The impact of firm prestige on executive compensation[☆]

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Abstract

We show that CEOs of prestigious firms earn less. Total compensation is on average 8% lower for firms listed in Fortune's ranking of America's most admired companies. We suggest that CEOs are willing to trade off status and career benefits from working for a publicly admired company against additional monetary compensation. Our identification strategy is based on matched sample analyses, difference-in-differences regressions, and a regression discontinuity design. We perform several robustness checks and exclude many alternative explanations, including that firm prestige just proxies for better corporate governance, or for increased exposure of the pay-setting process to media attention.

JEL classifications: G30, M52

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1. Introduction

> "To excel in any profession, in which but few arrive at mediocrity, it is the most decisive mark of what is called genius, or superior talents. The public admiration which attends upon such distinguished abilities makes always a part of their reward." Adam Smith, Wealth of Nations, chapter 10.I.

In this paper we propose that CEOs derive benefits from the public recognition of their firms and that this allows prestigious firms to pay their CEOs less. Specifically, we hypothesize that CEOs are willing to trade off part of their compensation against an increase in the public recognition of their firms. This idea complements Malmendier and Tate (2009), who show that "superstar CEOs" can extract additional pay from the firms they work for, whereas we show that "superstar firms" can pay their CEOs less. More broadly, prestige seems to be a valued resource, which enhances the bargaining power of the party who owns it, in our case the firm.

We show that CEOs of publicly listed firms are willing to accept lower monetary compensation if they work for prestigious firms: CEOs of firms ranked in the top 100 of Fortune's list of "America's Most Admired Companies" earn on average 8% less compared to CEOs of firms outside this ranking after controlling for a range of other factors. While CEOs do not explicitly draw attention to their compensation in public statements, some of them do highlight the prestige of their firms while accepting below-average compensation packages. For example, the CEO of Nordstrom Inc., a company which is listed frequently in the Fortune Most Admired Companies ranking, expressed his pride in working for this company in his first letter to customers, shareholders, and employees in the annual report of 2000 by stating that "Everyone at Nordstrom is proud of this company's accomplishments over the last century" (p. 13). At the same time, our data indicate that Nordstrom's CEO accepted about 11% lower pay growth compared to the companies' peer group.

The willingness to trade off prestige for compensation is not unique to CEOs: There is similar anecdotal evidence for corporate managers, MBA graduates, university professors,

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and university presidents (see Appendix A.1 for more anecdotal evidence). For example, Auger et al. (2013) survey MBA graduates and find that corporate reputation accounts for 7.5% of the value of a job offer; other industry sources put the discount for recruiting corporate managers at 10% for reputable brands. Our paper shows that CEOs work for about 8% less at prestigious firms, which aligns closely with these more anecdotal estimates.

We highlight two explanations for the effect we observe. First, CEOs could give up compensation at a prestigious firm because they value the enhancement in their social status from being associated with a firm that enjoys public admiration. The notion that wages involve compensating differentials not only for the attractiveness of a job, but also for the prestige associated with it goes back at least to Adam Smith and Max Weber.[1] Second, CEOs could value the career benefits from being associated with a prestigious firm and anticipate increased opportunities to obtain higher-paid appointments after their tenure with a prestigious firm.

We investigate the relation between firm prestige and CEO pay for the period from 1992 to 2010 and use a firm's appearance in Fortune's list of "America's Most Admired Companies" (MAC) to define firm prestige. This ranking is suitable, because it possesses the following characteristics: (1) it is positional and allows for an easy comparison of different firms, (2) it is scarce by construction, and (3) desirable for CEOs because it is widely covered in the news media and based on judgments of CEOs' peer group.

As a first step, we provide evidence for a negative pay-prestige relation based on panel regressions with a large range of controls and fixed effects. We further establish causality based on several matching estimators and difference-in-differences regressions. These approaches generate quantitatively similar estimates. When we separately investigate the effect of entries to and exits from the ranking in the difference-in-differences analysis, we

[1] See the passage from Adam Smith above. Weber (as cited in Weiss and Fershtman (1998), p. 804) develops the notion that status and monetary compensation are distinct categories of rewards and that status can partially substitute for monetary compensation.

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find that the efffect is concentrated in entries to the ranking. In addition, we also use a regression discontinuity design (RDD) to identify a causal link between ranking inclusion and subsequent changes in CEO pay. The results using RDD also suggest a pay drop, but the effect is much larger compared to what we find with the other approaches.

In the next step, we analyze potential channels that could drive our main effect. In particular, we investigate whether the negative pay-prestige relation is driven by status concerns and career concerns, as well as alternative explanations. To investigate whether preferences for status contribute to our main effect, we match company and CEO-level data with state-level data from the general social survey (GSS), which provides information about the status preferences of the population surveyed in the state of a firm's headquarters. We find that the main effect we show is stronger for firms headquartered in states where the population has stronger status preferences, which supports the social-status hypothesis. According to the career-concerns hypothesis, CEOs believe that the managerial labor market interprets working for a prestigious firm as a signal of CEO talent. We collect data on CEOs' subsequent employments and find that leading a prestigious firm improves CEOs' career prospects: They are more likely to obtain board seats and executive directorships compared to CEOs of non-prestigious firms.

Next, we hypothesize that strong and independent boards are better able to convince CEOs to accept lower pay for the benefit of working for a prestigious firm, whereas weak boards leave the prestige benefits to CEOs as a rent. In line with this view, we find that the effect of prestige on compensation is stronger in firms with independent compensation committees and small boards. This governance effect is independent of the channel, as strong boards could pay CEOs less for either career benefits or status benefits associated with their current position.

We investigate several other candidate explanations that could imply a negative relation between firm prestige and CEO compensation. For example, being ranked could expose firms to public limelight and increase the potential loss of reputation for directors who have awarded excessively high compensation packages before the firm enters the ranking, leading them to reverse executive pay policy. Results do not provide support for such a limelight effect.

The analysis in this paper contributes to several strands of the literature. The most closely related literature analyzes the impact of winning prestigious business awards on CEOs' compensation. Malmendier and Tate (2009) find that "superstar CEOs" who win prestigious business awards extract compensation benefits. Our results complement theirs by showing that "superstar firms," defined as firms that enter prestigious business rankings, pay their CEOs less. While Malmendier and Tate (2009) find that their effect is stronger in poorly-governed firms, the effect we establish is stronger in well-governed firms.

This paper identifies how non-monetary characteristics of the firm affect CEO compensation. The only other papers we are aware of that make a similar point are Deng and Gao (2013) and Otto (2014). Deng and Gao (2013) find that CEOs are paid more if firms' headquarters are located in a polluted or high-crime environment. Otto (2014) shows that boards may pay CEOs less if they realize that the CEO is optimistic and likely to overvalue the firm's equity-related compensation. None of these papers refers to preferences for firm prestige, which is a separate non-monetary determinant of CEO pay.

Huberman et al. (2004) conduct an experiment in which they show that individuals are willing to give up monetary rewards for being celebrated as a "winner," even though they have no monetary or other benefit from their winner-status within the experiment or outside. However, to the best of our knowledge, ours is the first paper to provide field evidence on the trade-off between public recognition and monetary rewards.

More generally, this paper contributes to the question of what explains the cross-sectional variation in CEO compensation. The vast literature on executive compensation has made significant progress on this question in recent years, for example by analyzing the market for CEO talent (Eisfeldt and Kuhnen (2013)), or by identifying the importance of corporate governance as a factor that explains pay levels (Hartzell and Starks, 2003; see also the surveys by Murphy 1999, 2013; Frydman and Jenter 2010). More recent papers on pay differentials include Custodio et al. (2013) who find a pay premium for CEOs with more general human capital, and Fernandes et al. (2013), who investigate why US CEOs earn more than CEOs in other countries. Still, conventional theories and existing empirical examinations of executive compensation seem to be limited in their ability to explain the variation in CEO pay (see, e.g., Yermack (1995)), suggesting that many as yet unidentified factors influence compensation.

Finally, the debate on whether contracting about executive compensation conforms to the tenants of the efficient contracting paradigm remains inconclusive, in particular regarding the level of pay, because it is difficult to measure CEOs' outside options to decide whether CEOs extract rents, as suggested by, e.g., Yermack (2009) and Bebchuk et al. (2011), or whether they simply earn a competitive wage in the managerial labor market. A number of theoretical contributions emphasize the relevance of the participation constraint for compensation contracts, but empirical inferences on this constraint are only indirect (see Oyer, 2004; Gabaix and Landier, 2008; Terviö, 2009). Our results suggest that the participation constraint of the CEO matters in well-governed firms, an observation that provides support for efficient contracting theories for this subset of companies.

2. Data and summary statistics

This section describes the data on firm prestige in Section (2.1) and on executive compensation in Section (2.2), which also provides summary statistics.

2.1. Data on firm prestige

We use Fortune's Most Admired Companies ranking (MAC) as our definition of firm prestige. This ranking is based on surveys among approximately 15,000 senior executives, outside directors and financial analysts in the U.S and published annually in widely-read business magazines and newspapers such as Fortune, the New York Times, and the Wall Street Journal. To create an overall ranking of the most admired companies within the U.S., Hay Group, on behalf of Fortune, asks survey participants to select ten companies out of the Fortune 1000 which they admire most based on eight different attributes using a Likert scale ranging from zero (poor) to ten (excellent). The attributes that determine the ranking have not been changed since the mid-1980s. They comprise the following items: (1) quality of management; (2) quality of products or services; (3) financial soundness; (4) innovativeness; (5) long-term investment; (6) ability to attract, develop, and keep talented people; (7) community and environmental responsibility; and (8) wise use of corporate assets. Only these items are provided on the survey, and no additional definitions or interpretations of their meaning are given. Fortune encourages survey respondents to rank firms according to their knowledge of the firm or based on what they have heard about the firm. Thus, interpreting the meaning of these attributes is left to survey respondents. A firm's overall ranking is computed based on the average of the attribute scores the firm obtained in the survey.

There are several advantages of using the MAC ranking to define prestige in our context. First, it is unlikely that a company can actively influence inclusion in this ranking on a short term basis. Survey questions and variables cannot be easily influenced as they are determined by a third party (i.e., Hay Group on behalf of Fortune) and do not change over time. Also, it is impossible for a company to find out the names of all executives, directors, and financial analysts who are surveyed and to influence them correspondingly. Second, it is unlikely that compensation practices influence rank inclusion. None of the attributes explicitly asks about executive compensation, and it is rather unlikely that survey respondents (that is, other top executives and directors) would admire another company for paying their top executives less.

We manually collect MAC rankings from printed editions of Fortune magazines from 1990 to 2010. Overall, the number of companies included in the ranking varies from 305 to 593. This variation is mainly driven by the number of industries included in the pool. Even though most industries are covered by the MAC ranking, a large fraction of ranked companies come from industries such as manufacturing, business equipment, and materials. To obtain a first impression of the firms (not) appearing in the MAC ranking, Appendix A.2

contains a list of firms that appear either regularly or never in the MAC ranking, respectively.

2.2. Compensation data and descriptive statistics

Data on CEO compensation are obtained from ExecuComp. ExecuComp contains the value of total annual compensation for the top five executives of each company in the S&P1500 (ExecuComp variable *tdc1*). Due to a major change of some ExecuComp variables in 2006, there are several data adjustments regarding the computation of variables like option values and restricted stock that we need to apply. As a measure of the Black Scholes value of stock options granted to a CEO in a given year (options), we use data item *opt_blk_valu* before 2006 and its post-2006 equivalent, *option_awards_fv*, afterwards. We follow Walker (2011) and adjust the total pay variable *tdc1* from its pre-2006 format to the new format: Before 2006, ExecuComp's data item *tdc1* was supposed to capture the total compensation given to the CEO in that year, but in fact it did not measure the ex-ante value of performance shares. Therefore, we first subtract the value of long term incentive plans (ExecuComp variable *ltip*), which measures the ex-post value of performance shares from *tdc1*. Then, we multiply the target number of performance shares granted to the CEO (ExecuComp variable *shrtarg*) by a firm's year-ending stock price to compute the ex-ante value of performance shares in a given year, which is added to *tdc1*. For the post-2006 period we use *tdc1* as provided in ExecuComp.

Similarly, the pre-2006 data item *rstkgrnt* (restricted stock) indicates the value of non-performance contingent stock awards but not that of performance shares. For the period 2006 to 2010, a different data item, *stock_awards_fv*, measures all stock awards (restricted stock plus performance shares). We construct a comparable variable for the pre-2006 period by adding the value of performance shares to data item *rstkgrnt*.[2]

Firms in the MAC ranking are selected from the Fortune 1000 list, which is compiled

[2]In one robustness check, we restrict our sample to the pre-2006 period, which somewhat reduces significance (t-statistic of 1.92 instead of 2.10 in column (1) of Table 2).

based on revenues, whereas the ExecuComp universe is based on the S&P 1500 and therefore includes smaller firms. We do not have access to the Fortune 1000 list but restrict the ExecuComp sample to the 1,000 largest firms based on revenues in each year to ensure that non-ranked firms are sufficiently similar to firms that appear in the MAC ranking. The Fortune MAC ranking does not cover all industries that are included in ExecuComp. Therefore, we drop all firm-year observations from non-ranked industries to again make sure that our sample only comprises firms that are sufficiently similar to the firms that appear in the MAC ranking. Overall, our main sample consists of 12,707 firm-year observations during 1992 to 2010 and covers 1,711 unique firms.

Descriptive statistics for compensation data as well as for rankings and control variables are provided in Panel A of Table 1. Mean total compensation in our sample is about 5.9 million USD, while median compensation is lower at 3.7 million USD. Overall, descriptive statistics on compensation data are slightly higher than in previous work on CEO compensation based on ExecuComp data (e.g., Peters and Wagner (2014)), because we restrict the sample to the largest 1,000 firms. Table 1 indicates that 11% of all firm-year observations are from firms that appear in the top 100 of the MAC ranking.

3. The impact of firm prestige on CEO pay

In this section we investigate our main conjecture that firm prestige is negatively related to CEO compensation by adopting alternative empirical strategies. If we could run a perfect experiment, we would compare CEO pay of firms that are ranked as one of the top 100 most admired companies by Fortune in a particular year to CEO pay of the same firm in the same year, had it not been ranked. This counter-factual situation is not observed. Therefore, we have to rely on second-best experiments, in which we compare a firm-CEO combination for a prestigious firm to another, sufficiently similar firm-CEO pair for a non-prestigious firm. These second-best experiments invariably involve a trade-off: If the comparison is very close we have to focus on a small subset of our sample, whereas analyses based on larger samples sometimes rely on control firms that differ more from the prestigious firms. Thus, we effectively face a trade-off between establishing causality for a small set of comparable firms and the generalizability of our results to a large sample of firms. We follow several empirical strategies, which resolve this trade-off in different ways. We start with panel regressions including a saturated set of fixed effects in Section 3.1. Section 3.2 presents results based on matching and Section 3.3 presents results of a difference-in-differences analysis. Section 3.4 provides additional evidence based on a regression discontinuity design.

3.1. Panel regressions with firm fixed effects

We begin by presenting panel regressions of the logarithm of CEO compensation on *Prestige* and a range of control variables and fixed effects. This approach maximizes the generalizability of our results by covering firms in the top 100 MAC ranking and all control observations from the largest 1,000 firms belonging to ranked industries in ExecuComp. Including a range of fixed effects in addition to a large set of control variables allows us to control for unobserved heterogeneity. However, this approach relies critically on the assumption that firm-CEO matches outside the top 100 in our sample are comparable to the ranked observations after removing the influence of these controls through regression analysis.

The main dependent variable is the logarithm of total compensation and the independent variable of interest, $Prestige_{i,t-1}$, is a dummy variable that is equal to one if firm i appears in the top 100 MAC ranking in the previous year $t-1$, and zero otherwise.[3] We use the representation of firm prestige as a dummy variable, because it is an easy-to-interpret summary of the data. We provide robustness checks on the chosen cut-off of 100 in Section 5 below. Specifically, we estimate

$$y_{it} = \alpha_0 + \gamma Prestige_{i,t-1} + \beta' X_{i,t-1} + \alpha_i + \alpha_k \times \alpha_t + \varepsilon_{it}. \tag{1}$$

[3] In the following and for brevity, we use the term “ranked” and “ranked in the top 100” synonymously.

Here, y_{it} is the logarithm of total compensation paid to the CEO of firm i in year t, $X_{i,t-1}$ is a vector of control variables, α_i denotes firm fixed effects, and α_k denotes industry fixed effects (Fama French 48 classification), which we interact with year fixed effects α_t. Closer inspection of the MAC ranking reveals some time-varying industry clustering, so we include time-varying industry effects. The regression also includes size deciles based on lagged market value and an interaction of firm size and industry fixed effects. Hence, identification in Eq. (1) comes from within-firm variation after subtracting time-varying industry and size effects, which provides a high hurdle for identifying the impact of firm prestige on CEO pay. Standard errors are clustered at the firm level.

We include several control variables measured as of time $t-1$. Since better performing managers are paid more, we include lagged stock returns and return on assets as control variables. We use the lagged logarithms of firm sales and total assets as measures for firm complexity and firm size (e.g., Baker et al. (1988)), which are known to influence executive compensation (e.g., Murphy (1999)). Controlling for size is generally critical in compensation regressions, but even more so in our setting, since larger firms are more likely to be included in the MAC ranking. To account for the fact that *Prestige* could still proxy for higher-order influences of firm size that are not captured by the linear influence of our control variables, we additionally include size splines based on market value in our regressions.

We need to control for growth opportunities, since CEOs of firms with many growth opportunities may give up some current compensation in return for higher expected future compensation. We therefore use past sales growth and the market-to-book ratio to control for growth opportunities, and follow Core et al. (1999) in using the average of the market-to-book ratio over the last five years. The market-to-book ratio also controls for potential confounding effects between *Prestige* and the visibility of "glamor firms" (Rau and Vermaelen (1998)).

Principal-agent theory suggests that compensation depends critically on firm risk, because CEOs demand a risk premium for accepting higher firm risk (e.g., Lambert et al.

(1991)). We therefore include the standard deviation of a firm's daily stock returns in the previous year. We also control for CEO ownership to ensure that our main effect is not driven by the negative relation between CEOs' equity holdings and compensation (e.g., Core et al. (1999)). Finally, we include the CEO's tenure, because we expect CEOs with longer tenure to have more firm-specific human capital, which may affect their pay. A detailed description of the main variables is provided in Appendix A.3. Note that differences in time-invariant firm characteristics, such as its founding status (i.e., family firms vs. non-family firms), are accounted for by including firm fixed effects in the regression. Correlation checks reveal that multicollinearity is not an issue. The highest correlation between the different size proxies (sales, total assets, and market value) is 0.8, all other correlations are below this value.

Table 2 presents the estimation results. We find that firm prestige is significantly negatively related to CEO compensation in all regressions. Column (1) is our baseline specification as described in Eq. (1) and suggests that total compensation is on average 7.9% lower if a CEO works for a prestigious firm (based on the point estimate of -8.2%, using $e^{\beta} - 1$). The impact of prestige on compensation is also economically large if compared to the impact of other variables studied in the literature (e.g., Core et al. (1999)) and corresponds to about half a million USD in absolute terms for the average CEO in our sample.

The coefficient estimates for the control variables are mostly insignificant, showing that there is too little time-series variation in these variables to show an effect in a regression with a large array of fixed effects, including firm fixed effects. As expected, firm performance measured by stock returns is significantly positively related to CEO pay. In contrast, total assets show a negative coefficient, which probably picks up higher-order effects and is difficult to interpret on its own given the range of other size controls we include. The coefficient on the market-to-book ratio is also negative, similar to the findings in Otto (2014) for a recent sample. After controlling for all other factors, CEOs of firms with many growth opportunities accept lower pay, possibly because they expect to profit from higher compensation in the future.

In column (2), we raise the hurdle even further and add an interaction of CEO and firm fixed effects. The point estimate declines numerically to -5.7%, but still remains significant at the 10% level. The reduction of the coefficient may result because our main effect is stronger for CEOs who are newly appointed to a prestigious firm (a conjecture we test below) and is less pronounced in fixed CEO-firm pairs.

In column (3), we estimate Eq. (1) again, but omit firm fixed effects. Instead, we include the lagged dependent variable. Core et al. (1999) show that there is a strong relation between current and lagged CEO pay. This specification may help us to better control for unobserved time-varying firm and CEO characteristics. The estimate of the effect is -9.0%, which is numerically close to our baseline specification, but more precisely estimated; it becomes statistically somewhat stronger and reaches the 1% significance level. This is unsurprising, given that the fixed effects absorb a significant portion of variation in pay. In this specification, *Sales*, *Firm Risk, and CEO Ownership* become significant, whereas *Total Assets* becomes insignificant. Note, that the lower level of compensation for CEOs of prestigious firms does not necessarily imply that money is taken away from CEOs if a firm is more prestigious. It only implies that growth rates are lower at prestigious firms.

Including lagged dependent variables in a dynamic panel might induce biased estimates. We therefore apply generalized method of moments (GMM) estimation based on Arellano and Bond (1991) and Arellano and Bover (1995) in columns (4) and (5) and again find a very similar effect as in the previous columns, with point estimates of -7.8% and -7.7%, respetively, which are significant at the 10% level. We implement GMM by following the procedure of Wintoki et al. (2012). The extensive specification tests associated with this procedure can be found in Tables IA.2 to IA.4 of the Internet Appendix. It turns out that *Total Pay* is very persistent and the model is dynamically complete only if we include at least five lags of the dependent variable. We perform all three tests suggested by Wintoki et al. (2012) for the strict exogeneity of the control variables: (1) regressing each control variable on the first lag of all control variables and lagged *Total Pay*; (2) repeating the same test for the first difference of each control variable; (3) and regressing *Total Pay* on the control variables and the first lead of each control variable. We then instrument for all control variables that are not strictly exogenous according to these tests, which is nearly all control variables except market value, stock returns, CEO ownership, and return on assets. Interestingly, we cannot reject the hypothesis that *Prestige* is strictly exogenous. The specification in column (5) includes industry and year fixed effects and passes all standard specification tests. Column (6) corresponds most closely to Eq. (1) and interacts the industry fixed effects with year effects. This specification passes the Sargan test. It fails the Hansen test, presumably because the large number of moments arising from the interacted fixed effects leads to an imprecise estimation of the weighting matrix, which is singular. The coefficient estimates are almost identical for both GMM specifications.

Taken together, results from Table 2 show that there is a negative impact of firm prestige on CEO pay. CEOs working for prestigious firms earn on average 8% less than CEOs working for non-prestigious firms.

3.2. Matched sample analysis

While results in the previous section hold in a large sample of firms and generalizability of our results should be high, there might still be unobserved heterogeneity that can only be taken care of if the set of control firms is narrowed down to a more comparable subsample. Therefore, in this section we construct a set of control groups to which we apply several matching estimators. Matching improves on the panel regressions above by not assuming the global validity of the regression function. Moreover, the covariates displayed in Table 2 could interact and then the linear regression approach may well turn out to be inadequate.

The main challenge to applying matching estimators is that assignment to the "treatment" condition - inclusion in the top 100 of the MAC ranking in a particular year - is by design not random and is most likely based on criteria such as performance and growth opportunities of the firm, which are also relevant for setting CEO pay. In Panel B of Table 1, we compare the values of the control variables for non-prestigious and prestigious firms.

In columns (1) to (5), we use the full sample for comparison, while columns (6) to (10) are based on the matched sample described below. We perform standard t-tests, but mainly follow Imbens and Wooldridge (2009) and Imbens and Rubin (2015), who argue that for the purposes of matching, comparisons should be based on normalized differences, defined as

$$\Delta_X = \frac{\overline{X}_P - \overline{X}_{NP}}{\sqrt{S_P^2 + S_{NP}^2}}. \tag{2}$$

Here, $\overline{X}_P = \frac{1}{N_P} \sum_{i,t} X_{it}$ and $\overline{X}_{NP} = \frac{1}{N_{NP}} \sum_{i,t} X_{it}$ are the sample means of the covariates for prestigious firms ("P") and non-prestigious matching firms ("NP"), respectively; S_P^2 and S_{NP}^2 are the corresponding estimates of the variances. The rationale for favoring Δ_X over t-statistics is that it captures whether the means of two distributions are sufficiently different to pose problems for statistical inference. Normalized differences do not depend on sample size, whereas t-statistics do, and sample size should not have an impact on how we evaluate the quality of matching. We report the normalized differences, Δ_X, in columns (3) and (8) of Panel B in Table 1. Imbens and Wooldridge (2009) argue that they should not exceed 0.25 to retain good statistical properties. We also follow Imbens and Rubin (2015) and report the logarithm of the ratio of the sample standard deviations, $ln\,(S_P^2/S_{NP}^2)$, to compare the dispersion of the distributions in columns (4) and (9).

Results based on the full sample show that prestigious firms are larger, older, more profitable, and have more growth opportunities as measured by the five-year average of the market-to-book ratio, whereas differences in stock returns, firm risk, and sales growth are statistically significant (t-statistic), but not economically significant in terms of the normalized difference Δ_X. Unsurprisingly, the statistically and economically largest differences can be observed for the three size proxies.

We proceed in two steps to find a matched sample without significant differences in those variables that are relevant for inclusion in the ranking and also relevant for compensation. In the first step, we investigate which independent variables of our main analysis determine ranking inclusion by running a Probit regression of *Prestige* on a comprehensive set of variables. In addition, we control for year fixed effects, industry fixed effects, and a range of controls for corporate governance. Table 3 shows results from two specifications. The specification in column (1) includes all variables that might influence ranking inclusion. However, including corporate governance variables reduces the sample size significantly, because they are only available for a subset of observations. With the exception of busy boards, all governance variables are statistically insignificant. In column (2) we omit the governance variables and estimate a reduced specification on a larger sample. Dropping the corporate governance variables more than doubles the number of observations in column (2). Most coefficient estimates and the pseudo R-squared remain similar.

As expected, variables with high explanatory power for ranking inclusion are proxies for firm size. Lagged return on assets (ROA) and lagged stock returns also predict ranking inclusion. Surprisingly, lagged stock returns are negatively related to *Prestige*. The reason is that we also include the first lag of the market value of the stock, which includes past returns (note that $ln\,(MV_{t-1}) = ln\,(MV_{t-2}) + ln\,(R_{t-1})$, where R denotes 1 + stock return). The coefficient on market value in column (2) is about three times larger in magnitude compared to the coefficient on stock returns, so the net effect of lagged returns on ranking inclusion is still positive. This also implies that size in previous periods is more important for the likelihood of being prestigious than last year's market return. The market-to-book ratio, firm age, and lagged total compensation are all insignificant.

In Table 4, we construct matched samples based on propensity scores calculated from the Probit regressions in Table 3. Rosenbaum and Rubin (1983) show that treated and untreated subjects with the same propensity scores should have identical distributions for all baseline variables to avoid biasing the estimated treatment effects. We first check whether the balancing property is satisfied for our matched sample of treated and control firms. We compare prestigious and non-prestigious firms by plotting density estimates for distributions of the six key covariates for the whole sample in Fig. 1 and for the matched sample in Fig.

2.[4]

Visual inspection shows that means for the whole sample differ in terms of the three size-related variables between prestigious and non-prestigious observations, whereas those for ROA, sales growth, and stock returns are reasonably similar. After matching, only small differences between prestigious and non-prestigious firms remain. Statistics in Panel B of Table 1 confirm this impression. Normalized differences are all comfortably below the threshold of 0.25 recommended by Imbens and Wooldridge (2009). The largest differences are those for market value (0.16) and sales (0.13), which are the only variables for which the difference is also statistically significant (t-statistic: 2.84 and 2.25, respectively). Prestigious firms are on average about 25% (15%) larger than matching firms in terms of market value (sales). However, this remaining difference biases the estimates against the predicted effect, since firm size has a positive influence on compensation. The log-ratio of standard deviations also becomes small for all variables. We conclude from the graphs and the tests in Panel B of Table 1 that matching based on the propensity scores estimated from Table 3 yields a comparable set of treatment and control firms that allows us to isolate the impact of firm prestige on CEO compensation.[5]

In Table 4 we report the results on the impact of prestige on total pay based on five matching estimators. The first one is a standard one-to-one nearest-neighbor estimator based on the propensity score (Rosenbaum and Rubin, 1983; Rubin, 1973), whereas the second is a four-to-one nearest neighbor estimator, i.e., we select four matches for each prestigious firm. The third estimator uses all potential matches from the full sample, but weights them based on the distance between the propensity score of the treated observation and those of the matching observation using Kernel weighting (Heckman et al. (1998)).

[4]Additional density plots for further covariates, including the distributions of the governance variables, are available as Fig. IA.1 (whole sample) and Fig. IA.2 (matched sample) in the Internet Appendix.

[5]To provide a first impression of pay differences between prestigious firms and non-prestigious firms with the same propensity score, we provide several examples in Table IA.1 in the Internet Appendix.

The fourth method divides the sample into subsets based on suitable cut-offs for the propensity scores and is referred to as stratification (Rosenbaum and Rubin (1983)). Then, we rerun our main regression from Table 2 to obtain estimates for the coefficient on *Prestige* for each sub-interval, which effectively assumes that all observations within the same interval are homogeneous. Finally, we obtain the stratification matching estimator as a weighted average of these coefficients, using the proportions of observations in the combined sample of ranked and non-ranked firms as weights.

The fifth and final estimator is the bias-adjusted matching estimator of Abadie and Imbens (2011), which adjusts for the differences in covariates between treated observations and control observations by using regression analysis. The first step estimates an auxiliary regression as in column (1) of Table 2, but without *Prestige* and only on the non-ranked observations. The second step uses the slope coefficients from this regression to adjust the log of total compensation for each control observation for the differences in covariates between the treated observation and its control. Bias-adjusted matching effectively performs a local adjustment to the compensation of the matching firm.

We follow two different approaches to obtain matched sample results for the impact of firm prestige on CEO pay. The first approach is the standard matching estimator applied to total compensation, for which we display the results in Panels A and B of Table 4. In Panel A, we use all control variables and matching is based on the propensity scores from column (1) in Table 3. In Panel B, we exclude the governance variables and compute propensity scores from column (2) in Table 3. In this case, the sample is more than twice as large. For both matching procedures, we generally obtain significant and slightly larger estimates in absolute terms for the influence of firm prestige on CEO pay compared to the regression approach adopted in Table 2. The one-to-one nearest-neighbor estimator is close to the regression estimates in magnitude (-10.3% and -7.7% in Panel A and B, respectively), but not significant at conventional levels (t-statistic: 1.55 and 1.48, respectively). All other estimates are significant at least at the 5% level and range between -10.0% (for Kernel estimation, Panel B) and -15.8% (for the bias-adjusted estimator by Abadie and Imbens (2011), Panel B). The relatively large size for the bias-adjusted estimator in Panel B might arise because it accounts for differences between ranked and non-ranked observations in terms of size and profitability, which have a positive impact on compensation. Since prestigious firms are larger and more profitable, bias-adjustment increases the estimates. This effect is less pronounced in Panel A.

The second approach repeats the panel regression analysis from Table 2 based on matched samples. Panels C and D of Table 4 display the results for the two different matched samples. In both cases, we either use our baseline specification from column (1) in Table 2 including firm fixed effects, or estimate the regression with a lagged dependent variable as in column (3) in Table 2. The regressions in columns (1) and (2) restrict the sample to the common support of the propensity scores, which reduces sample size by about 10%. Columns (3) and (4) rely on the same one-to-one nearest-neighbor matching we use in Panels A and B, which reduces the sample by about 80%. The estimates for *Prestige* range from -6% to -15%. They are statistically significant except for columns (3) and (4) in Panel A, in which we impose stricter sample restrictions.

Overall, our matching results show that prestigious firms pay their CEOs about 10% less in total compensation. They are close to the previous multivariate regression results in terms of statistical and economic significance.

3.3. Difference-in-differences analysis

As a further identification strategy, we apply a difference-in-differences analysis. The advantage of this analysis is that it controls for differences in levels between prestigious and non-prestigious firms that may arise from unobservable factors we cannot otherwise control for. The central identifying assumption of this approach is that the average *change* in pay would be the same for prestigious and non-prestigious firms, if prestigious firms did not enter the top 100 ranking ("common trends assumption"). If firms that become prestigious and their matching control firms differ systematically in terms of time-varying but unobservable factors, and if these factors influence growth in compensation, then the common trends assumption would be violated and our estimates would be biased.

We check the common-trends assumption by performing two sets of tests. First, we calculate one-year (three-year) compensation growth rates of firms that are ranked in the top 100, but have not been ranked in the top 100 in the previous year (three years). Growth is calculated for the years before the ranking event, e.g., from t-2 to t-1 for one-year growth, where the ranking event is in t-1 and the change in compensation is measured from t-1 to t. We compare compensation growth of these firms to that of non-prestigious firms, which remain outside the top 100 for the one-year horizon. Panel B of Table 1 shows these tests. Second, we repeat the same calculations, but now we use residual compensation instead of raw compensation, where the residual is obtained from a regression of the log of total compensation on the control variables included in Table 2. For both sets of tests and both sets of time periods, we cannot reject the common-trends assumption. We repeat the same tests for exit events, which are defined as firms being ranked in the previous year but not ranked in the current year. We again find no significant differences in pay growth between these firms and firms that remain ranked. These results provide some justification for conducting a difference-in-differences analysis. Recall also that *Prestige* was one of the few variables for which we could not reject the strict-exogeneity hypothesis for the GMM tests in Table 2. However, we caution the reader that these tests may still not be able to fully capture the influence of unobservable time-varying factors mentioned above, and that the non-prestigious firms may therefore not attain the standard of a randomly assigned control group.

We first conduct a regression analysis based on the full sample and estimate Eq. (1) in first differences. We use annual growth rates in total compensation, $\Delta TotalPay$, as the dependent variable and include $\Delta Prestige$ as well as all control variables in first differences. Note that by definition, $\Delta Prestige$ equals +1 for entries into the ranking and -1 for exits from the ranking, while $\Delta Prestige$=0 for all other firm-years. Results are presented in column (1) of Table 5.

We obtain a negative point estimate of -5.3%, which is about half the size of our baseline estimate and statistically insignificant. A potential explanation for the lower effect of might be that the pay adjustment process takes more than one year after entry to or exit from the ranking. Then, this specification would not reflect such a delayed response and the control group with $\Delta Prestige = 0$ would comprise prestigious firms with slow-growing pay and non-prestigious firms with faster-growing pay. Moreover, $\Delta Prestige$ averages the effect of entries to ($\Delta Prestige = +1$) and exits from the ranking ($\Delta Prestige = -1$), which might be different. We address these points by conducting a separate analysis of entries and exits and by carefully constructing appropriate control groups. Specifically, we rely on the differenced version of regression (1):

$$\Delta y_{it} = \Delta\alpha_0 + \gamma D^{Event}_{i,t-1} + \beta' \Delta X_{i,t-1} + \alpha_k \times \alpha_t + \Delta\varepsilon_{it}. \tag{3}$$

In Eq. (3) the dummy variable $D^{Event}_{i,t-1}$ refers to four different events, which define $D^{Event}_{i,t-1}$ as (first) entries to or (first) exits from the MAC ranking and correspond to columns (2) to (5) in Table 5. In column (2) of Table 5, the event dummy becomes one whenever a firm enters the MAC ranking. Specifically, we require that a firm is ranked in the top 100 in $t-1$, but not in $t-2$. In addition, we require that control observations are restricted to firms that are not ranked in $t-2$ and remain unranked in $t-1$. Results are similar if we use all non-treated firms as control observations (see Table IA.5 in the Internet Appendix). We find a significantly negative coefficient on entries into the ranking of -8.6%. In column (3), we focus on a firm's first entry into the top 100 of the MAC ranking within our sample period and define the event dummy accordingly. We find a larger and significant effect with a drop in pay of about -14.0%. The difference in coefficients between columns (2) and (3) indicates that entering the MAC ranking for the first time has a larger impact on subsequent CEO pay than just re-entering the ranking in a later year, which is intuitive.

In columns (4) and (5) we repeat the analysis of columns (2) and (3) for exits. Column (4) corresponds to column (2) and includes all firms that are ranked in period $t-2$, but not ranked anymore in period $t-1$; control firms are restricted to firms that are ranked in $t-2$ and remain ranked in period $t-1$. The coefficient estimate is positive, as expected, but statistically insignificant, potentially because the results of losing prestige status take more than one year to materialize. Finally, column (5) defines the event as the last exit from the top 100 within our sample period. The coefficient is smaller and insignificant, probably because we have only 136 last exits in our sample.

In the next step, we extend the analysis of Panel A by hypothesizing that the decline in compensation growth falls mainly on newly-hired CEOs. When a firm becomes prestigious, it may not wish to reduce the existing CEO's pay. Moreover, the CEO may be given some credit for the ranking success of the firm, an effect which may counteract the pure prestige effect. We therefore expect that the difference in compensation is particularly large if the firm hires a new CEO, who benefits from the prestige of the firm, cannot claim any credit for the ranking success, and does not have a reference compensation at the same firm yet. We define a dummy variable, *New CEO*, which equals one in the year in which the firm hires a new CEO, and zero otherwise. In Panel B of Table 5 we provide a triple-differences analysis and interact *New CEO* with $\Delta Prestige$ or the event dummies used in Panel A, respectively. Coefficient estimates in column (1) indeed suggest that our main effect is stronger for new CEOs. We obtain negative coefficients on the baseline effect of $\Delta Prestige$ as well as on its interaction with the dummy variable reflecting new CEOs. However, coefficients are not statistically significant. For entries into the ranking (columns (2) and (3)), the estimates also conform to our predictions. We obtain negative estimates for the interaction effect as well as for the baseline effect of prestige on CEO pay. While the interaction effect is only significant for first entries into the ranking, the baseline impact of firm prestige on changes in CEO pay always remains significantly negative. The interaction for first entries in column (3) appears very large, although it is somewhat imprecisely estimated and based on only 22 cases in which a first entry is associated with a newly hired CEO. With respect to exits, we also observe coefficients that point into the expected direction, but all of them are statistically insignificant.

3.4. Regression discontinuity analysis

Finally, we apply a regression discontinuity design (RDD) to our data. To employ RDD, we need a cut-off that separates prestigious firms from non-prestigious firms. For our analysis we choose a rank of 100, which seems salient since companies do not seem to advertise the fact that they have been included in the MAC ranking with a rank greater than 100. In Panel A of Table IA.6 in the Internet Appendix we test for differences between firms ranked below and above rank #100 using the same statistics as in Table 1 and find that the differences are insignificant.

We first provide a visual impression of the RD analysis in Fig. 3, which is based on a kernel regression to the right and the left of the cut-off at rank 100. We observe a small but discontinuous jump reflecting an increase in compensation for firms ranked below rank #100. Table 6 shows estimation results of the RD analysis. We use the optimized bandwidth following Imbens and Kalyanaraman (2012). We report the differences in the logarithm of total compensation between prestigious and non-prestigious firms, defined as being ranked above or below rank #100 of the MAC ranking. Column (1) reports results for a window from ranks between 90 and 110, and column (2) reports results for a window from ranks between 80 and 120. Column (3) reports a placebo test, for which we assume an arbitrary cut-off of 80 and perform the regression discontinuity analysis around rank 80.

Columns (1) and (2) both show a decline in compensation for firms with a top 100 rank in the MAC ranking, which is significant at the 10% (5%) level if we use the optimized bandwidth and the window from [90;110] ([80;120]). In economic terms, the coefficient of -0.563 corresponds to a 43% ($e^{-0.563}-1$) reduction in pay, which is much larger compared to the findings based on other methodologies. By contrast, the placebo test shows that there is no significant drop around rank #80. In addition, we multiply the bandwidth by a factor of 1.5 (0.5) to check the robustness of our result. We obtain statistically significant results if we increase the bandwidth, but not if we shrink it; we cannot even estimate the coefficient around rank #80 in this case. RDD estimates rely on a small number of observations close to the chosen cut-off. The number of observations effectively used in the estimation declines even further if we shrink the bandwidth. Accordingly, the results in Table 6 are based on a few hundred observations only. Therefore, results are imprecisely estimated and statistical significance turns out to be sensitive to excluding extreme observations. We thus caution the reader with respect to the reliability of the estimated magnitude of the effects.

It is important to note that our analysis relies on the salience of the cut-off at rank #100. To address concerns that companies ranked above 100 may also benefit from being listed as a prestigious company, we repeat the RD analysis with a different ranking, which does not suffer from this shortcoming and present results in the Internet Appendix. There, we use *Fortune's list of the Best Companies to Work For* (BCW), which only publishes the ranking for companies ranked #100 or better. We obtain additional ranking information about companies that applied for the ranking but were not included in the top 100 from the Great Place to Work Institute, which publishes the ranking. From a statistical point of view, this ranking is more suitable for RDD as it comes with a natural cut-off at rank #100. Results reported in Table IA.7 and Fig. IA.3 of the Internet Appendix are qualitatively similar and statistically significant, but the same caveats regarding the interpretation of the economic magnitude applies. A disadvantage of the BCW ranking in our context is that its link to firm prestige is conceptually weaker, since it is based on surveys among employees, whereas the MAC ranking is based on surveys among executives, outside directors, and financial analysts. The latter should provide a more accurate measure of firm prestige from a CEO's point of view.

Discussion of identification strategies. The identification strategies employed in this section differ in how they try to establish causality. Matching is on observables and therefore strong on considering the influence of the covariates included in the analysis, whereas the panel regressions and difference-in-differences regressions allow controlling for unobserved differences between firms. Regression discontinuity offers the advantage that firms close to the cut-off should be very similar and entrance into the ranking can be considered a random event for these firms. However, the reduction in sample size for RDD renders the estimation results less precise. We take comfort in the fact that all approaches to identification suggest a negative relation between firm prestige and CEO compensation, and many of them are consistent with a reduction in pay of around 8%. However, for RDD, economic magnitudes differ and in some cases (e.g., diff-in-diff based on exits) results lose statistical significance.

4. Channels of the prestige-pay relation

The analysis in the previous section shows that firm prestige has a negative impact on CEO compensation. In this section, we provide a more in-depth assessment of the different channels through which firm prestige affects CEO pay. Specifically, we follow up on the status-concerns hypothesis and the career-concerns hypothesis discussed in the Introduction (Section 4.1) and we ask whether our main result depends on cross-sectional differences in corporate governance (Section 4.2). All results in this section are based on our main specification from column (1) in Table 2. We provide estimates based on regressions in first differences in the Internet Appendix.

4.1. Status concerns and career concerns

Our first set of tests investigates whether the prestige-pay relation is more pronounced for CEOs with stronger status concerns or career concerns. Results are presented in Table 7. We rely on several proxies to measure status concerns and career concerns.

In Panel A, we define a dummy variable *High Preference*, which equals one if the CEO should have high status or career concerns according to a particular proxy and interact it with our main *Prestige* variable. In the first step, we collect data on status preferences across U.S. states from the General Social Survey (GSS). This survey is conducted among the U.S. population and elicits opinions on different topics such as religion, politics, and cultural values. To measure status concerns, we compute the fraction of survey respondents in each U.S. state who agreed to the following statements: "I am proud to be working for this organization" (GSS variable *PROUDORG*) and "I would turn down another job that offered quite a bit more pay in order to stay with this organization" (GSS variable *STAYORG*). We label these questions as "Pride" and "Loyalty" for easier reference and argue that being proud to work for an organization and rejecting other jobs offering more pay reflects obtaining social status from this employment. We argue that the prestige-pay relation should be stronger for firms headquartered in states where people value this kind of job-related status. This assumes that status preferences of the local population surrounding a firm's headquarters are correlated with the CEO's preferences. Yonker (2015) finds that more than 30% of CEOs work for firms in those states where they were born and raised. It is therefore plausible that these CEOs share the attitudes of the local population. Moreover, CEOs probably self-select into a culture that reflects their own attitudes. In line with this view, previous papers have shown that a firm's local environment is a good proxy to predict CEOs' decision making behavior (Hilary and Hui, 2009; Schneider and Spalt, 2015) and that their actions respond to their social environment (Shue (2013)).

Fig. 4 displays how status preferences vary across U.S. states. Results are based on the average fraction of survey respondents in a given state who agree with the two GSS statements listed above. In columns (1) and (2) of Table 7, we define *High Preference* to equal one if the fraction of respondents in a state who agree with the "Pride" or "Loyalty" statement is above the median for the entire United States, and zero otherwise.[6] We match these state-level dummy variables to company data by the state in which firms are headquartered. Note that the inclusion of firm fixed effects absorbs the baseline effect of *High Preference*, as firms rarely change headquarters in our sample period. Columns (1) and (2) of Table 7 show economically large coefficient estimates on the interactions of *Prestige* with *High Preference*

[6] Fig. IA.4 and Fig. IA.5 in the Internet Appendix display above and below-median states according to the "Pride" or "Loyalty" GSS statements.

which are significant at the 5% level. This suggests that the prestige-pay relation is indeed stronger for firms located in states where preferences for status are high.

In column (3), we look at the distribution of firm prestige across industries and compute the fraction of prestigious firms for each industry-year in our sample. If there are only few prestigious firms in a given industry, then CEOs with strong preferences for status should be more willing to trade off prestige against monetary compensation compared to industries in which several prestigious firms exist and CEOs may find it easier to switch to a prestigious competitor, which reduces the bargaining power of the firm. We therefore expect our results to be stronger in industries with below-median fractions of prestigious firms. Note again, that the inclusion of industry times year fixed effects absorbs the baseline effect of *High Preference* in column (3), as this variable varies on the industry-year level.

The coefficient estimates in column (3) of Table 7 are consistent with our hypothesis. Interacting *Prestige* with a dummy variable indicating a low fraction of prestigious firms in a given industry yields a stronger decrease in pay. However, the interaction term is not statistically significant.

Our next set of tests examines whether CEOs may be willing to sacrifice current income at a prestigious firm because they believe that it will help them to improve their future career prospects.[7] Note that this expectation would be justified only if CEOs anticipate that the managerial labor market interprets firm prestige as an indicator of CEO talent or enhanced human capital. We hypothesize that career concerns are stronger for younger CEOs compared to older CEOs and weaker for CEOs who are close to retirement (Gibbons and Murphy (1992)). To test these hypotheses, we first interact *Prestige* with a dummy variable equal to one if the CEO is younger than the median CEO in our sample, and zero otherwise. Results in column (4) of Table 7 support the view that younger CEOs are more

[7] The importance of career concerns for the managerial labor market was already emphasized by Fama (1980) and developed theoretically by Gibbons and Murphy (1992), Dewatripont et al. (1999), and Holmstrom (1999).

willing to sacrifice monetary compensation for the benefit of working at a prestigious firm. Specifically, we find that the prestige-pay relation is weakly significantly stronger for younger CEOs. Second, we interact *Prestige* with a dummy variable equal to one if the CEO does not retire after the current job (ExecuComp variable *reason*). We do not find any significant differences between CEOs who retire after their current job, and those who do not (column (5)).

In Panel B, we examine CEOs' career prospects after their current job more directly. If career concerns contribute to the prestige-pay relation, we would expect that CEOs use prestigious firms to start their careers and are more likely to switch to more profitable jobs afterwards than CEOs of non-prestigious firms. Thus, CEOs might profit from working for a prestigious firm in the long run, because they are hired for other directorships or accept other well-paid jobs that we do not observe in the ExecuComp data. To follow CEOs' careers after their exit, we link our ExecuComp data with BoardEx. The link is established based on a firm's Central Index Key (CIK) and the CEO's name. Where in doubt, links were verified manually. Data on what CEOs earn at their subsequent positions are insufficient, but we observe the number of board seats (column 1), the number of executive directorships (column 2) and the number of board seats on publicly listed companies (column 3) that CEOs accept after their current job. We use the levels of these measures as dependent variables to be able to include observations that assume a value of zero. We then regress these different measures of future career opportunities on *Prestige* and *CEO Exit*, a dummy variable equal to one if the CEO ceases to be the chief executive at the firm after period t-1, and an interaction between the two variables. Our main variable of interest is the interaction between *Prestige* and *CEO Exit*, which indicates how employment outcomes after the CEO leaves the firm differ depending on whether she led a prestigious firm or a non-prestigious firm. If leading a prestigious firm is associated with improved future employment opportunities, we would expect a positive interaction coefficient. The baseline effect of *Prestige* measures employment outside the firm during the CEO's tenure for prestigious firms, while the baseline effect of

CEO Exit measures the effect of retiring from the role of CEO on the employment of CEOs of non-prestigious firms.

In column (1), the dependent variable is the number of board seats. We find a positive coefficient of 0.203 on the interaction term, which is statistically significant at the 10% level. This coefficient indicates that CEOs who exit a prestigious firm hold about 0.20 more board seats in the year after their current employment than CEOs from non-prestigious firms. In addition, we find a negative but insignificant baseline effect on *CEO Exit*, showing that CEOs of non-prestigious firms hold 0.028 board seats less after losing their CEO position. This suggests that CEOs at prestigious firms hold about 0.17 (0.20 - 0.028) more board seats after exiting the firm. Moreover, we find a positive but insignificant effect on *Prestige*, suggesting that CEOs of prestigious firms do not gain significantly more additional board seats during their tenure at a prestigious firm. Results are similar if we use the number of executive directorships (column (2)) as the dependent variable. If we use the number of board seats on publicly listed firms (column (3)) as the dependent variable, the interaction term is not significant anymore.

The previous discussion shows that CEOs of prestigious firms enjoy career benefits from subsequent employments. The natural question is therefore whether these career benefits are large enough to explain the magnitude of the effect of firm prestige on CEO pay. Using BoardEx data on post-CEO compensation, we perform a simple analysis in which we relate the effect of firm prestige to the career benefits for the average CEO and present these calculations in Table 8. We calculate the annual dollar difference in pay from firm prestige as -7.87% ($= e^{-0.082} - 1$, see column (1) of Table 2) of average CEO pay (5.94 million USD, see Table 1). We abstract from discounting and multiply the resulting value with the average tenure of 7.50 years to obtain a total dollar value of pay given up for firm prestige over the CEO's tenure, which is 3.51 million USD. From Panel B, CEOs of prestigious firms hold 0.20 board seats more after they leave the firm compared to CEOs of non-prestigious firms. We multiply this number with the average tenure on the board (4.36 years) and the average annual remuneration for each board seat (205,000 USD). We obtain an average gain of 182,000 USD from additional board seats for CEOs of prestigious firms. The same calculations for executive directorships yield a gain of 1,696,000 USD. Since public board seats are only a subset of the overall board seats, we do not separately report their compensation in the table. The total career benefits can therefore explain about half of the lower pay from firm prestige. Naturally, these simple calculations abstract from the correlation among the components and conditioning variables that may influence the costs and benefits of working for a prestigious firm, and these simplifications may bias our simple calculation. We also do not include the effect of firm prestige on the average compensation per board seat post-CEO employment, since BoardEx provides insufficient data and using the available data yields insignificant results. For these reasons, we cannot conduct a more sophisticated analysis that takes these additional effects into account. Thus, based on our back-of-the-envelope calculation we conclude that career benefits can explain a significant portion but probably not the entire prestige effect on CEO pay.

4.2. Prestige and corporate governance

In this section we analyze two governance-related aspects of our main effect. We first ask if the prestige effect is concentrated in well-governed firms. We then investigate if firms pay more attention to CEO compensation after being ranked in the top 100 due to increased public scrutiny.

4.2.1. Outside options and corporate governance

The effect of prestige on compensation should be particularly pronounced in well-governed firms, because it requires that boards do not leave the non-monetary benefits from prestige as a rent to CEOs. We hypothesize that paying less due to prestige requires strong governance, in particular of the pay-setting process. This hypothesis is independent of where preferences for firm prestige come from. Such preferences could come from concerns about social status or from career concerns. Moreover, the board must be in a position to take this information into account when setting the CEO's compensation.

We address the hypothesis that the negative prestige-pay relation is stronger for firms with good governance in Table 9. We use six different measures of good corporate governance based on the prior literature, and for each measure we define a dummy variable *Good Governance* to indicate a high corporate governance standard. For binary variables, it equals one if firms conform to the corresponding standard, and zero otherwise. For other variables we divide the sample at the median and *Good Governance* equals one for the firms with above-median governance standards. We then interact firm prestige with the dummy variable indicating good corporate governance.

We use the following governance measures. In column (1), we follow Ferris et al. (2003) and use the independence of the compensation committee to measure good corporate governance. *Good Governance* equals one if the majority of members of the compensation committee is independent, and zero otherwise. In our context, this is probably the best governance proxy, because it relates directly to executive compensation. Based on prior work, (e.g., Rosenstein and Wyatt (1990)), we expect compensation contracts to be more efficient if the compensation committee is independent.

It has been shown that large boards and busy boards are weak monitors (e.g., Yermack, 1996; Fich and Shivdasani, 2006). Therefore, we use the number of board members as a measure for board size and the average number of outside board seats of board members as a measure for busyness in columns (2) and (3). We conjecture that large and busy boards are less likely to set contracts efficiently and to pay their CEOs less if the firm is prestigious.

The literature argues that CEO duality weakens the ability of the board to monitor the CEO and leads to poor corporate governance (Shivdasani and Yermack, 1999; Ryan and Wiggins, 2004). In column (4), *Good Governance* equals one if the CEO is not the chairman of the board. In column (5), we investigate CEO entrenchment and the relevance of external corporate governance by using the Gompers et al. (2003) governance index as a measure of good governance. Finally, in column (6) we include a dummy variable for the presence of at least one blockholder as an indicator for good corporate governance (e.g., Core et al. (1999)).

The governance hypothesis predicts that the effect of *Prestige* is concentrated in well-governed firms, i.e., the coefficient on the interaction of *Prestige* with *Good Governance* is negative and larger in absolute value than the coefficient for the baseline impact of *Prestige* on compensation. For all of the governance measures, we find coefficient estimates consistent with this prediction. Differences in estimates are also statistically significant for the first two measures: If companies have independent compensation committees or small boards, they pay their CEOs significantly less. Overall, this supports the view that our main effect is concentrated in well-governed prestigious firms, in which boards have enough power to extract the prestige premium from the CEO.

4.2.2. Governance and outrage: The limelight hypothesis

Another governance-related interpretation of our results suggests that firms are exposed to more publicity and enter the public limelight when they are ranked. Consequently, prestigious firms are more visible, under higher public scrutiny, and may pay their CEOs less, because they are more likely to face public outrage (Bebchuk and Fried, 2004; Kuhnen and Niessen, 2012). In that case, firm prestige would lead to lower CEO pay because of public scrutiny and anticipated public outrage. Boards of prestigious firms may just be more reluctant to grant CEOs large compensation packages and may be inclined to reverse previously awarded excessive compensation.

The limelight hypothesis emphasizes the impact of directors' reputational concerns and we treat it as distinct from the governance-related hypotheses developed above. Theoretical work by Levit and Malenko (2014) shows that reputational mechanisms have an ambiguous impact on corporate governance outcomes. In our context, directors may want to develop a reputation for not being restrictive on CEO pay if such a stance enhances their employment opportunities. However, it might be the case that boards revert previously awarded compensation if public scrutiny increases. Table 10 addresses these concerns.

According to the limelight hypothesis, the effect of *Prestige* should be stronger in firms with higher public scrutiny in the years leading up to their inclusion in the ranking. In column (1), we reason that the limelight effect is strongest immediately after the firm enters the ranking and should not be present two or more years later. We therefore lag *Prestige* by two years but still obtain a significantly negative coefficient on firm prestige. In columns (2) to (4), we interact *Prestige* with variables indicating whether a firm had high media coverage over the past year before it entered the ranking. Specifically, for each firm-year, we compute the number of articles on each firm in our sample published in four national US newspapers (New York Times, USA Today, Wall Street Journal, and Washington Post) as well as 37 local US newspapers contained in the LexisNexis database.[8] In columns (2) and (3), we define dummy variables equal to one for all firms in the top 10% or top 5% of the media coverage distribution, respectively. We then interact these dummy variables with our *Prestige* variable. Alternatively, in column (4), we divide firms into quartiles according to their media coverage in the previous year and interact these quartiles with our main variable, *Prestige*. Our results show that media coverage of firms does not explain the baseline impact of firm prestige on compensation. The interaction terms in columns (2) and (3) are all insignificant and provide no support for the hypothesis that media coverage in the top 5% or top 10% increases the effect of firm prestige. Similarly, the interactions for all quartiles in column (4) are insignificant, suggesting that the effect of *Prestige* does not vary significantly with media coverage. We still observe a negative coefficient on *Prestige* in all cases, although it loses significance in column (4). Finally, in column (5), we interact firm prestige with quartiles of firm visibility measured by the number of analysts following a particular firm. The findings are similar to those in column (4). Taken together, the results in Table 10 do not provide evidence for a higher impact of firm prestige on CEO compensation for firms that are highly visible to the public.

[8] We thank Hillert et al. (2014) for sharing their raw data on firms' media coverage.

5. Alternative explanations and robustness checks

In this section we consider further potential explanations for our result of a negative impact of firm prestige on CEO compensation and conduct several robustness checks.

One remaining concern with our results could be that CEOs who care more about prestige could also be more likely to end up working for prestigious firms and be more willing to give up compensation for the status associated with firm prestige than an average CEO would be. We already introduced this possibility in Section 4 above and consider it to be a legitimate interpretation of our results. It means that CEOs who do not work for prestigious firms may not be willing to give up pay for prestige if given the opportunity.

Another potential concern could be that association with a prestigious firm provides incentives, and CEOs of prestigious firms thus may receive less incentive compensation and a high fraction of fixed salary. These CEOs may therefore demand a lower risk premium and lower total pay. Investigating the different components of CEO pay shows that this is not likely to be the case (see Table IA.12 in the Internet Appendix). Although bonus payment to CEOs at prestigious firms tend to be lower, we find no evidence that firm prestige substitutes for explicit incentive provision by option or restricted stock grants. Moreover, pay-performance sensitivity is not lower for CEOs at prestigious firms.

Finally, we run several robustness checks regarding the econometric specification of the main regression. Results are reported in Table 11. In column (1) we cluster standard errors by firm and year. We obtain slightly larger standard errors, but the point estimate on the impact of firm prestige on CEO compensation remains significant at the 10% level. In column (2), we restrict the sample to the time period before 2006 because in that year ExecuComp changed the composition of some data items so that several adjustments need to be made if data are used beyond 2005 (see Section 2). Although we lose about 20% of the observations, we still find a weakly significantly negative impact of firm prestige on total compensation.

To control for the influence of outliers on our results, we truncate the sample and drop the observations with the largest and the smallest values for total compensation. This should alleviate concerns that our results are driven by CEOs with one-dollar salaries (Loureiro et al. (2011)) or by erroneous data points. In column (3) of Table 11, we truncate the overall distribution of CEO pay at the top and bottom 1% separately for each year in our sample (instead of once for the whole time period). Our results for *Prestige* are unaffected.

Throughout the paper, we define *Prestige* to be equal to one if a firm is ranked in the top 100. In columns (4) to (6) of Table 11, we re-run our standard regressions with alternative definitions of *Prestige.* In column (4), *Prestige* equals one for all firms in the top 75 MAC ranking, while we use the top 125 MAC ranking as the cut-off in column (5). In column (6), *Prestige* is defined to be one for all ranked firms, and zero otherwise. Coefficients are still negative and statistically significant except for the cut-off at 75 which is negative but not significant at conventional levels. Finally, in column (7), we include a higher-order lag of prestige. We still observe a negative coefficient for *Prestige* and a significant and negative coefficient on its lag.

As mentioned before, our results point into the same direction if we estimate regressions in first differences and thus condition on entries into and exits from the ranking only, but several coefficients are not statistically significant anymore (see Internet Appendix).

6. Conclusion

We investigate the relation between CEO compensation and firm prestige and find that CEOs of firms that are included in the Fortune 100 Most Admired Companies ranking earn on average 8% less than CEOs of non-ranked companies after controlling for other factors that affect compensation. We argue that CEOs value firm prestige because working for these firms increases their social status and improves their future career prospects. Accordingly, they are willing to trade off firm prestige against monetary compensation and accept lower growth rates of pay.

We also find that the negative impact of firm prestige on compensation is concentrated in well-governed firms. We conclude from this observation that only sufficiently strong boards can pay CEOs less and that weaker boards leave the benefits from working for a prestigious firm to the CEO as a rent.

Appendix

A.1. Anecdotal evidence

This appendix summarizes several sources on the relation between prestige and compensation.

- **Corporate Managers**

  According to the Hiring Site, "it will cost companies a 21 percent increase in compensation to lure candidates who feel a company's brand is unattractive, compared to an 11 percent premium for candidates who feel a brand is attractive." This comparison is the basis for the 10% discount for managers mentioned in the Introduction.

  Source: http://thehiringsite.careerbuilder.com/2013/10/01/candidates-will-accept-lower-salary-good-brands/

- **MBA Students**

  Auger et al. (2013) examine how 303 MBA and executive MBA students chose among 16 pairs of experimentally designed job offers.They offer these students hypothetical employment contracts which include specific salary offers, allowing the authors to estimate each individual participant's willingness to pay for corporate reputation as well as other features of the job offer. Results show that corporate reputation accounts for 7.5% of the value of the job contracts on offer.

- **University Professors**

  Leef and Sanders (1999) report on compensation of full professors at all major public and private schools in the US adjusted for cost-of-living and find that out of 86 research institutions, Harvard (Stanford, Yale, MIT) ranks only 75 (77, 43, 80) in this list. Furthermore, Johnston (2004) report for the UK that "Oxford and Cambridge are able to lure staff with lower salaries for the privilege of working at institutions with great reputations".

- **University Presidents**

  Statistics in the *Chronicle of Higher Education* show that among the 10 most highly-paid university presidents of private colleges in the U.S. in 2011, only five lead colleges ranked in the top 100 colleges by Forbes. For example, the president of Northeastern University (Forbes rank #236) earns $3.1 million, compared to the presidents of Harvard, Princeton (both $0.9 million), or Stanford ($1.1 million).

  Source: http://chronicle.com/article/Executive-Compensation-at/143541/).

A.2. Firms in (not in) MAC ranking

This table presents 25 firms that appear in the MAC ranking most often (column (1)) as well as 25 randomly selected firms that never appear in the MAC ranking (column (2)).

	Frequently ranked in MAC (1)	Never ranked in MAC (2)
1.	Abbott Laboratories	Altera Corp.
2.	Alcoa Inc.	Aon Corp.
3.	AMR Corp.	Avery Dennison Corp.
4.	Apple Inc.	Bed Bath & Beyond Inc.
5.	AT&T Inc.	Big Lots Inc.
6.	Berkshire Hathaway Inc.	Carnival Corp.
7.	Boeing Co.	CBS Corp.
8.	Caterpillar Inc.	CenturyLink Inc.
9.	Chevron Corp.	Cincinnati Financial Corp.
10.	Coca-Cola Co.	Citrix Systems Inc.
11.	Colgate-Palmolive Co.	DTE Energy Co.
12.	Deere & Co.	Ecolab Inc.
13.	Dow Chemical Co.	Freeport-McMoran Copper
14.	E. I. du Pont de Nemours and Co.	Frontier Communications
15.	Eli Lilly and Co.	Hasbro Inc.
16.	Exxon Mobil Corp.	Hershey Co.
17.	Hewlett-Packard Co.	Linear Technology Corp.
18.	International Business Machines Corp.	Molex Inc.
19.	J. C. Penney Company Inc.	Molson Coors Brewing Co.
20.	Johnson & Johnson	Monster Worldwide Inc.
21.	Kroger Co.	Nabors Industries Ltd.
22.	PepsiCo Inc.	Rockwell Automation Inc.
23.	Procter & Gamble Co.	Rowan Companies Inc.
24.	Safeway Inc.	Schlumberger Ltd.
25.	Target Corp.	Sigma-Aldrich Corp.

A.3. Variable description

This table contains detailed descriptions on the construction of all variables used in the empirical analysis.

Variable	Definition
Total $\text{Pay}_{i,t}$	Log of total pay at firm i in year t ($ln(tdc1)$). Source: ExecuComp.
$\text{Prestige}_{i,t}$	Dummy variable equal to one if firm i appears in the top 100 of Fortune's Most Admired Companies (MAC) ranking in year t, and zero otherwise. Source: Fortune.
Stock $\text{Return}_{i,t}$	Annual stock return of firm i in year t. Source: Center for Research in Security Prices (CRSP)/Compustat.
$\text{ROA}_{i,t}$	Return on assets of firm i in year t (ExecuComp variable *roa*). Source: ExecuComp.
$\text{Sales}_{i,t}$	Log of sales in millions of firm i in year t. Source: CRSP/Compustat
Sales $\text{Growth}_{i,t}$	Sales growth in percent of firm i in year t (ExecuComp variable *salechg*). Source: ExecuComp.
Market $\text{Value}_{i,t}$	Log of market value of firm i at the end of year t. Source: CRSP/Compustat.
Total $\text{Assets}_{i,t}$	Log of total assets of firm i at the end of year t. Source: CRSP/Compustat.
Firm $\text{Risk}_{i,t}$	Annualized standard deviation of firm i's daily stock return in year t. Source: CRSP.
Firm $\text{Age}_{i,t}$	Log of difference between current year and the year firm i first appears in the CRSP stock database. Source: CRSP.
$\text{MTB}^{5y}_{i,t}$	Market to book ratio of firm i in year t, measured as the average over the previous five years. MTB is measured as assets (Compustat data # 6) less book value of equity (Compustat data #60) plus market value of equity (Compustat data #199 $\times$ # 25) scaled by total assets. We drop observations with MTB $>$ 10. Source: Compustat.
CEO $\text{Ownership}_{i,t}$	CEO ownership in percent at firm i in year t (ExecuComp variable *shrownpc*). Source: ExecuComp.
CEO $\text{Tenure}_{i,t}$	Log of fractional years CEO is in her position at firm i in year t. Fractional years are defined as the number of months the CEO has been in office divided by 12. Source: ExecuComp.
CEO Pay $\text{Slice}_{i,t}$	Fraction of the total compensation of the CEO of firm i in year t relative to all other top five directors. Source: ExecuComp.
Ind. $\text{Committee}_{i,t}$	Percentage of independent members of firm i's compensation committee in year t. Source: Risk Metrics.
Large $\text{Board}_{i,t}$	Number of board members of firm i in year t. Source: Risk Metrics.
Busy $\text{Board}_{i,t}$	Number of outside appointments of all board members of firm i in year t. Source: Risk Metrics.
$\text{CEO/Chair}_{i,t}$	Dummy variable equal to one if CEO of firm i in year t is also chairman of the board, and zero otherwise. Source: Risk Metrics.
$\text{G-Index}_{i,t}$	Corporate governance index of firm i in year t as in Gompers, Ishii, and Metrick (2003).
$\text{Blockholder}_{i,t}$	Dummy variable equal to one if the fraction of blockholders at firm i in year t is larger than the median in the sample, and zero otherwise. Source: Risk Metrics.

Variable	Definition
$\text{Entry}_{i,t}$	Dummy variable equal to one if firm i enters the top 100 in the MAC ranking in year t, and zero otherwise. Source: Fortune.
$\text{First Entry}_{i,t}$	Dummy variable equal to one if firm i enters the top 100 in the MAC ranking in year t for the first time in our sample period, and zero otherwise. Source: Fortune.
$\text{Exit}_{i,t}$	Dummy variable equal to one if firm i drops out of the top 100 in the MAC ranking in year t, and zero otherwise. Source: Fortune.
$\text{Last Exit}_{i,t}$	Dummy variable equal to one if firm i drops out of the top 100 in the MAC ranking in year t and does not re-enter in our sample period, and zero otherwise. Source: Fortune.
$\text{New CEO}_{i,t}$	Dummy variable equal to one if firm i's CEO has changed in year t, and zero otherwise. Source: Fortune.
$\text{Pride}_{i,t}$	Dummy variable equal to one if the headquarter of firm i in year t is located in a US region with above-median values of the GSS variable PROUDORG, and zero otherwise. Source: General Social Survey (GSS).
$\text{Loyalty}_{i,t}$	Dummy variable equal to one if the headquarter of firm i in year t is located in a US region with above-median values of the GSS variable STAYORG, and zero otherwise. Source: GSS.
$\text{Prestige Dispersion}_{i,t}$	Dummy variable equal to one if firm i operates in an industry with a fraction of prestigious firms below the median in year t, and zero otherwise. Source: Compustat/Fortune.
$\text{Retirement}_{i,t}$	Dummy variable equal to one if firm i's CEO in year t does not retire after her current position. Source: ExecuComp.
$\text{\# Board Seats}_{i,t}$	The number of board seats in public and private companies that the CEO of firm i holds in year t. Source: BoardEx.
$\text{\# Executive Directorships}_{i,t}$	The number of executive directorships the CEO of firm i holds in year t. Source: BoardEx.
$\text{\# Public Board Seats}_{i,t}$	The number of board seats in public companies the CEO of firm i holds in year t. Source: BoardEx.
$\text{Media Coverage}_{i,t}$	The number of articles published on firm i in 41 national and local US newspapers in the year $t-1$. Source: LexisNexis and Hillert et al. (2014).
$\text{Analyst coverage}_{i,t}$	The number of analysts following firm i in year t. Source: I/B/E/S.

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Table 1: Summary statistics

This table presents summary statistics for the variables used in our analysis. Compensation data are obtained from ExecuComp. The sample goes from 1992 to 2010 and contains yearly data. Panel A presents summary statistics on the whole sample. Panel B compares characteristics of prestigious and non-prestigious firms. In Panel B, results in columns (1) to (5) are based on the full sample of firms, while results in columns (6) to (10) are based on a matched sample of prestigious firms and their nearest neighbor among the non-prestigious firms according to a propensity score analysis; the propensity score is based on the specification in Table 3, column (1). Columns (1) and (6) ((2) and (7)) report the sample means for the prestigious (non-prestigious) firms. Columns (3) and (8) report the normalized differences Δ_X from Eq. 2. Columns (4) and (9) report the logarithm of the standard deviations of prestigious and non-prestigious firms as in Imbens and Rubin (2015). Columns (5) and (10) report two-sample t-statistics. Standard errors are clustered at the firm level.

Panel A: Summary statistics	Mean	Standard Deviation	Median	1^{th} Percentile	99^{th} Percentile	Obs.
Prestige	0.11	0.32	0.00	0.00	1.00	12,707
Total Pay (in ’000)	5,937	11,643	3,663	383	36,698	12,707
Stock Return	0.07	0.76	0.02	-0.84	1.87	12,707
ROA	0.05	0.07	0.05	-0.22	0.21	12,707
Sales (in ’000,000)	5,200	5,798	2,736	635	26,947	12,707
Market Value (in ’000,000)	7,611	15,022	3,057	132	71,608	12,707
Total Assets (in ’000,000)	12,471	31,005	3,489	357	164,000	12,707
Sales Growth	0.12	0.86	0.08	-0.37	1.01	12,707
MTB^{5y}	1.87	1.06	1.50	0.94	6.16	12,707
Firm Age (in years)	33.50	20.08	32.00	5.00	82.00	12,707
Firm Risk	0.35	0.21	0.30	0.10	1.15	12,707
CEO Ownership	0.51	2.86	0.00	0.00	10.70	12,707
CEO Tenure (in years)	7.50	7.03	5.42	0.50	34.00	12,707

Table 1: Summary statistics, continued

Panel B:	Full Sample					Matched Sample				
Normalized differences	Prestige (1)	No Prestige (2)	Δ_X (3)	$Ln(\frac{SD_P}{SD_{NP}})$ (4)	t-stat (5)	Prestige (6)	No Prestige (7)	Δ_X (8)	$Ln(\frac{SD_P}{SD_{NP}})$ (9)	t-stat (10)
Stock Return	-0.02	0.08	-0.11	-0.59	-8.74	-0.03	-0.02	-0.02	0.04	-0.42
ROA	0.07	0.04	0.26	-0.11	8.37	0.07	0.06	0.05	0.01	1.01
Sales	8.94	7.86	0.86	-0.13	21.76	8.95	8.80	0.13	-0.10	2.25
Market Value	9.35	7.79	0.88	-0.05	21.88	9.40	9.16	0.16	0.02	2.84
Total Assets	9.20	8.13	0.58	-0.10	14.90	9.19	9.05	0.08	-0.05	1.42
Sales Growth	0.09	0.14	-0.05	-1.52	-4.97	0.10	0.11	-0.05	-0.33	-1.01
MTB^{5y}	2.38	1.83	0.32	-0.13	6.73	2.46	2.33	0.06	0.03	1.15
Firm Age	3.44	3.19	0.24	0.29	5.80	3.49	3.45	0.05	0.01	0.70
Firm Risk	0.30	0.36	-0.20	-0.21	-7.94	0.29	0.30	-0.02	-0.12	-0.34
CEO Ownership	0.42	0.50	-0.02	-0.11	-0.66	0.41	0.43	0.00	0.01	-0.06
CEO Tenure	1.63	1.57	0.05	0.00	1.49	1.55	1.56	0.00	0.02	-0.04
Δ Total $Pay_{Entry;t-2,t-1}$	0.06	0.09	-0.03	0.14	-0.73	0.06	0.12	-0.05	-0.06	-0.94
Δ Total $Pay_{Entry;t-4,t-1}$	0.26	0.29	-0.02	0.17	-0.63	0.32	0.44	-0.08	-0.19	-1.26
Δ Residual Total $Pay_{Entry;t-2,t-1}$	0.01	0.00	0.00	0.15	0.04	0.00	0.03	-0.03	-0.10	-0.46
Δ Residual Total $Pay_{Entry;t-4,t-1}$	0.01	0.01	0.00	0.14	-0.09	0.04	0.09	-0.05	-0.06	-0.68
Δ Total $Pay_{Exit;t-2,t-1}$	0.04	0.06	-0.02	0.19	-0.45	0.08	0.09	-0.01	0.21	-0.11
Δ Total $Pay_{Exit;t-4,t-1}$	0.17	0.29	-0.07	0.18	-1.52	0.22	0.27	-0.04	-0.01	-0.39
Δ Residual Total $Pay_{Exit;t-2,t-1}$	0.00	-0.02	0.02	0.26	0.38	0.01	0.01	0.00	0.12	0.01
Δ Residual Total $Pay_{Exit;t-4,t-1}$	0.05	0.00	0.04	0.10	0.76	0.02	-0.03	0.05	0.02	0.43

Table 2: Average impact of firm prestige on CEO pay

This table presents panel and GMM regressions with *Total Pay* as the dependent variable. The main independent variable, *Prestige*, is a dummy variable indicating a top 100 rank in the Fortune Most Admired Companies (MAC) ranking. The regressions also include a separate linear size control for each size decile. Columns (1) to (3) report ordinary least squares (OLS) regression results. Columns (4) and (5) report results from dynamic Arellano and Bond (1991) GMM regressions using forward orthogonal deviations as in Arellano and Bover (1995) to account for the firm fixed effects. Both specifications include five lags of *Total Pay* to ensure dynamic completeness of the model, but we only display the first lag for brevity. All control variables are described in Appendix A.3. Standard errors are clustered at the firm level and provided in parentheses. *,**, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

Method	OLS (1)	OLS (2)	OLS (3)	GMM (4)	GMM (5)
$\text{Prestige}_{i,t-1}$	–0.082**	–0.057*	–0.090***	–0.078*	–0.077*
	(0.039)	(0.033)	(0.029)	(0.041)	(0.041)
$\text{Stock Return}_{i,t-1}$	0.025**	0.034***	0.030**	0.043	0.043
	(0.012)	(0.011)	(0.014)	(0.041)	(0.042)
$\text{ROA}_{i,t-1}$	0.275	0.243	0.014	–0.101	–0.608
	(0.169)	(0.204)	(0.160)	(0.844)	(1.254)
$\text{Sales}_{i,t-1}$	0.029	0.021	0.058***	–0.023	–0.018
	(0.051)	(0.055)	(0.017)	(0.156)	(0.158)
$\text{Total Assets}_{i,t-1}$	–0.110**	–0.113**	0.029	–0.208	–0.333
	(0.047)	(0.051)	(0.021)	(0.272)	(0.444)
$\text{Sales Growth}_{i,t-1}$	0.007	0.006	0.002	0.061	0.111
	(0.006)	(0.004)	(0.004)	(0.175)	(0.197)
$\text{Firm Risk}_{i,t-1}$	0.054	0.011	0.163***	–0.317	–0.576
	(0.055)	(0.053)	(0.052)	(0.462)	(0.620)
$\text{Firm Age}_{i,t-1}$	0.037	–0.013	–0.015	–0.020	–0.021
	(0.089)	(0.094)	(0.012)	(0.062)	(0.066)
$\text{MTB}^{5y}_{i,t}$	–0.070**	–0.026	–0.024	–0.165	–0.195
	(0.028)	(0.039)	(0.016)	(0.107)	(0.144)
$\text{CEO Own}_{i,t-1}$	–0.004	–0.003	–0.008***	–0.006	–0.009
	(0.004)	(0.005)	(0.003)	(0.018)	(0.015)
$\text{CEO Tenure}_{i,t}$	–0.006	0.021	–0.009	–0.204	–0.186
	(0.010)	(0.023)	(0.008)	(0.160)	(0.136)
$\text{Pay}_{i,t-1}$			0.498***	0.202	0.256*
			(0.023)	(0.154)	(0.146)
AR(1) test (p-value)				0.032	0.041
AR(6) test (p-value)				0.919	0.746
Sargan test (p-value)				0.167	0.290
Hansen test (p-value)				0.419	0.000
Size spline	yes	yes	yes	yes	yes
Year fixed effects	no	no	no	yes	no
Industry fixed effects	no	no	no	yes	no
Industry × size fixed effects	yes	yes	yes	no	yes
Industry × year fixed effects	yes	yes	yes	no	yes
Firm fixed effects	yes	no	no	–	–
Firm × CEO fixed effects	no	yes	no	–	–
Adj. R^2	0.610	0.686	0.538	–	–
Number of observations	12,707	12,707	12,612	8,447	8,447

Table 3: Determinants of prestige

This table presents results from Probit regressions with *Prestige* as dependent variable. *Prestige* is a dummy variable indicating a top 100 rank in the Fortune Most Admired Companies (MAC) ranking. Column (1) presents results including all control variables, column (2) includes a reduced set of control variables; governance variables are dropped. The table displays coefficient estimates. All control variables are described in Appendix A.3. Standard errors are clustered at the firm level and provided in parentheses. *,**, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Probit All controls (1)	Probit Without governance controls (2)
$\text{Pay}_{i,t-1}$	-0.037	-0.049
	(0.064)	(0.031)
Stock $\text{return}_{i,t-1}$	-0.151**	-0.227***
	(0.062)	(0.042)
$\text{ROA}_{i,t-1}$	1.523**	1.360***
	(0.709)	(0.477)
$\text{Sales}_{i,t-1}$	0.563***	0.560***
	(0.101)	(0.081)
Sales $\text{Growth}_{i,t-1}$	-0.269	-0.290**
	(0.168)	(0.126)
Market $\text{Value}_{i,t-1}$	0.649***	0.639***
	(0.110)	(0.078)
Total $\text{Assets}_{i,t-1}$	-0.286**	-0.356***
	(0.130)	(0.091)
Firm $\text{Age}_{i,t-1}$	-0.114	-0.070
	(0.084)	(0.056)
$\text{MTB}^{5y}_{i,t}$	-0.013	-0.033
	(0.058)	(0.045)
$\text{CEO/Chair}_{i,t-1}$	0.057	
	(0.082)	
Ind. $\text{Committee}_{i,t-1}$	-0.178	
	(0.214)	
CEO Pay $\text{Slice}_{i,t-1}$	-0.323	
	(0.339)	
Busy $\text{Board}_{i,t-1}$	-0.014**	
	(0.007)	
Large $\text{Board}_{i,t-1}$	0.022	
	(0.020)	
$\text{G-Index}_{i,t-1}$	0.025	
	(0.020)	
Size spline	yes	yes
Year fixed effects	yes	yes
Industry fixed effects	yes	yes
Pseudo-R^2	0.363	0.363
Number of observations	5,517	12,493

Table 4: Matching results

This table presents matching results. Panels A and B show average treatment effects of being ranked in the top 100 of the Fortune Most Admired Companies (MAC) ranking on CEO compensation. In Panels A and C, matching is based on the propensity score derived from column (1) in Table 3. In Panels B and D, matching is based on the propensity score derived from column (2) in Table 3. A common support is created by eliminating all treated observations with a propensity score below the smallest or above the largest propensity score for the control group. Matching is done with replacement. Matching algorithms are as indicated in the table and described in Section 3.2. Control variables are as in Table 2. Standard errors are clustered at the firm level. *,**, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Total Pay Coefficient	Total Pay t-stat	Number of treated obs.	Number of control obs.
Panel A: Matching using all controls				
	(1)	(2)	(3)	(4)
Nearest Neighbor 1:1	-0.103	-1.55	713	435
Nearest Neighbor 4:1	-0.146	-2.42	713	1,704
Kernel	-0.112	-2.00	713	4,569
Stratification	-0.117	-2.14	713	4,569
Bias-adjusted	-0.117	-2.14	713	435
Panel B: Matching without governance controls				
	(1)	(2)	(3)	(4)
Nearest Neighbor 1:1	-0.077	-1.48	1,390	894
Nearest Neighbor 4:1	-0.120	-2.84	1,390	3,411
Kernel	-0.100	-2.58	1,390	11,050
Stratification	-0.116	-2.88	1,390	11,050
Bias-adjusted	-0.158	-3.96	1,390	894

Panel C: Regressions based on matched sample using all controls	Common support		Nearest neighbor 1:1	
	(1)	(2)	(3)	(4)
$\text{Prestige}_{i,t-1}$	-0.113***	-0.094*	-0.057	-0.075
	(0.039)	(0.056)	(0.054)	(0.069)
Controls included	yes	yes	yes	yes
Size spline	yes	yes	yes	yes
Industry × year fixed effects	yes	yes	yes	yes
Industry × size fixed effects	yes	yes	yes	yes
Firm fixed effects	no	yes	no	yes
LDV	yes	no	yes	no
Adj. R^2	0.572	0.707	0.633	0.859
Number of observations	5,052	5,065	1,088	1,089

Panel D: Regressions based on matched sample without governance controls	Common support		Nearest neighbor 1:1	
	(1)	(2)	(3)	(4)
$\text{Prestige}_{i,t-1}$	-0.087***	-0.075*	-0.071*	-0.147*
	(0.030)	(0.042)	(0.040)	(0.084)
Controls included	yes	yes	yes	yes
Size spline	yes	yes	yes	yes
Industry × year fixed effects	yes	yes	yes	yes
Industry × size fixed effects	yes	yes	yes	yes
Firm fixed effects	no	yes	no	yes
LDV	yes	no	yes	no
Adj. R^2	0.558	0.684	0.571	0.727
Number of observations	11,344	11,380	2,100	2,102

Table 5: Difference-in-differences results

This table presents difference-in-differences regressions with $\Delta Total\ Pay$ as the dependent variable. In column (1), $\Delta Prestige$ is equal to one if a firm enters the top 100 in the MAC ranking, equal to minus one if it drops out of the top 100, and zero otherwise. In column (2), $Entry$ is a dummy variable indicating that the firm enters the top 100 in the MAC ranking. In column (3), $First\,Entry$ is a dummy variable indicating the first time a firm enters the ranking. In column (4), $Exit$ is a dummy variable indicating the time when a firm drops out of the top 100 in the MAC ranking. In column (5), $Last\,Exit$ is a dummy variable indicating the last time a firm drops out of the ranking in our sample period. In Panel B, $New\,CEO$ is a dummy variable indicating that the CEO has changed from year t-2 to t-1. Control variables are as in Table 2, but in first differences, i.e. $\Delta Stock\ Return$, ΔROA, $\Delta Sales$, $\Delta Total\ Assets$, $\Delta Sales\,Growth$, $\Delta Firm\,Risk$, $\Delta Firm\,Age$, ΔMTB, $\Delta CEO\,Own$, and$\Delta CEO\,Tenure$. The size spline consists of size decile dummies based on $Market\,Value$ and interactions between $\Delta Market\,Value$ and each size decile. Moreover, an interaction between each industry dummy and $\Delta Market\,Value$ is included. Standard errors are clustered at the firm level and provided in parentheses. *,**, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

Panel A: Baseline	(1)	(2)	(3)	(4)	(5)
Δ Prestige$_{i,t-1}$	–0.053				
	(0.042)				
Entry$_{i,t-1}$		–0.086**			
		(0.044)			
First Entry$_{i,t-1}$			–0.140**		
			(0.056)		
Exit$_{i,t-1}$				0.047	
				(0.091)	
Last Exit$_{i,t-1}$					0.025
					(0.127)
Δ controls included	yes	yes	yes	yes	yes
Δ size spline	yes	yes	yes	yes	yes
Industry × Δ size fixed effects	yes	yes	yes	yes	yes
Industry × year fixed effects	yes	yes	yes	yes	yes
Event no.	822	472	300	350	136
Adj. R^2	0.046	0.046	0.046	–0.048	–0.048
Number of observations	11,320	10,138	10,138	1,182	1,182

Table 5: Difference-in-differences results, continued

Panel B: New CEOs	(1)	(2)	(3)	(4)	(5)
Δ Prestige$_{i,t-1}$	–0.022				
	(0.033)				
Δ Prestige$_{i,t-1}$ $\times$ New CEO$_{i,t-1}$	–0.300				
	(0.267)				
Entry$_{i,t-1}$		–0.068*			
		(0.034)			
Entry$_{i,t-1}$ $\times$ New CEO$_{i,t-1}$		–0.259			
		(0.287)			
First Entry$_{i,t-1}$			–0.141**		
			(0.056)		
First Entry$_{i,t-1}$ $\times$ New CEO$_{i,t-1}$			–0.300**		
			(0.150)		
Exit$_{i,t-1}$				0.015	
				(0.078)	
Exit$_{i,t-1}$ $\times$ New CEO$_{i,t-1}$				0.382	
				(0.539)	
Last Exit$_{i,t-1}$					0.018
					(0.128)
Last Exit$_{i,t-1}$ $\times$ New CEO$_{i,t-1}$					0.026
					(0.388)
New CEO$_{i,t-1}$	0.156***	0.142***	0.136***	0.283**	0.389*
	(0.034)	(0.037)	(0.039)	(0.122)	(0.214)
Δ controls included	yes	yes	yes	yes	yes
Δ size spline	yes	yes	yes	yes	yes
Industry $\times$ Δ size fixed effects	yes	yes	yes	yes	yes
Industry $\times$ year fixed effects	yes	yes	yes	yes	yes
Event no.	822	472	300	350	136
Interaction no.	87	49	22	38	19
Adj. R^2	0.050	0.048	0.049	–0.028	–0.033
Number of observations	11,320	10,138	10,138	1,182	1,182

Table 6: Regression discontinuity analysis for the MAC ranking

This table presents results for a regression discontinuity analysis with a kernel regression using a triangular kernel and the optimized Imbens and Kalyanaraman (2012) bandwidth (bw=1) for the MAC ranking. We modify the optimized bandwidth by factors of 1.5 and 0.5 to check the robustness of our results. A sharp regression discontinuity design is assumed, where the treatment variable (MAC ranking) jumps from one to zero at rank 100. We run this analysis for all firms ranked between 90 and 110 (column (1)), and 80 and 120 (column (2)). Column (3) displays a placebo test for the hypothetical cut-off set to 80 and firms in the rank interval between 70 and 90. Standard errors are provided in parentheses. *,**, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Cut-off 100; Window size [90;110] (1)	Cut-off 100; Window size [80;120] (2)	Cut-off 80; Window size [70;90] (3)
$Prestige_{i,t-1}$ (bw=1)	-0.563*	-0.578**	0.216
	(0.297)	(0.288)	(0.497)
$Prestige_{i,t-1}$ (bw=1.5)	-0.561**	-0.551**	0.002
	(0.259)	(0.253)	(0.344)
$Prestige_{i,t-1}$ (bw=0.5)	0.314	0.046	–
	(0.244)	(0.490)	–
Number of observations	477	886	423

Table 7: Channels of the prestige-pay relation

This table presents results on the channels for the association between *Prestige* and CEO compensation. *Prestige* is a dummy variable equal to one if a firm belongs to the top 100 in the MAC ranking, and zero otherwise. In Panel A, the dependent variable is *Total Pay*. We interact *Prestige* with a dummy variable indicating high status or career concerns. In columns (1) and (2), *High Preference* is equal to one if the firm's headquarter is located in a US state with above median status concerns according to GSS variables *PROUDORG* or STAYORG, respectively. In column (3), *High Preference* is equal to one if the firm operates in an industry with a fraction of prestigious firms below the sample median, and zero otherwise. In column (4), *High Preference* is equal to one if the CEO's age is below the sample median, and zero otherwise. In column (5), *High Preference* is equal to one if the CEO does not retire after her current position, and zero otherwise. In Panel B, we test the association between *Prestige* and CEO employment. We regress different measures of employment on *Prestige* and *CEOExit*, which is a dummy variable equal to one if the CEO ceases to be the chief executive at the firm after $t-1$, and an interaction between the two variables. In column (1) the dependent variable is *#Boards*, which is the number of board seats. In column (2), we use *#Executive Directorships*, which is the number of executive directorship positions, and in column (3) we use *#Public Boards*, which is the number of board seats at publicly listed companies. The data on executive employment are from BoardEx. The specifications are otherwise based on on column (1) in Table 2. Standard errors, clustered at the firm level in Panel A and the CEO level in Panel B, are provided in parentheses. *,**, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

Panel A: CEO Pay	Pride (1)	Loyalty (2)	Prestige dispersion (3)	CEO age (4)	Retire-ment (5)
$\text{Prestige}_{i,t-1}$	–0.012	–0.035	–0.077**	–0.028	–0.097**
	(0.039)	(0.039)	(0.038)	(0.041)	(0.047)
$\text{Prestige}_{i,t-1} \times \text{High Pref.}_{i,t-1}$	–0.217**	–0.200**	–0.069	–0.108*	0.049
	(0.093)	(0.100)	(0.047)	(0.059)	(0.052)
$\text{High Preference}_{i,t-1}$	–	–	–	0.038	–0.032
	–	–	–	(0.039)	(0.027)
Controls included	yes	yes	yes	yes	yes
Size spline	yes	yes	yes	yes	yes
Industry × size fixed effects	yes	yes	yes	yes	yes
Industry × year fixed effects	yes	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes	yes
Adj. R^2	0.592	0.593	0.610	0.612	0.610
Number of observations	9,283	9,223	12,707	12,694	12,707

Table 7: Channels of the prestige-pay relation, continued

Panel B: CEO Employment	# Board Seats (1)	# Executive Directorships (2)	# Public Board Seats (3)
$\text{Prestige}_{i,t-1}$	0.034	0.015	0.006
	(0.044)	(0.018)	(0.036)
$\text{Prestige}_{i,t-1} \times \text{CEO Exit}_{i,t-1}$	0.203*	0.202***	0.144
	(0.109)	(0.056)	(0.091)
$\text{CEO Exit}_{i,t-1}$	–0.028	–0.281***	–0.058*
	(0.050)	(0.027)	(0.033)
Controls included	yes	yes	yes
Size spline	yes	yes	yes
Industry × size fixed effects	yes	yes	yes
Industry × year fixed effects	yes	yes	yes
Firm fixed effects	yes	yes	yes
Adj. R^2	0.883	0.831	0.812
Number of observations	11,556	11,556	11,556

Table 8: Career benefits of firm prestige.

This table compares costs and career benefits from being a CEO of a prestigious firm based on the estimates in previous tables. To obtain the monetary costs to the CEO from leading a prestigious firm, we multiply the estimate of the coefficient γ on Prestige from Eq. 1 and column (1) in Table 2 with the average tenure and average annual compensation from Table 1. Career benefits from subsequent employments are calculated as the product of column (2) , i.e. the increase in the number of board seats due to firm prestige based on the estimate of the coefficient on the interaction of *Prestige* with *CEO Exit* in Panel B of Table 7 , column (3), i.e. the average tenure on the board, and column (4), i.e. the average annual compensation on the board (in thousand dollars). Data for columns (2), (3), and (4) for directors are from BoardEx. Calculations are performed separately for executive directorships and for non-executive directorships.

	Percentage effect of prestige	Level effect of prestige	Years of tenure	Annual compensation ('000 USD)	Total effect ('000 USD)
	(1)	(2)	(3)	(4)	(5)
CEO employment	-7.87%	–	7.50	5,937.00	-3,505.56
Subsequent employment					
Board seats	–	0.20	4.36	205.47	181.92
Executive directorships	–	0.20	2.19	3,830.33	1,696.00
Career effect					1,877.92
Career effect / CEO employment effect					-53.57%

Table 9: Corporate governance and the prestige-pay relation

This table presents results on the association between *Prestige* and corporate governance. The main independent variable, *Prestige,* is a dummy variable equal to one if a firm belongs to the top 100 in the MAC ranking, and zero otherwise. We interact *Prestige* with a dummy variable for good corporate governance. In column (1), *Good Governance* is equal to one if the compensation committee of the company is independent, and zero otherwise. In column (2), *Good Governance* is equal to one if the number of board members is smaller than the median number of board members in the sample, and zero otherwise. In column (3), *Good Governance* is equal to one if the number of outside appointments of all board members is smaller than the median number of outside appointments in the sample, and zero otherwise. In column (4), *Good Governance* is equal to one if the CEO is not at the same time chairman of the board, and zero otherwise. In column (5), *Good Governance* is equal to one if the Gompers et al. (2003) G-index of a firm is smaller than the median G-index in the sample, and zero otherwise. In column (6), *Good Governance* is equal to one if the fraction of blockholders is larger than the median in the sample, and zero otherwise. The specifications are otherwise based on column (1) in Table 2. Standard errors are clustered at the firm level and provided in parentheses. *,**, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Indep. Comm. (1)	Small Board (2)	Non-Busy Board (3)	CEO Chair (4)	G-Index (5)	Blk holder (6)
Prestige$_{i,t-1}$	0.050	–0.018	–0.051	–0.062	0.008	–0.094
	(0.073)	(0.035)	(0.041)	(0.040)	(0.049)	(0.113)
Prestige$_{i,t-1}$ × Good Gov.$_{i,t-1}$	–0.136*	–0.193**	–0.080	–0.040	–0.099	–0.086
	(0.079)	(0.092)	(0.057)	(0.068)	(0.069)	(0.131)
Good Governance$_{i,t-1}$	0.038	0.031	–0.044	–0.025	–0.091**	–0.086
	(0.033)	(0.026)	(0.028)	(0.029)	(0.046)	(0.091)
Controls included	yes	yes	yes	yes	yes	yes
Size spline	yes	yes	yes	yes	yes	yes
Industry × size fixed effects	yes	yes	yes	yes	yes	yes
Industry × year fixed effects	yes	yes	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes	yes	yes
Adj. R^2	0.596	0.608	0.608	0.608	0.622	0.659
Number of observations	8,760	9,936	9,936	9,936	8,815	1,517

Table 10: Is there a limelight effect?

This table presents results on the association between *Prestige* and public scrutiny. The main independent variable, *Prestige*, is a dummy variable equal to one if a firm belongs to the 100 most admired companies according to the MAC ranking, and zero otherwise. In column (1), we lag the main variable, *Prestige*, by one additional year. In columns (2) and (3), we interact *Prestige* with indicators for firms in the top 10% or top 5% of the distribution in media coverage, respectively. In column (4), we interact *Prestige* with quartiles of firms' media coverage. Media coverage is calculated based on the number of articles published on a firm in 41 national and local US newspapers in year t-1. In column (5), we interact *Prestige* with indicators for quartiles of analyst coverage based on the sum of analysts following a firm according to IBES. The specifications are otherwise based on on column (1) in Table 2. Standard errors are clustered at the firm level and provided in parentheses. *,**, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	Lagged Prestige (1)	Media Coverage Top 10% (2)	Top 5% (3)	Quartiles (4)	Analyst Coverage (5)
$\text{Prestige}_{i,t-2}$	–0.090**				
	(0.036)				
$\text{Prestige}_{i,t-1}$		–0.067**	–0.072**	–0.072	–0.071
		(0.033)	(0.033)	(0.046)	(0.062)
$\text{Prestige}_{i,t-1} \times \text{Top } 10\%_{i,t-1}$		–0.029			
		(0.053)			
$\text{Top } 10\%_{i,t-1}$		–0.027			
		(0.022)			
$\text{Prestige}_{i,t-1} \times \text{Top } 5\%_{i,t-1}$			–0.025		
			(0.056)		
$\text{Top } 5\%_{i,t-1}$			–0.041*		
			(0.024)		
$\text{Prestige}_{i,t-1} \times \text{Q2}_{i,t-1}$				0.021	0.099
				(0.072)	(0.093)
$\text{Prestige}_{i,t-1} \times \text{Q3}_{i,t-1}$				0.010	0.098
				(0.053)	(0.069)
$\text{Prestige}_{i,t-1} \times \text{Q4}_{i,t-1}$				–0.048	–0.089
				(0.057)	(0.077)
$\text{Quartile } 2_{i,t-1}$				–0.004	0.019
				(0.022)	(0.034)
$\text{Quartile } 3_{i,t-1}$				–0.010	–0.003
				(0.025)	(0.037)
$\text{Quartile } 4_{i,t-1}$				–0.030	0.001
				(0.042)	(0.044)
Controls included	yes	yes	yes	yes	yes
Size spline	yes	yes	yes	yes	yes
Industry × size fixed effects	yes	yes	yes	yes	yes
Industry × year fixed effects	yes	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes	yes
Adj. R^2	0.607	0.257	0.258	0.610	0.610
Number of observations	11,761	12,707	12,707	12,707	12,707

Table 11: Robustness checks

This table presents tests for alternative specifications. The main independent variable, *Prestige*, is a dummy variable equal to one if a firm belongs to the 100 most admired companies according to the MAC ranking, and zero otherwise. The dependent variable is *Total Pay* and specifications are based on column (1) in Table 2. In column (1), we cluster standard errors by year and firm. In column (2), we restrict the data to all years before 2006. In column (3), we exclude the top and bottom 1% of total compensation in each year. In columns (4) and (5), we define *Prestige* as a dummy equal to one if a firm belongs to the 75 (125) most admired companies according to the MAC ranking, respectively. In column (6), we define *Prestige* as a dummy equal to one if a firm is included in the MAC ranking at all. In column (7), we include two lags of *Prestige*. If not indicated differently, standard errors are clustered at the firm level. Standard errors are provided in parentheses. *,**, and *** denotes significance at the 10%, 5%, and 1% level, respectively.

	TwoWay Cls (1)	Year < 2006 (2)	Drop 1% Outl. (3)	Top 75 (4)	Top 125 (5)	All Ranked (6)	Higher Lag (7)
$\text{Prestige}_{i,t-1}$	–0.082*	–0.077*	–0.069**	–0.055	–0.064*	–0.060**	–0.059
	(0.045)	(0.040)	(0.032)	(0.038)	(0.033)	(0.027)	(0.040)
$\text{Prestige}_{i,t-2}$							–0.075**
							(0.034)
Controls included	yes	yes	yes	yes	yes	yes	yes
Size spline	yes	yes	yes	yes	yes	yes	yes
Industry × size fixed effects	yes	yes	yes	yes	yes	yes	yes
Industry × year fixed effects	yes	yes	yes	yes	yes	yes	yes
Firm fixed effects	yes	yes	yes	yes	yes	yes	yes
Adj. R^2	0.548	0.550	0.597	0.203	0.203	0.555	0.542
Number of observations	12,707	10,194	12,581	12,707	12,707	13,884	11,761

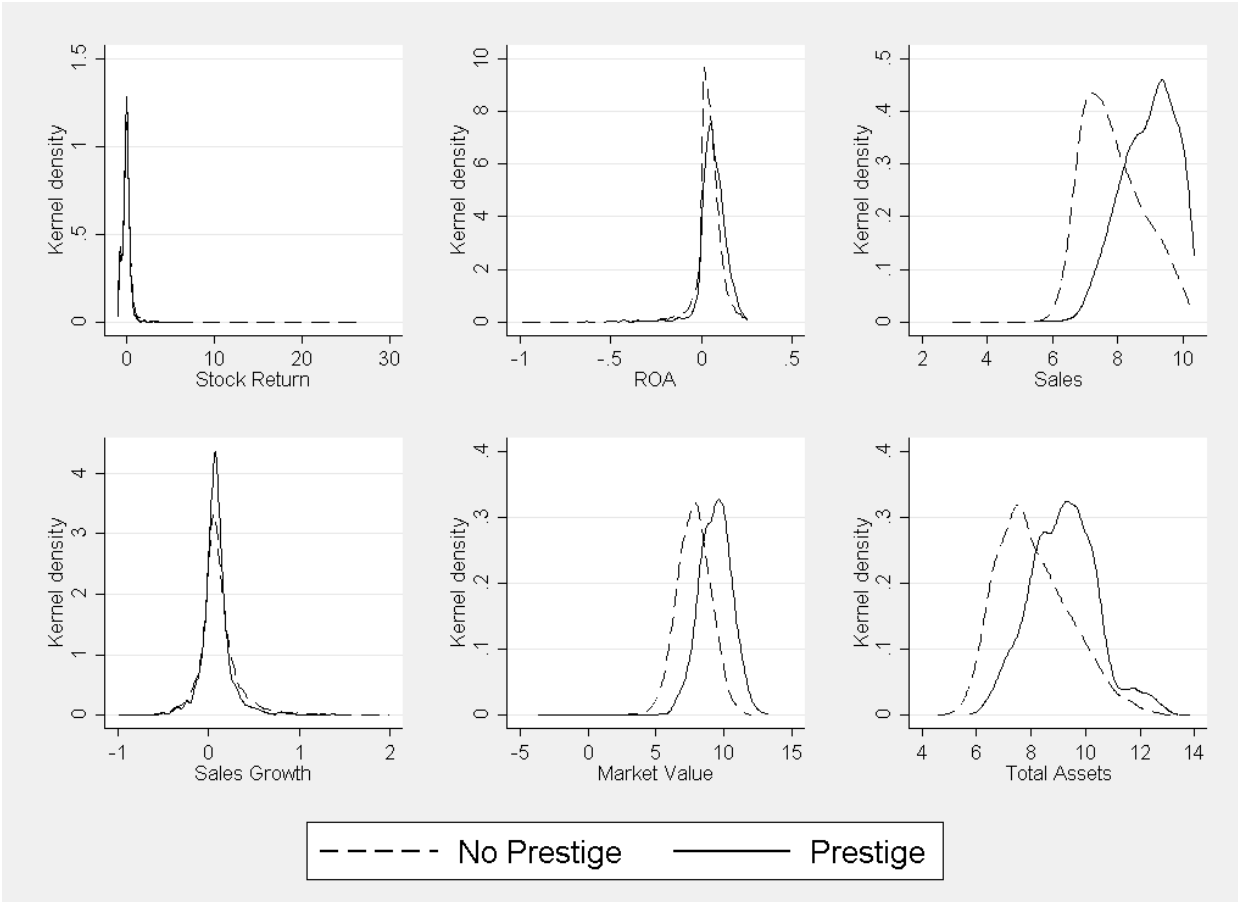

Figure 1: Distributions based on whole sample

This figure presents the distribution of key variables in the propensity score analysis for prestigious and non-prestigious firms according to the MAC ranking. The distributions are based on the whole sample.

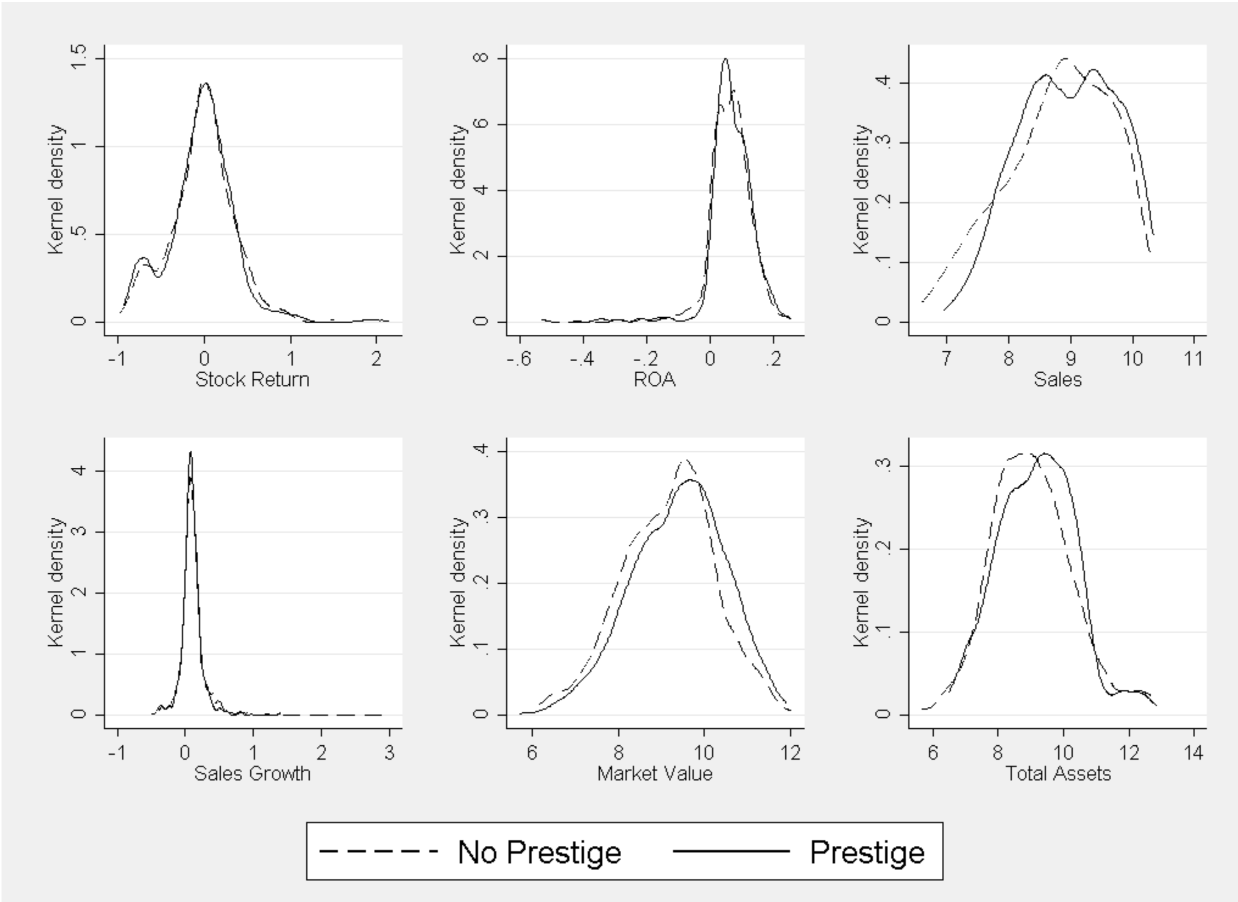

Figure 2: Distributions based on matched sample

This figure presents the distribution of key variables in the propensity score analysis for prestigious and non-prestigious firms according to the MAC ranking. The distributions are based on the matched sample, where the propensity score is based on the specification in Table 3, column (1).

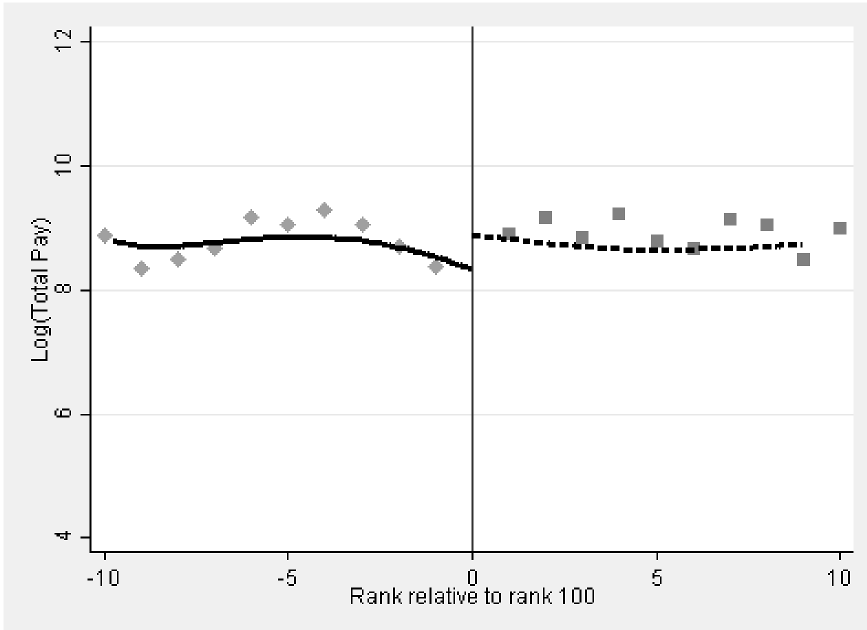

Figure 3: Discontinuity at Top 100 for the MAC ranking

This graph presents non-parametric estimates of two local polynomial regressions based on the log of total compensation. The cut-off is set at rank 100 of the MAC ranking. The sample comprises all firms ranked between 90 and 110.

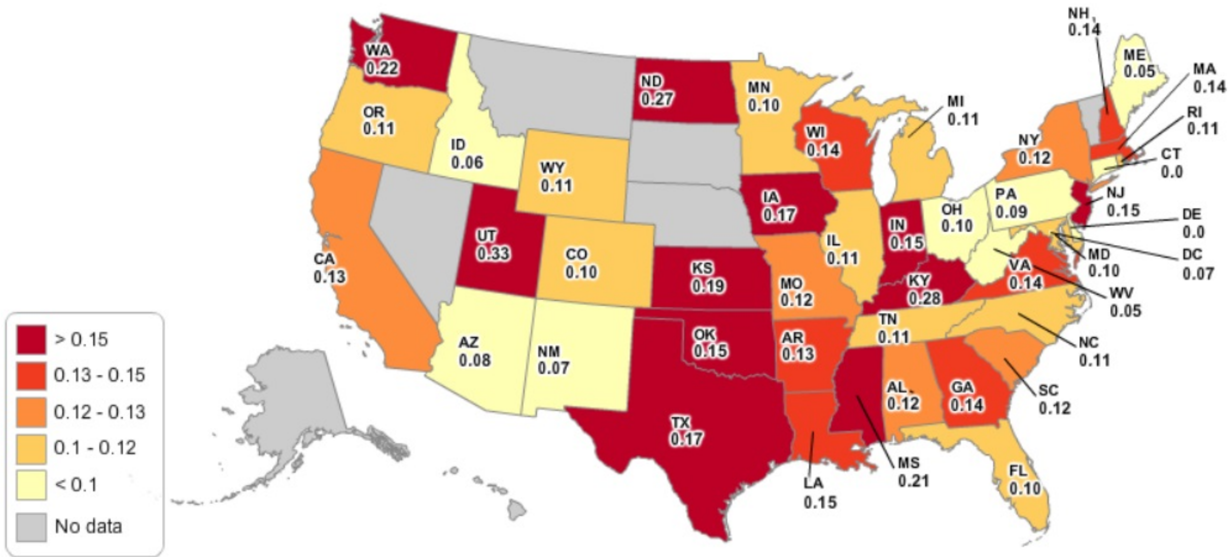

Figure 4: Geographic dispersion of status concerns

This graph presents the average fraction of survey respondents per US state that indicate to have high status concerns. Status concerns are measured by the fraction of survey respondents who agreed to the statements "I am proud to be working for this organization" (GSS variable *PROUDORG)* and "I would turn down another job that offered quite a bit more pay in order to stay with this organization" *(*GSS variable *STAYORG*).