

The second prediction is that CEO turnover should be more sensitive to *firm-specific* performance in recessions than in booms. We test this prediction in Table VIII by interacting firm-specific performance with indicators for low, medium, and high industry performance over years *t*-1 and *t*-2 in the CEO turnover regressions. The marginal effects are reported in Panel B. The results support the prediction when firm performance is measured in year *t*-1 before the turnover

decision: The effect of firm-specific performance on CEO turnover is larger when industry performance is low than when it is high. The differences in slopes and marginal effects are significant when using equal-weighted but not when using value-weighted industry returns as the measure of peer performance. The prediction is not supported when using firm-specific performance in *t*-2, which has its largest effect on CEO turnover when industry performance is at intermediate levels.[17]

[Table VIII]

The third and final prediction is that the effect of peer performance on CEO turnover should be largest for new CEOs. A CEO with long tenure should have already proven her ability in both good and bad times. Hence, if we believe that recessions reveal deficiencies in CEO skill to boards, then the effect of recessions on turnover should be largest for new CEOs. We test this prediction by allowing for different effects of industry performance on turnover for CEOs with up to four years of tenure, for CEOs between five and eight years of tenure, and for CEOs with more than eight years of tenure. Contrary to our prediction, there is no evidence that the effects of peer performance on forced turnover are larger early in CEO tenures. These results are available in the Internet Appendix.

C. Systematic Performance Attribution Errors

The lack of relative performance evaluation in CEO turnover may also suggest that corporate boards make systematic mistakes in attributing performance, and credit or blame CEOs for performance caused by factors beyond their control.[18] This idea is difficult to test directly. However, the result that underperforming CEOs are more frequently dismissed in recessions than in booms, while outperforming CEOs are unaffected by peer performance, is consistent with this idea. Even with systematic attribution errors, outperforming CEOs should only rarely be dismissed

as they can always point out that competitors are performing worse and induce the board to use relative performance evaluation. Underperforming CEOs, in contrast, 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. This asymmetry between under- and outperformers is exactly what the results in Table VII show.

Systematic performance attribution errors are also suggested by the observation that boards do a better job filtering the performance of the value-weighted industry and of the overall market when deciding on CEO retention. This suggests that boards take the most salient benchmarks, such as the largest firms in their industry, into account when evaluating CEOs, but fail to properly adjust for other, less obvious exogenous performance components.[19]

D. CEO Power and Relative Performance Evaluation

We next examine whether CEO power affects the relationship between peer performance and CEO turnover. More powerful CEOs may be better able to defend themselves against "unfair" dismissals, weakening the effect of peer performance on forced turnovers. Alternatively, more powerful CEOs may be better able to hide behind peer group performance in good times, thus strengthening the effect of peer performance on turnovers.

We test whether CEOs who are founders, CEOs with large equity stakes, CEOs with insider-dominated boards, and CEOs with more excess compensation are more or less affected by peer performance than other CEOs. The detailed results are available in the Internet Appendix. We find no consistent effects of CEO power on firms' propensity to use relative performance evaluation. To the extent that CEO power affects the relation between performance and CEO turnover, the effect is the same for peer performance and for idiosyncratic performance.

V. Conclusion

We document that industry and, to a lesser extent, market returns affect the frequency of forced CEO turnovers. There is some evidence that boards partially adjust for peer group performance when assessing CEOs, but the adjustment is too small to remove all of the peer performance effect. We conclude that boards allow exogenous shocks to firm performance to affect their CEO turnover decisions.

The effect of peer performance on CEO turnover concentrates among CEOs who have underperformed their peers. Such underperformers are much more likely to be fired following bad industry returns, while outperforming CEOs see at most a small increase in their firing probability. Moreover, there is some evidence of more effective filtering of more visible indicators of outside performance, such as the value-weighted industry and the market, suggesting that boards benchmark performance only against more salient measures of peer performance.

More research is needed to conclusively identify the cause of the peer performance effect on CEO turnover. Our results are consistent with the idea that boards mistakenly credit and blame CEOs for performance beyond their control, but also with the idea that performance in recessions reveals more (or more important) information about CEO quality than performance in booms.

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Table I
Summary Statistics

This table presents an overview of the 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 firm characteristics and performance by CEO retention outcome. Panel C reports the frequency of forced CEO turnovers by industry performance quintile. Dollar values and employee numbers are winsorized at the 1% level. All dollar values are in 2009 dollars.

Panel A: Frequency of voluntary and forced CEO turnovers

Number of firm-years	Number of forced CEO turnovers	Number of voluntary CEO turnovers	Percentage of firm-years with at least one CEO turnover	Percentage of firm-years with at least one forced CEO turnover	Percentage of firm-years with at least one voluntary CEO turnover
31,185	875	2,490	10.25%	2.77%	7.85%

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 ($m)	9,739	11,577	10,572
Market value of equity ($m)	6,115	7,203	5,244
Sales ($m)	4,537	5,544	5,242
Number of employees	15,511	18,424	19,045
Firm and industry performance			
Stock return in the 12 months before the CEO turnover	21.65%	8.12%	-19.39%
[S.E.]	[0.43]	[1.18]	[1.61]
EW industry stock return in the 12 months before the CEO turnover [S.E.]	15.73% [0.21]	13.96% [0.66]	10.78% [1.14]
VW industry stock return in the 12 months before the CEO turnover [S.E.]	11.19% [0.16]	11.08% [0.53]	7.98% [0.91]

Panel C: CEO dismissal probabilities by industry performance quintile

	Quintile				
	1	2	3	4	5
EW industry stock return in the year before the CEO turnover [S.E.]	4.10% [0.26]	2.82% [0.22]	2.53% [0.21]	2.50% [0.20]	2.38% [0.20]
VW industry stock return in the year before the CEO turnover [S.E.]	3.62% [0.24]	2.98% [0.22]	2.60% [0.21]	2.72% [0.21]	2.41% [0.20]

Table II
Two-Stage Hazard 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 industry definitions follow the Fama and French (1997) classification into 48 industries. The second-stage Cox hazard regressions predict forced CEO turnover using the predicted values and the residuals from the first-stage regression as estimates of the peer-group component and the idiosyncratic component of company stock returns, respectively. A CEO is of retirement age if she is between 63 and 66 years old, and CEO equity ownership is high if she owns more than 5% of all outstanding shares. All *t*- and *z*-statistics are calculated with robust standard errors clustered at the industry level. *, **, and ** denote significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)
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	Firm stock return in year *t*-1	Firm stock return in year *t*-1
Constant	0.064	0.064	0.079	0.079
	[6.92]***	[6.92]***	[8.49]***	[8.49]***
EW industry stock return in year *t*-1	0.826	0.826		
	[20.60]***	[20.60]***		
VW industry stock return in year *t*-1			1.007	1.007
			[20.74]***	[20.74]***
R^2	0.16	0.16	0.14	0.14
	Firm stock return in year *t*-2	Firm stock return in year *t*-2	Firm stock return in year *t*-2	Firm stock return in year *t*-2
Constant	0.086	0.086	0.095	0.095
	[7.46]***	[7.46]***	[7.81]***	[7.81]***
EW industry stock return in year *t*-2	0.819	0.819		
	[19.27]***	[19.27]***		
VW industry stock return in year *t*-2			1.011	1.011
			[18.12]***	[18.12]***
R^2	0.13	0.13	0.11	0.11
Panel B: Second-stage hazard regressions of CEO dismissals on peer-group induced and idiosyncratic firm performance				
	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover
Idiosyncratic stock return in year *t*-1	-2.542	-2.723	-2.659	-2.780
	[-11.71]***	[-11.66]***	[-11.20]***	[-11.55]***
Industry-induced stock return in year *t*-1	-1.602	-1.940	-1.264	-1.662
	[-8.66]***	[-8.50]***	[-7.75]***	[-8.86]***
Idiosyncratic stock return in year *t*-2	-0.783	-0.796	-0.718	-0.764
	[-6.08]***	[-6.06]***	[-5.87]***	[-6.21]***
Industry-induced stock return in year *t*-2	-0.112	-0.587	-0.439	-0.692
	[-0.62]	[-1.97]**	[-2.09]**	[-2.45]**
CEO of retirement age	-0.900	-0.872	-0.892	-0.875
	[-4.20]***	[-4.09]***	[-4.14]***	[-4.10]***
CEO with high equity ownership	-0.787	-0.828	-0.827	-0.837
	[-3.59]***	[-3.57]***	[-3.70]***	[-3.62]***
Year fixed effects	No	Yes	No	Yes

Table III

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 and are reported in Panel A of Table II. Columns (1) and (2) use equal-weighted and columns (3) and (4) use value-weighted industry returns as the measure of peer group performance. The second-stage logit regressions shown below predict forced CEO turnover using the predicted values and the residuals from the first-stage regression as measures of the peer-group component and the idiosyncratic component of company stock returns, respectively. The industry definitions follow the Fama and French (1997) classification into 48 industries. A CEO is of retirement age if she is between 63 and 66 years old, and CEO equity ownership is high if she owns more than 5% of all outstanding shares. All *z*-statistics are calculated with robust standard errors clustered at the industry level. *, **, and ** denote significance at the 10%, 5%, and 1% level, respectively.

	EW Industry		VW Industry	
	(1)	(2)	(3)	(4)
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
Idiosyncratic stock return in year *t*-1	-2.594	-2.714	-2.648	-2.736
	[-10.64]***	[-10.40]***	[-9.98]***	[-10.08]***
Industry-induced stock return in year *t*-1	-1.383	-1.832	-1.212	-1.666
	[-8.10]***	[-7.41]***	[-7.79]***	[-8.33]***
Idiosyncratic stock return in year *t*-2	-0.721	-0.721	-0.684	-0.717
	[-4.80]***	[-4.60]***	[-4.91]***	[-4.83]***
Industry-induced stock return in year *t*-2	-0.261	-0.763	-0.541	-0.780
	[-1.27]	[-2.46]**	[-2.15]**	[-2.54]**
CEO of retirement age	-1.053	-1.036	-1.039	-1.033
	[-4.59]***	[-4.46]***	[-4.51]***	[-4.46]***
CEO with high equity ownership	-0.773	-0.798	-0.809	-0.806
	[-3.84]***	[-3.80]***	[-3.96]***	[-3.83]***
CEO tenure in months	-0.004	-0.004	-0.004	-0.004
	[-4.73]***	[-4.78]***	[-4.77]***	[-4.82]***
Constant	-3.323	-3.943	-3.292	-3.876
	[-24.70]***	[-13.93]***	[-25.50]***	[-13.80]***
Year fixed effects	No	Yes	No	Yes

Table IV
Implied Probabilities of a Forced CEO Turnover

The implied probabilities of a forced CEO turnover are calculated using the turnover regression coefficients from columns (1) and (3) of Table III. For the base case probability, all independent variables are set to their actual values and the associated implied probabilities are averaged across all observations. This implied probability equals the observed frequency of forced CEO turnovers in the data. The implied probability is then varied by setting the predicted component of firm performance equal to either its 10th or its 90th percentile value. The other independent variables remain at their actual values.

Implied probabilities of a forced CEO turnover for different levels of peer group performance		
	Per group performance measured as EW industry returns	Per group performance measured as VW industry returns
	Implied probability of a forced CEO turnover	
Base case	2.86%	2.86%
Peer-group induced stock return in *t*-1 at 10th percentile	4.14%	4.05%
Peer-group induced stock return in *t*-1 at 90th percentile	2.05%	2.12%
Peer-group induced stock return in *t*-2 at 10th percentile	3.03%	3.26%
Peer-group induced stock return in *t*-2 at 90th percentile	2.69%	2.52%
Peer-group induced stock return in both *t*-1 and *t*-2 at 10th percentile	4.36%	4.59%
Peer-group induced stock return in both *t*-1 and *t*-2 at 90th percentile	1.92%	1.88%

Table V
Single-Stage Hazard Regressions of Forced CEO Turnover on Firm and Industry Performance

The single-stage Cox hazard regressions predict forced CEO turnover using company and industry stock returns. The industry definitions follow the Fama and French (1997) classification into 48 industries. A CEO is of retirement age if she is between 63 and 66 years old, and CEO equity ownership is high if she owns more than 5% of all outstanding shares. All *z*-statistics are calculated with robust standard errors clustered at the industry level. *, **, and ** denote significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)
Single-stage hazard regressions of CEO dismissals on firm performance and industry performance				
	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover
Firm stock return in year *t*-1	-2.542	-2.723	-2.659	-2.780
	[-11.71]***	[-11.66]***	[-11.20]***	[-11.55]***
EW industry stock return in year *t*-1	0.777	0.646		
	[6.92]***	[5.93]***		
VW industry stock return in year *t*-1			1.405	1.126
			[7.65]***	[5.80]***
Firm stock return in year *t*-2	-0.783	-0.796	-0.718	-0.764
	[-6.08]***	[-6.06]***	[-5.87]***	[-6.21]***
EW industry stock return in year *t*-2	0.549	0.171		
	[4.27]***	[0.68]		
VW industry stock return in year *t*-2			0.282	0.073
			[1.37]	[0.26]
CEO of retirement age	-0.900	-0.872	-0.892	-0.875
	[-4.20]***	[-4.09]***	[-4.14]***	[-4.10]***
CEO with high equity ownership	-0.787	-0.828	-0.827	-0.837
	[-3.59]***	[-3.57]***	[-3.70]***	[-3.62]***
Year fixed effects	No	Yes	No	Yes

Table VI
Two-Stage Hazard Regressions of Forced CEO Turnover on Firm and Market Performance

The first-stage regressions use stock market returns to predict contemporaneous company stock returns. The second-stage Cox hazard regressions predict forced CEO turnover using the predicted values and the residuals from the first-stage regression as estimates of the peer-group component and the idiosyncratic component of company stock returns, respectively. A CEO is of retirement age if she is between 63 and 66 years old, and CEO equity ownership is high if she owns more than 5% of all outstanding shares. All *t*- and *z*-statistics are calculated with robust standard errors clustered at the industry level. *, **, and ** denote significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)
Panel A: First-stage regressions of firm performance on market performance		
	Firm stock return in year *t*-1	Firm stock return in year *t*-1
Constant	0.066	0.097
	[12.02]***	[15.55]***
EW market return in year *t*-1	0.824	
	[10.77]***	
VW market return in year *t*-1		0.961
		[9.82]***
R^2	0.08	0.07
	Firm stock return in year *t*-2	Firm stock return in year *t*-2
Constant	0.096	0.118
	[13.81]***	[15.18]***
EW market return in year *t*-2	0.781	
	[8.73]***	
VW market return in year *t*-2		0.927
		[7.29]***
R^2	0.06	0.04
Panel B: Second-stage hazard regressions of CEO dismissals on peer-group induced and idiosyncratic firm performance		
	Forced CEO turnover	Forced CEO turnover
Idiosyncratic stock return in year *t*-1	-2.685	-2.718
	[-12.27]***	[-12.18]***
Market-induced stock return in year *t*-1	-0.873	-0.462
	[-3.65]***	[-1.80]*
Idiosyncratic stock return in year *t*-2	-0.747	-0.709
	[-6.08]***	[-5.84]***
Market-induced stock return in year *t*-2	0.553	-0.499
	[2.60]***	[-1.50]
CEO of retirement age	-0.884	-0.879
	[-4.14]***	[-4.06]***
CEO with high equity ownership	-0.832	-0.858
	[-3.78]***	[-3.77]***

Table VII
Two-Stage Hazard 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 II. Columns (1) and (2) use equal-weighted and columns (3) and (4) use value-weighted industry returns as the measure of peer group performance. The second-stage Cox hazard regressions shown below predict forced CEO turnover using the predicted values and residuals from the first-stage regression as estimates of the peer-group component and the idiosyncratic component of company stock returns, respectively. Underperformers (outperformers) are defined as firms with negative (positive) firm-specific residual performance in the first-stage regression. Industry-induced performance in year *t*-1 is interacted with underperformer (outperformer) dummies based on residual performance in year *t*-1, and industry-induced performance in year *t*-2 is interacted with underperformer (outperformer) dummies based on the summed residual performance in years *t*-1 and *t*-2. The industry definitions follow the Fama and French (1997) classification into 48 industries. A CEO is of retirement age if she is between 63 and 66 years old, and CEO equity ownership is high if she owns more than 5% of all outstanding shares. All standard errors are clustered at the industry level. The baseline hazards are set to one for the marginal effects calculations in Panel B. *, **, and ** denote significance at the 10%, 5%, and 1% level, respectively.

	EW Industry		VW Industry	
	(1)	(2)	(3)	(4)
Panel A: Second-stage hazard regressions of CEO dismissals on peer-group induced and idiosyncratic firm performance				
	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover
Idiosyncratic stock return in year *t*-1	-2.770	-2.949	-2.819	-2.933
	[-13.48]***	[-13.57]***	[-11.96]***	[-12.22]***
Industry-induced stock return in year *t*-1 for outperformers	0.027	-0.355	-0.022	-0.420
	[0.07]	[-0.79]	[-0.04]	[-0.72]
Industry-induced stock return in year *t*-1 for underperformers	-1.829	-2.160	-1.478	-1.860
	[-10.53]***	[-10.24]***	[-8.30]***	[-9.51]***
Idiosyncratic stock return in year *t*-2	-0.855	-0.879	-0.737	-0.784
	[-5.57]***	[-5.55]***	[-5.50]***	[-5.73]***
Industry-induced stock return in year *t*-2 for outperformers	0.346	-0.016	-0.240	-0.461
	[0.94]	[-0.03]	[-0.73]	[-1.26]
Industry-induced stock return in year *t*-2 for underperformers	-0.188	-0.679	-0.460	-0.715
	[-1.05]	[-2.45]**	[-2.06]**	[-2.36]**
CEO of retirement age	-0.892	-0.865	-0.888	-0.869
	[-4.15]***	[-4.05]***	[-4.12]***	[-4.07]***
CEO with high equity ownership	-0.796	-0.836	-0.834	-0.845
	[-3.69]***	[-3.67]***	[-3.80]***	[-3.72]***
Year fixed effects	No	Yes	No	Yes

	EW Industry		VW Industry	
	(1)	(2)	(3)	(4)
Panel B: Marginal effects of peer-group induced performance on CEO dismissals for underperformers and outperformers				
	Industry-induced stock return in year *t*-1	Industry-induced stock return in year *t*-1	Industry-induced stock return in year *t*-1	Industry-induced stock return in year *t*-1
Marginal effect for outperformers	0.02	-0.24	-0.02	-0.27
	[0.07]	[0.79]	[0.04]	[0.72]
Marginal effect for underperformers	-1.04	-1.05	-0.82	-0.91
	[10.53]***	[10.24]***	[8.30]***	[9.51]***
Difference in marginal effects	1.06	0.81	0.80	0.64
	[3.55]***	[2.76]***	[1.99]*	[1.62]
	Industry-induced stock return in year *t*-2	Industry-induced stock return in year *t*-2	Industry-induced stock return in year *t*-2	Industry-induced stock return in year *t*-2
Marginal effect for outperformers	0.24	-0.01	-0.15	-0.26
	[0.94]	[0.03]	[0.73]	[1.26]
Marginal effect for underperformers	-0.12	-0.35	-0.28	-0.38
	[1.05]	[2.45]**	[2.06]**	[2.36]**
Difference in marginal effects	0.36	0.34	0.12	0.12
	[1.39]	[1.39]	[0.64]	[0.69]

Table VIII

Two-Stage Hazard Regressions of Forced 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 II. Columns (1) and (2) use equal-weighted and columns (3) and (4) use value-weighted industry returns as the measure of peer group performance. The second-stage Cox hazard regressions shown below predict forced CEO turnover using the predicted values and residuals from the first-stage regression as estimates of the peer-group component and the idiosyncratic component of company stock returns, respectively. The second-stage regressions allow for different effects of idiosyncratic performance on CEO turnover depending on whether industry performance over the prior two years is in the bottom, middle, or top third of all observations. The industry definitions follow the Fama and French (1997) classification into 48 industries. A CEO is of retirement age if she is between 63 and 66 years old, and CEO equity ownership is high if she owns more than 5% of all outstanding shares. All standard errors are clustered at the industry level. The baseline hazards are set to one for the marginal effects calculations in Panel B. *, **, and ** denote significance at the 10%, 5%, and 1% level, respectively.

	EW Industry		VW Industry	
	(1)	(2)	(3)	(4)
Panel A: Second-stage hazard regressions of CEO dismissals on peer-group induced and idiosyncratic firm performance				
	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover	Forced CEO turnover
High industry performance: Idiosyncratic stock return in year *t*-1	-1.988 [-7.83]***	-2.281 [-7.30]***	-2.480 [-9.06]***	-2.616 [-9.13]***
Medium industry performance: Idiosyncratic stock return in year *t*-1	-2.552 [-7.85]***	-2.540 [-7.48]***	-3.029 [-9.95]***	-3.017 [-9.30]***
Low industry performance: Idiosyncratic stock return in year *t*-1	-3.089 [-16.74]***	-3.216 [-17.24]***	-2.721 [-7.89]***	-2.800 [-8.26]***
Industry-induced stock return in year *t*-1	-1.047 [-4.42]***	-1.457 [-4.50]***	-1.143 [-5.59]***	-1.495 [-5.87]***
High industry performance: Idiosyncratic stock return in year *t*-2	-0.455 [-2.17]**	-0.425 [-1.89]*	-0.561 [-3.29]***	-0.601 [-3.57]***
Medium industry performance: Idiosyncratic stock return in year *t*-2	-1.418 [-6.37]***	-1.492 [-6.63]***	-1.218 [-4.52]***	-1.211 [-4.53]***
Low industry performance: Idiosyncratic stock return in year *t*-2	-0.652 [-4.57]***	-0.672 [-4.64]***	-0.546 [-3.69]***	-0.592 [-3.76]***
Industry-induced stock return in year *t*-2	0.204 [0.90]	-0.227 [-0.62]	-0.329 [-1.35]	-0.566 [-1.65]*
CEO of retirement age	-0.890 [-4.16]***	-0.867 [-4.07]***	-0.881 [-4.12]***	-0.866 [-4.07]***
CEO with high equity ownership	-0.795 [-3.73]***	-0.825 [-3.63]***	-0.836 [-3.64]***	-0.840 [-3.54]***
Year fixed effects	No	Yes	No	Yes

	EW Industry		VW Industry	
	(1)	(2)	(3)	(4)
Panel B: Marginal effects of idiosyncratic performance on CEO dismissals given high and given low industry performance				
	Idiosyncratic stock return in year *t*-1	Idiosyncratic stock return in year *t*-1	Idiosyncratic stock return in year *t*-1	Idiosyncratic stock return in year *t*-1
Marginal effect if industry performance high	-1.36 [7.83]***	-1.32 [7.30]***	-1.48 [9.06]***	-1.39 [9.13]***
Marginal effect if industry performance low	-2.11 [16.74]***	-1.85 [17.24]***	-1.63 [7.89]***	-1.49 [8.26]***
Difference in marginal effects	0.75 [4.78]***	0.53 [3.20]***	0.15 [0.64]	0.10 [0.48]
	Idiosyncratic stock return in year *t*-2	Idiosyncratic stock return in year *t*-2	Idiosyncratic stock return in year *t*-2	Idiosyncratic stock return in year *t*-2
Marginal effect if industry performance high	-0.31 [2.17]**	-0.25 [1.89]*	-0.34 [3.29]***	-0.32 [3.57]***
Marginal effect if industry performance low	-0.45 [4.57]***	-0.39 [4.64]***	-0.33 [3.69]**	-0.32 [3.76]*
Difference in marginal effects	0.13 [0.72]	0.14 [0.84]	-0.01 [0.08]	-0.00 [0.04]

[1] See, for example, Hirshleifer and Thakor (1994, 1998), Hermalin and Weisbach (1998, 2003), Warther (1998), Adams and Ferreira (2007), and Taylor (2010).

[2] See Eisfeldt and Kuhnen (2013) for a model in which industry downturns are associated with changing CEO skill requirements.

[3] For more evidence on whether the observed turnover patterns are optimal, we examine the stock price reactions to CEO turnover announcements. We find no evidence of different 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. These results are presented in an Internet Appendix available in the online version of the article on the Journal of Finance website.

[4] In contemporaneous work, Kaplan and Minton (2012) 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 (but not external) CEO turnover to be significantly related to firm-specific performance, industry performance, and the performance of the overall market.

[5] See, for example, Huson, Parrino, and Starks (2001), Holmström and Kaplan (2001), Hermalin (2005), and Kaplan (2008).

[6] For the empirical evidence see Murphy (1985), Coughlan and Schmidt (1985), Antle and Smith (1986), Gibbons and Murphy (1990), Janakiraman, Lambert, and Larker (1992), Garen (1994), Aggarwal and Samwick (1999a, 1999b), Murphy (1999), and the reviews in Bebchuk, Fried, and Walker (2001) and Frydman and Jenter (2010).

[7] See Barro and Barro (1990), Himmelberg and Hubbard (2000), Core and Guay (2001), Jin (2002), Jenter (2002), Core, Guay, and Larcker (2003), Garvey and Milbourn (2003), Hall and Murphy (2003), and Oyer (2004).

[8] Even though CEO turnover could serve as punishment in a pay-for-performance scheme, threatening to use turnover in this manner is not likely to be credible as the board would not find it optimal to actually fire the CEO after bad performance if the CEO is still the best person for the job.

[9] An earlier version of this paper contains a formal derivation of this result (Jenter and Kanaan (2008)). For earlier and much more general treatments, see Holmström (1982) and Gibbons and Murphy (1990).

[10] See, for example, Janakiraman, Lambert, and Larcker (1992) and Albuquerque (2009).

[11] Corporate boards have access to more precise measures of peer group performance than the market and industry benchmarks we employ. Using a less informative benchmark biases us in favor of accepting the relative performance evaluation hypothesis.

[12] In the notation of equations (1) and (2), complete filtering of industry performance from firm performance implies that $-\gamma'_1/\gamma'_2 = \beta_1$. Using the estimates from column (1) in Tables II and V, we have $-\gamma'_1/\gamma'_2$ = 0.777/2.542 = 0.306 but β_1 = 0.826. Hence, theory predicts two to three times more intense filtering than observed in the data.

[13] Aggarwal and Samwick (1999a) show that optimal compensation contracts may put positive weight on both own-firm and rival-firm performance in an environment with strategic interactions between firms.

[14] The results are available in the Internet Appendix.

[15] This idea is explored in more detail in Eisfeldt and Kuhnen (2013).

[16] Including main effects, that is, dummy variables for CEOs who have underperformed their peer group, leaves all other coefficients unchanged and produces insignificant coefficients on the dummy variables.

[17] We also test whether CEO *compensation* is more sensitive to performance during bad times than good times. We find no evidence that the effect of firm-specific performance on CEO compensation is stronger when industry performance is low.

[18] 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 et al. (2001) find that experimental subjects underweight group size when assessing the ability of group leaders to inspire coordination outcomes. Wolfers (2002) shows that U.S. voters reward state governors for economic fluctuations that are unrelated to gubernatorial actions.

[19] An earlier version of this paper also examines whether the lack of relative performance evaluation in CEO turnovers may be caused by limited investor attention (see Jenter and Kanaan (2008)). The idea is that otherwise passive shareholders scrutinize CEOs only after their attention has been triggered by bad performance. However, we find that the effects of peer performance on CEO turnover persist even subsequent to very low returns. Furthermore, relative performance evaluation is also rejected for large and high-profile firms, which are likely to be continuously monitored by the press and investors.