

Shining Brighter Than The Stars? Corporate Evidence On Competing With Superstars^{*}

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Abstract

This paper investigates the effect of superstar CEOs on their competitors. Exploiting shocks to CEO status due to prestigious media awards, we document a significant outperformance of superstar CEOs' competitors over a sample of competitors of matched non-superstar CEOs. This treatment effect is more pronounced for competitors who have not previously received an award and who are not entrenched. We observe an increase in risk-taking of superstars' competitors and identify higher innovation activity as a channel of increased risk-taking. Our results suggest a positive overall welfare impact of corporate superstar systems due to the incentivizing effect on superstars' competitors.

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1. Introduction

How do Chief Executive Officers (CEOs) react when one of their peers is elevated to superstar status? Are competitors of superstar CEOs inspired to take actions that create or destroy economic value? This paper provides an analysis of the consequences that corporate superstars have on their competitors. Given the prevalent system of managerial superstars in today's corporations, how corporate superstars influence their competitors is a timely and economically relevant question. However, the empirical consequences of competing with superstar CEOs are not a priori clear. Recent evidence from the economics literature suggests that competing with superstars in sports has a detrimental effect on performance (see e.g., Brown, 2011, using the example of golf professionals competing with Tiger Woods). On the other hand, seeing a peer being elevated to superstar status can also incentivize competing CEOs to perform better (see e.g., Ehrenberg and Bognanno, 1990, for theoretical evidence). In this paper, we study which of these two effects ultimately prevails.

Our empirical identification strategy follows Malmendier and Tate (2009). We start by constructing a sample of CEOs who experience a status shock through an award from a major newspaper or magazine, such as Business Week, Forbes or Time. The competitors of these superstar CEOs are our treatment sample. We take advantage of the emergence of text-based competition measures first developed and provided by Hoberg and Phillips (2010a, b) to define the set of competitors. We also construct a control sample to contrast the effect of competing with a superstar CEO with its counterfactual (i.e., what would have happened to the same CEO if he had not been competing with a superstar CEO). We use propensity score matching (see e.g., Malmendier and Tate, 2009) to obtain a set of pseudo- or predicted superstar CEOs. These are CEOs that are very similar to actual CEO winners but do not end up winning an award. The competitors (again obtained using the Hoberg and Phillips dataset)

of these predicted award winners are our control sample. In our empirical analyses, we determine the treatment effect of competing with superstar CEOs by comparing the post-award behavior of our treatment and our control sample. It is important to note that our empirical strategy produces treatment and control samples that do not differ significantly from each other in nearly all observable characteristics we employ in our matching estimator.

Our results provide strong support for positive firm value effects of competing with superstar CEOs. Comparing the performance of competitors of actual award winners to competitors of predicted award winners, the treatment sample outperforms the control sample over one, two and three year windows. Our analysis reveals that the control sample's performance is not significantly different from the overall market. The treatment sample's performance, on the other hand, significantly outperforms the control sample by 4% (one year) to 29% (three years) over the years after the award. We interpret this finding as evidence that superstar CEOs have a beneficial impact on incentive levels of competing CEOs. Cross-sectional analyses show that this result is not driven by a set of known return predictors or by effects pertaining to a specific industry or year (for instance a peak in a certain industry). Our documented treatment effects are heterogeneous. The effect is most pronounced for CEOs who have not previously received an award themselves, are geographically close to the award-winning competitor, have a comparatively short tenure and a large number of competitors and who are not entrenched. Additional analyses further suggest that the effect we document does not simply reflect a reshuffling of market shares from award winners to competitors, but that the positive returns associated with competing with superstar CEOs result in a positive overall effect on economic welfare. Our documented treatment effect is also robust to a battery of robustness tests, most importantly different definitions of the control sample and different matching techniques as well as the use of sub-samples to ensure that our results are not affected by the market turbulence of the recent financial crisis.

When we investigate the channels through which the documented outperformance takes place, our results reveal that competitors of award-winning CEOs significantly increase their risk-taking and their engagement in patenting activities. In the three post-award years, competitors of superstars experience a significant increase in their stock return volatility compared to the control sample. This change in overall risk is driven by an increase in idiosyncratic risk showing that CEOs take on riskier projects (measured by filed patent applications) subsequent to a competitor being elevated to superstar status. The increase in patenting (innovation) activity is accompanied by an increase in innovation efficiency and an increase in innovation quality. Superstars' competitors also increase their ability to generate private returns of their patents (innovation appropriability). The findings on innovation efficiency and quality provide substantial economic grounds for abnormal positive stock returns.

Our paper contributes to the emerging literature on CEO status. Whereas existing theoretical and empirical work usually links CEO status to CEO risk-taking incentives or behavior (see e.g., Auriol and Renault, 2008; Malmendier and Tate, 2009; Roussanov, 2010; Roussanov and Savor, 2013; Shemesh, 2010) or compensation (see e.g., Maug et al., 2013), our paper investigates the effect of shocks to CEO status on the firm valuation of their competitors. We also add to recent work assessing the economic and financial impact of the superstar culture found among CEOs today. As such, our paper extends the work of Malmendier and Tate (2009) by looking at competitors of superstar CEOs and the work by Brown (2011), who investigates competition with superstars in sports. We document that competing with superstars can have positive effects in the corporate world in contrast to the sports world and, in addition, to the negative effects documented for the superstar CEOs' own companies. Finally, we also contribute to the recent literature investigating tournament incentives inside (Kale et al., 2009; Kini and Williams, 2012) or outside (Coles et al., 2013)

companies and to the growing literature making use of industry classifications that are more sophisticated than classifications using SIC codes by using text-based analysis provided by Hoberg and Phillips (Hoberg and Phillips, 2010a, b; Foucault and Fresard, 2014; Leary and Roberts 2014).

The remainder of this paper is organized as follows. In Section 2 we describe the data set and the competitor measure of Hoberg and Phillips (2010a, b). In Section 3 we explain the methodology which we use. The results of our event study are presented in Section 4. Section 5 investigates the channel through which competitors of superstar CEOs outperform the control sample. Section 6 concludes.

2. Data

2.1 Construction of the CEO award database

The core of our dataset is a hand-collected list of CEO awards published between 1992 and 2008. Several organizations and magazines confer such awards, for instance the “Best Manager of the Year” award by Business Week. Following Malmendier and Tate (2009), we only consider awards that are prominent enough to plausibly affect CEO status and that, potentially, could be given to any CEO in the US. The award, thus, has to be nationwide and not subject to any constraint in terms of industry focus, age or gender of the CEO. Based on these criteria, we consider the awards of nine media outlets: *Business Week*, *Chief Executive*, *Electronic Business Magazine*, *Financial World*, *Forbes*, *Industry Week*, *Morningstar*, *Time* and *Time/CNN* (see Appendix for more information on the media outlets). Figure 1 presents a histogram of the CEO awards by year and magazine, indicating that Business Week and Forbes are the predominant awards. Especially due to the ceasing of Business Week’s Best Manager award (after 2005), the average number of awards per year decreases in later years

of the sample. As the ceasing of the Business Week's Best Manager award does not seem to have a systematic reason, we do not expect this to bias our results¹.

The CEO award data is matched with information on CEO characteristics, firm characteristics and performance and firm competitors. We use Compustat's Execucomp database to obtain information on CEO characteristics such as age, tenure and gender. Tenure is calculated as the difference between each year in the sample and the year the person became CEO. Execucomp does not provide age and tenure information for all CEOs in our sample. If the information is missing, we hand-collect it from proxy statements². In a few occasions, a CEO served more than once as the CEO of the same company. In these cases the tenure count always starts with the new appointment³.

2.2 Company characteristics and firm performance

We merge Compustat and CRSP data to our CEO award dataset to measure company characteristics and firm performance. Market capitalization is the stock price multiplied by common shares outstanding (in log form). The book-to-market ratio is book equity over market equity⁴. As market return we use the value-weighted return on all NYSE, AMEX and NASDAQ stocks provided by CRSP. Fama-French risk factors (Fama and French, 1993) and data on the momentum factor (Carhart, 1997) are obtained from Kenneth French's database.

¹ Our results are similar if we only consider awards up to 2005 (2004) and, if anything, higher, suggesting that the observed effect was smaller during the financial crisis (see Column 3 of Table 9).

² An example for such multiple terms as CEO is James R. Houghton. He served as the CEO of Corning Inc. from 1983 until 1996 when he retired. In 2002 he resumed his role as CEO. He finally retired in April 2005.

³ Alternatively we also calculate a cumulative tenure variable. Robustness tests show that the results do not depend on the choice of the tenure variable.

⁴ We use the book equity definition of Malmendier and Tate (2009). They calculate book equity as stockholders' equity (SEQ) (if available, else book value of common equity (CEQ) + par value of preferred stock (PSTK) or assets (AT) – total liabilities (LT) [in that order]) plus balance sheet deferred taxes and investment tax credit (TXDITC), if available, minus the book value of preferred stock (redemption (PSTKRV), liquidation (PSTKL), or par value (PSTK) [in that order] depending on availability).

To measure corporate governance, we use the Governance Index (G-Index) developed by Gompers et al. (2003).

We calculate different risk measures to proxy for the risk-taking behavior of CEOs. Following Low (2009) we use equity risk measures and consider total risk, idiosyncratic risk and systematic risk. Total risk is measured as the standard deviation of monthly returns per firm-year. Using the monthly value-weighted CRSP index as the benchmark, idiosyncratic risk is calculated as the standard deviation of the residuals per firm-year. The beta of this regression is used as the systematic risk proxy.

We also measure the geographic distance between CEOs and their competitors. Geographic distance is a potentially important factor when considering the peer effects of a shock to CEO status. Other CEOs might not only be influenced by observing the shock (the award) but also by observing the impacts of these shocks (increased compensation, more leisure time). Whereas some effects, for instance the increase in compensation or the increase in publishing activity (Malmendier and Tate, 2009), can be observed nationwide, others might be more geographically limited (e.g. golf handicaps). Therefore, we would ideally measure the geographic distance between CEO domiciles. However, as we do not have data on CEOs' residences we use the companies' headquarter locations as a proxy. Geographic proximity is then determined based on postal ZIP codes. ZIP codes are from Compustat and merged with latitude and longitude data obtained from the 2010 Census Gazetteer Files. The actual distance is calculated using the Haversine method (Bouwman, 2014)⁵.

⁵ Using the location of headquarters as a proxy for CEO domiciles might lead to measurement errors due to the backfilling of firms' location data in Compustat (Heider and Ljungqvist, 2013) and CEOs living far away from their companies' headquarters (Liu and Yermack, 2012). However, we think that it is still a valid method as the distortion due to the errors of the distance measure should bias against finding significant results.

2.3 Definition of competition

The definition of competition is integral to our empirical investigation. We use the Hoberg-Phillips TNIC-3 data to define companies' competitors. Hoberg and Phillips (2010b) use a text-based analysis of companies' 10-K filings to determine the product similarity between companies, thereby estimating their degree of competitiveness. The firm-by-firm similarity measure is based on the similarity of companies' product descriptions. Hoberg and Phillips (2010b) measure the relative number of words that product descriptions of two companies have in common. This measure is bounded between zero and one. The higher the value of the measure, the more likely it is that two companies are competitors of each other. Hoberg and Phillips (2010b) use a 21.3% threshold to define competitors. The 21.3% threshold is chosen to target the same average number of competitors per company as the SIC-3 measure would provide. The main advantage of the TNIC-3 data compared to traditional measures such as SIC-3 is that it is time-varying and non-transitive⁶. A detailed explanation of the TNIC-3 data can be found in Hoberg and Phillips (2010a) and Hoberg and Phillips (2010b). We use the TNIC-3 data to assign competitors to each winner and each predicted winner. Using the Hoberg-Phillips data limits our sample to the Hoberg-Phillips universe. Thus, we do not consider any awards prior to 1996 or after 2008.

2.4 Innovation proxies

To measure innovation we use information on patents awarded by the U.S. Patent and Trademark office (USPTO) and the citations to these patents between 1995 and 2006. The data is obtained via the National Bureau of Economic Research (NBER). The NBER patent database contains information on all patents approved by the USPTO between 1976 and 2006.

⁶ Consider three companies A, B and C. If company A is a competitor of B and B is a competitor of C, then A would be a competitor of C in case of transitivity (for instance SIC-3). In contrast if being a competitor of B does not automatically make A and C competitors, it would be a non-transitive competition measure (for instance TNIC-3).

The data includes information on the year of the patent application, the year of approval and the forward and backward citations of each patent. On average, there is a two year gap between the application and the approval of a patent. This gap leads to potential time truncation issues if applied patents have not been approved yet. Hall et al. (2001) suggest ending the sample period two to three years prior to the actual end of the database. As this suggestion would substantially reduce our sample, we do not follow it but use time fixed effects instead. This approach is also suggested by Hall et al. (2001) and used by Hirshleifer et al. (2012), to reduce the potential truncation issues.

Besides the number of patents we calculate several other innovation variables. Patents measure the innovation output of companies. We compare this output to the input of innovation which we proxy using the firms' research and developing expenses (R&D) scaled by total assets, in log form (Hirshleifer et al, 2012). Following Hall et al. (2005) and Trajtenberg et al. (1997) we use citations scaled by patents (Cites per patent), in log form, to proxy for patent quality. More influential patents receive more citations on average, as the front page of a granted patent has to contain information on the technological antecedents of the patent (Trajtenberg et al., 1997). Further quality proxies which we use are the number of zero citations per paper, suggested by Amore et al. (2013), and patent generality (Trajtenberg et al, 1997). Zero citations indicate poor patents as they are not cited by any later patent. Thus, an increase in zero citations per patent indicates that companies' innovation quality has decreased. The generality measure developed by Trajtenberg et al. (1997) measures the range of technical fields in which the patent is cited. It is calculates as

$$Generality_i = 1 - \sum_{k=1}^{N_i} \left(\frac{NCiting_{i,k}}{NCiting_i} \right)^2 \quad (1)$$

where i and k index firm and patent class (technical field), respectively. N_i is the number of different patent classes to which the citing patents belong. $NCiting$ is the number of citing

patents. The generality variable is bounded between zero and one, with larger values representing higher generality. A more general patent is cited in several different fields, indicating its influence on future innovation. We use the average generality of firm patents per year. While high quality innovations increase public benefits, they do not necessarily increase private returns. Trajtenberg et al. (1997) outline that companies mainly profit from high quality innovations if they are able to develop them further to extract returns. To determine whether companies are able to generate private returns from patents Trajtenberg et al. (1997) suggest measuring the number of self-citations (Self cites per patent). If companies successfully exploit the market opportunities due to their high quality patents, they should be able to further develop these patents leading to follow up patents and increased self-citations. To decrease the potential influence of outliers we winsorize all our innovation ratios at the 5% level. Our results remain similar when we use lower levels for winsorizing.

2.5 Summary statistics

Table 1 provides summary statistics for award winners, predicted award winners (to be defined below) and the full universe of companies with data available. It shows that winners significantly differ in almost all dimensions from non-winners. Winners tend to be significantly larger companies than non-winners. On average they have a market capitalization of \$23.6 billion compared to the non-winners average market capitalization of \$1.4 billion. The difference in book-to-market ratios implies that winner companies tend to be growth companies. Their average book-to-market ratio of 0.306 is substantially lower than the average of non-winners (0.552). There are significant differences in returns before the award date (over horizons of up to three years) as it seems that several awards are conferred based (at least partially) on past performance. Award-winning CEOs are also older and more likely to be female than non-winners, although both of these differences are statistically

insignificant. Winners have been in office longer than non-winners (10.8 years versus 8.9 years). This difference is significant at the 5% level.

3. Methodology

To study the effect of award-winning CEOs on their competitors, it would be optimal to randomly assign competitors to award-winning CEOs and then compare these randomly assigned competitors to a control sample. However, such a random assignment is not feasible in the real world. Furthermore, the awards themselves are not randomly conferred but at least partially based on past performance. This leads to an endogeneity problem as discussed by Malmendier and Tate (2009). A natural solution to this problem would be to compare the competitors of winners with the competitors of nominees. However, as nominees are not observable, this is not feasible. Instead, we follow the approach used by Malmendier and Tate (2009) and use a nearest-neighbor matching estimator to identify so-called predicted winners and, in a next step, their competitors. Predicted winners are non-winning CEOs who are observationally equivalent to award winners.

We apply a two-step matching procedure. We first use a logit regression to predict CEO awards based on firm and CEO characteristics. As the award criteria are not observable, we assume that the awards are usually conferred based on the CEO characteristics age, gender and tenure as well as firm characteristics and past performance. These are the same criteria as the ones used by Malmendier and Tate (2009). The sample consists of all month-firm combinations in months when an award is conferred. We use an indicator variable equal to one if the company's CEO received an award in the respective month and zero otherwise. Following Malmendier and Tate (2009), we use market capitalization two months prior to the award month (in log form), book-to-market ratio based on the last fiscal year which ended at least six month prior to the award month, returns for months 2-3, 4-6, 7-12 and 13-36 before

the award month as well as CEO age, gender and tenure as predictive variables. Additionally, we also include award, industry and year fixed effects.

Columns 1 to 3 in Table 2 show the results of the logit regression with different fixed effects. Awards tend to be conferred to CEOs of large companies with a comparatively low book-to-market ratio and high past performance. All these company characteristics are significant at the 1% level irrespective of the fixed effects included. Regarding CEO characteristics, we observe that CEO tenure, CEO gender and CEO age (except for the specification including all fixed effects) are also statistically significant award criteria. According to these findings, a female and young CEO who has served for several years as the CEO of a large and well-performing company has the highest chances of receiving an award. Apart from age, these findings are in line with the univariate results shown in Table 1.

We use the predicted values from the logit regression with no fixed effects to perform propensity score matching. We use the predicted values of the logit regression without any fixed effects because Levine and Painter (2003) and List et al. (2003) point out that the propensity score matching's analogy to fixed effects is to match within the specific categories (e.g. years) instead of applying dummy variables (e.g. year fixed effects)⁷. In each award month (equivalent to year-month-fixed effects) we choose, without replacement, the CEO with the propensity score closest to the score of an award-winning CEO. We refer to these as predicted winners. Table 1 shows summary statistics of the predicted winners alongside the statistics for award winners and non-winners. Along all matching variables, there is no significant difference between award winners and predicted winners, an indication of the high matching quality.

⁷ As a robustness test we repeat the matching procedure using the same approach as Malmendier and Tate (2009), i.e. we fix the year and additionally use award, industry and year fixed effects. We use the resulting treatment and control groups to perform our event study and observe even higher and more significant cumulative abnormal returns than in the approach we have adopted in the paper (see Column 6 of Table 9).

In the subsequent step we merge the competitors to the winners and predicted winners. Using the TNIC-3 competition database developed by Hoberg and Phillips (2010a, b), we assign competitors to each winner and to each predicted winner in a given year. The competitors of winners correspond to our treatment sample whereas competitors of predicted winners form our control sample. At first sight, our definition of treated and control companies could appear similar to the one of Malmendier and Tate (2009), however, their definition and our definition differ substantially. Whereas the treatment group of Malmendier and Tate (2009) comprises actual award winners, we consider the competitors of the actual award winners as the treatment group. To further ensure that we measure the effect on competitors and do not accidentally capture the effect of winners or predicted winners, we exclude winners and predicted winners from our treatment and control group even if they are defined to be a competitor according to the Hoberg-Phillips database^{8,9}.

The assignment of competitors to winners and predicted winners potentially leads to situations in which a company is a competitor of a winner and a predicted winner at the same time (award date)¹⁰. This would make it impossible to identify any treatment effect. We

⁸ This situation can only occur if a winner (predicted winner) is the competitor of a different winner (predicted winner) or the competitor of a predicted winner (winner) it is not matched to.

⁹ Although we exclude predicted winners from our treatment group, we might still include observationally equivalent non-winning CEOs (second or third nearest neighbors) in our treatment group and thereby accidentally replicate the Malmendier and Tate (2009) study. We believe that it is unlikely that we accidentally replicate the Malmendier and Tate (2009) study for two reasons. First, we compare the competitors of winners to the competitors of predicted winners and therefore winning CEOs are excluded from both the treatment and the control group. Second, by comparing the data on predicted winners in Table 1 and the data on competitors of winners (our treatment sample) in Table 3, we observe that the two samples are different along several dimensions. Therefore, it seems unlikely that we accidentally replicate the study of Malmendier and Tate (2009). Instead, we capture the effect of CEO awards on competing CEOs.

¹⁰ An example would be the case of Concord Communications. On the 10th of January 2000, Timothy Koogle, the CEO of Yahoo, received the Best Manager award of Businessweek. The nearest neighbor match of Timothy Koogle and as such the predicted winner is Kevin K. Kalkhoven the CEO of JDS Uniphase. However, according to the TNIC-3 database, JDS Uniphase as well as Yahoo are competitors of Concord Communications. This means that Concord Communications would be part of the treatment as well as the control group. As we would not be able to study the treatment effect in such cases, we exclude them.

therefore only consider companies as competitors of either winners or predicted winners if they are not assigned to the other group 365 days prior or after the respective award date. Setting this threshold is a trade-off between either precisely differentiating treated and control companies or keeping the characteristics of the initial sample. While the differentiation precision between the treatment and the control sample increases with the number of days used as a threshold, we face the problem that this can lead to a change of the sample. The latter might occur because we are naturally more likely to exclude companies with a high number of competitors. This likelihood further increases if we increase the threshold. If companies with many competitors are systematically different compared to companies with few competitors, a long threshold would bias the sample. Considering this trade-off between differentiation precision and sample bias, we believe that a threshold of 365 days is a reasonable setting. T-tests (untabulated results) show that the characteristics of the sample do not significantly change when imposing a 365 days threshold. We further test the robustness of our results by using other thresholds ranging from zero to infinity, obtaining similar results (see below for more details).

In addition to the time threshold, we impose further restrictions on our sample. Equivalent to the restrictions imposed on the set of award-winning companies, we only consider competitors who are part of the Execucomp universe. We relax this restriction in a robustness test and show that the effect on our results is minimal.

In Table 3 we report the summary statistics for the competitors of winners (treatment group) and the competitors of predicted winners (control group). Along all dimensions, except market capitalization, there are no significant differences between the competitors of winners and predicted winners, a sign of the high matching quality. Regarding market capitalization, competitors of winners are slightly larger on average than the competitors of predicted

winners (significant at the 5% significance level), however the median market capitalizations of the treatment and the control group are not significantly different. Further tests below will show that this economically small difference is not a driver of our results.

We differ from other studies using a matching procedure in some ways. The usual approach would be to match companies directly on the treatment group (competitors of award winners; see e.g., Malmendier and Tate, 2009). Instead, we match award winners with similar companies (predicted winners) and then compare the competitors of winners (treatment group) and the competitors of predicted winners (control group) with each other. This matching does not necessarily lead to comparable treatment and control samples. That we still observe very similar treatment and control samples provides evidence for the high matching quality of award winners and predicted winners and mitigates concerns that our results are due to a sample bias. Despite the similarity of our treatment and control samples, a direct match of companies on the competitors of award winners would lead (by construction) to even more similarity. As a robustness test, we repeat all our analyses using this alternative matching approach and obtain similar results (see Column 7 of Table 9 for performance results; difference-in-differences results are not tabulated). In the main specifications used in the paper, we match award winners and predicted winners. Our treatment and control groups are then the competitors of award winners and the matched predicted winners. By using the matched award winners and predicted winners, we aim to ensure that both the treatment and the control group compete with a very successful CEO.

We employ an event study to analyze the stock market performance of superstars' competitors around the award announcement. For magazine awards, we use the date of the publication as the event date. For other awards, we use the date of the first public announcement as the event date. If the announcement date falls on a weekend or a holiday we

use the next trading day as the event day. We calculate the abnormal returns around the event date using a market model with the CRSP value-weighted returns. The market model is specified as

$$R_{i,t} = \alpha_i + \beta_i R_{m,t} + \varepsilon_{i,t} \quad (2)$$

where $R_{i,t}$ and $R_{m,t}$ are the period t excess returns on firm i and the market, respectively. α and β are the parameters of the market model which are estimated based on an estimation period of 755 weekdays starting 778 weekdays before the event. We use the parameters estimated in Equation (2) to predict the normal return of firms around the event date. The abnormal return of firm i is the difference between the observed return and the predicted return. We aggregate the abnormal returns over various time periods to obtain the cumulative abnormal return:

$$CAR_i(\tau_1, \tau_2) = \sum_{t=\tau_1}^{\tau_2} AR_{i,t} \quad (3)$$

where τ_1 and τ_2 mark the beginning and the end of the event period, respectively. Following Malmendier and Tate (2009), we use four different event windows, specifically $[-5,+5]$, $[+6,+255]$, $[+6,+510]$ and $[+6,+765]$. The $[-5,+5]$ event window might appear to be a rather long short-term window. However, it is not always possible to clearly identify the announcement day of an award. Therefore, we follow the suggestion of Malmendier and Tate (2009) and use a longer short-term window to ensure that the announcement date lies within the event period.

In addition to the event study setting, we employ a difference-in-differences research design to study whether competitors of superstar CEOs change their risk-taking and innovation behavior subsequent to an award. The difference-in-differences model is specified as

$$\sigma_{i,t} = \alpha_{i,t} + \beta_1 D_{Treatment,i} + \beta_2 D_{Award,t} + \beta_3 D_{Treatment,i} D_{Award,t} + FE_{i,t} + \varepsilon_{i,t} \quad (4)$$

where i and t are a firm and time index, respectively. The dependent variable is the equity volatility $\sigma_{i,t}$ which we use as a proxy for risk-taking. In further tests, we use other risk measures and innovation variables as our dependent variable. Depending on the nature of the dependent variable we use OLS, Tobit or Poisson regressions. $D_{Treatment,i}$ is a dummy variable which is one if firm i is part of the treatment group and zero otherwise. $D_{Award,i}$ is a dummy variable which is one in the award an all subsequent years and zero otherwise. The interaction term of these two dummy variables is our main variable of interest as it shows how CEOs are influenced if they observe one of their peers being promoted to superstar status, differencing out any time trends and omitted variables. We also include award, industry and year fixed effects.

4. Event study results

4.1 Main results

We study the stock market performance of superstars' competitors following an award by means of an event study. The outcome of the event study is a priori unclear as competitors could either be incentivized (Ehrenberg and Bognanno, 1990) or discouraged by the superstar status of their peer (Brown, 2011). Table 4 presents strong evidence that superstars' competitors significantly outperform competitors of predicted winners. The treatment group (Column 1) has a positive cumulative abnormal return in every event window, ranging from 0.5% in the 5-day window to 26.1% in the three years window. The magnitude of these results increases monotonically over time and is significant at the 10% level for the shortest event window and significant at the 1% level for all longer event windows. In contrast, we do not observe any significant deviation from the control groups' predicted returns over the three

years following the event (Column 2). Put together, these findings lead to the result that the difference between the treated and the control sample is positive and significant (Column 3).

Following Malmendier and Tate (2009) we use the market model in our event study. However, previous literature (e.g. Barber and Lyon, 1997; Lyon, Barber and Tsai, 1999) has shown that the market model might capture too much noise over longer event periods. To control for further risk factors we repeat the event study using a Fama-French and a Carhart model. As the market model as well as the Fama-French and the Carhart model are models that explain risk factor loadings but not alphas, we estimate our results with and without alphas. Table 5 presents the results of these additional tests. Columns 1 to 3 show the results using the estimated alpha to calculate predicted returns whereas we ignore these alphas in Columns 4 to 6. For brevity, we only present the results of the treatment group as the cumulative abnormal returns of competitors of predicted winners (control group) do not significantly differ from zero for any model or event window. The results of the Fama-French and the Carhart model (with and without alpha) support the previous finding of the market model. Again we observe positive and significant cumulative abnormal returns of superstars' competitors subsequent to the award. Compared to the market model the magnitude of the results decreases to about 13% over a three year horizon. This indicates that observing a peer being promoted to superstar status leads to an abnormal return per year of about 4%. We further test the robustness of these results using various other event study settings (untabulated results). Following MacKinlay (1997) we use monthly returns instead of daily returns as these are less sensitive to noise in long run event studies. To check that our results are not driven by a change in parameters (due to the event) we use a post event estimation period ranging from 788 days after the event to 1043 days (following Agrawal et al., 1992). For all these settings we obtain similar results as in Table 4 and Table 5.

In addition to the event study, we also use a calendar time portfolio approach comparing the performance of competitors of award winners and the performance of competitors of predicted winners. We construct a zero-cost portfolio which is rebalanced monthly. The portfolio is long in competitors of award winners and short in the competitors of predicted winners. Firms enter the portfolio in the first month after the award and exit the portfolio 1, 2 and 3 years later, or if the CEO changes. The calendar time approach has the advantage that it does not rely on any return expectations. However, in this setting, it has the disadvantage that the portfolio is not practically implementable, as it relies on future information which is used for the matching of winners and predicted winners. We run a weighted least square regression of the equally weighted portfolio returns on the three Fama-French (1993) factors – size (SMB), book-to-market (HML) and market risk premium (Mktrf) – and the momentum factor of Carhart (1997). Following Loughran and Ritter (2000), we use a weighted least square approach to give equal weights to each observation instead of each period. The number of shares varies from more than a thousand portfolio companies in some months to less than ten companies in the later months of the sample. We obtain similar results if we use ordinary least squares instead or if we use returns of a value weighted portfolio. The alpha of this long-short portfolio has a significant alpha of roughly 40 basis points per month over two and three years (Table 6). Economically this effect translates to about 10% and 15% over two and three years, respectively. This is remarkably similar to our event study results and provides strong support for these findings.

We interpret the findings of the event study and the calendar time portfolio approach as evidence that competing with a CEO who achieves superstar status leads to an incentive impact on competing CEOs, inducing them to outperform a sample of companies that compete with CEOs who are observationally equivalent to superstars but do not win an award themselves.

4.2 Cross-sectional analysis

We next use cross-sectional analysis to study whether the observed outperformance of superstars' competitors compared to competitors of predicted winners is driven by a set of known return predictors or by effects pertaining to a certain industry or year. For each of the four time horizons investigated in Table 4, we regress the respective cumulative abnormal return on a treatment dummy which is one if the CEO is a competitor of a winning CEO and zero otherwise. We include award, industry and year fixed effects as well as other control variables such as firm characteristics, past performance and CEO characteristics.

Table 7 presents the results of this cross-sectional analysis. Overall, the results presented in Table 4 are confirmed in this multivariate setting. The treatment dummy is positive and statistically significant for the longer time horizons. Our results do not seem to be affected by the small difference in size between the treatment and control groups (see Table 3), as the inclusion of a control for firm size does not account for the significance of our treatment dummy. Furthermore, the fact that our results survive the inclusion of industry fixed effects ensures that our documented effect is not simply attributable to industry peaks. Considering for known return predictors reduces the magnitude of the observed difference to around 16%. This is very similar to the results we obtain when using event study models with more than one risk factor or the calendar time portfolio approach, indicating that the magnitude of the effect of competing with a corporate superstar is in the range of 13% to 17%. Overall, the results presented in Table 7 are consistent with an incentive impact of superstar CEOs on their competitors, inducing them to outperform CEOs that compete with matched non-superstar CEOs. This difference in performance between our treatment and our control sample is not due to other return predictors or factors specific to an industry, a year or an award.

4.3 Heterogeneous treatment effects

We use heterogeneous treatment effects to study whether our results are more pronounced for certain CEOs in our sample. In particular, we analyze whether the effect is related to CEOs who have previously already received an award themselves, geographic proximity, CEOs' tenure, the number of competitors and corporate governance. We split our sample into groups based on each of these characteristics. Previous Award is a binary variable which is one if the competing CEO has previously already received one or more awards and zero otherwise. If a CEO previously received an award, we further differentiate whether he received the award within the last three years or more than three years ago. For the G-Index, the number of competitors and tenure we split the sample in two groups based on the sample median of each variable. For distance, we also split the sample in two groups but do not use the median as this would correspond to a distance of about 500 miles, which seems too large to proxy for geographical closeness. Instead, we use 100 miles as a threshold. The results do not notably change if we use other alternative distances such as 50 or 200 miles. If anything, the size of the effect tends to increase as we reduce the distance but due to the decreasing sample size, the significance also decreases.

The results of the heterogeneous treatment tests are presented in Table 8. Panel A shows that CEOs competing with a superstar CEO are positively affected if they have not previously received an award themselves. CEOs who have already received an award are not influenced if they received their last award more than three years ago. CEOs who have received an award within the last three years have a significantly negative CAR, supporting the findings of Malmendier and Tate (2009). Our findings indicate that CEOs who are perceived as being a superstar (as they previously received an award themselves) and who are no longer influenced by the negative effect of their own award are not affected by a

competitor being promoted or reconfirmed as a superstar¹¹. This could either be because they perceive themselves as superstars and therefore are not incentivized if they see a peer being promoted or because they have a higher status themselves (more awards) and therefore do not consider the winning CEO as a superstar compared to themselves.

The treatment effect is also stronger if CEOs are geographically close to the winning CEO (Panel B). As mentioned above, this could be due to the fact that some impacts of the awards are only observable within a limited geographic area. For instance, some symbols of improved status such as an improvement in golf handicaps (Malmendier and Tate, 2009) are presumably only observed by CEOs who live close to the winning CEO and ideally are even a member of the same golf club.

The results in Panel C indicate that CEOs with lower tenure are more strongly affected by a status shock to a competing CEO. Following Prendergast and Stole (1996) and Yim (2013), this finding could be due to higher incentives of younger CEOs to promote themselves in the labor market. As they are still at the beginning of their careers, the market opinion is still vague regarding their skills. By outperforming comparable companies, young CEOs have the opportunity to distinguish themselves as potential future superstars.

The results regarding the number of competitors and the G-Index presented in Panel D and E indicate that the size of the treatment effect is influenced by the level of entrenchment of competing CEOs. As competitiveness and corporate governance are related in the sense that competitiveness can be a substitute for good corporate governance (Giroud and Mueller,

¹¹ In an untabulated test, we find that the effect on competitors does not notably differ depending on whether the winning CEO is promoted or reconfirmed as a superstar. The reconfirmation effect tends to be larger than the promotion effect only in the longest event window. This provides some (albeit weak) evidence that CEOs who receive more than one award have a higher status than CEOs who only won one award and thereby have a larger incentivizing effect on their competitors. This finding is supported when studying the number of awards a winning CEO previously received. Winners with 4 or more previous awards on average tend to have a higher effect on their competitors than first time winners (untabulated).

2010, 2011), we interpret the more pronounced effect for companies with many competitors (Panel D) as evidence that less entrenched companies are more affected by a shock to CEO status. Entrenched CEOs only weakly react to a status change of a competitor as they are less concerned with their market environment. This finding is corroborated by the results in Panel E, which show that the treatment effect is lower for companies with worse corporate governance (high G-Index).

4.4 Robustness Tests

We perform several robustness tests to demonstrate the validity of our results. Table 9 shows a selection of these robustness tests. First, we relax the sample restrictions which we impose in our main specifications, namely that competitors have to be part of Execucomp and that there has to be a 365 day gap between being assigned to the treatment or the control group. The results do not change if we drop the Execucomp restriction (Column 1). Reducing the time constraint to one (Column 2) and zero days also yields similar results. Increasing the time constraint to three years and infinity yields economically the same results while being only marginally significant in case of the infinity constraint because of the reduced sample size and a selection bias towards more volatile small cap companies. Second, we also check if our results are distorted by the financial crisis. We therefore reduce the sample and only include awards (1) before 2004 (Column 3) and (2) only before 2003. As the longest event window is three years, we thereby ensure that our sample finishes before the start of the crisis. For this reduced sample, the magnitude slightly increases indicating that the effect decreases during the financial crisis. Regarding the sample construction, we also test the robustness of our competition definition. As an alternative to the Hoberg-Phillips measure, we repeat the analysis using SIC-2, SIC-3 (Column 4) and SIC-4 as competition measures. All three SIC measures support the previous findings. As we would expect the magnitude of the results increase from SIC-2 to SIC-4 as the competition classification becomes narrower.

A second group of robustness tests varies the matching procedure. First, we repeat the matching three times using an approach with replacement and one, two and three neighbors (Column 5), respectively. All three specifications confirm the previous findings. Second, we only consider those matches that are supported. The support of a match is defined as the difference of the propensity scores of award winners and their matches. We test two different support thresholds: 0.1 and 0.05. Restricting the sample to only those matches which are supported does not change our results. Third, we change the calculation of the propensity scores by (1) including award, industry and year fixed effects (Malmendier and Tate, 2009; Column 6) and (2) by excluding the age variable as it is not significant for all specifications of the logit regression. The inclusion of the fixed effects leads to an increase in magnitude and significance of our results. The exclusion of the age variable does not change the results. Additionally, we change the focus of our matching. Instead of matching winners to predicted winners we directly match control CEOs to the competitors of award-winners (treated CEOs). Column 7 in Table 9 shows that this alternative matching again yields similar results.

4.5 Alternative Explanation

We show that competitors of superstar CEOs earn a significant positive cumulative abnormal return and significantly outperform a group of comparable companies over three years subsequent to an award. We interpret this as evidence consistent with the theory put forward in Ehrenberg and Bognanno (1990) that superstars have an incentivizing effect on their peers. However, an alternative explanation of our finding could be that competitors of winning CEOs benefit from the documented loss of award winners (Malmendier and Tate, 2009) and gain part of the superstars' market shares. As such, the superstar system would not lead to an economic welfare gain as implied by Ehrenberg and Bognanno (1990) but solely to a redistribution of wealth among award winners and their competitors, equivalent to a zero

sum game. This alternative interpretation could explain the observed positive CARs of the treatment group.

We try to assess the likelihood of such an alternative explanation by studying the change in market capitalization of winners and their competitors compared to predicted winners and their competitors. To do this, we aggregate the market capitalization for winners (predicted winners) and their competitors. Market capitalization is measured 13 months and 1 month prior to the award and 11, 23 and 35 months subsequent to the award. Thereby we obtain one year intervals for which we can calculate the percentage change in market capitalization. Table 10 shows the means of the changes in market capitalization and t-tests for the differences in means, once without fixed effects and once with industry and year fixed effects. We observe that in the year prior to the award, the market capitalization of winners and their competitors as well as the market capitalization of predicted winners and their competitors significantly increase but there is no significant difference between the two. This lends further support to our finding in Table 1 and the finding of Malmendier and Tate (2009) that winners and predicted winners have a high performance prior to the award. It also further bolsters our confidence in the high quality of our matching procedure as there is no significant difference between the two groups. Subsequent to the award, however, we observe a significant change in market capitalization for the winners and their competitors only¹². The significant positive change indicates that the increase in market capitalization of competitors of winners is higher than the decrease in market capitalization of the award winner. In contrast, we do not observe a significant change in aggregate market capitalization for the

¹² We interpret this result as a lower bound as the actual positive economic welfare effect of superstars is likely to be larger. Note that we are likely to underestimate the effect because we only use competitors who we also use in our main event study in Table 4. This means that competitors who are dropped due to our sample restrictions are not included in the calculations. Given that robustness tests with less restricted samples lead to the same results (with sometimes even larger magnitudes) we consider the results presented in Table 10 as conservative estimates.

predicted winners and their competitors. The difference between the two groups is significant which provides support for the incentivizing theory of Ehrenberg and Bognanno (1990)¹³. If our results were purely due to a redistribution of wealth effect, we should not see a significant difference between superstars and their competitors and predicted winners and their competitors. We interpret our finding in Table 10 as further evidence that superstars truly have an incentivizing effect on their competitors and as indicative evidence of a positive welfare effect of a superstar system among corporate leaders.

5. Outperformance through risk-taking?

Competitors of award-winning CEOs have significantly higher positive abnormal returns compared to a sample of competitors of matched non-award winners. What might be potential reasons for this outperformance? As the outperformance is triggered by the award of the competitor, we are interested in actions managers consider when they try to catch up with a competing superstar. One possibility would be for managers to engage in increased risk-taking. Aron and Lazear (1990) theoretically show that companies which are less dominant in existing markets are more likely to try to enter new markets as they enjoy a relative benefit from high-variance strategies. Aron and Lazear (1990) motivate this argument by relating it to trailing football teams that throw long passes in the last minutes of a game. In these situations, a low-variance strategy would almost certainly lead to a defeat. This theory is supported by the tournament literature, in particular by Cabral (2002) who shows that laggards benefit from deviating from leaders. Besides changes in systematic risk, managers also have the possibility to increase the idiosyncratic risk of their company. Roussanov and Savor (2013) study the influence of CEOs' status concerns on their risk-taking activities. They observe that CEOs

¹³ Note that the fixed effects we use in the fourth column ensure that the difference between the two groups is not simply driven by different market conditions.

who are not married and therefore more concerned about their status engage in more risky projects, leading to an increase in idiosyncratic risk. Recent studies on mutual funds provide similar evidence on managers' asset allocations (Basak et al., 2007; Chen and Pennachi, 2009) suggesting that managers who are in the lead decrease their idiosyncratic risk (tracking error) whereas underperforming managers increase their idiosyncratic risk.

An increase in risk-taking is a viable option for managers to catch up with competing superstars. But when managers change their risk-taking attitude, there is no definite theoretical prediction whether managers either increase systematic or idiosyncratic risk. Potentially they could even do both. We use a difference-in-differences research design to study the effect of CEO awards on competitors' risk-taking. This allows us to control for omitted variables, common trends and shocks which affect the treatment as well as the control group. Consistent with prior literature on companies' risk taking such as Low (2009), we proxy risk-taking by stock return volatility, idiosyncratic volatility and market beta and compare the data of the year prior to the award year to the award year itself and the three subsequent years.

The risk-taking of award-winning competitors increases significantly in the years subsequent to the awards (Table 11). The return volatility of the treatment group increases by 3.9% over the three years after the CEO award compared to the control group. When we split total stock price risk into the idiosyncratic and the systematic part to study which component mainly drives the documented change in risk, we observe a significant increase of idiosyncratic risk. The systematic risk of the treatment group, however, does not significantly change compared to the control group and the pre-treatment level. Idiosyncratic risk increases by 2.6% compared to the control group and the pre-treatment level. These results show that the outperformance by competitors of winning CEOs documented above goes hand in hand

with an increase in more risk-taking. This increase in risk-taking is mainly driven by an increase in idiosyncratic risk. The increase in total risk and idiosyncratic risk is also economically significant. In the pre-award year the mean total risk is 0.42 and the mean idiosyncratic risk is 0.35. Thus, observing a competitor being promoted to superstar status leads to a 9.2% and a 6.3% increase in total and idiosyncratic risk, respectively.

Recent literature outlines that increased risk-taking can manifest itself through an increase in innovation activity. In their study on CEO overconfidence and risk-taking, Hirshleifer et al. (2012) show that innovation activity and risk-taking are closely linked. Other studies such as Guay (1999) and Roussanov and Savor (2013) use R&D spending as a proxy for risk-taking. As Hirshleifer et al. (2012) point out, R&D spending is the input to innovation whereas patents are the output of it. Following this reasoning, we use R&D spending and patent data to further study risk-taking behavior. This allows us to provide additional insights into our result on risk-taking by means of an increase in idiosyncratic risk of Table 11. Considering the R&D and innovation activity of companies establishes a channel to explain how managers actually achieve to increase the risk of their company. Similar to our investigation of risk-taking, we apply a difference-in-differences research design. We use aggregated yearly patent data on the firm level and compare the data of the year prior to the award year to the award year itself and the three subsequent years. To investigate whether the companies innovation quality changes, we further study the number of citations per patent, the number of zero citations per patent and the average patent generality. As higher patent quality does not necessarily increase the companies' private benefit (Trajtenberg et al., 1997) we also study the appropriability. We proxy for appropriability by using self-citations per patent (Trajtenberg et al., 1997). As patent data is a count variable, we use a Poisson regression when patents are the dependent variable. If the data is left-bounded at zero (e.g. cites per

patent), we use a log transformation and OLS regressions. In case of two-sided bounds (0 and 1), we use Tobit regressions. All standard errors are clustered on the firm level.

Our difference-in-differences analysis shows that there is a significant increase in the number of patents in the years after a competitor received an award (Column 1 of Table 12). This result provides evidence that the innovation output of superstars' competitors increases subsequent to an award. It also supports our previous finding that managers increase their risk-taking after a competitor received an award. While the results of the Poisson regression are highly statistically significant, it is difficult to give them economic meaning. Therefore, we repeat the analysis using an OLS regression and find that observing a competitor being promoted to superstar status leads on average to 14 patents more per year. Compared to 60 patents which companies filed on average in the year prior to the award, being a competitor of a superstar leads to a 23% increase in innovation activity, an increase that is thus economically significant. When we compare this increase in innovation output to the change in innovation input we observe that R&D expenditures do not significantly increase subsequent to an award. Consequently, this means that managers achieved to increase their innovation output at constant innovation inputs. Following Hirshleifer et al. (2012), we interpret this finding as evidence of an increase in innovation efficiency. Hirshleifer et al. (2012) show that higher innovation efficiency leads to higher stock market performance. Therefore, the finding that companies increase their innovation efficiency subsequent to an award provides a potent explanation for the positive stock market performance we observe in Section 4. We next assess whether companies also produce patents of higher quality. Columns (3) and (4) of Table 12 show that treated companies increase the number of citations per patent and decrease the number of zero citations per patent, both strong indicators of an increase in patent quality. Citations per patent is a direct measure of patent quality as it measures the number of descendant patents. To mitigate the effect of potential outliers (i.e.

patents receiving a very high number of citations), we also use the number of zero citations per patent as a further innovation quality proxy. It is an indirect measure of patent quality and is not vulnerable to positive outliers as it counts the number of poor patents. Both effects are also economically significant. Citations per patent increase by 23% compared to the pre-award mean of 1.16 and the number of zero cites per patent decreases by 22% compared to the pre-award mean of 0.41. The increase in patent quality is further corroborated by the observed increase in patent generality, which is significant for all three years following the award. This finding is also economically significant as a coefficient of 0.116 (in the third year) represents a 30% increase compared to the pre-award mean of 0.39. Finally, we investigate the number of self-citations per patent. Trajtenberg et al. (1997) outline that an increase in patent quality increases public welfare. However, it does not necessarily increase the private return of the company. To profit from high quality patents, a company has to be able to further develop the innovation. Following Trajtenberg et al. (1997), we measure this appropriability using the number of self-citations. Column (6) of Table 12 indicates that self-citations significantly increase after a CEO observes a competitor being promoted to superstar status. Over the three years following an award the (log) number of self-citations increases by 0.389. Compared to the pre-award mean of -0.74 this is an economic increase of 53%. This finding suggests that superstars' competitors do not only increase their innovation activity and quality but are also able to extract profits from the increased innovation quantity and quality. Overall, the results in Table 12 indicate more patenting and innovation activity as one channel through which competitors of award-winning CEOs significantly increase their risk-taking.. This observation is closely related to the finding of Roussanov and Savor (2013) that CEOs who seek higher status increase the tails of their return distribution.

We use a number of robustness checks to test the validity of our innovation results. Following the suggestion of Hall et al. (2001), we restrict our sample to patents which have

been applied for before 2004 (3 years prior to the end of the patent database) thereby mitigating the time truncation issue. Using this limited sample does not change our results. Following Hirshleifer et al. (2012), we also pay particular attention to firm-years with zero patents or zero citations. If we rerun our analysis only for firm-years with non-zero patents or citations, the results either do not notably change or become more significant. Instead of using a Poisson regression for the count data, Hirshleifer et al. (2012) apply an OLS regression on the logarithm of the count data. Repeating the analysis of Column 1 with the logarithm of the variable and an OLS regression does not change our results. Amore et al. (2013) only consider manufacturing companies (SIC codes up to 4000) based on the findings of Balasubramanian and Sivadasan (2011) that most patenting activity takes place in the manufacturing sector. When we restrict our sample to manufacturing firms, the significance of the results for the number of citations per patent decreases slightly, but the result remains significant. We also check that our results of cites per patent and zero cites per patent are not solely driven by the increase in self-citations per patent. If we measure cites per patent and zero cites per patent ignoring self-citations, the significance of the results does not change. However, the magnitude slightly decreases as expected due to the increase in self-citations per patent. In addition to measuring the average generality of firm patents in a given year, we also use the median generality and the patent with the maximum generality in a given year. While the median generality yields similar results as the mean generality, we observe slightly larger results for maximum generality.

6. Conclusions

In this paper, we conduct an empirical investigation of the effect of superstar CEOs on their competitors. Exploiting shocks to CEO status due to awards provided by major media outlets, we provide empirical evidence that CEOs who compete with superstar CEOs

outperform a control sample of companies that compete with CEOs who are very similar to actual superstar CEOs but do not win an award. Further, we document heterogeneity in this treatment effect. CEOs who have not previously received an award themselves, who are geographically close and who are not entrenched are on average more strongly affected if one of their peers is promoted to superstar status. Our results do not critically depend on the definition of the control sample, the matching technique employed, or the timeframe used for our analysis and are not explained by industry peaks or a redistribution of market shares from superstars to competitors. We identify an increase in risk-taking, in particular by becoming more efficient and better innovators, as a possible channel through which competitors of superstars achieve the observed outperformance. Studying the patenting activity of competitors of award-winning companies compared to other companies reveals that the competitors of winners significantly increase their number of patents and their innovation quality. In addition to the existing evidence documenting the negative effects of superstar CEOs and the negative effect of competition with superstars in the sports world, our paper sheds light on a surprising and previously undocumented ex post benefit of a superstar CEO culture: the incentives such superstars provide for their competitors.

Appendix: CEO Awards

Business Week (circulation: 993,000): Since 1988 the editorial staff chooses the Best Manager of the year and since 1991 they additionally pick the Best Entrepreneur of the year. Between 1990 and 2001 the latter has been awarded 71 times. The number of Best Entrepreneurs per year varies, ranging from none in 2000 to ten in 1995. Between 1988 and 2008 317 Best Managers were chosen. There was no award in 1989 and from 2005 until 2009. On average 18.6 Best Managers were chosen per year. The lowest number of awards is 6 in 1988 and 1990 and the highest is 29 in 2000.

Chief Executive (circulation: 42,000): Chief Executive has named a CEO of the year each year since 1987. The winner is chosen by a panel of CEOs.

Electronic Business Magazine (circulation: 65,000): The editorial staff has named a CEO of the year each year from 1997 until 2006.

Financial World (circulation: 430,000): From 1975 until the time the magazine ceased publication in 1997 the editorial staff of Financial World named a CEO of the year. There was a gold award (one winner per year) and a silver award (about ten winners per year, one winner per industry in 1995 summing up to a total of 70 winners in this year and five winners in 1997).

Forbes (circulation: 924,000): The editorial staff of Forbes started publishing a list of Best Performing CEOs in 2001. In 2001 five winners were chosen. From 2002 until 2008 ten winners per year were named. After 2008 the staff ceased to further publish the list.

Industry Week: The Industry Week award “CEO of the year” is based on a CEO survey. From 1993 until 2005 one winner per year was chosen.

Morningstar.com: Since 1999 the editorial staff of Morningstar names a CEO of the year in 1999 and 2001 two persons had the honor to receive this price. In the remaining years only one CEO received the award.

Time: For more than fifty years the Time magazine has named a person of the year. The winners are chosen by the editorial staff. In 1991, 1997, 1999 and 2010 the man of the year was a CEO.

Time/CNN: In 2001 Time in corporation with CNN picked 25 Most Influential Global Executives.

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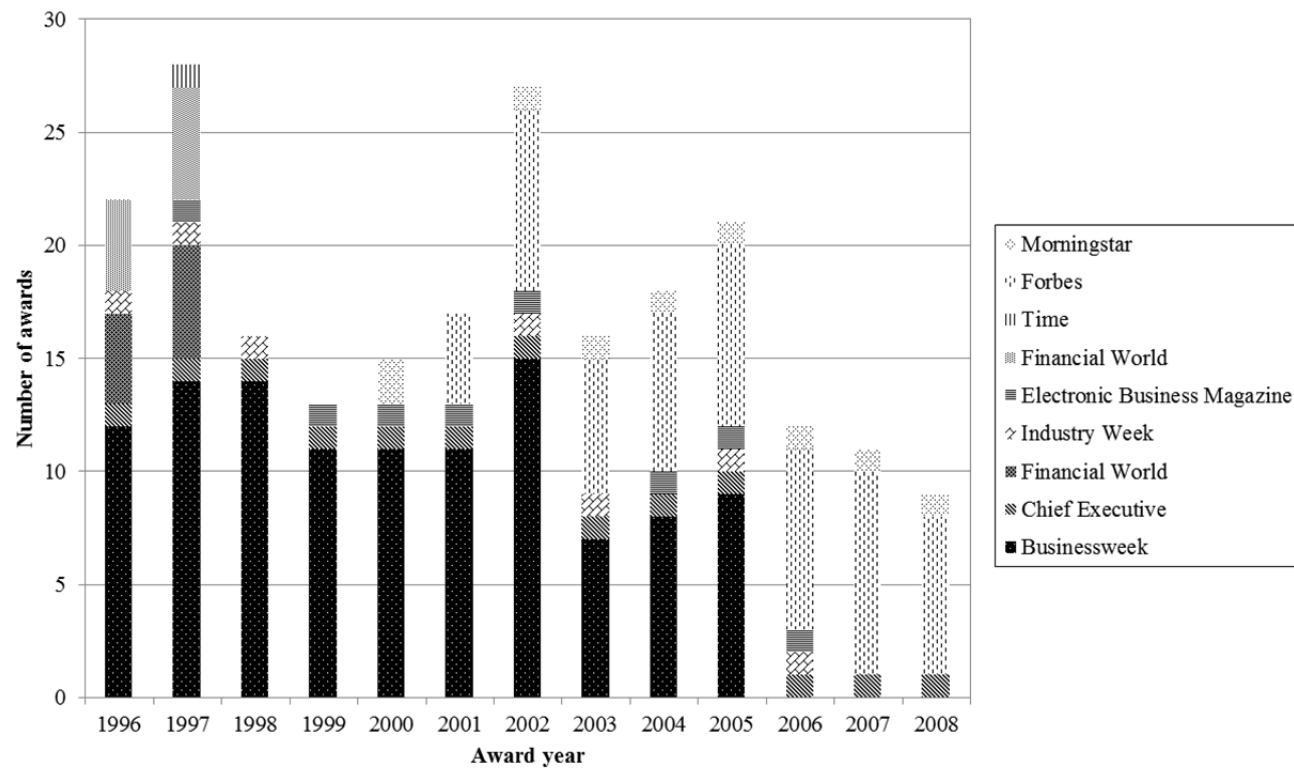


Figure 1
CEO awards by year and magazine

This figure shows the number of awards per magazine/media outlet from 1996 until 2008 where the number of awards is on the y axis and the respective year on the x axis.

Table 1
Descriptive statistics of winners, non-winners and predicted winners

	CEO award winners (W)			All non-winners (A)			Predicted winners (P)			Differences in means	
	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.	Obs	mean	Std. Dev.	p(W-A)	p(W-P)
Market capitalization	221	10.070	1.350	82,362	7.275	1.619	221	10.021	1.222	0.000 ***	0.776
Book-to-market ratio	221	0.306	0.471	82,362	0.552	0.741	221	0.321	0.274	0.000 ***	0.702
Returns_2_3	221	0.057	0.174	82,362	0.007	0.193	221	0.074	0.167	0.003 ***	0.271
Returns_4_6	221	0.061	0.206	82,362	0.001	0.238	221	0.059	0.191	0.004 ***	0.934
Returns_7_12	221	0.179	0.276	82,362	0.052	0.311	221	0.144	0.269	0.000 ***	0.205
Returns_13_36	221	0.489	0.608	82,362	0.207	0.627	221	0.436	0.529	0.000 ***	0.359
CEO female (dummy)	221	0.023	0.149	82,362	0.017	0.129	221	0.009	0.095	0.685	0.318
CEO age	221	56.407	8.363	82,362	55.972	7.582	221	56.805	6.851	0.647	0.732
CEO tenure	221	10.783	7.532	82,362	8.932	7.766	221	10.303	9.155	0.015 **	0.752

This table reports the descriptive statistics of all award winners, all non-winners and all CEOs who are defined to be predicted winners. The sample includes all months in all years from 1996 to 2008 in which a CEO award is conferred. Market capitalization is calculated as the stock price times common shares outstanding. It is measured two months prior to the award month and it is in log form. Book-to-market ratio is book value over market capitalization. Returns_x_y are total compound returns from the yth to the xth month prior to the award month. The column p(W-A) shows the p-value of the t-test of the difference in means between winners and all non-winners. p(W-P) shows the p-values of the differences in means between all winners and all non-winners. *, ** and *** represent significance at the 10%, 5% and 1% level, respectively. Standard errors are clustered on firm level.

Table 2
Determinants of award winners

	(1)	(2)	(3)
Market capitalization	0.924 *** (22.41)	1.024 *** (22.63)	1.053 *** (22.06)
Book-to-market ratio	-0.260 *** (-3.00)	-0.258 *** (-3.08)	-0.253 *** (-2.84)
Returns_2_3	1.456 *** (3.26)	1.565 *** (3.17)	1.557 *** (3.18)
Returns_4_6	1.102 *** (3.07)	2.000 *** (4.81)	2.067 *** (4.97)
Returns_7_12	1.140 *** (4.63)	1.780 *** (6.27)	1.752 *** (6.16)
Returns_13_36	0.408 *** (3.38)	0.719 *** (5.55)	0.710 *** (5.42)
CEO female (dummy)	0.767 * (1.66)	0.901 * (1.92)	0.956 ** (2.01)
CEO age	-0.022 ** (-2.13)	-0.023 ** (-2.08)	-0.015 (-1.37)
CEO tenure	0.043 *** (4.81)	0.043 *** (4.72)	0.039 *** (4.18)
Year fixed effects	no	yes	yes
Award fixed effects	no	yes	yes
Industry fixed effects	no	no	yes
Pseudo R ²	0.22	0.29	0.32
Observations	82,583	82,583	82,583

This table reports the results of a logit regression which is used to determine the relevant factors for winning an award. The sample includes all months in all years from 1996 to 2008 in which a CEO award is conferred. We use a binary variable equal to 1 if a CEO received an award in the respective month and 0 otherwise as the dependent variable. Market capitalization is calculated as the stock price times common shares outstanding. It is measured two months prior to the award month and it is in log form. Book-to-market ratio is book value over market capitalization. Returns_x_y are total compound returns from the yth to the xth month prior to the award month. *, ** and *** represent significance at the 10%, 5% and 1% level, respectively.

Table 3
Descriptive statistics of competitors of winners and competitors of predicted winners

	Competitors of winners (CW)				Competitors of predicted winners (CP)				Differences in means
	Obs	Mean	Median	Std. Dev.	Obs	Mean	Median	Std. Dev.	p(CW-CP)
Market capitalization	1,875	7.513	7.399	1.615	1,999	7.707	7.528	1.647	0.029 **
Book-to-market ratio	1,864	0.636	0.527	0.513	1,991	0.658	0.546	0.609	0.365
Returns_2_3	1,872	0.025	0.028	0.197	1,999	0.035	0.032	0.169	0.127
Returns_4_6	1,870	0.016	0.031	0.263	1,997	0.015	0.034	0.226	0.930
Returns_7_12	1,865	0.072	0.071	0.333	1,994	0.080	0.086	0.303	0.485
Returns_13_36	1,729	0.247	0.240	0.664	1,902	0.272	0.267	0.591	0.394
CEO female (dummy)	1,875	0.011	0.000	0.105	1,999	0.015	0.000	0.122	0.428
CEO age	1,875	55.269	56.000	7.884	1,998	55.816	56.000	7.516	0.202
CEO tenure	1,875	8.626	6.000	7.343	1,999	8.770	7.000	7.243	0.687

This table reports the descriptive statistics of all competitors of award winners and predicted winners. The sample includes the competitors of winners and predicted winners for all months in all years from 1996 to 2008 in which a CEO award is conferred. Market capitalization is calculated as the stock price times common shares outstanding. It is measured two months prior to the award month and it is in log form. Book-to-market ratio is book value over market capitalization. Returns_x_y are total compound returns from the yth to the xth month prior to the award month. The column p(W-A) shows the p-value of the t-test of the difference in means between winners and all non-winners. p(W-P) shows the p-values of the differences in means between all winners and all non-winners. *, ** and *** represent significance at the 10%, 5% and 1% level, respectively. Standard errors are clustered on firm level.

Table 4
Stock performance of competitors of award winners vs. competitors of predicted winners

	(1) Treatment	(2) Control	(3) Difference
Event Window	0.005 *	0.001	0.003
[-5,+5]	(1.76)	(0.56)	(0.99)
Event Window	0.040 ***	-0.007	0.047 **
[+6,+255]	(2.56)	(-0.61)	(2.42)
Event Window	0.136 ***	-0.023	0.158 ***
[+6,+510]	(4.96)	(-1.14)	(4.63)
Event Window	0.261 ***	-0.033	0.294 ***
[+6,+765]	(6.13)	(-1.16)	(5.69)

This table reports the cumulative abnormal returns of the treatment and the control sample. The treatment sample consists of the competitors of award winners and the control sample consists of the competitors of predicted winners. Competition is defined using the Hoberg-Phillips database. The competitors are only chosen from companies listed in the Execucomp database. Companies are excluded if they classify as treatment as well as control within a 365 days period. Predicted winners are chosen using a nearest neighbor propensity score matching with controls for firm size, book-to-market ratio, returns 2-3, 4-6, 7-12 and 13-36 months prior to the award month, CEO age, CEO tenure and CEO gender. Matching is done in every month in which an award is conferred, without replacement. Expected returns are calculated using a market model with the value weighted CRSP index as market returns and an estimation period ranging from -775 to -23 days prior to the award date. Event windows are in trading days. T-statistics are in parenthesis. Standard errors are clustered on the firm level. *, ** and *** represent significance at the 10%, 5% and 1% level, respectively.

Table 5
Stock performance of competitors of award winners for different return models

	(1)	(2)	(3)	(4)	(5)	(6)
	With alpha			Without alpha		
	Market	Fama-French	Carhart	Market	Fama-French	Carhart
Event Window	0.005 *	0.003	-0.004	0.003	0.002	-0.004
[-5,+5]	(1.76)	(1.10)	(-1.60)	(1.18)	(0.88)	(-1.60)
Event Window	0.040 ***	0.021	0.026 *	0.026 *	0.010	0.029 **
[+6,+255]	(2.56)	(1.46)	(1.79)	(1.86)	(0.86)	(2.44)
Event Window	0.136 ***	0.055 **	0.052 *	0.113 ***	0.043 **	0.068 ***
[+6,+510]	(4.96)	(2.03)	(1.90)	(5.48)	(2.06)	(3.24)
Event Window	0.261 ***	0.131 ***	0.133 ***	0.223 ***	0.127 ***	0.173 ***
[+6,+765]	(6.13)	(3.37)	(3.32)	(8.02)	(4.55)	(5.78)

This table reports the cumulative abnormal returns of competitors of award winners (treatment group) using several different estimation models. Competition is defined using the Hoberg-Phillips database. The competitors are limited to companies listed in the Execucomp database. Companies are excluded if they classify as treatment as well as control within a 365 days period. Expected returns are calculated using a market model with the value weighted CRSP index as market returns, the Fama-French model and the Carhart model. Data on the Fama-French and the momentum factor are obtained from Kenneth French's database. The estimation period ranges from -775 to -23 days prior to the award date. In Column 1 to 3 we estimate predicted returns including the alpha estimates whereas the alpha estimates are not included in Column 4 to 6. Event windows are in trading days. T-statistics are in parenthesis. Standard errors are clustered on the firm level. *, ** and *** represent significance at the 10%, 5% and 1% level, respectively.

Table 6
Calendar time portfolio returns

	Year 1	Year 2	Year 3
Alpha	0.002 (0.61)	0.004 * (1.70)	0.004 * (1.94)
Mktrf	0.207 ** (2.58)	0.218 *** (3.51)	0.213 *** (3.99)
SMB	0.076 (0.78)	0.186 ** (2.42)	0.316 *** (4.83)
HML	0.193 * (1.76)	-0.027 (-0.31)	-0.178 ** (-2.44)
UMD	-0.120 * (-1.86)	-0.071 (-1.43)	-0.010 (-0.24)
Observations	164	176	188
R ²	0.08	0.16	0.30

This table reports results of an equally-weighted portfolio that is long in competitors of award winners and short in competitors of predicted winners. The portfolio is rebalanced monthly. Firms enter the portfolio at the first month after the award and remain in the portfolio for 1, 2 and 3 years. Firms exit the portfolio if the CEO changes. Alpha is the alpha from a four factor model. Mktrf, SMB and HML are the Fama-French factors. UMD is the momentum factor. *, ** and *** represent significance at the 10%, 5% and 1% level, respectively.

Table 7
Cross-sectional analysis of the cumulative abnormal returns

	(1)	(2)	(3)	(4)
	[-5,+5]	[+6,+255]	[+6,+510]	[+6,+765]
Treatment	0.001 (0.20)	0.032 * (1.79)	0.118 *** (3.86)	0.163 *** (3.35)
Market capitalization	0.001 (0.21)	-0.039 *** (-3.14)	-0.133 *** (-6.48)	-0.210 *** (-6.73)
Returns_7_12	-0.012 *** (-3.85)	-0.101 *** (-7.64)	-0.186 *** (-8.56)	-0.258 *** (-8.16)
Book-to-market ratio	0.018 ** (2.42)	0.158 *** (3.20)	0.144 ** (2.20)	0.178 ** (2.15)
Age	0.002 (1.07)	0.012 (0.95)	0.034 * (1.67)	0.048 (1.55)
Tenure	-0.004 ** (-2.17)	-0.031 *** (-3.08)	-0.054 *** (-3.18)	-0.073 ** (-2.48)
CEO female (dummy)	0.001 (0.63)	-0.002 (-0.18)	0.008 (0.53)	0.010 (0.53)
Year fixed effects	Yes	Yes	Yes	Yes
Award fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Observations	3,880	3,216	2,707	2,240
R ²	0.03	0.11	0.16	0.18

This table reports the results of the cross-sectional regressions where the CARs of the different event windows are the dependent variables. Treatment is a binary variable which is one if the company is a competitor of an award-winning CEO and zero otherwise. Market capitalization is calculated as the stock price times common shares outstanding. It is measured two months prior to the award month and it is in log form. Book-to-market ratio is book value over market capitalization. Returns_x_y are total compound returns from the yth to the xth month prior to the award month. Distance is the distance in miles between the headquarters of a winner or predicted-winner and its competitor. T-statistics are in parenthesis. Standard errors are clustered on firm level. *, ** and *** represent significance at the 10%, 5% and 1% level, respectively.

Table 8
Heterogeneous treatment effects

	(1) [-5,+5]	(2) [+6,+255]	(3) [+6,+510]	(4) [+6,+765]
<i>Panel A - Has the Competitor previously received an award and if yes how long ago?</i>				
Treated x Previous Award (> 3 years)	-0.031 (-1.61)	-0.100 (-1.31)	-0.048 (-0.31)	0.006 (0.04)
Treated x Previous Award ($\leq$ 3 years)	0.005 (0.50)	-0.096 * (-1.66)	-0.185 ** (-2.20)	-0.362 ** (-2.70)
Treated x No Previous Award	0.001 (0.02)	0.032 * (1.67)	0.136 *** (4.10)	0.233 *** (4.90)
<i>Panel B - Distance between winner and competitor</i>				
Treated x Distance > 100	-0.001 (-0.32)	0.018 (0.91)	0.093 *** (2.72)	0.145 *** (2.94)
Treated x Distance $\leq$ 100	-0.001 (-0.05)	-0.018 (-0.37)	0.155 (2.05)	0.401 *** (3.53)
<i>Panel C - CEO tenure</i>				
Treated x CEO Tenure > 7	-0.008 * (-1.78)	0.003 (0.11)	0.086 ** (2.06)	0.162 *** (2.65)
Treated x CEO Tenure $\leq$ 7	0.005 (1.23)	0.038 (1.65)	0.139 *** (3.33)	0.227 *** (3.73)
<i>Panel D - Number of competitors</i>				
Treated x Num. Comp. > 77	0.001 (0.28)	0.011 (0.40)	0.127 *** (2.63)	0.288 *** (4.00)
Treated x Num. Comp. $\leq$ 77	-0.002 (-0.37)	0.036 (1.62)	0.108 *** (2.86)	0.119 ** (2.16)
<i>Panel E - G-Index</i>				
Treated x G-Index > 9	-0.001 (-0.03)	0.014 (0.53)	0.059 (1.34)	0.111 * (1.68)
Treated x G-Index $\leq$ 9	-0.006 (-1.16)	0.032 (1.23)	0.111 *** (2.69)	0.138 ** (2.33)
Award fixed effects	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes

This table shows the results of various tests for heterogeneous treatment effects. Treated is a binary variable equal to one if the company is a competitor of an award-winning CEO and zero otherwise. The other variables are also binary variables which are one in case the stated condition is fulfilled and zero otherwise. Previous Award is one if the competitor has previously won an award himself. Distance is the distance in miles between the headquarters of a winner or predicted-winner and its competitor. Num.Comp is the number of competitors each company has according to the Hoberg-Phillips database. G-Index is the governance index developed by Gompers et al. (2003). T-statistics are in parenthesis. Standard errors are clustered on firm level. *, ** and *** represent significance at the 10%, 5% and 1% level, respectively.

Table 9
Robustness tests (difference between treatment and control)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Event Window	0.016 ***	0.007 ***	0.005	0.011 ***	0.013 ***	0.005	0.009 **
[-5,+5]	(4.77)	(2.91)	(1.15)	(3.08)	(2.95)	(1.29)	(2.32)
Event Window	0.082 ***	0.061 ***	0.053 **	0.045 **	0.085 ***	0.123 ***	0.036 *
[+6,+255]	(4.64)	(4.75)	(2.15)	(2.08)	(3.66)	(5.96)	(1.82)
Event Window	0.152 ***	0.155 ***	0.180 ***	0.161 ***	0.158 ***	0.209 ***	0.110 ***
[+6,+510]	(5.00)	(6.31)	(4.22)	(4.14)	(3.92)	(5.96)	(2.98)
Event Window	0.272 ***	0.272 ***	0.362 ***	0.307 ***	0.218 ***	0.263 ***	0.156 ***
[+6,+765]	(6.13)	(7.62)	(5.61)	(5.52)	(3.76)	(4.98)	(2.68)

This table reports robustness tests of the results presented in Table 4. In Column 1 we drop the restriction that competitors have to be listed in the Execucomp database. Column 2 reports the results if we use a one day time restriction for competitors to be assigned to either treatment or control group. Results controlling for effects due to the financial crisis by excluding all awards after 2004 are reported in Column 3. In Column 4 we report the results using SIC-3 codes as an industry classification. In Column 5 we report the results based on a matching with replacement and three neighbors. In Column 6 we report results based on matching using award, industry and year fixed effects, identical to the setup of Malmendier and Tate (2009). In Column 7 we report results based on direct matching of companies to the competitors of winners. Expected returns are calculated using a market model with the value weighted CRSP index as market returns and an estimation period ranging from -775 to -23 days prior to the award date. Event windows are in trading days. T-statistics are in parenthesis. Standard errors are clustered on firm level. *, ** and *** represent significance at the 10%, 5% and 1% level, respectively.

Table 10
Change in market capitalization of winners and predicted winners and their competitors

	Winners plus competitors	Predicted winners plus competitors	Difference	Difference
Δ Market Capitalization [-1,0]	0.386 *** (3.92)	0.364 *** (4.27)	0.022 (0.24)	-0.007 (-0.09)
Δ Market Capitalization [0,1]	0.176 *** (2.63)	0.067 (1.52)	0.109 ** (2.00)	0.089 ** (2.09)
Δ Market Capitalization [0,2]	0.276 *** (3.05)	0.094 (1.34)	0.182 *** (4.85)	0.173 *** (4.47)
Δ Market Capitalization [0,3]	0.391 *** (2.88)	0.171 * (1.97)	0.220 *** (2.51)	0.187 ** (2.68)
Industry fixed effects	No	No	No	Yes
Year fixed effects	No	No	No	Yes

This table reports the percentage change in aggregate market capitalization for winners and predicted winners and their competitors before and after CEO awards. Market capitalization is measured 13 and 1 months prior to the award and 11, 23 and 35 months after the award. The first column shows the mean change in aggregate market capitalization for winners and their competitors. The second column shows the mean change in aggregate market capitalization for predicted winners and their competitors. The third and fourth columns show the differences in means with and without industry and time fixed effects. T-values are presented in parenthesis. *, ** and *** represent significance at the 10%, 5% and 1% level, respectively. Standard errors are clustered on firm level.

Table 11
Risk-taking activities of treatment and control companies

	$\sigma(\text{returns})$	$\sigma(\text{idiosyncratic CRSP})$	$\beta(\text{CRSP})$
[-1,0]	0.011 (1.55)	0.010 (1.54)	-0.054 (-0.94)
[-1,1]	0.021 ** (12.28)	0.013 * (1.81)	-0.070 (-1.10)
[-1,2]	0.039 *** (3.90)	0.026 *** (3.27)	0.080 (1.19)
[-1,3]	0.039 *** (3.39)	0.022 ** (2.51)	0.147 ** (2.16)
Year fixed effects	Yes	Yes	Yes
Award fixed effects	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes

This table reports results for difference-in-differences regressions for different time periods and risk measures as dependent variables. Year 0 is the award year. The other years are determined relative to the award years. $\sigma(\text{returns})$ is the volatility of monthly returns for the given firm-year. $\sigma(\text{idiosyncratic CRSP})$ measures the standard deviation of the residual of the regression of the monthly firm returns on the CRSP value-weighted index. $\beta(\text{CRSP})$ is the beta of this regression. For all regressions award, industry and year fixed effects are used. All regressions are OLS regressions. Standard errors are clustered on firm level. T-statistics are in parenthesis. *, ** and *** represent significance at the 10%, 5% and 1% level, respectively.

Table 12
Innovation activities of treatment and control companies

	(1) Patents	(2) R&D / TA	(3) Cites per patent	(4) Zero cites per patent	(5) Generality per patent	(6) Self cites per patent
[-1,0]	0.085 (1.32)	-0.001 (-0.07)	0.060 (0.84)	-0.005 (-1.09)	-0.007 (-0.88)	0.088 (1.04)
[-1,1]	0.252 ** (2.35)	0.018 (0.62)	0.165 (1.61)	-0.032 *** (-6.41)	0.059 *** (7.47)	0.255 ** (2.21)
[-1,2]	0.354 ** (2.32)	0.048 (1.35)	0.248 ** (2.42)	-0.053 *** (-8.33)	0.101 *** (11.35)	0.523 *** (3.59)
[-1,3]	0.523 *** (2.71)	0.054 (1.41)	0.265 ** (1.97)	-0.090 *** (-12.25)	0.116 *** (11.17)	0.389 ** (2.14)
Award Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Regression	Poisson	OLS	OLS	Tobit	Tobit	OLS

This table reports results for difference-in-differences regressions for different time periods and dependent variables studying the innovation activities of superstars' competitors. Year 0 is the award year. The other years are determined relative to the award year. Patents is the number of patents a company applied for in a given year. R&D/TA is research and development expenses scaled by total assets, in log form. Cites per patent are the number of all subsequent patents citing this year's patents divided by the number of this year's patents, in log form. Zero cites per patent is the number of this years' patents with no citations scaled by the number of this year's patents. Generality per patent is the average generality measure (Trajtenberg et al., 1997) per firm-year. Self cites per patent is the number of self-citations of this years' patents over the total number of this years' patents, in log form. Ratios are winsorized at the 5% level. For count data we use Poisson regressions whereas we use Tobit regression for variables which are bounded on both sides (zero cites per patent as well as generality per patent are bounded between 0 and 1). For the remaining variables we use an ordinary least square regression as they are unbounded due to the logarithmic transformation. All regressions include award, industry and year fixed effects. Standard errors are clustered on firm level. T-statistics are in parenthesis. *, ** and *** represent significance at the 10%, 5% and 1% level, respectively.