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Organization capital and executive performance incentives☆

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ABSTRACT

We conjecture that a firm's organization capital (OC) has a substitution effect on its executive pay-for-performance sensitivity (PPS) and empirically document a robust and significant substitution effect of OC on executive PPS. We use state-level unemployment insurance benefits as an instrumental variable for OC and show that the documented OC-PPS substitution effect is likely causal. Results are also robust to a stacked difference-in-differences estimation approach based on a quasi-natural experiment of exogenous CEO turnovers due to health-related issues. Our findings strongly suggest that greater OC substitutes for costly executive incentive compensation to sustain firm productivity and increase shareholder wealth.

1. Introduction

This paper examines the cross-sectional relationship between firms' organization capital (OC) and executive performance incentives. Our analysis reveals a robust and significant substitution effect of OC on executive pay-for-performance sensitivity (PPS). OC is the agglomeration of firm proprietary knowledge, business processes and systems that facilitates the match between labor and physical production facilities, and allows firms to use economic resources more efficiently.[1] Competitors cannot easily imitate a firm's OC because it is proprietary and tailor-made to the specific characteristics of an organization (Prescott and Visscher, 1980). A growing body of literature explores the role of OC in business production, firm performance, and corporate policies.

For example, Atkeson and Kehoe (2005) estimate that cash flows from OC account for more than 30% of the cash flows from physical capital. Lev and Radhakrishnan (2005) and Lev et al. (2009) confirm that OC is an important determinant of operating performance and firm value. Eisfeldt and Papanikolaou (2013) show that firms with greater OC have higher future stock returns and that greater OC renders the firm's cash flow to shareholders riskier (and thus shareholders demand a higher risk premium). Li et al. (2018) find that acquirers with superior OC achieve better deal performance and more synergy gains because high OC acquirers can, post integration, extract greater value from a more efficient matching of the original physical assets and the human resources of the target firm.[2]

We conjecture that productivity gains from an increase in OC can reduce a firm's reliance on managerial effort and hence, costly executive incentive compensation. For instance, a firm with efficient business processes and systems and well-trained employees would require less managerial effort (e.g., supervisory and planning effort) to maintain its competitive edge. However, because OC contributes to firm productivity and performance, the presence of OC effectively adds noise to the relationship between firm performance and managerial effort. As such, two implications for the relationship between OC and executive PPS may result. First, greater OC decreases the marginal effect of executive effort on firm out-

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[1] See, e.g., Prescott and Visscher (1980), Atkeson and Kehoe (2005), Lev, Radhakrishnan, and Zhang (2009), Carlin, Chowdhry, and Garmaise (2012), Eisfeldt and Papanikolaou (2013), and Li, Qiu, and Shen (2018).

[2] OC is also found to affect firm policies, such as capital structure and cash holdings (Berk, Stanton, and Zechner, 2010; Falato, Kadyrzhanova, and Sim, 2013). Moreover, Carlin, Chowdhry, and Garmaise's (2012) theory suggests that organization capital contributes to rising levels of total executive compensation, reduces employee turnover, and leads to a higher dispersion in the quality and pay of managers who have been internally promoted. Lustig, Syverson, and Van Nieuwerburgh (2011) develop a theory to show that successful firms accumulate more organization capital that then leads to growing inequality in within-industry executive pay and to sensitivity of compensation to size over time at the aggregate level.

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comes, which leads to an OC-PPS substitution effect. Second, due to the presence of OC and the resulting noise to the managerial effort-firm performance relationship, to elicit any given level of executive effort, incentives must be more high-powered. We derive both predictions from a simple extension to the standard principal-agent model and show that which effect dominates depends critically on whether the optimal effort level is determined endogenously.

Nevertheless, we expect that the substituting effect dominates for the following reasons. First, shareholders may not need to elicit full managerial effort due to the productivity gain from OC. Second, they may not be willing to elicit full executive effort because high-powered incentives are inherently costly, shown to be related to earnings management, accounting fraud, and managerial short-termism (e.g., Bergstresser and Philippon, 2006; Burns and Kedia, 2006; Goldman and Slezak, 2006; Efendi et al., 2007; Peng and Röell, 2008; Johnson et al., 2009; Benmelech et al., 2010; Qiu and Slezak, 2019). Equity-based pay is also shown to dilute shareholders and entrench managers (e.g., Denis et al., 1997). Lastly, if there exists an incentives cap of certain sort (e.g., explicitly in the form of legislation or implicitly via matching to peer firms), it can be implausible to elicit full executive effort in high OC firms where the cost of inducing effort is also higher and grows with OC.

We follow previous studies (e.g., Lev and Radhakrishnan, 2005; Eisfeldt and Papanikolaou, 2013; Li et al., 2018) to measure a firm's stock of OC using capitalized selling, general and administrative (SG&A) expenses. A large part of SG&A expenses consists of expenses on IT infrastructure, information systems, R&D, employee training, knowledge building, and the improvement of business processes, etc. Eisfeldt and Papanikolaou (2013) and Li et al. (2018) cross-validate the capitalized SG&A-based measure of OC in a number of ways. The results of these papers support the validity of the capitalized SG&A measure as a proxy for a firm's OC. Given that CEO is arguably a major contributor to the firm's OC, we focus our study on the impact of OC on the PPS of non-CEO executives, who do not contribute as substantially in the way the CEO does. In this way, we mitigate the potential endogeneity concern that CEO effort affects both OC and PPS.[3]

Using a merged U.S. sample from Compustat and Execucomp with 30,678 observations from 1992 to 2015, we show that OC is significantly and negatively associated with executive PPS measured by delta, which is the dollar change in executive wealth for a 1% change in firm value (Coles et al., 2006). On average, a one-standard-deviation increase in a firm's OC is associated with a 12.54% (22.58%) reduction in the delta of non-CEO executives, after controlling for year and industry (firm) fixed effects. Our results continue to hold if we measure executive PPS alternatively using the wealth-performance elasticity in Edmans et al. (2009), defined as delta scaled by the executive's total compensation (which reflects the percentage change in executive wealth for a 1% change in firm value). To address potential measurement errors in capitalized SG&A expenses resulted from expenses not related to investments in OC, we follow Li et al. (2018) in constructing an OC-based annual rank variable and show that our results remain robust. Moreover, we conduct a specification curve analysis (e.g., Simonsohn et al., 2020) to comprehensively verify the robustness of the documented OC-PPS substitution effect in a total of 32 model specifications. Specifically, we show that our finding is robust to alternative executive PPS measures, additional control of managerial ability, inclusion of CEO fixed effect and various fixed effect combinations, and to subsample periods with and without the global financial crisis.

To further identify the OC-PPS substitution effect, we start with an instrumental-variable regression approach using the state-level unemployment insurance (UI) benefits to extract the exogenous component of OC. Generous UI benefits are negatively associated with job switches to avoid unemployment and positively associated with employees' investment in marketable human capital (Levhari and Weiss, 1974; Brown and Kaufold, 1988; Light and Omori, 2004; Hassler et al., 2005). Li et al. (2018) argue that firms in states with more generous UI benefits invest more in OC because their employees have a lower turnover risk and stronger incentives to accumulate industry- and firm-specific human capital compared to employees of firms located in states with less generous UI benefits. Besides, state unemployment insurance is quite unlikely to directly affect top executive PPS because the size of state UI benefits is trivial compared to the level of executive compensation. Consistent with our expectation, results from the two-stage least-square (2SLS) regressions confirm that state-level UI benefits are significantly and positively associated with both the raw and rank measures of OC. More importantly, the instrumented OC measures remain significantly and negatively associated with the executive PPS measures, which suggests that the OC-PPS substitution effect is likely causal.

Moreover, we exploit exogenous CEO turnover as a quasi-natural experiment to help further identify the OC-PPS substitution effect. A CEO turnover is exogenous if the CEO's departure is not initiated by the firm but is due to factors exogenous to the firm such as planned retirement or health issues (as opposed to firm-specific reasons such as poor firm performance). Since CEO is arguably a major contributor to a firm's OC and the firm's OC is partially embodied in the CEO (e.g., Eisfeldt and Papanikolaou, 2013), we expect the exogenous CEO departure to represent a negative exogenous shock to the firm's OC. We further remove planned retirement turnover cases because such cases are, by definition, planned. The literature suggests that planned CEO retirements are intended to smoothen the transition process to reduce organizational disruptions (Naveen, 2006). Thus, we define our treatment firms as those that experienced an exogenous CEO turnover due to health-related issues. If OC indeed substitutes for executive PPS, we expect the treatment firms to upwardly adjust executive PPS following an exogenous CEO turnover due to well-specified health issues.

We conduct a difference-in-differences (DID) estimation similar to Gormley et al. (2013), where cohorts of treatment and control firms are stacked together in firm-year panel regressions. Control firms are in the same (2-digit SIC) industry as the treatment firm but without any CEO turnover, further filtered by a propensity score matching (PSM) on firm size and organization capital. We find that firms responded to health-related exogenous CEO turnovers, and thus to a loss of OC, by subsequently increasing PPS to other (non-CEO) executives. We rule out the possibility of non-CEO executive's turnover and promotion driving the result by further restricting the sample to executives who remain employed by the firm throughout the event window and whose ranks based on salary and bonus remain unchanged. Results are also robust to a dynamic DID estimation, which enables the exact timing of the treatment effect to be identified. We further show, in a placebo DID estimation using planned CEO retirements as the pseudo treatment events, that there is no positive treatment effect of planned CEO retirements on executive PPS. Overall, our findings from two sets of identification tests suggest that the impact of OC on executive PPS is most likely causal.

We also find that while both OC and executive PPS are positively associated with contemporaneous stock returns, a greater OC mitigates the positive relation between executive PPS and firm stock performance. Although this finding needs to be interpreted with caution, it supports our earlier conjecture that OC reduces the

[3] Nevertheless, in earlier versions of the paper we find a similar OC-PPS substitution effect using CEO's PPS measures.

marginal effect of executive effort on firm outcomes, which may lead shareholders of firms with greater OC to reduce costly executive PPS.

Our study contributes to the growing literature on the role of OC in business production, firm performance, and corporate policies. While previous empirical studies document the importance of OC in improving a firm's operational and stock performance (Lev et al., 2009; Edmans, 2011; Eisfeldt and Papanikolaou, 2013; Li et al., 2018), and in influencing the firm's capital structure and cash holdings (Berk et al., 2010; Falato et al., 2013), our empirical results show that a firm's OC affects its executive PPS. Our study also contributes to the executive compensation literature (e.g., see Murphy, 1999, and Edmans and Gabaix 2009, 2016) by revealing a novel OC-PPS substitution effect. Our findings suggest that corporate boards take OC into consideration when determining executive compensation schemes.

The rest of this paper is structured as follows. We develop our hypothesis in Section 2, describe the sample and variable construction in Section 3, and report the main results in Section 4. In Section 5, we address the endogeneity concerns using both an instrumental-variable approach and a quasi-natural experiment. Section 6 explores the implication of the OC-PPS substitution effect in relation to shareholder wealth. Section 7 concludes.

2. Hypothesis development

To illustrate the relationship of OC and executive PPS and motivate our empirical investigation, in the Online Appendix of the paper, we develop a standard principal-agent model following Holmstrom and Milgrom (1987) and add OC as an additional determinant of firm outcomes. Under the assumption that OC increases the firm's outcome but decreases the marginal effect of the agent's (i.e., the manager's) effort on firm outcome, the model shows that the relation between OC and executive pay-for-performance sensitivity depends critically on the optimal level of effort the principal (i.e., the board) wants to implement. If the principal wants to implement a fixed level of effort (e.g., full effort) from the agent, the optimal PPS is shown to be increasing in the firm's OC. This is because OC adds noise to the effort-outcome relationship—the agent may succeed with low effort because firm outcome is sustained by high OC or fail with high effort because of low OC. However, if the principal trades off the benefits and costs of high effort, the optimal PPS is shown to be decreasing in the firm's OC because OC increases firm outcome but decreases the marginal effect of the agent's effort. Thus, the model offers the following empirical implications. On the one hand, high OC firms may offer higher PPS to induce executive effort. On the other hand, PPS may be reduced in high OC firms as a result of efficiency gains from the substitution of OC for executive effort.

Nevertheless, we hypothesize a cross-sectional substitution effect of OC on executive PPS for the following reasons. First, the shareholders of high OC firms may not need to elicit full efforts from top executives, and thus the substituting effect dominates. This is because a firm with more efficient business processes and systems and well-trained employees should require less effort from its executives to maintain productivity and efficiency due to the productivity gain from OC. Second, the shareholders of high OC firms may not be willing to elicit full executive effort, and thus the optimal executive incentives are lower. This is because high-powered incentives are inherently costly, shown to be related to earnings management, accounting fraud, and managerial short-termism (e.g., Bergstresser and Philippon, 2006; Burns and Kedia, 2006; Goldman and Slezak, 2006; Efendi et al., 2007; Peng and Röell, 2008; Johnson et al., 2009; Benmelech et al., 2010; Qiu and Slezak, 2019). Equity-based pay is also shown to entrench managers (e.g., Denis et al., 1997).[4] Besides, if there exists an incentives cap of certain sort (e.g. explicitly in the form of legislation or implicitly via matching to peer firms), it can be implausible to elicit full executive effort in high OC firms where the cost of inducing effort is also higher and grows with OC. In the next section, we turn to an empirical examination of the relationship between OC and executive PPS.

3. Sample and variable construction

Since a firm's OC is partially embodied in the CEO (e.g., Eisfeldt and Papanikolaou, 2013), the CEO is a significant contributor to the firm's OC. While we admit that non-CEO executives may also contribute to the firm's OC, they arguably do not contribute substantially to the firm's OC in the way the CEO does. Thus, to mitigate the potential endogeneity concern that CEO effort affects both OC and PPS, our empirical investigation focuses on the relation between the firm's OC and the performance incentives paid to non-CEO executives.

3.1. Measuring organization capital

Consistent with Eisfeldt and Papanikolaou (2013), we measure a firm's stock of OC using capitalized SG&A expenses. SG&A expenses represent the overall nonproduction costs of operating a firm, including IT infrastructure, information system, R&D, employee training, advertising and marketing. These are expenses for developing a firm's knowledge and business processes and for improving the utilization of resources; in other words, investments in OC.

We obtain firm-year accounting data from the Compustat database and compute the stock of organization capital for firm i in year t using the perpetual inventory method that recursively calculates the stock of OC by accumulating the deflated value of SG&A expenses:

$$OC_{i,t} = (1-\delta_{OC})OC_{i,t-1} + \frac{SGA_{i,t}}{CPI_t} \tag{1}$$

where $SGA_{i,t}$ is firm i's SG&A expenses in year t; CPI_t is the consumer price index; and δ_{OC} is the depreciation rate of OC stock. The latter is set to 15%, as used by the U.S. Bureau of Economic Analysis (BEA) in their estimation of R&D capital in 2006 (Eisfeldt and Papanikolaou, 2013). The initial values of the stock of OC for firm i is set to:

$$OC_{i,0} = \frac{SGA_{i,1}}{g+\delta_{OC}} \tag{2}$$

where the average real growth rate of firm-level SG&A expenses, g, is industry- (at the two-digit SIC level) and decade-specific and depends on which year firm i first enters the Compustat database (Li et al., 2018); and $SGA_{i,1}$ is firm i's first-year SG&A expenses with non-missing data in Compustat.[5] Finally, we obtain our OC measure by standardizing the capitalized SG&A expenses by the firm's book value of total assets.

Eisfeldt and Papanikolaou (2013) and Li et al. (2018) carry out a total of seven empirical tests to cross-validate the capitalized SG&A measure as a robust proxy for OC. Eisfeldt and Papanikolaou (2013) adopt the following validation tests. First, they use 10-K filings to verify that high-OC firms (i.e., firms with high capitalized SG&A) are more likely to list the departure of key talents, which embodies a firm's OC, as a major risk factor facing these

[4] Note that these side costs of executive PPS are beyond the simple model discussed here. If these costs are brought into the current model, the substitution effect of OC on executive PPS will be even stronger.

[5] In our sample period from 1992 to 2015, 85% observations have valid non-missing SG&A expenses information. We treat subsequent missing values of firm i's SG&A expenses as zero.

firms. Second, the authors demonstrate that the capitalized SG&A measure is positively correlated with the managerial quality score constructed by Bloom and Van Reenen (2007). Third, the authors use the IT spending budget data from *Information Week* to verify that high-OC firms also tend to have greater demand for information technology. Fourth, they document higher levels of productivity in high-OC firms after accounting for physical capital and labor. Moreover, Li et al. (2018) find that the capitalized SG&A measure is positively correlated with the managerial ability score developed by Demerjian et al. (2012). Further, the authors find that high-OC firms tend to rank higher in Fortune magazine's "100 Best Companies to Work for in America" list. Finally, the authors find that high-OC firms also tend to rank higher in Computerworld's "100 Best Places to Work in IT" list.

Taken together, these findings from various validation tests support the view that the capitalized SG&A measure is a robust proxy for OC, which represents the firm's multi-faceted internal knowledge and expertise, business processes and systems. These characteristics of OC play a key role in enhancing the production output of a firm. To address potential measurement errors in capitalized SG&A expenses, we follow Li et al. (2018) and further construct the annual decile rank variable of firms' OC in the Compustat universe (RANK_OC). If the fraction of non-OC-related expenses (e.g., rent or mortgage on buildings, insurance, managerial perks, and audit fees) included in SG&A expenses does not vary across firms, this measurement error will not affect firms' rankings in terms of the ratio of OC to total assets.[6]

3.2. Measuring executive performance incentives

Throughout our empirical investigation, we use two alternative measures of PPS. Our first measure is delta, following Coles et al. (2006), defined as the dollar change in executive wealth for a 1% change in firm value :[7]

$$delta = \frac{\Delta \text{executive wealth}}{\%\Delta \text{firm value}} \tag{3}$$

where firm value is measured by stock price, and the change in executive wealth is calculated based only on vested and unvested shares and stock options. With regards to the stock-options-related wealth change, we follow Core and Guay's (2002) use of the Black-Scholes option valuation model that was modified by Merton (1973) to account for dividends. Our second measure of executive PPS is the wealth-performance elasticity (WPS) as in Edmans et al. (2009), defined as the delta scaled by the executive's total compensation. WPS reflects the percentage change in executive wealth for a 1% change in firm value. This measure better captures the incentives when executives have a multiplicative preference.

We obtain data on executive compensation from Standard & Poor's Execucomp database and data on stock returns and prices from the Center for Research in Security Prices (CRSP). We primarily focus on the average delta and WPS of all non-CEO executives (DELTA_MGMT and WPS_MGMT, respectively). Later, we also compute the average delta and WPS of non-CEO executives who stay in the firm and whose ranks based on salary and bonus do not change during the entire period in the difference-in-differences (DID) estimation.

[6] To address the concern that different accounting treatments of SG&A expenses across industries may introduce an industry-level measurement error in our OC measure, we include industry fixed effects or firm fixed effects in our main results and further use (2-digit SIC) industry-adjusted OC measures as a robustness check.

[7] Delta is also called *equity at risk* in Frydman and Jenter (2010) and Frydman and Saks (2010). Baker and Hall (2004) point out that this measure is more appropriate than the Jensen–Murphy statistic when the marginal product of executive efforts is increasing in firm size.

3.3. Sample and summary statistics

We merge data from CRSP, Compustat and Execucomp to form a sample containing 30,678 observations for 2795 firms (excluding utility and financial companies) over the sample period from 1992 to 2015. We consider the following firm characteristics: 1) market-to-book ratio (MTB), defined as the market value of equity to book value of equity; 2) leverage (LEVERAGE), defined as the book value of debt divided by the sum of book value of debt and market value of equity; 3) institutional ownership (IO_TOP5), defined as the fraction of shares outstanding held by the five largest institutional investors; 4) firm size (SIZE), defined as the natural logarithm of total assets; 5) return on assets (ROA), defined as income before extraordinary items scaled by total assets; 6) past stock returns (RET_FYEAR), defined as the buy-and-hold stock returns in the fiscal year; 7) asset tangibility (TANGIBILITY), defined as the book value of total net property, plant, and equipment scaled by total assets; and 8) investment (INVEST), defined as the capital expenditures scaled by total assets. Detailed definitions of these variables are provided in Table A1 in the Appendix.

Panel A of Table 1 provides an overview of the summary statistics for our organization capital measure OC, performance incentive measures DELTA_MGMT and WPS_MGMT for top executives, and the firm-specific characteristics. We winsorize all continuous variables at the 1st and 99th percentiles to limit the influence of outliers on our results. Mean (median) ratio of OC to total assets is 1.3 (0.98) with a standard deviation of 1.2. The mean (median) DELTA_MGMT is $104,338 ($37,973) and the mean (median) WPS_MGMT is 0.081 (0.038). These summary statistics are consistent with those documented in the aforementioned literature. Panel B of Table 1 shows the pairwise correlation matrix. We find that OC is negatively correlated with both executive PPS measures, consistent with our conjecture that OC is inversely related to executive PPS. To visualize the potential OC-PPS substitution effect, we further form annual quintiles based on organization capital of the sample firms and plot the average PPS and WPS of non-CEO executives for each OC quintile in Fig. 1. Consistent with our conjecture, Fig. 1 clearly shows an inverse relationship between executive incentive compensation and OC.

4. The substitution effect of organization capital on executive performance incentives

Our baseline model regresses the PPS measures of non-CEO executives, including the natural logarithm of DELTA_MGMT and WPS_MGMT, on lagged OC and firm-specific control variables.[8] Specifically, we estimate the following model based on a firm–year panel structure:

$$\begin{aligned} \text{PPS}_{i,t+1} = \beta_0 + \beta_1 OC_{i,t} + \beta X_{i,t} + Firm\ FE\ (or\ Industry\ FE) \\ + Year\ FE + \varepsilon_{i,t} \end{aligned} \tag{4}$$

where $PPS_{i,t+1}$ is either $\ln(DELTA\_MGMT_{i,t+1})$, the natural logarithm of average delta of non-CEO executives for firm i in year $t+1$ or $WPS\_MGMT_{i,t+1}$; $OC_{i,t}$ is our measure of OC for firm i in year t; and $X_{i,t}$ represents firm-specific control variables at year t. The control variables, as mentioned above, include firm size, market-to-book ratio, leverage, institutional ownership, ROA, asset tangibility, capital expenditure, and past stock returns (e.g., Hartzell and Starks, 2003; Fernandes et al., 2013;, Li et al., 2018). In all specifications, we control for year fixed effects (*Year FE*) and either firm fixed effects (*Firm FE*) or industry fixed effects

[8] We use the natural logarithm of DELTA_MGMT to facilitate an easier interpretation of the substitution effect. We find qualitatively similar and consistent results using raw DELTA_MGMT as the dependent variable.

Table 1
Summary Statistics.
Table 1. presents the summary statistics and correlation matrix of the variables used in our study. We merge data from CRSP and Compustat-Execucomp to form a sample which contains 30,678 observations for 2795 firms (excluding utility and financial companies) over the sample period from 1992 to 2015. Panel A presents the summary statistics of the variables. Panel B presents the pairwise correlations of the variables. Definitions of the variables are provided in the Appendix. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Summary Statistics

Variable	N	Mean	Standard Deviation	25th Percentile	Median	75th Percentile
OC	30,678	1.302	1.196	0.475	0.984	1.756
DELTA_MGMT	30,538	104.338	205.350	15.499	37.973	97.223
ln(DELTA_MGMT)	30,538	3.638	1.452	2.741	3.637	4.577
WPS_MGMT	30,284	0.081	0.161	0.020	0.038	0.071
SIZE	30,678	7.196	1.565	6.063	7.056	8.195
MTB	29,841	3.436	3.567	1.562	2.390	3.856
BLEVERAGE	30,677	0.321	0.282	0.066	0.298	0.477
IO_TOP5	30,678	27.858	9.806	21.194	27.405	33.981
ROA	30,676	0.038	0.112	0.016	0.052	0.090
TANGIBILITY	30,638	0.275	0.218	0.105	0.212	0.388
RET_FYEAR	30,674	15.892	51.819	−15.122	9.524	36.131
INVEST	30,678	0.057	0.054	0.022	0.040	0.072

Panel B: Pairwise Correlation

	OC	DELTA_MGMT	ln(DELTA_MGMT)	WPS_MGMT	SIZE	MTB	BLEVERAGE	IO_TOP5	ROA	TANGIBILITY	RET_FYEAR	INVEST
OC	1											
DELTA_MGMT	−0.088	1										
ln(DELTA_MGMT)	−0.193	0.683	1									
WPS_MGMT	−0.051	0.627	0.49	1								
SIZE	−0.291	0.338	0.497	0.026	1							
MTB	0.097	0.213	0.279	0.152	0.014	1						
BLEVERAGE	−0.137	−0.019	−0.023	−0.097	0.346	0.158	1					
IO_TOP5	0.015	−0.143	−0.136	−0.133	−0.116	−0.073	0.046	1				
ROA	−0.095	0.152	0.319	0.118	0.139	0.165	−0.175	−0.05	1			
TANGIBILITY	−0.228	−0.042	−0.043	−0.005	0.145	−0.093	0.209	−0.064	0.009	1		
RET_FYEAR	−0.015	0.122	0.228	0.123	−0.039	0.271	−0.073	−0.038	0.213	−0.044	1	
INVEST	−0.131	0.003	0.027	0.071	−0.029	0.022	0.009	−0.083	0.054	0.679	−0.03	1

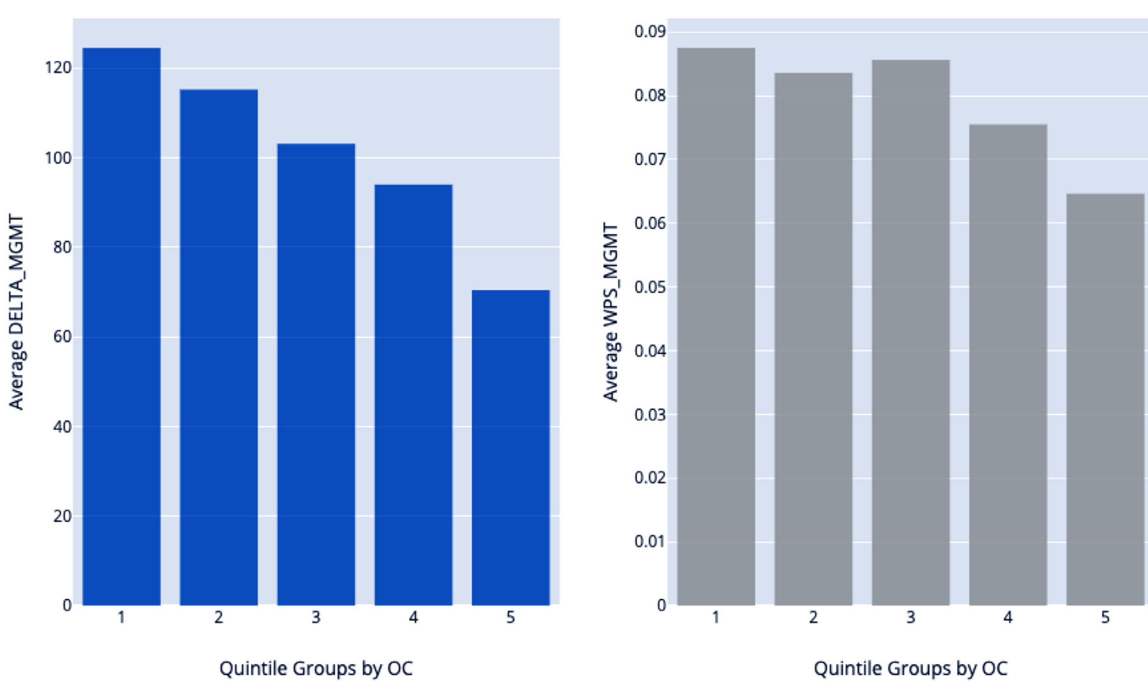

Fig. 1. Organization Capital and Executive Performance Incentives.
Fig. 1 shows the average delta of non-CEO executives (*DELTA_MGMT*) and the average wealth-performance elasticity of non-CEO executives (*WPS_MGMT*) across annual organization capital quintiles. We merge data from CRSP and Compustat-Execucomp to form a sample which contains 30,678 observations for 2795 firms (excluding utility and financial companies) over the sample period from 1992 to 2015. We then form annual quintiles based on the organization capital of the sample firms and calculate the average *DELTA_MGMT* and *WPS_MGMT* for each quintile. Definitions of the variables are provided in the Appendix.

Table 2
Organization Capital and Executive Performance Incentive.
Table 2. presents the baseline results of regressing the executive PPS measures on the lagged organization capital. In Panel A, the dependent variable is the natural logarithm of *DELTA_MGMT*. In Panel B, the dependent variable is the average wealth-performance elasticity as in Edmans et al. (2009) of non-CEO executives (*WPS_MGMT*). In all specifications, we include year fixed effects and either (2-digit SIC) industry fixed effects (columns (1) and (3)) or firm fixed effects (columns (2) and (4)), and lagged firm-specific control variables. For simplicity, in Panel B we report only the coefficient estimates of the OC measures. Heteroskedasticity-robust standard errors are clustered at the firm level. Definitions of the variables are provided in the Appendix. Numbers in parentheses are two-tailed *t*-statistics. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Dependent variables=ln(DELTA_MGMT)

	OC		*RANK_OC*	
	(1)	(2)	(3)	(4)
OC Measure	−0.112***	−0.214***	−0.042***	−0.101***
	(−6.363)	(−9.689)	(−5.438)	(−8.593)
SIZE	0.513***	0.208***	0.518***	0.217***
	(43.198)	(6.975)	(43.933)	(7.251)
MTB	0.086***	0.054***	0.085***	0.052***
	(18.343)	(14.275)	(18.114)	(14.128)
LEVERAGE	−1.163***	−0.721***	−1.164***	−0.732***
	(−15.162)	(−10.063)	(−15.049)	(−10.172)
IO_TOP5	−0.009***	−0.007***	−0.009***	−0.007***
	(−5.484)	(−5.427)	(−5.311)	(−5.282)
ROA	1.621***	0.880***	1.704***	0.957***
	(11.910)	(8.889)	(12.774)	(9.628)
TANGIBILITY	−0.687***	−0.315*	−0.705***	−0.319**
	(−5.984)	(−1.958)	(−6.118)	(−1.979)
RET_FYEAR	0.003***	0.002***	0.003***	0.002***
	(17.093)	(17.762)	(17.026)	(17.621)
INVEST	2.699***	1.305***	2.706***	1.329***
	(8.856)	(5.053)	(8.874)	(5.184)
Year Fixed Effect	Yes	Yes	Yes	Yes
Industry Fixed Effect	Yes		Yes	
Firm Fixed Effect		Yes		Yes
Observations	26,813	26,648	26,813	26,648
Adjusted R-squared	0.448	0.690	0.446	0.689

Panel B: Dependent variable=WPS_MGMT

	OC		*RANK_OC*	
	(1)	(2)	(3)	(4)
OC Measure	−0.010***	−0.018***	−0.005***	−0.010***
	(−4.432)	(−5.618)	(−4.973)	(−5.463)
Controls	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes
Industry Fixed Effect	Yes		Yes	
Firm Fixed Effect		Yes		Yes
Observations	26,650	26,484	26,650	26,484
Adjusted R-squared	0.085	0.401	0.086	0.401

(*Industry FE*). Moreover, we cluster standard errors at the firm level throughout to allow for possible correlations of firm outcomes over time.

Table 2 shows that, in all specifications, OC is negatively and significantly associated with the performance incentives paid to non-CEO executives at the 1% level. This result is robust to alternative OC measures and alternative executive PPS measures, supporting our conjecture of a OC-PPS substitution effect. The economic magnitude of the substitution effect is also large. For example, columns (1) and (2) of Panel A of Table 2 show that a one-standard-deviation increase in a firm's OC is associated with reductions of 12.54% (22.58%) in DELTA_MGMT, controlling for year fixed effects and industry (firm) fixed effects. In addition, Table 2 shows that the substitution effect is consistent across both industry and firm fixed effect models, with the magnitude being larger in the latter. The substantially larger impact of OC on PPS, after controlling for firm fixed effects, alleviates the concern that the OC-PPS substitution effect is driven by unobserved time-invariant firm heterogeneity.

Further, we conduct a specification curve analysis (e.g., Simonsohn et al., 2020) to verify the robustness of the documented OC-PPS substitution effect to a wide range of model specifications. First, we consider the inclusion of extra control for managerial ability, an important determinant of compensation as suggested by modern executive compensation theories. Specifically, we adopt the General Ability Index from Custódio et al. (2013) and compute the average General Ability Index of non-CEO executives (GAI_MGMT).[9] Second, we consider the inclusion of CEO fixed effects, which helps alleviate the concern that CEO affects both the firm's OC and non-CEO executive PPS. Third, we also examine the documented OC-PPS substitution effect in different subperiods with and without the global financial crisis (GFC).

Fig. 2 presents the result of the comprehensive specification curve analysis on the OC-PPS substitution effect, where we estimate a total of 32 models from different specification choices. Specifically, our variable of interest is OC; the dependent variable is either the natural logarithm of DELTA_MGMT or WPS_MGMT; the control variables are either as in the baseline model or baseline plus GAI_MGMT; the fixed effects are either industry and year, firm and year, or with CEO fixed effects included additionally; the sample period is either with or without GFC; finally, we cluster the standard errors at the firm level in all specifications. The upper panel of Fig. 2 plots the coefficient estimates of OC in various model specifications, in descending order, and the associated 95% confidence intervals. Sample size of each model specification is plotted as bar at the bottom of the upper panel. For simplicity, we annotate only the maximum and minimum coefficient estimates, as well as the threshold of zero. The lower panel reports the exact specification for each model, where colored dots indicate the choices from various specification alternatives.

First and most importantly, the specification curve shows that the coefficient estimates of OC are negative and statistically significant across all specifications, which suggests a robust OC-PPS substitution effect. Second, it shows that the inclusion of GAI_MGMT as a control variable does not materially affect the size and significance of the coefficients of OC, as the green-colored dots in the lower panel spread evenly. This helps alleviate the concern that managerial ability may explain the lower executive PPS in high OC firms. Third, the estimated coefficients of OC tend to be larger in magnitude in model specifications with more conservative fixed effects. For example, using the natural logarithm of DELTA_MGMT as the dependent variable, the coefficient of OC is −0.11 in the first model, which is the *minimum* estimated impact of OC given the ordered nature of the curve. The maximum estimated impact of OC is doubled at −0.22, where industry fixed effects are replaced with firm fixed effects and estimated on the sample excluding GFC. This further confirms that the documented OC-PPS substitution is robust and not due to model selection. Lastly, the OC-PPS substitution effect is also shown to be robust to subperiods with and without the GFC period.[10]

To summarize, the empirical evidence in this section strongly supports the hypothesis of a substitution effect of OC on executive PPS. The OC-PPS substitution effect is shown to be robust to alternative OC measures, alternative PPS measures, alternative model specifications and alternative subperiods. Hereafter, we focus on firm fixed effect models because they control for time-invariant unobservable firm factors.

[9] We thank Cláudia Custódio for generously sharing with us her updated GAI data.

[10] We also find that the substitution effect of OC on executive PPS is robust to industry-adjusted OC measures as shown in Table A2 in the Appendix.

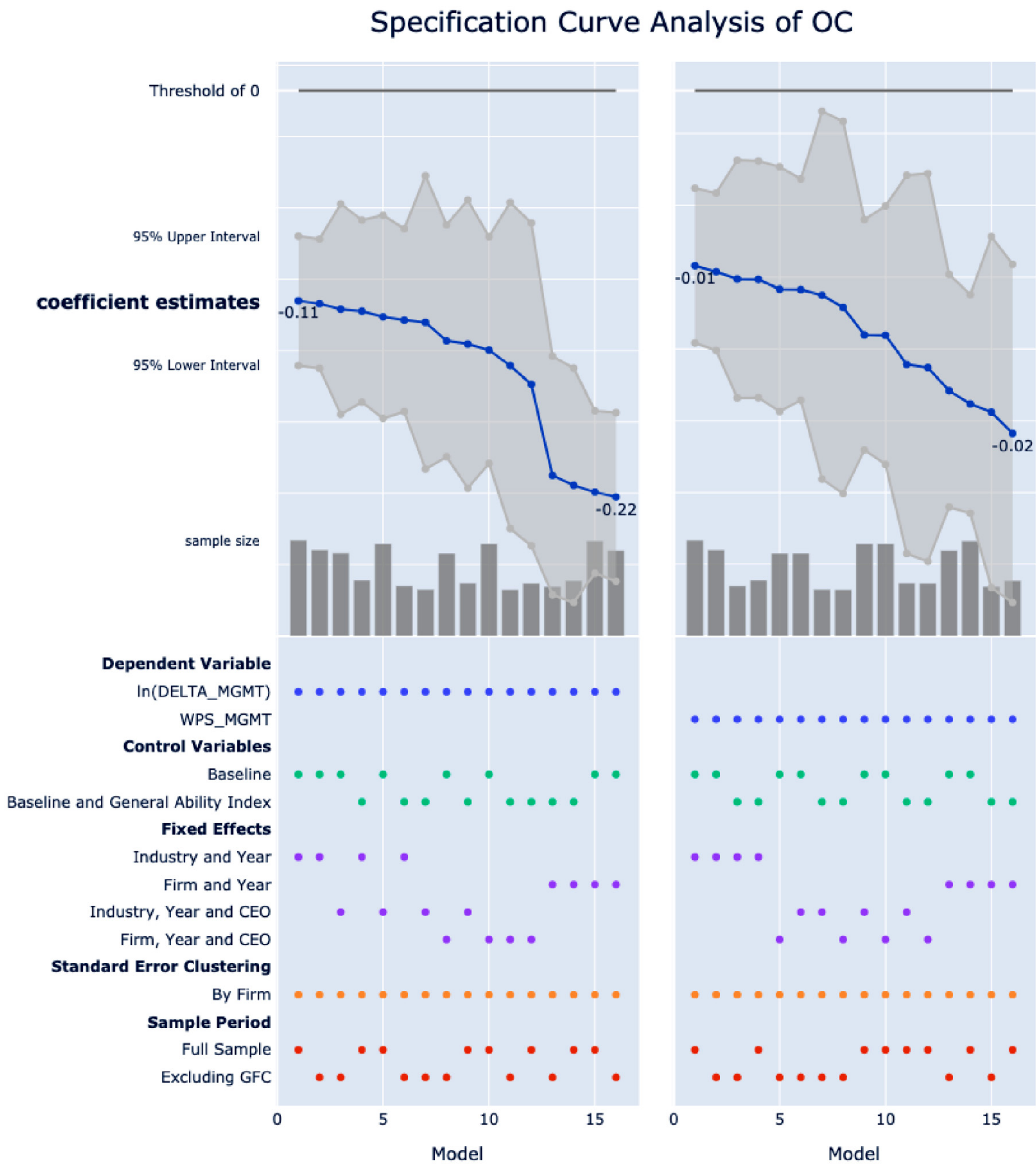

Fig. 2. Robustness using Specification Curve Analysis.
Fig. 2 presents the results of specification curve analysis of the relation between organization capital (OC) and non-CEO executive pay-for-performance sensitivity (PPS). Specifically, the upper panel plots the coefficient estimates of *OC* in various model specifications, in descending order, and the associated 95% confidence intervals. Sample sizes of each model are plotted as bars at the bottom of the upper panel. For simplicity, we annotate only the maximum and minimum coefficient estimates, as well as the threshold of zero. The lower panel reports the exact specification for each model, where colored dots indicate the choices from various specification alternatives. Definitions of the variables are provided in the Appendix. To interpret this specification curve, for example, *OC* has an estimated coefficient of −0.11 in the first model, which uses the natural logarithm of *DELTA_MGMT* as the dependent variable, and control variables as in the baseline model, including industry fixed effects and year fixed effects, clustering standard errors at the firm level, and is estimated on the full sample. Further, the ordered nature of the curve implies that this is the *minimum* estimated impact of *OC* on ln(*DELTA_MGMT*), whereas the maximum estimated coefficient is doubled at −0.22 when the industry fixed effects are replaced with the more conservative firm fixed effects and estimated on the sample excluding global financial crisis period. More importantly, in all specifications, we find the coefficient estimates of *OC* to be statistically significant, suggesting a robust OC-PPS substitution effect.

5. Identification

In this section, we employ two identification strategies to further identify the OC-PPS substitution effect and establish a causal interpretation for the documented OC-PPS substitution. The first identification strategy is an instrumental-variable regression approach, while the second strategy is a quasi-natural experiment.

5.1. Instrumental-variable approach

Li et al. (2018) use an instrumental-variable regression approach to extract the exogenous component of OC. The authors employ as instruments the staggered recognition of the Inevitable Disclosure Doctrine (IDD) by U.S. state courts and the state-level generosity of unemployment insurance (UI) benefits. The state adoption of the IDD restrains the mobility of knowledgeable key employees (e.g., Png and Samila, 2015) and thus should encourage firms' investments in OC. However, the adoption of IDD is not a good instrument in our setting because its restraining effect on executives' mobility and outside options can directly affect executives' bargaining power in compensation negotiations and their PPS, violating the exclusion condition of a valid instrument.

The generosity of state-level UI benefits also has a restraining effect on employee turnover (Light and Omori, 2004; Hassler et al., 2005) and can serve as a valid instrument in our setting for the following reasons. First, more generous UI benefits reduce job switches and the employee turnover risk so that firms located in states with more generous UI benefits should have stronger incentives to invest more in OC via accumulating industry- and firm-specific human capital (Levhari and Weiss, 1974; Brown and Kaufold, 1988), which satisfies the relevance condition. Second, the state-funded UI benefits are small in magnitude relative to executive compensation and intended to cover only basic needs of rank and file employees. Although we cannot rule out the possibility that state UI benefits may affect executive compensation design, the small UI benefits are unlikely to directly affect executive PPS, which satisfies the exclusion restriction.

Thus, we collect the data from the U.S. Department of Labor's Database on Significant Provisions of State UI Laws. We use the natural logarithm of the product of the maximum benefit amount and the maximum duration allowed as the measure of UI benefits generosity (UI_BENEFITS) (Hassler et al., 2005). We merge the state-year UI_BENEFITS measure with our firm-year panel based on the firm's *historical* headquarter state. Bai et al. (2020) find that, from 1969 to 2003, 87.50% firms never relocated, but the remaining 12.5% have conducted interstate relocation of their headquarter at least once. Because the Compustat database contains only the current headquarter state information, to obtain the firms' historical headquarter states, we start with the dataset from Bai et al. (2020) for the period from 1992 to 2003.[11] For the period after 2003, we follow the same approach and extract for each firm-year the headquarter state from the latest SEC 10 K/Q filing using the Augmented 10-X Header Data provided by the Notre Dame Software Repository for Accounting and Finance.[12]

Table 3 presents the results from the 2SLS regressions using the lagged UI_BENEFITS as the instrumental variable for OC. Panel A of Table 4 reports the results using the natural logarithm of DELTA_MGMT as the dependent variable, where column (1) (column (3)) reports the first-stage result where we regress OC (RANK_OC) on UI_BENEFITS and the same set of control variables as in the second stage. Heteroskedasticity-robust standard errors are clustered at the firm level. Consistent with the finding in Li et al. (2018), the lagged state-level UI benefits are significantly and positively associated with the firm's OC (RANK_OC) at the 5% (1%) level. The Cragg-Donald F-statistic of 47.76 (63.40 for RANK_OC) implies that our instrument does not suffer from the weak-instruments problem. In columns (2) and (4), we report the second-stage results where the firm's organization capital is measured by OC and RANK_OC, respectively. We find that the exogenous component of OC (RANK_OC) continues to be negatively and significantly related to the PPS of non-CEO executives at the 5% level. In Panel B of Table 4, we repeat the 2SLS regressions using WPS_MGMT as the alternative PPS measure. The first stage results are largely unchanged. In the second stage, the estimated coefficients of both instrumented OC and RANK_OC remain negative and are statistically significant at the 10% level.

In summary, our instrumental-variable regression results continue to show a negative OC-PPS relation, suggesting that the OC-PPS substitution effect is likely causal.

5.2. Quasi-natural experiment

Next, we turn to the second identification strategy by exploiting a quasi-natural experiment to further identify the OC-PPS relation. Given that CEO is a major contributor to a firm's OC and a firm's OC is partly embodied in its CEO (e.g., Eisfeldt and Papanikolaou, 2013), exogenous CEO departure poses a negative shock to the firm's OC. Therefore, we exploit exogenous CEO turnover as a quasi-natural experiment and use a difference-in-differences (DID) approach to identify the OC-PPS substitution effect. Exogenous CEO turnover refers to a situation where the CEO departure is not initiated by the firm and thus is not due to firm-specific reasons such as poor performance, but to exogenous factors, such as planned retirement or well-specified health issues. By contrast, when the CEO turnover is initiated by the firm, adjustments to executive incentives are more likely a result of endogenous factors (e.g., poor firm performance) that cause the turnover.

We use Eisfeldt and Kuhnen's (2013) CEO turnover dataset that records 2118 CEO departures from 1992 to 2006 based on the Execucomp database.[13] A CEO turnover event is identified when the executive holding the annual CEO title changes, and the event year is the last year of the departing CEO holding the title. Based on the Factiva news database, Eisfeldt and Kuhnen (2013) classify CEO departures into three categories following the procedure proposed by Parrino (1997). In 15.49% of all turnovers, the incumbent CEO was forced out, and in 29.18% of all cases, the CEO left because of exogenous reasons such as planned retirement or well-specified health problems. The remaining 55.33% of the cases cannot fit in these two categories and are labeled as unclassified. Using the Factiva database, Eisfeldt and Kuhnen (2013) also classify replacement-CEO types based on new CEO's former employer into either from inside the company, from within the industry but outside the company, or from outside the industry. While we focus on the exogenous CEO departures, we do not impose any restriction on the replacement CEO type because the replacement decision is made by the firm and thus endogenous. We further remove the retirement-related exogeneous CEO turnover cases. Planned CEO retirements, though unrelated to the firm's performance, are known in advance and the firms are expected to prepare for such events accordingly. For example, the board may designate an internal successor to minimize organizational disruption and human capital loss (Naveen, 2006), who is purposely trained to accumulate firm-specific knowledge and human capital for a number of years before the incumbent CEO steps down. Therefore, we remove those exogenous CEO departures with the stated reason of leaving being "RETIRED" from Execucomp. As a result, our exogenous CEO turnover events include only CEO departures due to well-specified health issues, which represent cleaner negative shocks to the treatment firms' OC.[14] We expect that the treatment firms will

[11] The dataset of firm historical headquarter state is based on the SEC filings post 1994 and hand-collected by the authors from the Moody's Manuals (later Mergent Manuals) and Dun & Bradstreet's Million Dollar Directory (later bought by Mergent). We thank the authors for making the data available at https://sites.google.com/utk.edu/matthew-serfling/research.

[12] We thank Bill McDonald for making this dataset available at https://sraf.nd.edu/data/augmented-10-x-header-data/.

[13] We thank the authors for making their dataset available at https://sites.google.com/site/andreaeisfeldt/.

[14] As shown in Table A3 in the Appendix, we find that our OC measures indeed drop following health-induced exogeneous CEO turnovers, statistically significant at the 10% level in the sample with propensity score matching and at the 1% level in a broader sample without propensity score matching. However, because our OC measure based on capitalized SG&A expenses is a stock measure, the reduction in OC can only result from the new CEO cutting SG&A expenses. This is consistent with the literature that new CEOs tend to reduce discretionary expenses, such as R&D, advertising and SG&A, to increase earnings in their early years of CEO service (e.g., Ali and Zhang, 2015; Pan, Wang and Weisbach, 2016).

Table 3
Instrumental Variable Regression
Table 3. presents the results of two-stage instrumental variable regressions using state-level unemployment insurance benefits (*UI_BENEFITS*) as the instrument for our OC measures. In Panel A, the dependent variable is the natural logarithm of *DELTA_MGMT*. In Panel B, the dependent variable is the average wealth-performance elasticity as in Edmans et al. (2009) of non-CEO executives (*WPS_MGMT*). In all specifications, we include year fixed effects, firm fixed effects, and lagged firm-specific control variables. Columns (1) and (3) report the first-stage result with the Cragg-Donald Wald F-test statistic for weak instrument; columns (2) and (4) report the second-stage result of using *OC* and *RANK_OC* as the OC measure, respectively. For simplicity, in Panel B we report only the coefficient estimates of the OC measures and *UI_BENEFITS*. Heteroskedasticity-robust standard errors are clustered at the firm level. Definitions of the variables are provided in the Appendix. Numbers in parentheses are two-tailed *t*-statistics. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Dependent variable=ln(DELTA_MGMT)

	OC		*RANK_OC*	
	1st Stage (1)	2nd Stage (2)	1st Stage (3)	2nd Stage (4)
OC Measure		−1.094**		−0.491**
		(−2.231)		(−2.231)
UI_BENEFITS	0.181**		0.403***	
	(2.428)		(2.605)	
SIZE	−0.641***	−0.363	−1.248***	−0.275
	(−21.682)	(−1.149)	(−25.567)	(−0.994)
MTB	0.003	0.055***	−0.002	0.050***
	(0.964)	(13.084)	(−0.367)	(12.684)
LEVERAGE	−0.136**	−0.856***	−0.386***	−0.896***
	(−2.199)	(−8.577)	(−3.920)	(−7.951)
IO_TOP5	0.001	−0.006***	0.003*	−0.005***
	(0.604)	(−4.555)	(1.795)	(−3.431)
ROA	−0.976***	0.009	−1.317***	0.430
	(−9.182)	(0.018)	(−8.672)	(1.408)
TANGIBILITY	1.196***	0.773	2.442***	0.665
	(8.388)	(1.261)	(8.899)	(1.173)
RET_FYEAR	−0.000	0.002***	−0.000***	0.002***
	(−1.168)	(15.401)	(−2.990)	(12.226)
INVEST	−1.364***	−0.084	−2.643***	0.110
	(−7.055)	(−0.115)	(−7.432)	(0.170)
Firm Fixed Effect	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes
Cragg-Donald Wald F-statistic for weak instrument	47.76		63.40	
Observations	25,860	25,733	25,860	25,733
Adjusted R-squared	0.861	0.687	0.899	0.687

Panel B: Dependent variable=WPS_MGMT

	OC		*RANK_OC*	
	1st Stage (1)	2nd Stage (2)	1st Stage (3)	2nd Stage (4)
OC Measure		−0.118*		−0.053*
		(−1.699)		(−1.699)
UI_BENEFITS	0.181**		0.403***	
	(2.428)		(2.605)	
Controls	Yes	Yes	Yes	Yes
Firm Fixed Effect	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes
Cragg-Donald Wald F-statistic for weak instrument	47.76		63.40	
Observations	25,860	25,574	25,860	25,574
Adjusted R-squared	0.861	0.390	0.899	0.390

upwardly adjust non-CEO executives' PPS following such negative shocks to the firms' OC.

We generally follow Gormley et al. (2013) in constructing cohorts of treatment and control firms. In each turnover-event cohort, we use firm-year observations from the three years before to three years after the exogenous CEO turnover event. To select control firms for the treatment firm in a turnover-event cohort, we start with all firms in the same (2-digit SIC) industry as the treatment firm but without any CEO turnover during the 7-year window. We then use propensity score matching method (PSM) to select those control firms with similar size and organization capital as the treatment firm. Specifically, we use the data immediately before the treatment events to estimate the propensity score of each firm to experience exogenous CEO turnover in the next year, conditional on firm size and organization capital. For each treatment firm, we choose the 4 control firms in the cohort with the nearest propensity scores. As with Lemmon and Roberts (2010), we choose 4 matches because it is not relying on too little information and also not incorporating observations that are not sufficiently similar.[15] We further use calliper matching and require the included control firms to have propensity scores within 0.01 from the treatment firm's score to alleviate the concern that even the 4 nearest matches may not be sufficiently similar. Control firms are selected with replacement (i.e., the same firm can be in the control groups of different treatment firms). Firms are not required to be

[15] Our results are robust to varying the number of control firm matches, for example, from 1 to 5.

Table 4
Propensity Score Matching Diagnostic.
Table 4 presents the pairwise comparison of our sample variables before and after our propensity score matching. We start by stacking together cohorts of treatment and control firms, where treatment firms are defined as those that experienced an exogenous CEO turnover due to well-specified health issues. Control firms in each cohort are those in the same (2-digit SIC) industry as the treatment firm but without any CEO turnover event during the three years before and three years after the event, further filtered by propensity score matching (PSM). Specifically, we use the data immediately before the treatment events to estimate the propensity score of each firm to experience the exogenous CEO turnover in the next year, conditional on the firm size and organization capital. For each treatment firm, we choose the 4 control firms in the cohort with the nearest propensity scores. We further use calliper matching and require the included control firms to have propensity scores within 0.01 from the treatment firm's score. Control firms are selected with replacement, i.e. the same firm can be in the control group of different treated firms. Firms are not required to be in the sample for the entire event window. Definitions of the variables are provided in the Appendix. Numbers in parentheses are standard errors. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Pre-Match			Post-Match		
	Control	Treatment	t-diff	Control	Treatment	t-diff
OC	1.26	1.29	−0.50	1.23	1.27	−0.61
	(0.01)	(0.06)		(0.03)	(0.06)	
SIZE	6.86	7.34	−7.45***	7.02	7.16	−1.64
	(0.01)	(0.08)		(0.04)	(0.08)	
MTB	6.30	3.77	0.42	4.01	3.89	0.23
	(1.35)	(0.30)		(0.27)	(0.34)	
LEVERAGE	0.30	0.33	−1.67*	0.33	0.33	0.08
	(0.00)	(0.02)		(0.01)	(0.01)	
IO_TOP5	26.15	24.91	2.29**	25.50	25.41	0.14
	(0.12)	(0.50)		(0.30)	(0.54)	
ROA	0.03	0.04	−1.16	0.03	0.05	−1.52
	(0.00)	(0.01)		(0.01)	(0.01)	
TANGIBILITY	0.25	0.32	−6.39***	0.29	0.30	−1.22
	(0.00)	(0.01)		(0.01)	(0.01)	
RET_FYEAR	25.24	18.96	1.36	22.46	18.42	0.89
	(1.02)	(2.58)		(2.33)	(2.75)	
INVEST	0.06	0.07	−2.10**	0.07	0.06	1.06
	(0.00)	(0.00)		(0.00)	(0.00)	
Observations	7107	355		1054	310	

in the sample for the entire event window. We then stack cohorts of treatment and control firms to form a full sample for our DID analysis.

Table 4 presents the PSM diagnostic of the pairwise *t*-test results on the pre-match and post-match samples. We find that, before PSM, the treatment firms and control firms have a similar level of OC stock, although they differ in certain other characteristics. However, after matching on industry, firm size and OC, we show that the treatment firms and control firms are comparable across all firm characteristics examined. We then estimate the treatment effect in a standard DID regression:

$$PPS_{i,c,t} = \beta_0 + \beta_1 treatment_{i,c} \times post_{t,c} + \omega_{i,c} + \gamma_{t,c} + \varepsilon_{i,c,t} \quad (5)$$

where $PPS_{i,c,t}$ is either $\ln(DELTA\_MGMT_{i,c,t})$, the natural logarithm of average delta of non-CEO executives in firm i in cohort c in year t or $WPS\_MGMT_{i,c,t}$; $treatment_{i,c}$ is an indicator that equals 1 if firm i is the treatment firm in cohort c; $post_{t,c}$ is an indicator that equals 1 if year t in cohort c is after the event year in the cohort (the event year is excluded from estimation when we estimate Eq. (5)); $\omega_{i,c}$ is firm-cohort fixed effects and $\gamma_{t,c}$ is year-cohort fixed effects. We control for both firm-cohort fixed effects and year-cohort fixed effects to be more conservative in controlling for unobserved heterogeneity because the firm and year fixed effects can vary across cohorts. Consistent with Gormley et al. (2013), we deliberately do not include time-varying firm characteristics as controls to avoid the "endogenous control" problem (e.g., Angrist and Pischke, 2009; Gormley and Matsa, 2016). Moreover, the fact that the treatment and control firms in the PSM sample have very similar firm characteristics, as evident in Table 4, helps alleviate the concern of excluding firm-level control variables in Eq. (5).

Panel A of Table 5 presents the stacked DID estimation results. Consistent with our conjecture, column (1) shows that following exogenous CEO turnovers due to health-related issues, the average PPS of non-CEO executives (DELTA_MGMT) increases by 21.29%, statistically significant at the 1% level. However, one concern is that there can also be turnovers of those non-CEO executives over the event window in both treatment and control firms, which can potentially cause changes (and thus the difference in changes) in the aggregate measure of executive PPS. Besides, executives may receive promotions during the event window, leading to changes in their performance incentives along with their career advancement.

In columns (2) and (3), we address these issues by restricting to the non-CEO executives who stay in the firm throughout the event window and whose annual ranks based on salary and bonus remain unchanged throughout the window. Column (2) shows that there is a 74.37% increase in the average PPS of such non-CEO executives (DELTA_MGMT_UNCHANGERANK) following an exogenous CEO turnover, statistically significant at the 1% level. Further, we include in column (3) the contemporaneous annual average ranks of those non-CEO executives in the firm based on salary and bonus (EXECUTIVE_RANK) to explicitly control for promotion effect.[16] If the documented treatment effect is mainly driven by executive

[16] Executive receiving the highest salary plus bonus is ranked 1 and greater value of EXECUTIVE_RANK means lower average executive rank. Therefore, the expected relation between EXECUTIVE_RANK and executive PPS is negative, as evident in column (3) of Table 5.

Table 5
Standard Difference-in-Differences Estimation.
Table 5 presents the results of standard stacked difference-in-differences (DID) estimations examining the impact of exogenous CEO turnover due to well-specified health issues on executive PPS. *TREATMENT* is an indicator that equals 1 for treatment firms that experienced an exogenous CEO turnover due to well-specified health issues. Control firms in each cohort are those in the same (2-digit SIC) industry as the treatment firm but without any CEO turnover event during the three years before and three years after the event, further filtered by propensity score matching (PSM). Specifically, we use the data immediately before the treatment events to estimate the propensity score of each firm to experience the exogenous CEO turnover in the next year, conditional on the firm size and organization capital. For each treatment firm, we choose the 4 control firms in the cohort with the nearest propensity scores. We further use calliper matching and require the included control firms to have propensity scores within 0.01 from the treatment firm's score. Control firms are selected with replacement, i.e. the same firm can be in the control group of different treated firms. Cohorts of treatment and control firms are then stacked together in the firm-year panel regression. *POST* is an indicator that equals 1 for the years after the event year and 0 for the years before the event. Panel A presents the results where the executive PPS is measured by *delta* and Panel B presents the results using wealth-performance elasticity as in Edmans et al. (2009) as the alternative PPS measure. In both panels, column (1) uses the average PPS measures of the non-CEO executives who may join or leave the firm during the event window, where the dependent variable is the natural logarithm of *DELTA_MGMT* and *WPS_MGMT*, respectively. To alleviate the concern of changing composition of the management team and the promotion of managers over time, column (2) restricts to the non-CEO executives who stay inside the firm throughout the event window and whose ranking based on salary and bonus remain unchanged, where the dependent variable is the natural logarithm of *DELTA_MGMT_UNCHANGERANK* and *WPS_MGMT_UNCHANGERANK*, respectively. Column (3) further controls the contemporaneous average ranking of non-CEO executives (*EXECUTIVE_RANK*). In all specifications, we include firm-cohort and year-cohort fixed effects that can vary across cohorts and thus are more conservative than simple fixed effects. We deliberately do not control for time-varying accounting variables that are likely affected by the CEO turnover and hence confound the difference-in-differences estimates. Heteroskedasticity-robust standard errors are clustered at the firm level. Definitions of the variables are provided in the Appendix. Numbers in parentheses are two-tailed *t*-statistics. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: ln(DELTA_MGMT) and variants			
Dependent Variable =	ln(*DELTA_MGMT*)	ln(*DELTA_MGMT_UNCHANGRANK*)	
	(1)	(2)	(3)
TREATMENT × *POST*	0.193***	0.556***	0.538**
	(2.934)	(2.641)	(2.580)
EXECUTIVE_RANK			−0.211*
			(−1.939)
Firm-Cohort Fixed Effect	Yes	Yes	Yes
Year-Cohort Fixed Effect	Yes	Yes	Yes
Observations	7382	1757	1757
Adjusted R-squared	0.667	0.336	0.340
Panel B: WPS_MGMT and variants			
Dependent Variable =	*WPS_MGMT*	*WPS_MGMT_UNCHANGRANK*	
	(1)	(2)	(3)
TREATMENT × *POST*	0.044***	0.153***	0.152***
	(4.934)	(3.572)	(3.553)
EXECUTIVE_RANK			−0.029
			(−1.187)
Firm-Cohort Fixed Effect	Yes	Yes	Yes
Year-Cohort Fixed Effect	Yes	Yes	Yes
Observations	7411	1941	1941
Adjusted R-squared	0.407	−0.029	−0.029

promotion, then controlling for EXECUTIVE_RANK should render the treatment effect insignificant. However, after controlling for the promotion effect, we continue to find a negative and statistically significant (at the 5% level) treatment effect.

In Panel B of Table 5, we repeat the DID estimations using the average WPS of the non-CEO executives as the dependent variable. Consistent with the results on executive delta in Panel A, we continue to find a negative and statistically significant (at the 1% level) treatment effect on executive WPS across different models.

To verify the parallel trend assumption for DID estimation and examine also the timing of the treatment effect, in Table 6, we extend the standard DID model by replacing the $post_{t,c}$ indicator with year-specific indicators. Specifically, we include the event year in the estimation and estimate the following dynamic DID regression specification:

$$\begin{aligned} PPS_{i,c,t} = {} & \beta_0 + \beta_1 treatment_{i,c} \times d_{-2,t,c} + \beta_2 treatment_{i,c} \times d_{-1,t,c} \\ & + \beta_3 treatment_{i,c} \times d_{0,t,c} + \beta_4 treatment_{i,c} \times d_{1,t,c} \\ & + \beta_5 treatment_{i,c} \times d_{2,t,c} + \beta_6 treatment_{i,c} \times d_{3,t,c} \\ & + \omega_{i,c} + \gamma_{t,c} + \varepsilon_{i,c,t} \end{aligned} \tag{6}$$

where $PPS_{i,c,t}$ is one of the two executive PPS measures used in Table 5; $d_{j,t,c}$ is an indicator that equals 1 if year t in cohort c is the jth year relative to the event year; other notations follow previously given definitions. Such a dynamic DID model enables us to examine both the existence and timing of the treatment effect. If the increase in executive PPS is caused by exogenous CEO turnover, then we should expect zero difference-in-differences between the treatment firms and control firms prior to the CEO turnover event; this means β_1 and β_2 should be insignificant. We expect that the event-year DID estimate, β_3, to be either 0 or positively significant as it may take some time for the board to respond by adjusting executive performance incentives. Therefore, any increase in executive PPS due to negative shocks to OC induced by exogenous CEO turnover events should be measured by the post-event DID estimates, β_4 to β_6.

In Panel A of Table 6, we observe no significant difference between the changes of non-CEO executive PPS in treatment and

Table 6
Dynamic difference-in-differences estimation.
Table 6 presents the results of dynamic difference-in-differences (DID) estimations, extending the DID model in Table 5, examining the impact of exogenous CEO turnover due to well-specified health issues on executive PPS, where the definitions of treatment and controls follow Table 5. To verify the parallel trend assumption and identifying the timing of the treatment effect, we replace the single *POST* indicator with event-year specific indicators *d*, where $\boldsymbol{d_j}$ is an indicator that equals 1 if a year is the *j*-th year after the exogenous CEO turnover. In all specifications, we include firm-cohort and year-cohort fixed effects that can vary across cohorts and thus are more conservative than simple fixed effects. We deliberately do not control for time-varying accounting variables that are likely affected by the CEO turnover and hence confound the difference-in-differences estimates. Heteroskedasticity-robust standard errors are clustered at the firm level. Definitions of the variables are provided in the Appendix. Numbers in parentheses are two-tailed *t*-statistics. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: ln(DELTA_MGMT) and variants			
Dependent Variable =	ln(*DELTA_MGMT*)	ln(*DELTA_MGMT_UNCHANGRANK*)	
	(1)	(2)	(3)
TREATMENT × d_{-2}	−0.001	0.090	0.114
	(−0.015)	(0.468)	(0.590)
TREATMENT × d_{-1}	0.002	−0.060	−0.034
	(0.027)	(−0.242)	(−0.140)
TREATMENT × d_0	0.091	0.420*	0.446*
	(1.180)	(1.715)	(1.845)
TREATMENT × d_1	0.332***	1.559***	1.567***
	(3.810)	(5.721)	(5.739)
TREATMENT × d_2	0.136	0.071	0.032
	(1.437)	(0.227)	(0.104)
TREATMENT × d_3	0.070	−0.082	−0.084
	(0.694)	(−0.355)	(−0.370)
EXECUTIVE_RANK			−0.235**
			(−2.502)
Firm-Cohort Fixed Effect	Yes	Yes	Yes
Year-Cohort Fixed Effect	Yes	Yes	Yes
Observations	8688	1951	1951
Adjusted R-squared	0.688	0.383	0.388
Panel B: WPS_MGMT and variants			
Dependent Variable =	*WPS_MGMT*	*WPS_MGMT_UNCHANGRANK*	
	(1)	(2)	(3)
TREATMENT × d_{-2}	0.010	−0.017	−0.012
	(0.856)	(−0.415)	(−0.282)
TREATMENT × d_{-1}	0.013	0.049	0.054
	(1.060)	(0.780)	(0.863)
TREATMENT × d_0	0.017	0.130*	0.135*
	(1.438)	(1.841)	(1.918)
TREATMENT × d_1	0.055***	0.226***	0.230***
	(3.878)	(3.561)	(3.636)
TREATMENT × d_2	0.053***	0.187**	0.183**
	(3.747)	(2.472)	(2.437)
TREATMENT × d_3	0.044***	0.085	0.087
	(2.996)	(1.539)	(1.564)
EXECUTIVE_RANK			−0.033
			(−1.620)
Firm-Cohort Fixed Effect	Yes	Yes	Yes
Year-Cohort Fixed Effect	Yes	Yes	Yes
Observations	8717	2169	2169
Adjusted R-squared	0.434	0.026	0.028

control firms before exogenous CEO turnover events. The treatment effect is only observable at or after the event year across all specifications, as supported by the positive and statistically significant coefficient estimates of β_3 and, in particular, β_4. This finding suggests that the parallel-trend assumption for DID estimation is well satisfied and the treatment effect of an increase in executive PPS is mostly likely due to the exogenous CEO turnover and the resulting negative shock to the OC of treatment firms. Similarly, Panel B of Table 6 uses WPS as the alternative PPS measure and repeats all DID estimations described above. We find that the identified negative treatment effect again exists only at or after the event year.

As discussed, planned CEO retirements are planned by definition and known well in advance. Therefore, this type of turnover event is not expected to induce an unexpected negative shock to the firm's OC. This suggests that if we use exogenous CEO turnovers due to planned retirements as the pseudo treatment events, we should not expect to observe a positive treatment effect on executive PPS. Table 7 presents the results of such placebo DID estimations. As expected, the treatment effect is statistically insignificant in four out of six models, and in the remaining two models we find a significant and negative treatment effect (which suggests a reduction in other managers' PPS following planned CEO retirements).[17] These results clearly suggest that planned CEO retirements do not cause an increase in executive PPS.

[17] The treatment effect is only negative and significant in the two models where we restrict to non-CEO executives who stay in the firm throughout the event window and whose annual executive ranks remain unchanged. It is unlikely that a planned CEO retirement would lead to an increase in the firm's OC (and thus the re-

Table 7
Difference-in-Differences Estimation using Planned CEO Retirements.
Table 7 presents the results of standard stacked difference-in-differences (DID) estimations examining the impact of exogenous CEO turnover due to planned retirements on executive PPS. *TREATMENT* is an indicator that equals 1 for treatment firms that experienced an exogenous CEO turnover due to planned retirements. Control firms in each cohort are those in the same (2-digit SIC) industry as the treatment firm but without any CEO turnover event during the three years before and three years after the event, further filtered by propensity score matching (PSM). Specifically, we use the data immediately before the treatment events to estimate the propensity score of each firm to experience the exogenous CEO turnover in the next year, conditional on the firm size and organization capital. For each treatment firm, we choose the 4 control firms in the cohort with the nearest propensity scores. We further use calliper matching and require the included control firms to have propensity scores within 0.01 from the treatment firm's score. Control firms are selected with replacement, i.e. the same firm can be in the control group of different treated firms. Cohorts of treatment and control firms are then stacked together in the firm-year panel regression. *POST* is an indicator that equals 1 for the years after the event year and 0 for the years before the event. Panel A presents the results where the executive PPS is measured by *delta* and Panel B presents the results using wealth-performance elasticity as in Edmans et al. (2009) as the alternative PPS measure. In both panels, column (1) uses the average PPS measures of the non-CEO executives who may join or leave the firm during the event window, where the dependent variable is the natural logarithm of *DELTA_MGMT* and *WPS_MGMT*, respectively. To alleviate the concern of changing composition of the management team and the promotion of managers over time, column (2) restricts to the non-CEO executives who stay inside the firm throughout the event window and whose ranking based on salary and bonus remain unchanged, where the dependent variable is the natural logarithm of *DELTA_MGMT_UNCHANGERANK* and *WPS_MGMT_UNCHANGERANK*, respectively. Column (3) further controls the contemporaneous average ranking of non-CEO executives (*EXECUTIVE_RANK*). In all specifications, we include firm-cohort and year-cohort fixed effects that can vary across cohorts and thus are more conservative than simple fixed effects. We deliberately do not control for time-varying accounting variables that are likely affected by the CEO turnover and hence confound the difference-in-differences estimates. Heteroskedasticity-robust standard errors are clustered at the firm level. Definitions of the variables are provided in the Appendix. Numbers in parentheses are two-tailed *t*-statistics. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: ln(DELTA_MGMT) and variants			
Dependent Variable =	ln(*DELTA_MGMT*)	ln(*DELTA_MGMT_UNCHANGRANK*)	
	(1)	(2)	(3)
TREATMENT × *POST*	0.027	−2.250***	−1.886**
	(0.140)	(−4.442)	(−2.548)
EXECUTIVE_RANK			0.333
			(0.823)
Firm-Cohort Fixed Effect	Yes	Yes	Yes
Year-Cohort Fixed Effect	Yes	Yes	Yes
Observations	4167	718	718
Adjusted R-squared	0.676	0.837	0.840
Panel B: WPS_MGMT and variants			
Dependent Variable =	*WPS_MGMT*	*WPS_MGMT_UNCHANGRANK*	
	(1)	(2)	(3)
TREATMENT × *POST*	−0.018	−0.154	−0.028
	(−0.750)	(−1.170)	(−0.178)
EXECUTIVE_RANK			0.120
			(1.649)
Firm-Cohort Fixed Effect	Yes	Yes	Yes
Year-Cohort Fixed Effect	Yes	Yes	Yes
Observations	4179	778	778
Adjusted R-squared	0.427	0.516	0.530

We carry out further robustness tests by using a broader sample that include control firms (without any CEO turnover during the 7-year event window) from the same (2-digit SIC) industry as the treatment firm and with similar firm size (+/−15%) as the treatment firm. We then repeat the standard and dynamic DID estimations, as well as the placebo DID estimation (using planned CEO retirements as the pseudo treatment events). The results are reported in Table A4 to A6 in the Appendix. Our findings remain qualitatively unchanged.

In summary, our empirical results from the DID analyses suggest that the substitution effect of OC on executive PPS is most likely causal. Firms strategically increase executive PPS when they experience a loss of OC due to health-related exogenous CEO departures.

6. Implication for shareholder wealth

Finally, we explore the implication of the OC-PPS substitution effect for shareholder wealth. The literature documents mixed evidence on the relation between executive incentive pay and firm performance. Mehran (1995) finds a positive relation between executive PPS and firm performance. Cooper et al. (2016) show that in the recent decade, excess CEO compensation is negatively related to future firm returns. However, both theories and empirical evidence suggest that OC has a positive impact on firm performance. Table A7 in the Appendix shows that both OC and executive PPS are significantly and positively associated with the contemporaneous buy-and-hold stock returns. More importantly, we find that the interaction between OC and executive PPS is negative, statistically significant at the 1% level.[18] These results suggest

duction in executive PPS). Hence, we conjecture that the result is likely due to weak managers who stay with the firm even without any promotion after a planned CEO retirement receiving reduced incentive pay. To investigate this conjecture, in untabulated analysis, we lift the restriction of unchanged executive ranks (i.e., we allow for promotion of non-CEO executives after a planned CEO retirement) and only require non-CEO executives to stay with the firm throughout the event window. Consistent with our expectation, we find that the coefficient on *TREATMENT*×*POST* is statistically insignificant in the models. We thank an anonymous referee for suggesting this explanation to us.

[18] We perform the same panel regression using CEO's PPS and the results remain qualitatively unchanged.

that while a firm may benefit from higher executive PPS, greater OC mitigates the necessity to raise PPS to motivate executives. Although these results are subjected to endogeneity concerns and should be interpreted with caution, they are consistent with the assumption that OC reduces the marginal effect of executive effort on firm outcomes.

7. Conclusion

A firm with efficient business processes and systems and well-trained employees (i.e., a firm with greater OC) may require lower levels of effort from its executives to maintain its productivity and competitive edge. Thus, its board may optimally offer lower levels of executive incentive compensation. In this paper, we confirm this economic intuition and find that a firm's OC is significantly and negatively associated with executive incentive compensation measured by PPS. The documented OC-PPS substitution effect is robust to alternative measures of OC, alternative measures of PPS, various model specifications and different subperiods. Using state-level unemployment insurance benefits to extract the exogenous component of OC, we find that the instrumented OC continues to be negatively and significantly associated with executive PPS measures. This result suggests a likely causal substitution effect of OC on executive PPS. We obtain further supporting evidence from a quasi-natural experiment based on exogenous health-related CEO turnover and a stacked difference-in-differences estimation approach. Our results imply that greater OC investments reduce the need to raise executive PPS to maintain productivity and increase shareholder wealth. The findings may be of interest to academics and practitioners.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jbankfin.2020.106017.

Appendix

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Table A1
Variable definitions.
All firm characteristics are measured as of the end of the fiscal year. All continuous variables are winsorized at the 1st and 99th percentiles by fiscal year.

Variable	Definition
Organization Capital	
OC	Organization capital scaled by total assets, constructed by capitalizing SG&A expenses using the perpetual inventory method following Eisfeldt and Papanikolaou (2013). For a firm in Compustat, starting from the first year with non-missing SG&A expenses, we recursively construct the stock of organization capital by cumulating the CPI-deflated value of SG&A expenses using a depreciation rate of 15%. The initial stock of organization capital is calculated with an industry- and decade-specific real growth rate of SG&A expenses following Li et al. (2018).
RANK_OC	The annual decile rank of a firm's organization capital based on the Compustat universe.
INDADJ_OC	The organization capital minus the (2-digit SIC) industry median OC, scaled by total assets.
RANK_INDADJ_OC	The annual decile rank variable of industry-adjusted OC.
Executive Performance Incentives	
DELTA_MGMT:	The average dollar change (in thousands) in all non-CEO executives' wealth for a 1% change in firm value.
DLETA_MGMT_ UNCHANGERANK	The average dollar change (in thousands) in wealth of the management team consisted of only those non-CEO executives whose pay rank (based on salary and bonus) remain unchanged throughout the event window for a 1% change in firm value.
WPS_MGMT	The average wealth-performance elasticity as in Edmans et al. (2009) of all non-CEO executives, measured by the dollar change (in thousands) in the executive's wealth for a 1% change in firm value, scaled by his or her total pay (in thousands).
WPS_MGMT_ UNCHANGERANK	The average wealth-performance elasticity of those non-CEO executives whose pay rank (based on salary and bonus) remain unchanged throughout the event window.
Control Variables	
SIZE	The natural logarithm of total assets.
MTB	Market value of equity divided by book value of equity.
LEVERAGE	Book value of debt divided by the sum of book value of debt and market value of equity.
IO_TOP5	The fraction of shares outstanding held by the five largest institutional investors.
ROA	Income before extraordinary items scaled by total assets (in percentage points).
TANGIBILITY	Net property, plant, and equipment scaled by total assets.
RET_FYEAR	The buy-and-hold stock returns in the fiscal year.
INVEST	Capital expenditure scaled by total assets.
UI_BENEFITS	The natural logarithm of the product of the maximum unemployment insurance benefit amount and the maximum duration allowed at the state level, following Hassler et al. (2005).
EXECUTIVE_RANK	The annual ranking of an executive officer within the firm by salary and bonus.

Table A2
Industry-Adjusted Measures of Organization Capital.
Table A2 presents the results of baseline regression as in Table 2 where organization capital is measured by industry-adjusted OC (*INDADJ_OC*) and the annual decile rank of industry-adjusted OC (*RANK_INDADJ_OC*). In all specifications, we include year fixed effects and either (2-digit SIC) industry fixed effects (columns (1) and (3)) or firm fixed effects (columns (2) and (4)), and lagged firm-specific control variables. Heteroskedasticity-robust standard errors are clustered at the firm level. Definitions of the variables are provided in the Appendix. Numbers in parentheses are two-tailed *t*-statistics. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	INDADJ_OC		*RANK_INDADJ_OC*	
	(1)	(2)	(3)	(4)
OC Measure	−0.111***	−0.167***	−0.038***	−0.049***
	(−6.116)	(−7.772)	(−6.139)	(−6.026)
SIZE	0.513***	0.244***	0.517***	0.269***
	(43.223)	(8.440)	(43.936)	(9.492)
MTB	0.086***	0.054***	0.085***	0.053***
	(18.272)	(14.196)	(18.133)	(13.970)
LEVERAGE	−1.160***	−0.714***	−1.161***	−0.716***
	(−15.135)	(−9.932)	(−15.089)	(−9.891)
IO_TOP5	−0.009***	−0.007***	−0.009***	−0.007***
	(−5.503)	(−5.432)	(−5.349)	(−5.327)
ROA	1.631***	0.943***	1.690***	1.014***
	(12.004)	(9.406)	(12.614)	(10.039)
TANGIBILITY	−0.690***	−0.398**	−0.696***	−0.435***
	(−6.018)	(−2.461)	(−6.064)	(−2.679)
RET_FYEAR	0.003***	0.002***	0.003***	0.002***
	(17.060)	(17.636)	(17.047)	(17.615)
INVEST	2.716***	1.419***	2.698***	1.461***
	(8.902)	(5.473)	(8.854)	(5.665)
Year Fixed Effect	Yes	Yes	Yes	Yes
Industry Fixed Effect	Yes		Yes	
Firm Fixed Effect		Yes		Yes
Observations	26,813	26,648	26,813	26,648
Adjusted R-squared	0.448	0.689	0.447	0.688

Table A3

Impact of Exogenous CEO Turnover on Organization Capital.

Table A3 presents the results of standard stacked difference-in-differences (DID) estimations examining the impact of exogenous CEO turnover on the firm's OC measures. *TREATMENT* is an indicator that equals 1 for treatment firms that experienced an exogenous CEO turnover due to well-specified health issues. Control firms in each cohort are those in the same (2-digit SIC) industry as the treatment firm but without any CEO turnover event during the three years before and three years after the event. In Panel A, we filter the control firms by a propensity score matching (PSM). Specifically, we use the data immediately before the treatment events to estimate the propensity score of each firm to experience the exogenous CEO turnover in the next year, conditional on the firm size and organization capital. For each treatment firm, we choose the 4 control firms in the cohort with the nearest propensity scores. We further use calliper matching and require the included control firms to have propensity scores within 0.01 from the treatment firm's score. Control firms are selected with replacement, i.e. the same firm can be in the control group of different treated firms. In Panel B, we use a broader sample of control firms where we only restrict to firms with similar sizes (+/−15%). Cohorts of treatment and control firms are then stacked together in the firm-year panel regression. *POST* is an indicator that equals 1 for the years after the event year and 0 for the years before the event. In both panels, column (1) uses OC as the dependent variable and column (2) uses RANK_OC as the dependent variable. In all specifications, we include firm-cohort and year-cohort fixed effects that can vary across cohorts and thus are more conservative than simple fixed effects. We deliberately do not control for time-varying accounting variables that are likely affected by the CEO turnover and hence confound the difference-in-differences estimates. Heteroskedasticity-robust standard errors are clustered at the firm level. Definitions of the variables are provided in the Appendix. Numbers in parentheses are two-tailed *t*-statistics. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: PSM Sample		
	OC	*RANK_OC*
	(1)	(2)
TREATMENT × *POST*	−0.064*	−0.151*
	(−1.735)	(−1.839)
Firm-Cohort Fixed Effect	Yes	Yes
Year-Cohort Fixed Effect	Yes	Yes
Observations	7001	7001
Adjusted R-squared	0.870	0.890
Panel B: Broader Sample		
	OC	*RANK_OC*
	(1)	(2)
TREATMENT × *POST*	−0.081***	−0.195***
	(−2.894)	(−3.119)
Firm-Cohort Fixed Effect	Yes	Yes
Year-Cohort Fixed Effect	Yes	Yes
Observations	46,208	46,208
Adjusted R-squared	0.899	0.911

Table A4

Standard Difference-in-Differences Estimation.

Table A4 presents the results of standard stacked difference-in-differences (DID) estimations examining the impact of exogenous CEO turnover due to well-specified health issues on executive PPS. *TREATMENT* is an indicator that equals 1 for treatment firms that experienced an exogenous CEO turnover due to well-specified health issues. Control firms in each cohort are those in the same (2-digit SIC) industry and are of similar size (+/−15%) to the treatment firm but without any CEO turnover during the three years before and three years after the event. Firms are not required to be in the sample for the entire event window. Cohorts of treatment and control firms are then stacked together in the firm-year panel regression. *POST* is an indicator that equals 1 for the years after the event year and 0 for the years before the event. Panel A presents the results where the executive PPS is measured by *delta* and Panel B presents the results using wealth-performance elasticity as in Edmans et al. (2009) as the alternative PPS measure. In both panels, column (1) uses the average PPS measures of the non-CEO executives who may join or leave the firm during the event window, where the dependent variable is the natural logarithm of *DELTA_MGMT* and *WPS_MGMT*, respectively. To alleviate the concern of changing composition of the management team and the promotion of managers over time, column (2) restricts to the non-CEO executives who stay inside the firm throughout the event window and whose ranking based on salary and bonus remain unchanged, where the dependent variable is the natural logarithm of *DELTA_MGMT_UNCHANGERANK* and *WPS_MGMT_UNCHANGERANK*, respectively. Column (3) further controls the contemporaneous average ranking of non-CEO executives (*EXECUTIVE_RANK*). In all specifications, we include firm-cohort and year-cohort fixed effects that can vary across cohorts and thus are more conservative than simple fixed effects. We deliberately do not control for time-varying accounting variables that are likely affected by the CEO turnover and hence confound the difference-in-differences estimates. Heteroskedasticity-robust standard errors are clustered at the firm level. Definitions of the variables are provided in the Appendix. Numbers in parentheses are two-tailed *t*-statistics. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: ln(DELTA_MGMT) and variants			
Dependent Variable =	ln(*DELTA_MGMT*)	ln(*DELTA_MGMT_UNCHANGRANK*)	
	(1)	(2)	(3)
TREATMENT × *POST*	0.126***	0.454***	0.452***
	(2.632)	(4.009)	(4.006)
EXECUTIVE_RANK			−0.055
			(−1.046)
Firm-Cohort Fixed Effect	Yes	Yes	Yes
Year-Cohort Fixed Effect	Yes	Yes	Yes
Observations	78,220	23,341	23,341
Adjusted R-squared	0.771	0.621	0.622
Panel B: WPS_MGMT and variants			
Dependent Variable =	*WPS_MGMT*	*WPS_MGMT_UNCHANGRANK*	
	(1)	(2)	(3)
TREATMENT × *POST*	0.036***	0.123***	0.123***
	(4.602)	(3.730)	(3.730)
EXECUTIVE_RANK			−0.010
			(−0.864)
Firm-Cohort Fixed Effect	Yes	Yes	Yes
Year-Cohort Fixed Effect	Yes	Yes	Yes
Observations	79,094	25,338	25,338
Adjusted R-squared	0.548	0.421	0.421

Table A5
Dynamic Difference-in-Differences Estimation.
Table A5 presents the results of dynamic difference-in-differences (DID) estimations, extending the DID model in Table A4, examining the impact of exogenous CEO turnover due to well-specified health issues on executive PPS, where the definitions of treatment and controls follow Table A4. To verify the parallel trend assumption and identifying the timing of the treatment effect, we replace the single *POST* indicator with event-year specific indicators d, where $\boldsymbol{d_j}$ is an indicator that equals 1 if a year is the j-th year after the exogenous CEO turnover. In all specifications, we include firm-cohort and year-cohort fixed effects that can vary across cohorts and thus are more conservative than simple fixed effects. We deliberately do not control for time-varying accounting variables that are likely affected by the CEO turnover and hence confound the difference-in-differences estimates. Heteroskedasticity-robust standard errors are clustered at the firm level. Definitions of the variables are provided in the Appendix. Numbers in parentheses are two-tailed t-statistics. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: ln(DELTA_MGMT) and variants			
Dependent Variable =	ln(*DELTA_MGMT*)	ln(*DELTA_MGMT_UNCHANGRANK*)	
	(1)	(2)	(3)
TREATMENT × d_{-2}	−0.002	−0.004	−0.000
	(−0.053)	(−0.032)	(−0.002)
TREATMENT × d_{-1}	−0.023	−0.186	−0.178
	(−0.440)	(−1.165)	(−1.123)
TREATMENT × d_0	0.057	0.206	0.217
	(1.065)	(1.407)	(1.486)
TREATMENT × d_1	0.272***	1.098***	1.105***
	(4.284)	(7.823)	(7.866)
TREATMENT × d_2	0.035	0.039	0.040
	(0.530)	(0.222)	(0.224)
TREATMENT × d_3	0.019	−0.116	−0.122
	(0.263)	(−0.808)	(−0.855)
EXECUTIVE_RANK			−0.070
			(−1.378)
Firm-Cohort Fixed Effect	Yes	Yes	Yes
Year-Cohort Fixed Effect	Yes	Yes	Yes
Observations	92,856	27,482	27,482
Adjusted R-squared	0.779	0.631	0.631
Panel B: WPS_MGMT and variants			
Dependent Variable =	*WPS_MGMT*	*WPS_MGMT_UNCHANGRANK*	
	(1)	(2)	(3)
TREATMENT × d_{-2}	0.002	−0.022	−0.021
	(0.243)	(−0.815)	(−0.798)
TREATMENT × d_{-1}	0.006	0.026	0.027
	(0.604)	(0.717)	(0.755)
TREATMENT × d_0	0.003	0.080*	0.082**
	(0.311)	(1.937)	(1.979)
TREATMENT × d_1	0.052***	0.205***	0.206***
	(4.124)	(5.324)	(5.355)
TREATMENT × d_2	0.035***	0.116**	0.116**
	(2.923)	(2.499)	(2.507)
TREATMENT × d_3	0.027**	0.038	0.037
	(2.097)	(1.032)	(1.019)
EXECUTIVE_RANK			−0.009
			(−0.796)
Firm-Cohort Fixed Effect	Yes	Yes	Yes
Year-Cohort Fixed Effect	Yes	Yes	Yes
Observations	93,856	29,822	29,822
Adjusted R-squared	0.561	0.417	0.417

Table A6
Difference-in-Differences Estimation using Planned CEO Retirements.
Table A6 presents the results of standard stacked difference-in-differences (DID) estimations examining the impact of exogenous CEO turnover due to planned retirements on executive PPS. *TREATMENT* is an indicator that equals 1 for treatment firms that experienced an exogenous CEO turnover due to planned retirements. Control firms in each cohort are those in the same (2-digit SIC) industry and are of similar size (+/−15%) to the treatment firm but without any CEO turnover during the three years before and three years after the event. Firms are not required to be in the sample for the entire event window. Cohorts of treatment and control firms are then stacked together in the firm-year panel regression. *POST* is an indicator that equals 1 for the years after the event year and 0 for the years before the event. Panel A presents the results where the executive PPS is measured by *delta* and Panel B presents the results using wealth-performance elasticity as in Edmans et al. (2009) as the alternative PPS measure. In both panels, column (1) uses the average PPS measures of the non-CEO executives who may join or leave the firm during the event window, where the dependent variable is the natural logarithm of *DELTA_MGMT* and *WPS_MGMT*, respectively. To alleviate the concern of changing composition of the management team and the promotion of managers over time, column (2) restricts to the non-CEO executives who stay inside the firm throughout the event window and whose ranking based on salary and bonus remain unchanged, where the dependent variable is the natural logarithm of *DELTA_MGMT_UNCHANGERANK* and *WPS_MGMT_UNCHANGERANK*, respectively. Column (3) further controls the contemporaneous average ranking of non-CEO executives (*EXECUTIVE_RANK*). In all specifications, we include firm-cohort and year-cohort fixed effects that can vary across cohorts and thus are more conservative than simple fixed effects. We deliberately do not control for time-varying accounting variables that are likely affected by the CEO turnover and hence confound the difference-in-differences estimates. Heteroskedasticity-robust standard errors are clustered at the firm level. Definitions of the variables are provided in the Appendix. Numbers in parentheses are two-tailed t-statistics. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: ln(DELTA_MGMT) and variants			
Dependent Variable =	ln(*DELTA_MGMT*)	ln(*DELTA_MGMT_UNCHANGRANK*)	
	(1)	(2)	(3)
TREATMENT × *POST*	−0.251**	−0.484	−0.504
	(−2.562)	(−1.049)	(−1.098)
EXECUTIVE_RANK			−0.040
			(−0.239)
Firm-Cohort Fixed Effect	Yes	Yes	Yes
Year-Cohort Fixed Effect	Yes	Yes	Yes
Observations	69,467	21,140	21,140
Adjusted R-squared	0.785	0.691	0.691
Panel B: WPS_MGMT and variants			
Dependent Variable =	*WPS_MGMT*	*WPS_MGMT_UNCHANGRANK*	
	(1)	(2)	(3)
TREATMENT × *POST*	−0.020*	−0.100	−0.101
	(−1.879)	(−1.244)	(−1.256)
EXECUTIVE_RANK			−0.004
			(−0.188)
Firm-Cohort Fixed Effect	Yes	Yes	Yes
Year-Cohort Fixed Effect	Yes	Yes	Yes
Observations	70,553	22,697	22,697
Adjusted R-squared	0.557	0.444	0.444

Table A7
Implication for Shareholder Wealth.
Table A7 presents the regression results of annual buy-and-hold returns on organization capital, executive performance incentives, and their interactions after controlling for firm characteristics including size, market-to-book ratio, leverage, ROA, investment, and lagged stock return. In all specifications, we include year fixed effects and either (2-digit SIC) industry fixed effects (columns (1) and (3)) or firm fixed effects (columns (2) and (4)). Panel A reports the results using the natural logarithm of DELTA_MGMT as the executive PPS measure. Panel B uses WPS_MGMT as the alternative PPS measure. Heteroskedasticity-robust standard errors are clustered at the firm level. Definitions of the variables are provided in the Appendix. Numbers in parentheses are two-tailed *t*-statistics. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: ln(DELTA_MGMT)				
	OC		*RANK_OC*	
	(1)	(2)	(3)	(4)
OC Measure	2.175***	4.544***	0.639*	2.176***
	(3.497)	(4.344)	(1.864)	(3.326)
ln(DELTA_MGMT)	9.367***	14.358***	9.806***	15.577***
	(21.898)	(20.747)	(18.941)	(18.378)
OC Measure X ln*(DELTA_MGMT)*	−1.126***	−0.924***	−0.492***	−0.621***
	(−5.574)	(−2.962)	(−5.338)	(−4.340)
SIZE	−6.032***	−14.111***	−6.104***	−15.182***
	(−19.083)	(−12.277)	(−19.368)	(−13.763)
MTB	2.999***	5.056***	3.058***	5.083***
	(15.100)	(18.425)	(15.434)	(18.564)
LEVERAGE	−6.704***	−26.199***	−7.486***	−26.157***
	(−3.669)	(−8.808)	(−4.091)	(−8.776)
IO_TOP5	0.026	0.045	0.019	0.043
	(0.675)	(0.878)	(0.507)	(0.835)
ROA	71.797***	98.262***	71.675***	97.546***
	(17.837)	(19.935)	(18.379)	(20.106)
TANGIBILITY	7.440***	−49.702***	6.741**	−47.955***
	(2.785)	(−7.409)	(2.517)	(−7.182)
RET_FYEAR	−0.142***	−0.243***	−0.144***	−0.244***
	(−20.790)	(−38.377)	(−21.155)	(−38.600)
INVEST	−85.500***	−109.301***	−86.311***	−111.634***
	(−9.687)	(−9.718)	(−9.764)	(−9.918)
Year Fixed Effect	Yes	Yes	Yes	Yes
Industry Fixed Effect	Yes		Yes	
Firm Fixed Effect		Yes		Yes
Observations	26,714	26,547	26,714	26,547
Adjusted R-squared	0.276	0.348	0.278	0.348

Panel B: WPS_MGMT				
	OC		*RANK_OC*	
	(1)	(2)	(3)	(4)
OC Measure	−1.671***	−0.152	−1.247***	−1.094**
	(−4.322)	(−0.167)	(−7.378)	(−2.526)
WPS_MGMT	23.571***	29.626***	25.261***	34.868***
	(5.550)	(5.413)	(5.206)	(5.783)
OC Measure X WPS_MGMT	−4.800*	−2.562	−2.055**	−2.243**
	(−1.780)	(−0.720)	(−2.189)	(−2.005)
Controls	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes
Industry Fixed Effect	Yes		Yes	
Firm Fixed Effect		Yes		Yes
Observations	26,552	26,384	26,552	26,384
Adjusted R-squared	0.253	0.314	0.255	0.315

CRediT authorship contribution statement

Mingze Gao: Conceptualization, Methodology, Data curation, Software, Writing - original draft. **Henry Leung:** Conceptualization, Writing - review & editing. **Buhui Qiu:** Conceptualization, Methodology, Data curation, Writing - review & editing.

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