CEO CONTRACT HORIZON AND INNOVATION

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Abstract

Innovating firms face a dilemma when setting contractual terms for management. Competing theories make opposing predictions on the relationship between contract-duration and innovation. Using novel data, we estimate that an additional year of CEO-contract-duration leads to 6.5% higher-quality innovation. We support a causal interpretation by exploiting exogenous variation spurred by CEO contract-limits regulation. Our evidence illustrates the process of changing innovation quality. Longer-contract-horizon CEOs allocate more resources to exploratory R&D and set longer term incentives for CROs. The evidence is consistent with the view that longer contracts facilitate long-term investment and greater risk-taking by mitigating managerial myopia and career concerns.

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Most innovation decisions today are delegated: from government to research institutes, from granting bodies to researchers, or from owners to managers. To mitigate possible conflicts of interest, principals offer incentive contracts to agents; many of these contracts are, in practice, valid only for a specified time horizon. The optimal contract duration is subject to debate. Some practitioners and researchers believe that too short a contract can hinder research and economic growth[1] because such a contract sets shorter decision horizons (Narayanan, 1985; Stein 1988 and 1999; Edmans et al., 2012) and may not provide the "tolerance for failure" that is necessary for truly innovative research (Holmstrom, 1989; Manso, 2011; Powell, 2015).[2] Others argue that the flexibility to remove bad performers promptly is essential for disciplining agents (Weisbach, 1988; Morck et al., 1989; Denis et al., 1997; Mikkelson and Partch, 1997; Hartzell, 2001) and for reacting to market developments (Abraham and Houseman, 1993, 1994; Burgess et al., 2000; Autor et al., 2007). In line with this argument, regulators have discussed and implemented limits to the length of executive contracts (UK: House of Commons, 2002; Switzerland: Wagner and Wenk, 2015). Yet the potential associated innovation effects from these policies—and, more generally, the effect of contract length on innovation—remain understudied, mainly because contract data is seldom publicly disclosed. Using a novel sample of Chief Executive Officer (CEO) employment contracts, we estimate a robust positive correlation between contract duration and the quality of innovation (as measured by patent citations).

[1] Larry Fink, the founder of BlackRock, in an interview with Adi Ignatius (2015); see also National Science Board (2012), and Porter (1992).

[2] Consistent with this argument, the compensation literature shows that long-term *compensation* features are indeed associated with more innovation output (Yanadori and Marler, 2006; Lerner and Wulf, 2007; Baranchuk et al., 2014; Chang et al., 2015).

To document this evidence, we combine information on contract terms for CEOs of public firms in the United States—which are required to disclose their managers' contracts—and on innovation data from patent records (Kogan et al., 2015). We exploit cross-sectional, within-firm, and within–CEO–firm-pair variation to show that one additional year remaining on the CEO's contract is associated with 6.5% more annual citations per patent (adjusted by technology class and application year) in excess of the firm's mean and relative to all other CEOs in the same industry, of the same age, with the same overall tenure in the firm, and the same contract-age (i.e., the same contract start year). At contract renewal, when the contract horizon is reset, citations increase by 0.208. These effects are economically important: innovation quality increase by 0.14 standard deviations with a one–standard deviation increase in contract horizon, and by 17% relative to the mean upon contract renewal.

Through a battery of tests, we show that results are robust to several controls for time-varying heterogeneity in innovation opportunities and the compensation structure. Our conclusions hold also using a measure of innovation importance based on market value (the market reaction to the announcement of a patent grant; cf. Kogan et al., 2015).

Further, we present evidence illustrating the process by which longer-contract-horizon CEOs affect innovative processes within the firm. Longer-horizon CEOs allocate more financial and human resources to research and development than CEO's with short horizons. Under longer-contract-horizon CEOs, Chief Research Officers also receive longer contracts. Additionally, firms with longer horizon CEOs pursue more exploratory innovation strategies: they invest into newer and more varied technologies when they have more time remaining under their contract.

The evidence is consistent with the view that increased contract-duration facilitates long-term investment and greater risk-taking by mitigating managerial myopia and career concerns. The theoretical literature on managerial myopia argues that managers with a short horizon may fail to invest in valuable long-term investment opportunities (Narayanan, 1985; Stein, 1988; 1989; Edmans et al. 2012). The theoretical literature on CEO career-concerns argues that career-concerned managers take less risk than desired by a diversified shareholder (Jensen and Meckling, 1976; Holmstrom, 1999; Gormley and Matsa, 2016). Longer contracts can align managers by setting longer performance horizons and decreasing the sensitivity of compensation to performance. Indeed, Cziraki and Groen-Xu (2016) show that longer contracts decrease turnover-performance sensitivity.

Interpretation of results under the light of these theories is however not straightforward in three further respects: causality, problems with accurately measuring and timing innovation output, and the efficiency of long-term contracts.

First, a correlation between innovation output and contract horizon need not reflect a causal relationship. For example, if firms set longer contracts in anticipation of future investment opportunities, then reducing contract duration may have no effect on innovation. We provide evidence suggestive of a causal decrease in innovation quality following the introduction of UK regulations that exogenously shortened CEO employment contracts in 2002 (House of Commons, 2002). We show that, for firms listed in both the United States and the United Kingdom, and relative to other US firms (in the same industry and with CEOs of the same age and tenure), enactment of this UK provision was followed by reduced average contract duration and also by a decline in the quality of innovation. Affected firms saw contract duration decrease by 2 years and citations decline by 15% in relation to their mean, and relative to all other firms in the same industry, and with CEOs of the same age and tenure.

Second, the veracity of our preferred interpretation depends on whether we have correctly characterized the timestamp of the innovation process. For example, we match contract horizon to concurrent patent applications, yet there could be lags between innovation decisions and patents. If such lags are systematic, then different interpretations might explain the results. For example, contract deadlines can discipline CEOs (as other forms of threats of dismissal do, see Hart, 1983; Bertrand and Mullainathan, 2003); this dynamic, when observed with a lag, results in better patents during the *next* contract only—leading us to attribute, erroneously, the innovation increase to the subsequent CEO's longer contract horizon. To address this concern, we explore whether and when there are lags between innovation decisions and output. We use the methodology of Hall et al. (1986) and find that lags do occur between research and development (R&D) expenditures and the number of patents produced, though only for a small number of industries. The longest lags are prevalent in industries where it is arguably more difficult to file intermediary patents (e.g., agriculture and food products). We then show that our results are strongest for the vast majority of industries that do not exhibit such a lag.

Third, the positive correlation we find between contract horizon and innovation quality does not imply that a long-term contract is necessarily efficient. However, our results do imply that there is at least one reason—innovation quality—why short-term contracts can be detrimental for the principals. Countries seeking to reform managerial contract terms should weigh the potential benefits of such reform against the potential innovation costs that we document here.

Our paper contributes to the literature that explores incentive conflicts when innovation is delegated. In the context of early-stage cancer trials, Budish et al. (2015) show that long commercialization lags may lead to underinvestment in innovation. In the context of academia, Azoulay et al. (2011) show that holders of long-term grants produce more important research because they have greater freedom to experiment. We focus on corporations for several reasons. First, they are key to innovation (some 45% of US patent production can be attributed to US firms[3]); second, in corporations the incentive conflict is exacerbated because firms seek to maximize shareholder value and therefore not always pursue innovation-related goals; third, stock prices and quarterly earnings announcements provide investors with short-term performance measures that may further increase short-term pressure. Our findings complement the extant research because grants are not the only delegation contracts: indeed, scientists are typically compensated also via fixed-term employment agreements.

We contribute to the literatures exploring the relation between corporate innovation and CEO myopia and CEO career concerns. Most empirical work on CEO myopia explores how firms can provide longer-term incentives to CEOs through the compensation structure (Yanadori and Marler, 2006; Lerner and Wulf, 2007; Baranchuk et al., 2014; Chang et al., 2015, Edmans et al., 2016). Instead, we focus on how the length of employment contracts sets longer-term incentives and performance horizons for CEOs. Similarly, most empirical work on CEO career concerns explores how the firms' ownership structure (e.g., institutional investors) or legal setting (e.g., employment laws) can increase corporate innovation by decreasing sensitivity of pay or turnover to performance (Lerner et al., 2011; Aghion et al., 2013; Acharya et al. 2014; Tian and Wang, 2014; Asker et al., 2015; Bebchuk et al., 2015; Barrot 2016). Instead, we focus on how explicit commitments by boards to be tolerant to failure, in the form long-term contract provision, affect innovation choices. Since employment agreements in governmental or educational institutions seldom rely on complex compensation schemes voted by block-holder owners, our novel focuses are more likely to be relevant for noncorporate innovation contexts. Moreover, our paper is the first to make explicit estimates of the economic effects of legislated reductions in CEO contract duration

[3] http://www.uspto.gov/web/offices/ac/ido/oeip/taf/topo_14.htm#PartA1_1b

(House of Commons, 2002). This research can serve to inform the current regulatory debate about the consequences of such legislation for the real economy.

This paper contributes as well to the literature on how managers affect corporate behavior and performance (see e.g. Bertrand and Schoar, 2003; Chemmanur et al., 2015; Custodio et al., 2015). Our work complements findings by Acemoglu et al. (2014), Yim (2013), Bereskin and Hsu (2013), and Pan et al. (2014) that link investment decisions to CEO age and tenure. In addition, Antia et al. (2010) show that more (realized) time to turnover is associated with better performance and Dechow and Sloan (2991) that firms invest less into R&D in the last years of a CEO's tenure. In contrast to those papers, we document a robust correlation between innovation quality and contract horizon that *cannot* be explained by tenure or CEO-, firm-, or contract-age effects. We also shed light on the "black box" of corporate innovation by documenting some of the mechanisms via which CEOs can affect innovation.

The rest of this paper proceeds as follows. Section 1 presents the data sources, describes the construction of our data set, and defines the main variables of interest. Section 2 presents our methodology and results. In Section 3, we describe managerial practices associated with longer contract horizons. After interpreting our results in Section 4, we conclude in Section 5.

1. DATA

1.1. Contract data

We use novel data on fixed-term employment agreements between CEOs and US public firms. Here we give a brief description of the data; for a detailed account of the sample construction, see Cziraki and Groen-Xu (2016), where the data were introduced.

The disclosure of employment agreements between firms and CEOs is mandatory (Regulation S-K, Item 402) in the United States. Approximately half of public firms sign explicit fixed-term contracts with their managers (Gillan et al., 2009). Absent a fixed-term contract, CEOs are employed at-will; in that case, either the manager has no written agreement with the firm or the agreement covers only some aspects of the employment relation (e.g., control, nondisclosure, noncompete clauses, nonsolicitation agreements) yet has no explicit termination date. For a more detailed discussion on employee protection provided by contracts see also Chen et al. (2015).

For each fixed-term contract, information on its effective date (start date) and its duration is directly available from SEC filing exhibits. Cziraki and Groen-Xu (2016) collect these data and complement them with details on renewals from proxy statements and actual separation dates from Boardex and Execucomp.

For this paper, the sample includes all firm-years in which the contract between a firm and its CEO is valid—that is, until it expires naturally (73% of all contracts) or is terminated prior to expiration (e.g., the CEO leaves because the firm is acquired by or merges with another). Because contracts typically change after acquisitions, we do not follow target firms afterwards; thus we abstract from possible externally acquired innovation and patent reassignments. We also exclude information on co-CEOs.

1.2. Innovation data

We use patent filings to characterize innovation output. We retrieve the data from Kogan et al. (2015), who match public firms' patent records from the US Patent and Trademark Office with those firms' returns as reported by the Center for Research in Security Prices and then estimate the adjusted stock market reaction to patent grants.

Patent-based innovation proxies have two advantages over accounting metrics: timing accuracy and measurement quality. The prevailing legal incentives to file patents immediately after incremental steps in the development process—and so to claim priority (Branstetter, 2006)—usually render patent data less prone than accounting data to reflect "window dressing", timing inaccuracies, or strategic behavior by CEOs. Thus both patent citations and the stock market reaction to patent grants are reliable proxies for the quality and value of innovation *output* (Jaffe and Trajtenberg, 2002; Lerner and Seru, 2014; Kogan et al., 2015); in contrast, R&D expenditures measure the amount of innovation *input* (which we also study, retrieved from Compustat).[4] In addition, patents identify their inventor and are assigned to a technology class. We obtain this information from the Harvard Business School patent data (Lai et al., 2011).

Following standard practice in the literature, we time patents based on their application year (because it better reflects, than does the grant year, the time of the invention) and characterize them using the count of citations (excluding self-citations) made within three years of the grant date, which we call "*Raw cites*" (cf. Hall et al., 2001; Lerner et al., 2011). The lag between the application for and the award of a patent has the effect of censoring patent citation data. That is, patents granted later have less time to be cited than do patents granted earlier; this effect is well documented in the literature (e.g., Lerner and Seru, 2014). To remove that potential bias, we follow Hall et al. (2001) and scale citations based on the average number of forward citations to patents filed in the same technology class and application year. Such scaling offsets the censoring bias but may introduce trends to our dependent variable, as we shall discuss in more detail. In other specifications, year fixed effects—which we allow to vary by industry—also absorb such potential censoring issues (see Section 2). For reading convenience, hereafter we refer to scaled citations as *Cites* unless stated otherwise ("*Raw cites*").

[4] Citations are important in patent filings because they delimit the property rights of inventions and delineate the scope of granted claims. Hence patents with more forward citations are typically interpreted as having more quality or as being relatively more influential (Hall et al., 2001; Lerner et al., 2011).

1.3. Sample composition

The final sample contains information on 1,344 fixed-term employment contracts between 970 CEOs and 914 firms that were granted at least one US patent during the period 1993–2008. Table 1 describes the composition of this sample.

[[INSERT Table 1 about Here]]

Panel A of the table reveals a decreasing trend in average contract length throughout the sample period: whereas the average length for new contracts entered into during 1993 was 4.00 years, that average fell to 2.67 years for contracts entered into during 2008. This decline parallels growing public pressure to increase the accountability and decrease the job security (and entrenchment) of CEOs, and it is consistent with the significant decline in CEO tenure over the past few decades (Kaplan and Minton, 2012; Peters and Wagner, 2013). Patent filings exhibit a U-shaped pattern over the sample period, which reflects the well-documented truncation bias in patent data: patents applied for in the panel's later years may yet be granted (Hall et al., 2001).

Panel B of Table 1 reports the number of new and renewal contracts by their length. Most new contracts are less than six years long, with a mode of three years (167 contracts) followed by two-year (119) and one-year contracts (83). The distribution is similar for renewal contracts.[5]

Panels C and D in the table display the sample firms' distribution across, respectively, the Top 5 US states and the Top 5 (Fama–French) industries relative to all firms in Compustat. As in the universe of Compustat firms, the majority of firms in our sample are headquartered in California (followed by Massachusetts, New York, and Texas). The sample

[5] Of the contracts that are longer than nine years, 12 are explicitly linked to the executive's retirement age. In total, 28 of our sample's contracts are explicitly linked to age. As Jenter and Lewellen (2015) document, such linkage tends to occur at the age of 65 (23 contracts) or thereabouts.

is concentrated in the electronics and pharmaceutical industries, reflecting industry-specific needs to protect intellectual property.

Panel E of Table 1 presents summary statistics for the main variables used in our empirical analysis. Average cites (resp. raw cites) to patents in our sample are 1.16 (resp. 5.24). The average firm has assets of $3.46 billion (US), spends 10% of its assets annually on R&D, and files 33.63 patents per year. The mean CEO is 53.79 years old, has tenure of 4.24 years, owns 15.4% of the company's stock, and receives an annual awarded (realized) compensation of $6.29 million ($5.54 million), called TDC1 (TDC2). The average *contract horizon*—that is, the number of years remaining on the contract before expiration—is 2.32 years.

1.4. Assembling the database

The observations in our main data set are patents. From each employment contract, we retrieve two main variables: the effective (start) year and the planned duration. We then use these variables to construct our main explanatory variable, *Horizon*: the number of years remaining on the CEO's contract. We structure the data in "horizon event time". To illustrate the sample structure, Figure 1 introduces excerpts from the employment contract between Advanced Micro Devices Inc. (AMD) and its new CEO Hector Ruiz. In this contract, the effective year is 2002 and the planned duration is five years: "This Agreement shall be effective upon the signing by both parties …, and shall expire five (5) years after the Commencement Date." Figure 2 shows part of our data for AMD and Ruiz: *Horizon* = 5 for Patent #6445174 (filed during 2002) whereas *Horizon* = 3 for Patent #7183152 (filed during 2004).

[[INSERT Figure 1 about Here]]

[[INSERT Figure 2 about Here]]

Our main dependent variable is *Cites*. Other than *Horizon*, there are two main explanatory variables: *Start year of contract*, and *Tenure* (i.e., number of years that the incumbent CEO has headed the firm). The *Start year of contract* variable is constant across all patents filed under the same contract, whereas *Tenure* is increasing within a contract. In the case of Ruiz, for example, *Tenure* = 0 for patents filed in 2002 but *Tenure* = 4 for those filed in 2006.

1.5. Basic patterns of innovation and contract horizon

We show basic patterns involving innovation and contract horizon in Table 2. Here, for a first univariate comparison we classify patents according to CEO horizon above and below the median of three years—long (> 3 years) or short (< 3 years). Long–contract horizon CEOs are associated with significantly more influential innovations: on average, 6.53 (resp. 4.39) raw citations are made to patents produced under a long (resp. short) contract horizon.

[[INSERT Table 2 about Here]]

These univariate results could be driven by heterogeneity in innovation opportunities or in the economic and institutional conditions faced by firms setting long- versus short-horizon employment contracts. To control for such conditions, we match the firms in our sample to firms with *no* fixed-term CEO contracts using a nearest-neighbor matching procedure of propensity scores (see Rosenbaum and Rubin, 1983; Hirano et al. 2003 and Roberts and Whited, 2012), as explained in Appendix A. We then repeat our comparison across contract horizons but adjust our measure of raw citations by subtracting the corresponding benchmark (i.e., average raw citations of matched firms' innovation). The results continue to show a positive correlation between contract horizon and *relative* innovation quality. More specifically, patents filed when CEOs have long contract horizons are cited 1.86 more times than those filed under short-horizon CEOs (relative to their noncontracted peers).

Since firms with and without fixed-term contracts differ also in other respects (Gillan et al., 2009), propensity score matching might not adequately control for dissimilarities in the innovation opportunities encountered by short– versus long–contract horizon firms—a common concern in research that relies on synthetic control groups for heterogeneous subjects of study (see Furman and Stern, 2011). An advantage of our setting is that there is variation in the start year of contracts across firms, which allows us to study the correlation between contract horizon and innovation for only those companies with fixed-term contracts. So in our main analysis, we explore the relation between contract terms and innovation using within-firm variation in the terms and timing of contract setting only for firms with fixed-term CEO contracts. As we explain in what follows, this approach has its own set of challenges. Of these, perhaps the most vexing is to disentangle innovation trends from possible contract horizon effects.

2. METHODOLOGY AND RESULTS

In this section we describe the methodology used and present our main results. The structure of our contract data allows for two main sets of statistical exercises, both using *within-firm* variation. First, we explore variation in average innovation quality as horizon decreases over the course of a contract between the firm and its CEO. Second, we explore changes in innovation quality around the time of (ex ante scheduled) contract renewal.

2.1. Exploiting within-firm variation in the contract horizon

We compare citations in the same firm when the CEO is contracted under a long versus short horizon while controlling for other potential drivers of the innovation choices made by CEOs. For this purpose, we estimate the following type of regression:

$$y_p = \alpha + Horizon_p + \varphi \boldsymbol{x}_p^{Ci} + \eta_p^i + \gamma_p^\tau + \delta_p^{Ci\tau} + \varepsilon_p; \tag{1}$$

where p indexes patents, i indexes firms, C indexes CEOs, and τ indexes the contract start year. The variable $Horizon_p$ corresponds to the number of years remaining on the CEO's employment contract when patent p is filed, and the vector of additional controls, $\boldsymbol{x}_p^{Ci}$, varies across specifications as we test the robustness of our results to a broad set of control variables. Standard errors are clustered at the firm level to adjust for heteroscedasticity and for within-company correlation over time.

In some specifications, we replace the continuous variable $Horizon_p$ with a set of "contract horizon" indicator variables, D_p^h, that equal to 1 when patent p is filed with h years remaining on the CEO's employment contract (and set to 0 otherwise). To avoid multicollinearity, we exclude the indicator variable for contracts with less than a year before expiration (D_p^0).

As is the case with all "time-to-event"-type data, the main challenge is distinguishing the potential effects of contract horizon from aggregate and firm-level innovation trends. We control for several sources of such trends and other sources of bias parametrically. We include contract start-year fixed effects, γ_p^τ—interacted with industry indicators in some specifications—that absorb aggregate trends (e.g., decreasing trend in contract length) and potential "*contract-age*" effects (e.g., CEOs in the first year of a contract may be more active than in the subsequent years). We include tenure-fixed effects, $\delta_p^{Ci\tau}$—and CEO-age fixed effects in some specifications—to control for potential CEO life-cycle–driven trends in innovation (e.g., younger and less experienced CEOs may innovate more; see Bereksin and Hsu, 2013; Yim, 2013; Acemoglu et al., 2014; Pan et al., 2014). Finally, we include firm fixed effects, η_p^i, that control for time-invariant heterogeneity across firms (e.g., the companies with the best innovation opportunities may be able to attract the best CEOs and so may offer them longer contracts). In addition, through the scaling procedure, we absorb any remaining aggregate innovation trends in innovation quality proxy.

To avoid collinearity among the many fixed effects, we follow Berndt and Griliches (1993) and omit a minimal subset of dummy variables to achieve a maximal parameterization that can still be identified (see also the discussion in Hall et al., 2007). Alternatively, we control for tenure and age effects using high-order polynomials of those variables (Furman and Stern, 2011).

The coefficients of interest in equation (1) are the β_h, which in the baseline specifications correspond to the average (scaled) *cites* filed by CEOs with *h* years remaining in their contracts, *relative* to CEOs with *equal* contract-age and tenure *but* with only zero years remaining in their employment agreements. The variation that accounts for our estimates of β_h is the firm's CEO's reduced contract horizon over time, relative to other CEOs with the same age, firm tenure and contract age.

2.2. Results

Table 3 reports ordinary least-squares (OLS) estimates of different specifications of equation (1). The covariates used in each specification are indicated in the table's lower portion.

[[INSERT Table 3 about Here]]

Column 1 presents estimates for β_h in equation (1) while including firm, tenure, and contract start-year fixed effects. The coefficients reveal that cites are increasing in CEO contract horizon. The coefficient for the five-year–horizon dummy is 0.435; thus, for a given firm, patents filed when its CEO has five more years before expiration receive 0.435 more cites than do those filed when less than one year remains. This difference is relative to patents filed by all other CEOs who started their contract in the same year and have the same overall tenure. Hence the correlation measures the relation between innovation quality and horizon that cannot be explained by "contract-age" (e.g., shareholders prefer to start major strategy changes in the beginning of a contract) or tenure effects (e.g., shareholders prefer to start major strategy changes with a new CEO).

Reported in the table's bottom rows are the differences between coefficients of longer contract horizons and β_1; the positive and sizable differences imply that the increasing pattern observed between citations and contract duration is significant. Estimated as a slope instead of using horizon dummies, the pattern remains positive and is equally significant (column 2). One extra year in the contract horizon of a company's CEO is associated with patents that receive 0.065 more cites within three years of being granted (relative to other CEOs with the same contract-age and same overall tenure). This effect is economically important: an increase of one standard deviation in contract horizon increases relative cites by 0.14 standard deviations.[6]

Figure 3 displays the increasing pattern of innovation quality in contract horizon while controlling for start-year, firm, and CEO tenure fixed effects. In the graph, Table 3's column 1 coefficients are plotted (solid line) together with their 95% confidence intervals (dashed lines). Longer horizons are associated with more influential innovation, and this association is not explained by differences in contract-age or CEO tenure. We are nevertheless interested in potential tenure effects on innovation given recent evidence on such effects in tangible investments (see Pan et al., 2014). In Appendix B we show that, consistently with the findings of Pan et al. (2014), CEOs in the first two years of their job are associated with less influential patents.

[[INSERT Figure 3 about Here]]

The estimates in columns 3-6 of Table 3 show that the positive correlation between quality of innovation and contract horizon is robust to the inclusion of start year cross industry fixed effects (column 3), to CEO age and firm age fixed effects (column 4), and to replacing fixed effects for tenure, CEO age, and firm age with respective high-order polynomials (column 5). In column (6) we show that results continue to hold when we

[6] According to Table 1, the standard deviations of contract horizon and scaled citations are 2.00 and 2.22, respectively; thus 0.14 = 0.065 × 2.22.

substitute firm fixed effects with CEO fixed effects and then estimate the effect of contract horizon based only on the subset of CEOs with multiple contracts in the sample period. Finally, column 7 shows that our findings are robust to restricting the sample to contract *renewals*, which indicates that we are not capturing a "new CEO" effect.

2.3. Robustness

Here we show that significant correlation between contract horizon and innovation quality is robust to (i) controlling for compensation measures known to be correlated with citations, (ii) using an alternative approach to control for aggregate trends, (iii) using a market-based measure of innovation importance, (iv) various sample splits, and (v) controlling explicitly for demand- and supply-driven cycles that may coincide with the contract horizon.

Compensation features can be set to coincide with the contract horizon and so might explain the positive correlation between contract horizon and citations. Against this claim, in column 1 of Table 4, Panel A we show our results continue to hold when we control for the potential effects of equity-based CEO compensation. We consider the following equity-based CEO compensation features shown by prior work to affect innovation (or investment): the fraction of shares owned by the executive (Lerner and Wulf, 2007), the fraction of incentive pay compared to total compensation, the sensitivity of compensation to stock price increases of unvested equity (Baranchuk et al., 2014; calculated following Core and Guay, 2002), the value of vesting stocks and options (Edmans et al., 2016), the duration of remaining vesting periods (Gopalan et al., 2014), the maximum years to vesting of all restricted grants held by the CEO (Baranchuk et al., 2014), and clawback provisions (Babenko et al., 2013). We control for the executive's level of compensation (log of TDC1) to capture size effects. Information on the vesting-related compensation measures is available only for a subset of firms (9.1% of the sample). Our regression code each missing vesting-related compensation feature to zero and including a dummy variable indicating the firms for which such replacement coding was made to avoid selection bias.

[[INSERT Table 4 about Here]]

Further, in Figure 4 we show these compensation measures actually do not correlate with contract horizon. We regress them against horizon indicators, controlling for firm, start-year, and tenure fixed effects, and find no significant relationship between compensation and horizon. Intuitively, the difference in contract and "compensation horizon" arises because the former is set ex ante and proceeds to shorten over time. Alterations to it are not easily implemented: unplanned contract terminations, and especially unplanned renewals occur in less than, respectively, 27% and 1% of all cases. In contrast, firms often actively replenish the compensation horizon such that it remains stable over time (Carter and Lynch, 2001).

Next we address the possibility that our procedure for scaling citations induces an additional trend that the γ_p^τ may not fully capture. For example, if average scaled citations increased over time then they would, albeit mechanically, decline over a contract's lifespan because the denominator in our scaled citations calculation would then be exhibiting a positive trend. The figures reported in column 2 of Table 4, Panel A show that the effect of contract horizon is robust to using raw cites as the dependent variable while including year fixed effects in the estimation. Column 3 shows that results continue to hold even after we exclude observations after calendar year 2005 to address possible censoring of citation data.

In column 4 of Panel A we use an alternative dependent variable: the adjusted stock market reaction to the announcement of patent grants (see Kogan et al., 2015). This variable measures the monetary value of innovation to the firm itself. A particular advantage of this variable is its relative imperviousness to aggregate innovation trends. We find that CEOs employed under longer-horizon contracts are associated with patents of greater value to shareholders.

Results in column 5 and 6 of Panel A show that our results are not driven by short (<=3 years) or only long (>3 years) contracts. They also continue to hold when we exclude firms with headquarters in California (column 7).

Contract horizons may be set to concur with other, predictable cycles that affect innovation (Anderson et al., 2013). While the contract start-year fixed effects control for aggregate cycles, further controls for *firm-specific* cycles may be desirable. Columns 1-4 of Table 4, Panel B show that results continue to hold when we include controls for predictable firm-level innovation cycles. We proxy such firm-level cycles as the time elapsed between peaks in the historical growth of capital expenditures and other innovation demand- and supply-related variables (peaks correspond to years in which a variable rises more than 5% annually before subsequently shrinking). For every calendar year we then calculate the remaining time under these predicted cycles and include it in the estimation. The cycles so constructed average three years in length, which is remarkably close to our sample's average CEO contract length. The effect of contract horizon remains significant after these cycles are included in the estimation. The downside of this approach is its restriction of the sample to firms with cycles, which leaves us with 22,318 observations when using capital expenditure cycles (the least restrictive cycle generator).

Further, columns 5-6 in Table 4, Panel B show that the results also continue to hold after we include controls for more specific industry- and macro-level sources of innovation cycles. We consider *supply*-side innovation determinants such as: the (logarithm of the) number of PhD graduates in any given year, a proxy for the availability of researchers; and the total or industry-specific (e.g., agricultural engineers for agricultural firms) number of graduates; and *finance*-side innovation determinants: the (logged) number of within-industry finance transactions—including mergers, acquisitions, and equity offerings (Schlingemann et al., 2002)—to measure the availability of equity capital; and the real interest rate to measure the availability of debt capital.

In Appendix C we demonstrate further the low probability that unobserved mean-reverting trends explain our results. We simulate mean-reverting trends and contract tenures and show that the former almost never result into a pattern comparable to the one we observe.

2.4. Differences in innovation quality around contract renewals

Around the time of a scheduled renewal, contract horizon increases sharply. If our results are indeed driven by contract horizon, then such a sharp increase should result in an equally dramatic improvement in innovation outcomes. In this section we test that prediction using variation in the timing of contract renewals across CEO–firm pairs in an "multiple event time study analysis" framework.

This framework is similar to a traditional event-study analysis but with the difference that each observation can simultaneously relate to several renewals of the same CEO or same CEO–firm pair. For illustration, take a CEO with a first renewal in 2000 and a second renewal in 2002. For this CEO, year 2001 was the first year after the first renewal (*t+1*), and at the same time, the year before the next renewal (*t-1*). We follow convention (for multiple event-time–study analyses) by assuming that the effects are the same for each of the renewal events (i.e., the first, second, and subsequent ones), and estimate the following regression:[7]

$$y_p = \alpha + \sum_j \beta_j Event_{i,t+j} + \varphi \boldsymbol{x}_p^{Ci} + \eta_p^{Ci} + \gamma_p^{\tau} + \varepsilon_p \,, \tag{2}$$

where $Event_{it+j}$ are indicator variables of the years before and after contract renewals, η_p^{Ci} are CEO–firm pair fixed effects, and all other variables remain as before.

[7] For more details on this procedure, see Sandler and Sandler (2014). The authors use Monte Carlo simulations to show that other ways of dealing with multiple events—for instance, ignoring subsequent events or duplicating observations so there will be at least one observation per discrete event time—can create trends in the outcome variable before and after an event and thus lead to biased estimates.

The series of coefficients β_j are the main estimates of interest. For $j > 0$, the term β_j is an estimate of the change in (scaled) citations j years *after* the contract renewal—for firms that renewed their CEO contract j periods ago—relative to all other firms whose CEOs did not have a contract renewal j periods ago (but did so before and may or may not do so afterward). The estimates β_j for $j < 0$ allow us to determine whether there are any trends in innovation in the years *before* renewal. The coefficients are normalized relative to the year before contract renewal, which is excluded in the regression. As in Section 2.1, standard errors are clustered at the firm level to adjust for heteroscedasticity and within-company correlation over time. As before, the regression includes three types of fixed effects—CEO–firm pair, contract-start-year and tenure—that account for many common sources of bias. The variation accounting for our estimates of β_h in equation (2) stems either from within-CEO changes or from changes within CEO–firm pairs.

[[INSERT Table 5 about Here]]

Table 5 reports the β_j coefficients from estimating equation (2) for the period spanning 7 years before the contract renewal date to 16 years after that date. We report values for the window consisting of 3 years before and 3 years after contract renewal (since the average contract duration is 3 years). There is evidence of decreasing innovation immediately prior to contract renewal: β_{-2}, relative to the year before renewal, is negative and statistically significant in some specifications. At the actual time of renewal, an abrupt shift in the mean is evident.[8] In this table, column 1 (resp., column 2) gives the results when we control for firm fixed effects (resp., for CEO–firm pair fixed effects). On average, from $j = -1$ to $j = 0$ we see that cites increase by 0.190 when controlling for firm fixed effects and by 0.208 when controlling for CEO–firm fixed effects. According to Table 1, the mean of cites is 1.16; hence

[8] Section 4.2 shows average lags between R&D expenditure and patent outcomes: consistent with Hall et al. (1986) we find that in most industries no such lag exists. This suggests that a within-year reaction of patent outcomes to incentives is indeed plausible.

the estimated increase reported in column 2 of Table 5 corresponds to a 17% rise over the sample mean. Results are similar for the subsample of CEOs and CEO–firm pairs that experience at least one renewal during the sample period (unreported). This dramatic increase in innovation quality around renewal confirms that the correlation between contract horizon and citations is not likely to be driven by tenure effects or by underlying trends in innovation.

3. MANAGEMENT OF INNOVATION

CEOs and boards of public firms are hierarchically distant from the inventors who actually implement innovation (Lerner and Wulf, 2007; Chang et al., 2015). Thus the question arises: how can CEO incentives be related to innovation outcomes at all? Before turning to the interpretation of our results (including causality considerations) in Section 4, here we discuss different ways in which CEOs are involved with the innovation process in firms.

CEOs often argue that they influence innovation via changes in corporate strategy: control and distribution of resources, assignment of personnel to the resulting projects, and dissection of the project pipeline. For example, in a recent survey on the role of management on innovation to 600 global business executives, respondents explained: “[We hold] quarterly meetings to see how the businesses are performing versus their innovation targets, and there are meetings every six months to dissect the innovation pipeline… in the long-term view, we try to replace people who don’t take part in our permanent innovation process and tell people to look for another company to work for” (Barsh et al., 2008).

[[INSERT Table 6 about Here]]

Tables 6 and 7 provide evidence consistent with these arguments. Column 1 of Panel A, in Table 6 shows that long-horizon CEOs allocate more *physical* capital resources to innovation; thus they are associated with larger R&D investments (normalized by the size of book assets). These greater resources are not part of an expanded innovative scale, as measured by patent filings (column 2), so they are more likely to reflect a change in innovation strategy. More *human* capital resources to innovation are also allocated under longer CEO horizons: the likelihood that inventors who are new to the firm file patents is greater than for short-horizon managers (column 3). An inventor is classified as "new to the firm" in a given year if she first assigns a patent to the company during that year. One limitation of this analysis is that we detect inventor arrivals only when they patent and assign the innovation to the firm; hence "late filers" will be erroneously classified as new workers.

Panel B of Table 6 documents evidence consistent with long-horizon managers setting long-term incentives for the firm's other innovation-related executives. We use a novel small sample comprising 157 Chief Research, Scientific, and Medical Officers (CROs, for short) and 561 Chief Financial Officers (CFOs)—for which firms file employment contracts. Column 1 shows a significant association between CEO contract horizon and that of such executives' contract duration. Because CEO and other executive contracts may be set simultaneously by the board, each regression controls for the CEO's overall contract duration. For comparison, we also document results from the same regression for 123 Chairman contracts—on which CEOs should have little influence—and find no relation to the CEO's horizon.

Finally, Table 7 reveals differences in the project pipeline dissected by CEOs with long and short contract horizons, based on several patent-based metrics. Column 1 shows that long–contract horizon CEOs pick more varied projects that have a lower *Technological focus* as measured by the distribution of patent filings across technology classes. The projects also appear more varied in nature as measured by the V*ariance* of raw citations to patents (column 2)—while on average innovation quality increases with contract horizon, the number of misses is also higher.

Columns 3 and 4 show that longer horizon CEOs pick more exploratory pipelines that rely less on innovation produced by the firm in the past as measured by patent's *Self-citations* *(*i.e., the number of backward citations to other inventions previously filed by the same firm) and propensity to be filed in technology classes not explored previously by the company (*New technology class)*. Moreover, Column 5 shows that the more influential innovation of long-horizon CEOs is particularly associated with those patents in technological areas that are new to the firm. The coefficients for our *New technology class* dummy and its interaction with *Contract horizon* are both positive and significant; that is, patents in new fields are more influential, and their impact increases substantially with longer contract horizons.

[[INSERT Table 7 about Here]]

Finally, columns 6-8 show that the differences in the project pipeline do not appear to reflect complete overhauls to the nature of the company's technology, further strengthening the management and strategy (as opposed to scientific) arguments given by executives regarding how they affect firm innovation. There are no differences in the *Originality* or *Generality* of patents (i.e., dispersion across technology classes of backward and forward citations, respectively; see Jaffe and Trajtenberg, 2002). While the generality metric is positively associated with contract horizon, column 8 shows that the relation appears to be explained by aggregate trends—i.e., the positive association is no longer significant when we use a scaled version of the metric comparing patents to others filed the same year and in the same technology class. In unreported regressions we use a 2-digit (instead of 3-digit) technological classification (based on Hall et al., 2001); the results are qualitatively similar.

Overall, the results suggest that CEOs with longer horizons pursue more exploratory approaches, whereas those with shorter contract horizons are more technologically focused and concentrate on advancing previously successful technology.

4. INTERPRETATION

The robust correlation between contract horizon and patent citations is consistent with the theoretical literature on CEO myopia and career concerns. The myopia literature argues that managers concerned with the firm's short-term stock price may fail to invest in valuable opportunities if the benefits of such investments are only visible in the long run (Narayanan, 1985; Stein, 1988; 1989; Edmans et al. 2012). Longer contracts set longer performance horizons. Thus, managers with longer contracts may invest in high-quality projects that otherwise are rejected because of long time lags between the initial research and the availability of a commercially viable product.

Our results are also consistent with the theoretical literature on CEO career concerns. This literature argues that career-concerned, risk-averse managers take on less risk than desired by a diversified shareholder (Jensen and Meckling, 1976; Holmstrom, 1999; Gormley and Matsa, forthcoming). This misalignment is especially important for innovation investments where projects often fail for purely stochastic reasons (see Manso, 2011). Longer contracts can align managers by decreasing the sensitivity of compensation to performance. Indeed, Cziraki and Groen-Xu (2016) show that firms with bad performance are less likely to dismiss their CEO if their contract horizon is long. Thus, managers aligned with longer contracts may invest in higher quality projects because these contracts insulate managers against the reputational consequences of bad outcome realizations.

Although consistent with this theoretical work, interpreting our results in this light is not straightforward for three reasons: (i) the direction of causality is not clear, (ii) our innovation proxies may be inaccurate, and (iii) the efficiency implications are nuanced. We discuss these issues in detail now.

4.1. Causality

The positive correlation between contract horizon and innovation quality may not reflect a causal relationship. For example, if firms set longer contracts in anticipation of future scientific opportunities, then reducing the duration of contracts may not affect innovation.[9] We take two approaches to show that this noncausual effect is not the only interpretation of the findings.

4.1.1. Regulation change in the United Kingdom

Here we present evidence suggestive of a causal decrease in innovation after a government-mandated shortening of CEO employment agreements. We exploit a quasi-natural experiment for firms that are listed in the United States and also in the United Kingdom: a UK regulatory provision that shortened the average CEO contract length (House of Commons, 2002). In aiming to reduce "rewards for failure" and to increase equality in the employment conditions of white- and blue-collar workers, this provision recommended shortening the contract duration for CEOs and explicitly advocated one-year contracts as a best practice. While this recommendation is not legally binding, firms need to comply or explain —as usually occurs with the UK's advisory corporate governance standards.

We take advantage of a small subset of 30 firms in our sample that were also listed in the UK in 2002. For these firms, the reform breaks the link between contract setting by firms and innovation opportunities, thus a comparison of their innovation outcomes to that of non-dual listed, similar firms can provide insights on the causal effect of contract horizon. The identification assumption is that the UK's regulatory provision bears no positive relation to the innovation opportunities available to dual-listed versus US-only public firms. Indeed, Figure 5 shows that in the years leading up to the reform, non-dual listed firms followed a slightly more *negative* trend than that of dual listed firms.

First, we show that contract length indeed decreases for crosslisted firms after the reform. Panel A of Table 8 shows that, for these dual-listed firms—and relative to other US-

[9] Anecdotal evidence indicates that not all contract durations are matched to investment opportunities. For instance, we identify (in our sample of fixed-term contracts) five CEO turnover events that are due to sudden deaths. In each of these cases the successor was given a contract whose expiration date differed from the one of the deceased CEO.

listed firms (in the same industry and with CEOs of the same age and tenure)—there is indeed a decrease of 2.4 years, on average, in contract duration; see column 1.

[[INSERT Table 8 about Here]]

Next, we link the reform for dual-listed firms to innovation quality (column 2) using a differences-in-differences approach that controls for aggregate trends via year fixed-effects instead of scaling. Raw citations decrease on average by 0.79 for dual-listed firms after the reform (and relative to other similar US-listed firms), which corresponds to 15% of the average raw cites per year (5.24; see Table 1). The results persist whether or not we control for CEO age fixed effects (column 3). In column 4 of Table 8 (Panel A) we report results from regressing raw cites directly on contract length, which document a positive relationship that is similar to those revealed by previous regressions. Since this UK governance provision was most certainly not a reaction to changes in relative innovation opportunities of dual-listed firms, it follows that the correlation between innovation quality and contract horizon cannot be explained solely by unobserved scientific opportunities.

4.1.2. Historical contract length

We also address concerns about endogenous contract setting by exploiting the tendency of firms to reuse the same contract terms; for this purpose we employ the methodology of Cziraki and Groen-Xu (2016). In particular, we use the length of past contracts as a conditionally exogenous proxy for the actual contract length, while controlling for recurrent innovation cycles. The key idea behind this methodology is that even though past contract terms are predictive of future contract duration, they are unlikely to reflect accurate information about the firm's (nonrecurrent) innovation opportunities during *future* contract periods. The identifying assumption is that a firm's past contract terms are unrelated to future innovation opportunities once we control for its historical cycles of investment and other variables.

More specifically, we calculate predicted horizon as the number of years remaining until a predicted expiration year, which is the contract start year *plus* the average length of a firm's three previous contracts (our results are robust to using instead the firm's most recent contract). The resulting proxied contract length is comparable to that in our baseline sample, with a mean of three years (and a standard deviation of 0.23). The average difference between the real and proxied contract length is 0.61 years. Note that, because of the transformation necessary to obtain the number of years remaining, we cannot estimate the "first stage" simultaneously in a classical two-stage, least-squares setting. We estimate equation (1) again after replacing the horizon variable with the proxied contract horizon. This reduced-form estimation includes controls for potential recurring cycles in innovation, which are based on several demand- and supply-driven sources of cycles (as explained in Section 2.3).

The results are summarized in Panel B of Table 8. Our estimated coefficient for the predicted contract horizon is both positive and statistically significant. These results provide further evidence that the association between contract horizon and innovation quality is *not* explained by the purposeful matching (by boards) of CEO contract terms with the anticipated length of innovation opportunities.

4.2. Lags between innovation decisions and patent outputs

Patent-based proxies of innovation outcomes are not necessarily accurate measures of the innovation choices made by CEOs. For one thing, there can be lags between innovation inputs and outputs; for another, managers may time their patent filings based on strategic considerations. These potential mismatches between the time of patent filing (our observations) and the time of an innovation choice (our object of interest) are important because this paper relies on the time-varying nature of a contract horizon to make inferences about contractual incentives and innovation.

Consider systematic lags between innovation decisions and patent applications. Such lags could lead us to attribute current innovation outcomes to the choices of a CEO despite their true origin with prior managers (or with the same CEO but in earlier years). For example, the patterns we observe might be explained by the disciplinary effect of impending contract expiration and its obverse—potential renewal (Weisbach, 1988; Morck et al., 1989; Denis et al., 1997; Mikkelson and Partch, 1997; Hartzell, 2001; Bertrand and Mullainathan 2003) or by underperforming CEOs "gambling for resurrection" (Bebchuk and Stole, 1993).

To address these concerns, we explore in detail the existence of lags between innovation inputs and outputs as well as their prevalence across industries. We start by replicating the analysis of Hall et al. (1986) on a per-industry basis; doing so allows us to identify industries in which lagged R&D explains patent filings better than contemporaneous R&D expenditures do. Appendix D provides the classification details. In accordance with Hall et al. (1986) and with Gurmu and Pérez-Sebastián (2007), we find that contemporaneous R&D expenditures account for patent filings in most industries. One explanation is that, unlike academic publications, patents mark not only the full development of a product but also intermediate stages of research to prevent competitors from capitalizing on a promising technology (Pakes and Griliches, 1984; Granstrand, 1999; Blind et al., 2006; Mihm et al., 2015). Agriculture, food and business services are the industries with the longest lags—perhaps because it is difficult to document intermediate development in such industries.

We then sort industries in terms of the estimated lags and replicate our basic analysis by industry. We are reassured to find that, as reported in Panel A of Table 9, our results are strongest for industries with contemporaneous patent filings. Consistently with the existence of measurement inaccuracies, our results are weaker with increasing lags between expenditures and filings; this dynamic lends credence to the notion that patent metrics are inaccurate and also underscores the importance of cross-industry analysis. For industries characterized by lags of two or more years, none of our contract horizon dummies is significant.

[[INSERT Table 9 about Here]]

A different measurement issue concerns the possibly strategic considerations of CEOs as regards the *timing* of patent filings. Even absent systematic lags in innovation inputs and outcomes and even though there are legal incentives to file patents immediately after innovating, it is still possible that CEOs strategically time those filings so as, for example, to maximize their bargaining power in contract renegotiations (for evidence of contract rigging by powerful CEOs, see Morse et al., 2011). In that case, innovation patterns might be more indicative of CEOs' strategic decisions about when to file patents than of any real innovation outcomes. However, in practice this explanation appears to have little explanatory power as only 27% of our sample contracts are renegotiated before expiration. Moreover, we recall from Table 3 that the correlation between cites and CEO contract horizon is actually stronger for new contracts than for renewals. Nevertheless, in Panel B of Table 9 we show that the results continue to hold in better-governed firms. Consistently with powerful CEOs being less concerned about their careers, the citations of patents shepherded by CEOs with more power—as measured by the presence of insider boards, a Chairman-CEO, or a higher G-index number—are less related to contract horizon (though not significantly so for Chairman-CEOs) than are the patent figures for less powerful CEOs. We also find a positive relation between innovation and insider-dominated boards, which is in line with research (e.g., Kang et al., 2014) showing how board members connected to the CEO protects CEOs from market pressure.

4.3. Efficiency

The results reported in Section 4.1 indicate that forcing firms to reduce the length of their CEO contracts results in lower-importance innovation; however, that does not necessarily imply that it destroyed value. For one thing, we focus on innovation and do not consider other potentially negative effects (on managerial discipline) of longer contracts studied in the corporate governance literature. For instance, entrenched managers with longer contracts may overinvest (Jensen, 1986; Shleifer and Vishny, 1989; Jensen, 1993, Pan et al., 2014) as an empire-building strategy. For another thing, exploratory innovation is itself not always efficient; the firm may rationally decide to offer a shorter CEO contract because it wants to focus on "harvesting" and commercializing innovations (Ferreira et al., 2014). Indeed, Rhodes-Kropf and Nanda (2014) point out that without incentive structures that act as a "guillotine", organizations face inertia and avoid shutting down unprofitable projects.

Perhaps a better interpretation of our results is that they highlight one reason why principals may find it detrimental to adopt a short-horizon contract: innovation quality. In the context of the UK reform, our results imply that—following the proposed average decrease of two years in contract length—affected firms exhibited a 15% decline in average innovation importance (see Section 4.1.1). Among the potential consequences of such a decline is underinvestment in long-term and especially in exploratory projects (e.g., research on early-stage cancer; Budish et al., 2015). In countries seeking to reform the contract terms offered to managers, regulators should weigh the potential benefits of such reforms against the possible costs to innovation, as documented in this paper.

5. CONCLUSION

We use a novel data set on CEO employment agreements to document for the first time a robust positive correlation between a firm's CEO contract duration and the quality of its innovation output. Using cross-sectional, within-firm and within–CEO–firm pair variation we show that managers with more years remaining in their contract are associated with more influential and varied innovations. This correlation holds in a context where contract duration is set in a manner that is exogenous to the innovation opportunities facing firms.

We provide evidence of how CEOs with longer horizons change innovation. CEOs with more years remaining in their contract allocate more financial resources to R&D, hire new inventors, set longer term incentives for CROs, and pursue more exploratory innovation. These managerial practices can help explain differences in innovation across firms more broadly and shed light on how managers direct innovation even though they are hierarchically distant from inventors.

Overall our results are consistent with the theoretical literature on CEO myopia and career concerns. Longer contracts allow CEOs to invest in riskier, more exploratory and ultimately better quality innovation by setting lengthier performance horizons for managers and by protecting managers from early dismissal on the grounds of short-term underperformance.

The UK shortened CEO contract terms to do away with "the culture of rewards for failure". In the same spirit, Switzerland adopted the shortening of director terms to one year in 2015 (Wagner and Wenk, 2015) and further measures have been debated ever since. Our results show that regulatory limits on CEO contract duration can reduce the quality of corporate innovation. This potential negative consequence should be considered in debates over limits to CEO contract duration.

The contract horizon is likely to also be relevant in other settings such as academic research. Exploring how researcher contract terms affect research directions and innovation in research-oriented institutions, such as universities and government laboratories, promises to be a fruitful avenue for future work.

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Figure 1 – Excerpt from contract between Hector Ruiz and Advanced Micro Devices, Inc.

EMPLOYMENT AGREEMENT

Dear Hector:

On behalf of the Board of Directors of Advanced Micro Devices, Inc. (including as successor thereto, "AMD"), I am pleased to offer you the position of President and Chief Executive Officer of AMD on the terms set forth below.

1. Position.

(a) You will be employed by AMD as its President and Chief Executive Officer commencing on April 26, 2002 (the "Commencement Date"). You will have overall responsibility for the management of AMD and will report directly to its Board of Directors ("Board"). During the Employment Period (as defined below), you will also be nominated to and, if elected by the stockholders of AMD, shall serve on the Board and such committees that you may be appointed to by the Board. You shall succeed to the position of Chairman of the Board when the person serving in such capacity on the Effective Date shall cease to hold that position.

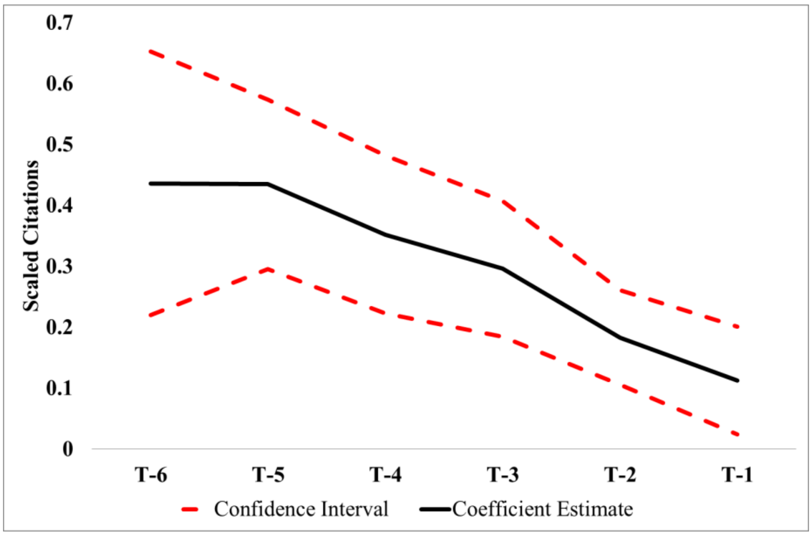
[…]

2. This Agreement shall be in effect upon the signing by both parties (the "Effective Date"), and shall expire five (5) years after the Commencement Date (the "Employment Period"), unless sooner terminated pursuant to Section 8 or extended pursuant to this Section 2.

Figure 2 – Corresponding excerpt from data set

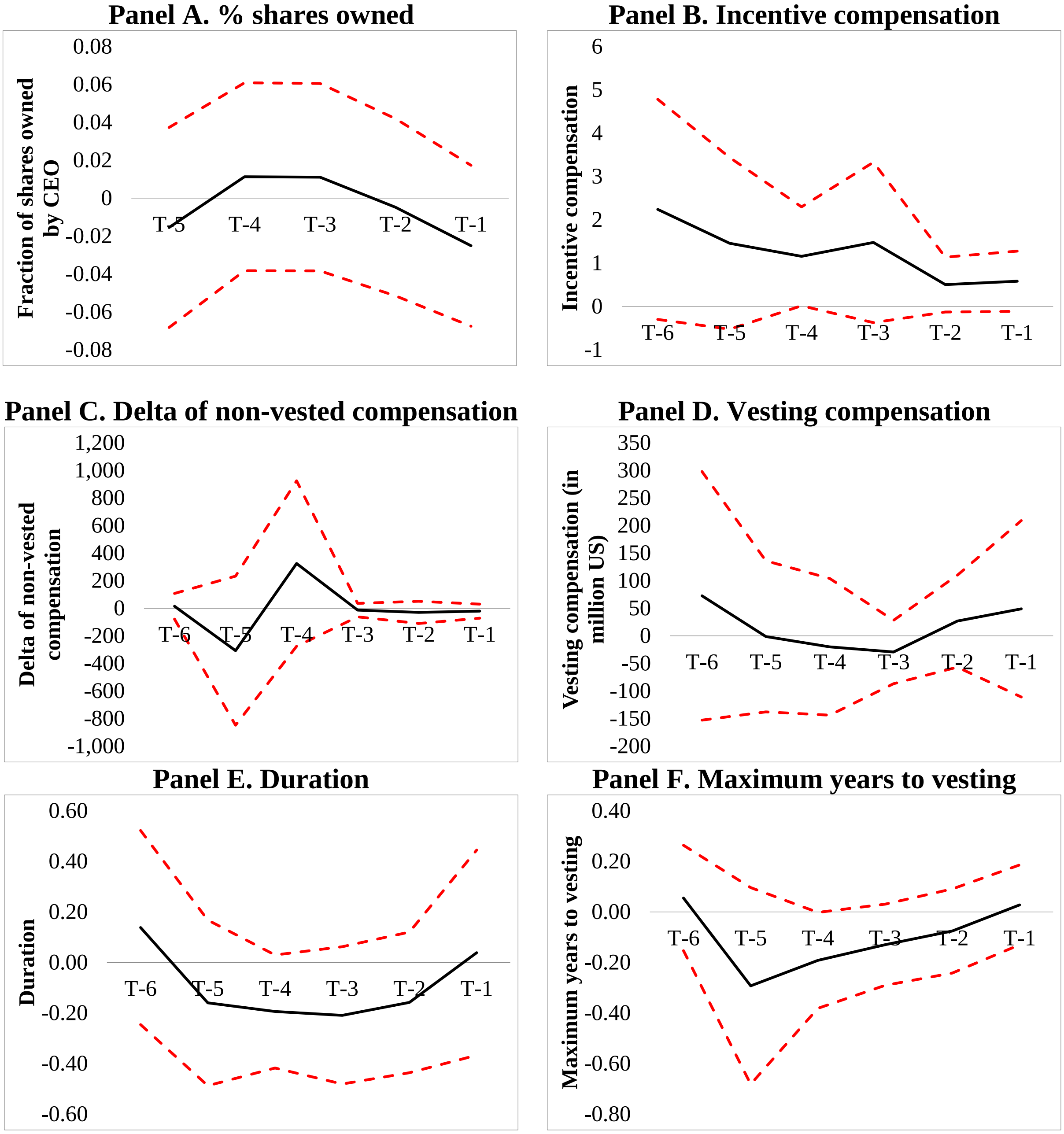
Firm	CEO name	Start year	Patent #	Appl. year	Horizon (years)	Tenure (years)	Cites
AMD	Hector Ruiz	2002	6445174	2002	5	0	0.00
AMD	Hector Ruiz	2002	6762448	2003	4	1	9.06
AMD	Hector Ruiz	2002	7183152	2004	3	2	0.56
AMD	Hector Ruiz	2002	7183629	2004	3	2	0.49
AMD	Hector Ruiz	2002	7200455	2005	3	3	0.00
AMD	Hector Ruiz	2002	7233835	2006	2	4	0.00

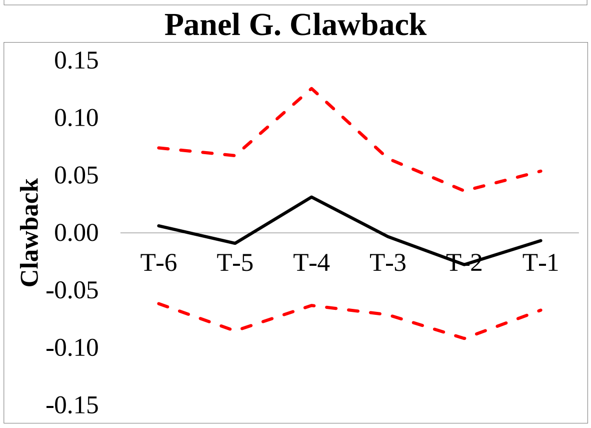
Figure 3 – Contract horizon and citations



This figure plots the coefficient estimates of equation (1); cites are regressed on contract horizon dummies, contract start year fixed effects, firm fixed effects, and tenure fixed effects. The solid line represents the estimated coefficients of the contract horizon dummies (from column 1 of Table 3); the dashed lines mark out the 95% confidence intervals around those estimates.

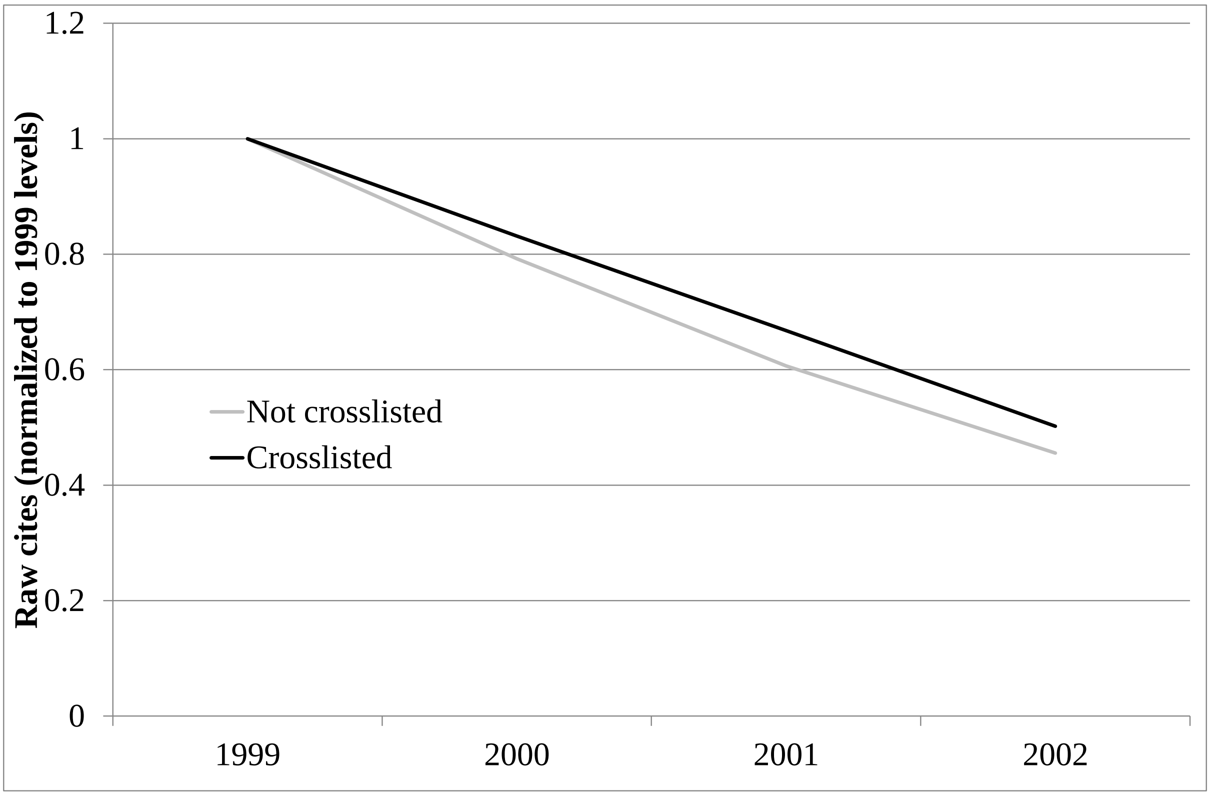
Figure 4 – Contract horizon and compensation





This figure plots the coefficient estimates of regressions where the independent variables are as in equation (1) and the dependent variables are as in the Panel name. The solid line represents the estimated coefficients of the contract horizon dummies; the dashed lines mark out the 95% confidence intervals around those estimates.

Figure 5 – Trends in raw cites prior to UK reform



This figure plots average raw cites from 1998-2002 for US firms that were vs. were not crosslisted in the UK, normalized to their 1999 levels.

Table 1 – Sample statistics
Panel A: Distribution by year

Year	Number of contracts		Mean contract length		Patents		Citations	
	New	Renewed	New	Renewed	Applications	Grants	Raw	Scaled
1993	2	9	4.00	5.13	2141	43	8.84	1.38
1994	29	11	5.21	3.25	1,821	703	5.29	1.40
1995	33	21	5.48	3.89	2,452	1,518	6.64	1.69
1996	37	46	5.14	3.35	4,369	1,999	8.48	1.52
1997	62	64	3.74	3.77	10,640	2,269	10.88	1.48
1998	60	51	3.96	2.72	7,995	4,545	8.08	1.48
1999	46	75	4.00	3.34	9,157	7,204	7.99	1.54
2000	61	50	3.76	3.32	8,419	7,892	6.55	1.51
2001	67	61	4.02	3.03	10,175	8,182	5.26	1.35
2002	62	38	3.35	3.13	8,478	8,342	4.35	1.25
2003	47	41	3.58	2.70	7,924	7,737	2.71	1.02
2004	55	37	2.95	3.15	7,277	7,021	1.39	0.74
2005	55	31	3.09	3.17	8,734	6,082	0.77	0.49
2006	44	44	2.65	3.16	5,859	7,359	0.40	0.34
2007	33	42	2.52	2.50	3,127	6,909	0.16	0.16
2008	17	13	2.67	2.63	962	7,091	0.08	0.06
2009					175	7,388	0.02	0.06
2010						7,421		
Total	710	634	3.76	3.21	99,705	99,705	5.24	1.16

Panel B: Distribution by contract length

Contract length	Number of contracts		
	New	Renewed	Total
1	83	55	138
2	119	103	222
3	167	188	355
4	74	45	119
5	73	55	128
6	33	14	47
7	13	1	14
8	12	8	20
9	8	1	9
>9	128	164	292
Total	710	634	1344

Panel C: Distribution by governing state (5 states with largest number of contracts)

State	CA	MA	NY	TX	NJ
Number of contracts	249	83	79	78	69
Average contract length	3.37	4.94	3.94	3.63	3.77
Sample distribution	19%	6%	6%	6%	5%
COMPUSTAT distribution	18%	1%	8%	6%	6%
Sample patent applications	19,303	2,888	3,056	6,661	14,797
% of all applications	19%	3%	3%	7%	15%
Raw Cites	4.67	4.78	3.58	3.89	3.56

Panel D: Distribution by industry (5 industries with largest number of contracts)

Industry	Electronic	Pharma	Medical	Software	Machinery
Number of contracts	140	133	97	91	82
Average contract length	4.02	3.22	3.42	3.72	3.26
Sample distribution	10%	10%	7%	7%	6%
COMPUSTAT distribution	5%	5%	3%	8%	2%
Sample patent applications	18,380	7,724	1,626	4,297	6,403
% of all applications	18%	8%	2%	4%	6%
Raw Cites	4.50	1.76	6.49	5.86	3.48

Panel E: Descriptive statistics

		Obs.	Mean	Std. Dev.	Min	Median	Max
			Patent level				
Patent	Raw Cites	99,705	5.24	10.20	2.00	0.00	328.00
	(Scaled) Cites	99,705	1.16	2.22	0.45	0.00	54.41
	Filtered market response	93,647	0.90	0.19	0.98	0.00	1.00
	Originality	57,473	0.82	0.28	0.95	0.00	1.00
	Generality	72,715	0.01	0.12	0.00	0.00	1.00
	New inventor	90,124	0.02	0.03	0.01	0.00	1.46
	Self-citations	71,776	0.65	0.48	1.00	0.00	1.00
			Firm-year level				
Firm	Assets (million USD)	3,470	3455.68	591.48	6766.53	4.12	29770.00
	Patents filed	3,958	33.63	3.00	194.17	1.00	4422.00
	R&D/Total assets	2,894	0.10	0.04	0.19	0.00	2.57
	Technological focus	3,958	0.63	0.56	0.33	0.02	1.00
	Variance in importance	2,285	6.12	3.83	7.87	0.00	127.33
CEO	Horizon	3,958	2.32	2.00	2.23	0.00	18.00
	Age	3,084	53.79	54.00	7.76	30	92.00
	Tenure	3,958	4.24	2.00	5.91	0	57.00
	% shares owned	3,958	15.4%	0.0%	1.53	0.0%	4000%
	TDC1 ('000 USD)	1,964	6,286.97	3,118.38	8,090.64	225.69	41,621.01
	TDC2 ('000 USD)	1,967	5,538.87	2,190.07	9,205.98	164.34	54,909.55
	Delta (vested)	1,611	87.50	17.46	314.34	-	4,847.08
	Delta (non vested)	1,611	56.23	11.43	369.88	-	12,268.78

The table presents summary statistics of the sample used for our regression analysis. The sample includes information on 1,344 fixed-term employment contracts by firms that were granted at least one US patent during the 1993–2008 period.

Table 2 – Differences in citations

Average raw cites	(1) Short horizon (< 3 years)	(2) Long horizon (> 3 years)	(3) Difference (Long – short)
All patents with contracts	4.38	6.53	2.14*** (0.072)
Treatment (Patents *with contracts* with matches)	3.84	5.90	
Control (Benchmark *without contracts)*	9.83	10.04	
Difference (Treatment minus control)	–5.99*** (0.0152)	–4.14*** (0.158)	
Difference in Differences			1.86*** (0.219)

This table presents the average *Raw cites* to patents filed by CEOs with short (less than 3 years) versus long (more than 3 years) horizons as measured by the number of years remaining on their fixed-term employment contracts. In the first row, averages are calculated over the sample of CEO–firm pairs with fixed-term contracts and column 3 reports the difference in average *Raw cites* for long versus short horizons. (Standard errors are reported in parentheses.) In rows 2–4 we match the CEO–firm pairs in our sample to CEO–firm pairs with no fixed-term contracts using a nearest-neighbor matching procedure of propensity scores (as explained in Appendix A). We report average *Raw cites* for those CEO–firm pairs for which we could find a suitable match (Treatment) and also for the matched firms (Control). The difference in average *Raw cites* between treatment and control firms for short- and long-horizon CEOs is included as the last row of this panel. Finally, in the last row we report the difference in the differences in average *Raw cites* between short- versus long-horizon contracts and treatment versus control firms. Asterisks indicate that the estimates are significantly different from zero at the ***1% level.

Table 3 – Contract horizon and citations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$D^{>5}$	0.436***						
	(0.110)						
D^5	0.435***						
	(0.071)						
D^4	0.352***						
	(0.066)						
D^3	0.296***						
	(0.057)						
D^2	0.183***						
	(0.040)						
D^1	0.113**						
	(0.045)						
Horizon		0.065***	0.064***	0.070***	0.062***	0.061***	0.073***
		(0.012)	(0.012)	(0.013)	(0.013)	(0.020)	(0.018)
Observations	95,801	95,801	95,793	82,583	82,587	95,665	56,148
R-squared	0.117	0.117	0.117	0.116	0.112	0.127	0.121
Test $D^5 - D^1 = 0$	1.16e-06						
Test $D^4 - D^1 = 0$	1.21e-05						
Test $D^3 - D^1 = 0$	7.60e-05						
Test $D^2 - D^1 = 0$	0.0517						
Tenure FE	Yes	Yes	Yes	Yes	No	Yes	Yes
CEO age, firm age FE	No	No	No	Yes	No	No	No
Tenure, CEO age, firm age polynomials	No	No	No	No	Yes	No	No
Only renewals	No	No	No	No	No	No	Yes

This table reports OLS estimates of different versions of equation (1); heteroskedasticity-robust and firm-clustered standard errors are reported in parentheses. The dependent variable is *Cites*. Main explanatory variables are the contract horizon dummies (the D^i) and *Horizon*. Both contract start year fixed effects and firm fixed effects are included in all estimations—except in column 3 (resp. 6), where we use start-year cross industry (resp. CEO) fixed effects. In column 7 we restrict the sample to contract renewals. The "Test" panel reports (in column 1) t-statistics for the differences between the estimated coefficients for dummies of longer than one-year horizons and D^1. Asterisks indicate that the estimates are significantly different from zero at the ***1% level or **5% level.

Table 4 – Robustness

Panel A: Robustness Part I

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:	*(Scaled) Cites*	*Raw Cites*	*Raw Cites*	*Market response*	*(Scaled) Cites*	*(Scaled) Cites*	*(Scaled) Cites*
Sample:	*All*	*All*	*All*	*All*	*Contract length <=3*	*Contact length >3*	*Excluding HQs in CA*
Horizon	0.064***	0.196*	0.169*	0.001**	0.083***	0.081***	0.061***
	(0.012)	(0.103)	(0.098)	(0.001)	(0.024)	(0.018)	(0.012)
% shares owned	-0.101***						
	(0.032)						
Incentive compensation	-0.000						
	(0.001)						
Delta of non-vested compensation	0.000						
	(0.000)						
Vesting compensation	-0.000***						
	(0.000)						
Duration	0.015						
	(0.022)						
Maximum years to vesting	-0.012						
	(0.019)						
Clawback	-0.140***						
	(0.051)						
Total Compensation (TDC1)	0.000						
	(0.000)						
Incentive compensation	0.012						
	(0.049)						
Vesting data dummy	0.064***						
	(0.012)						
Observations	95,801	96,401	66,775	86,662	29,568	66,198	76,771
R-squared	0.117	0.214	0.178	0.540	0.157	0.098	0.119
Start year FE	Yes	No	No	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	No	No	No	No
Only data until 2005?	No	No	Yes	No	No	No	No

Panel B: Robustness Part II

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	*Cites*	*Cites*	*Cites*	*Cites*	*Cites*	*Cites*
Cycle:	Capex	Sales	Receivables	Employees	Researchers	Finance
Horizon	0.112***	0.134***	0.094***	0.091***	0.075***	0.059***
	(0.021)	(0.025)	(0.017)	(0.018)	(0.012)	(0.012)
Alternative cycle	–0.028	–0.034	0.057**	–0.058**		
	(0.022)	(0.037)	(0.023)	(0.024)		
PhD graduates					–1.304	
					(1.12)	
Industry-specific graduates					–0.294	
					(0.713)	
Financial transactions						–0.018
						(0.053)
Real interest rate						0.018
						(0.022)
Observations	22,316	97,054	97,054	13,735	81,891	95,751
R-squared	0.112	0.071	0.071	0.077	0.077	0.073

This table reports OLS estimates of different versions of equation (1); heteroskedasticity-robust and firm-clustered standard errors are reported in parentheses. For Panel A, the dependent variable is as indicated by the column heading; for Panel B, the dependent variable is *Cites.* The main explanatory variable is *Horizon*. All regressions include both firm and tenure fixed effects. In Panel A, column 1, the vesting data dummy is an indicator variable set equal to 1 for observations with *no* information on average duration and maximum vesting years. In Panel B, all regressions include start-year fixed effects in addition to the tenure and firm fixed effects. Alternative cycles are the number of years remaining under cycles extrapolated using the distance in previous years between bottom and peak (each defined as years exhibiting reversals exceeding 5%) in the variable indicated by the "Cycle" row. Asterisks indicate that the estimates are significantly different from zero at the ***1% level, **5% level, or *10% level.

Table 5 – Innovation quality near contract renewals

	(1)	(2)
3 years before renewal	0.210*	–0.023
	(0.121)	(0.207)
2 years before renewal	–0.093	–0.316*
	(0.137)	(0.186)
Renewal	0.190**	0.208*
	(0.083)	(0.113)
1 year after renewal	0.047	0.082
	(0.069)	(0.120)
2 years after renewal	–0.005	0.043
	(0.124)	(0.161)
3 years after renewal	–0.207	–0.172
	(0.138)	(0.184)
Observations	95,801	95,801
R-squared	0.116	0.116
Firm FE	Yes	No
CEO–firm pair FE	No	Yes

This table reports OLS estimates of different versions of equation (2); heteroskedasticity-robust and firm-clustered standard errors are reported in parentheses. The dependent variable is *Cites*, and the main explanatory variables are event-time indicators of contract renewal. In the estimation we include a separate indicator for each year of the span from 7 years before to 16 years after the contract renewal date. We report values for the window from 3 years before to 3 years after contract renewal. The coefficients are normalized relative to the year *before* contract renewal (which is omitted from the regression). All regressions include tenure fixed effects and contract start year fixed effects. Asterisks indicate that the estimates are significantly different from zero at the **5% level or *10% level.

Table 6 – Allocation of resources to innovation

Panel A: R&D investments and new inventors

	(1)	(2)	(3)
Dependent variable:	*R&D / Total assets*	*Patent filings*	*New inventor*
Horizon	0.003***	–2.786	0.003**
	(0.001)	(3.476)	(0.001)
Observations	3,958	3,958	95,801
R-squared	0.751	0.898	0.236
Start year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	No
Tenure FE	Yes	Yes	No

Panel B: Long term incentives for Chief Research Officers (CROs)

	(1)	(2)	(3)
Dependent variable: Contract length of…	*CROs*	*CFOs*	*Chairmen*
CEO horizon	0.181*	0.173**	–0.124
	(0.1011)	(0.0671)	(0.2299)
CEO contract length	0.03	0.117*	0.530***
	(0.0644)	(0.0698)	(0.1540)
Constant	1.762	0.981	7.720**
	(1.092)	(1.5646)	(3.2734)
Observations	157	561	123
R-squared	0.08	0.146	0.177
Start year FE	Yes	Yes	Yes

Each panel of this table presents the results of OLS regressions, reporting coefficients along with (heteroskedasticity-robust and firm-clustered) standard errors in parentheses. For Panel A, the dependent variable is as indicated by the column heading; for Panel B, the dependent variable is the contract length of CROs (column 1), CFOs (column 2), or Chairmen of the Board (column 3). The explanatory variables are the number of years remaining on the CEO's contract and the length of that contract. All specifications include contract start year fixed effects. Asterisks indicate that the estimates are significantly different from zero at the ***1% level, **5% level, or *10% level.

Table 7 – Exploration

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable:	*Technological focus*	*Variance*	*Self-citations*	*New technology class*	*Cites*	*Generality*	*Originality*	*Scaled generality*	*Scaled originality*
Horizon	–0.014***	0.403***	–0.017**	0.003**	0.063***	0.015***	0.001	–0.002	–0.001
	(0.003)	(0.133)	(0.007)	(0.001)	(0.012)	(0.003)	(0.001)	(0.001)	(0.001)
New tech. class					–0.018				
					(0.100)				
Horizon × New tech. class					0.044				
					(0.032)				
Observations	3,958	2,285	95,801	95,801	95,801	55,630	90,083	55,629	90,083
R-squared	0.713	0.644	0.133	0.236	0.117	0.125	0.073	0.041	0.053

This table presents the results of OLS regressions, reporting coefficients along with (heteroskedasticity-robust and firm-clustered) standard errors in parentheses. The dependent variable is as indicated by the column heading. The main explanatory variable is *Horizon*, or the number of years remaining on the CEO's contract. All specifications include contract start year fixed effects, firm fixed effects, and tenure fixed effects. Asterisks indicate that the estimates are significantly different from zero at the ***1% level or **5% level.

Table 8 – Causal effects

Panel A: Dual listing

	(1)	(2)	(3)	(4)
Dependent variable:	*Contract Length*	*Raw Cites*	*Raw Cites*	*Raw Cites*
Cross-listed	1.128	–0.524***	–0.444	
	(2.081)	(0.169)	(0.311)	
Cross-listed × Post	–2.419**	–0.787*	–0.741**	
	(1.067)	(0.420)	(0.375)	
Contract length				1.431***
				(0.308)
Constant				–3.537***
				(1.274)
Observations	1,755	56,007	54,747	62,795
R-squared	0.848	0.191	0.195	
Year FE	Yes	Yes	Yes	No
Technology FE	No	Yes	Yes	Yes
Company FE	Yes	Yes	Yes	No
Tenure FE	Yes	Yes	Yes	No
CEO age FE	No	No	Yes	No

Panel B: Sticky contracts

	(1)	(2)	(3)	(4)	(5)	(6)
Cycle:	Capex	Sales	Receivables	Employees		
Predicted horizon	0.132***	0.226***	0.156***	0.208***	0.087***	0.100***
	(0.026)	(0.018)	(0.023)	(0.016)	(0.017)	(0.012)
Alternative cycle	0.048*	–0.029**	0.026	0.002		
	(0.025)	(0.01)	(0.021)	(0.007)		
PhD graduates					–0.907	
					(0.899)	
Industry-specific graduates					–0.362	
					(0.4)	
Financial transactions						–0.213**
						(0.09)
Real interestrate						0.004
						(0.015)
Constant	–0.373	–0.482**	3.084***	–2.269***	11.62	–0.357
	(0.271)	(0.18)	(0.48)	(0.125)	(6.8)	(0.602)
Observations	8,944	9,299	7,779	6,190	32,164	33,565
R-squared	0.108	0.077	0.086	0.107	0.095	0.095
Start year FE	Yes	Yes	Yes	Yes	Yes	Yes
Company FE	Yes	Yes	Yes	Yes	Yes	Yes
Tenure FE	Yes	Yes	Yes	Yes	Yes	Yes

This table presents the results of OLS regressions, reporting coefficients along with (heteroskedasticity-robust) standard errors in parentheses. The dependent variables in Panel A are those indicated by the column heading, and the data used are restricted to patents with application year after 1999 and contract start year before 2007. In Panel A, data are at the firm-year level for column 1 and at the patent level for all other columns. The "Cross-listed" dummy is set equal to 1 for firms that were cross-listed in the United Kingdom in 2003; the "Post" dummy is set equal to 1 for years after 2003. In Panel B, the dependent variable is *Cites*, and *Predicted horizon* is the number of years remaining until the start year of the contract *plus* the average length of the three previous contracts. Alternative cycles are the number of years remaining under cycles extrapolated using the distance in previous years between bottom and peak (each defined as years exhibiting reversals exceeding 5%) in the variable indicated by the "Cycle" row. Asterisks indicate that the estimates are significantly different from zero at the ***1% level, **5% level, or *10% level.

Table 9 – Measurement issues

Panel A: Industry heterogeneity

	(1)	(2)	(3)	(4)
Industry:	Lag = 0	Lag = 1	Lag = 2	Lag = 3
Horizon:	Actual	Actual	Actual	Actual
$D^{>5}$	0.500**	0.820***	0.605*	–2.464***
	(0.214)	(0.271)	(0.355)	(0.535)
D^{5}	0.635***	0.756***	0.058	–0.782
	(0.080)	(0.177)	(0.328)	(0.825)
D^{4}	0.413***	0.621***	–0.465*	–1.164
	(0.085)	(0.175)	(0.243)	(0.753)
D^{3}	0.425***	0.440***	–0.225	–0.324
	(0.090)	(0.128)	(0.270)	(0.605)
D^{2}	0.238***	0.259***	–0.264	–0.803*
	(0.045)	(0.087)	(0.264)	(0.414)
D^{1}	0.117**	0.097	0.281	–0.424*
	(0.046)	(0.099)	(0.306)	(0.242)
Constant	–1.465***	–0.898**	1.891	3.657**
	(0.439)	(0.450)	(1.526)	(1.425)
Observations	31,768	21,094	972	579
R-squared	0.144	0.091	0.177	0.153

Panel B: Governance

	(1)	(2)	(3)	(4)
Governance measure:	Insider board	Institutional ownership	Chairman -CEO	G-index
Horizon	0.071***	0.067***	0.077***	0.280***
	(0.012)	(0.013)	(0.029)	(0.105)
Governance measure	0.283*	0.022	0.082	0.084*
	(0.152)	(0.016)	(0.076)	(0.047)
Horizon ×	–0.070**	–0.001	–0.016	–0.018*
Governance measure	(0.029)	(0.003)	(0.028)	(0.011)
Observations	95,801	95,801	95,801	30,521
R-Squared	0.117	0.117	0.117	0.104

This table presents the results of OLS regressions, reporting coefficients along with (heteroskedasticity-robust and firm-clustered) standard errors in parentheses. The dependent variable is *Cites*. The main explanatory variables are the time to expiration dummies (D^h), which are set equal to 1 when there are *h* years remaining on the CEO's contract; and *Horizon*, the number of years remaining on that contract. All specifications include firm, contract start year, and tenure fixed effects. In Panel A, all regressions are run for an industry subsample based on the lag time indicated by the column heading (see Appendix D for details). Panel B reports regressions in which the explanatory variables include the governance measure indicated by the column heading as well the interaction between that measure and *Horizon*. Asterisks indicate that the estimates are significantly different from zero at the ***1% level, **5% level, or *10% level.

APPENDICES

Appendix A. Matching

We compare firms with varying CEO horizon to firms without contracts and therefore with no varying CEO horizon. To do so we construct a nearest-neighbor matching estimator following the approach of Rosenbaum and Rubin (1983). Although we do not observe the decision to sign contracts, the matching procedure reconstructs this information via the following observable characteristics: size (log of book assets), market-to-book ratio, return on assets (ROA), industry (Fama–French 49), year, and tenure.

We construct the control sample in two steps. First, we run a logit regression to predict the presence of a CEO contract based on firm and CEO characteristics. The sample used for this purpose is at the firm-year level. In Panel A of Table A.1 we report the coefficient estimates of the logit regression. The *p*-value of the chi-squared test and the R-squared value (of 19%) confirm that the specification captures a significant amount of variation in contract setting.

We then use the predicted probabilities from this estimation, the propensity scores, to perform a nearest-neighbor match with replacement. In particular, each firm with a fixed-term contract is paired with the ten firms (with no fixed-term contracts) whose propensity scores are the closest. This matching process ensures that we select the most similar matches as innovation benchmarks. Panel B of Table A.1 reports the average difference between the original samples and between matched sample and control; it also reports (in the last column) standard errors and t-statistics for a test of the hypothesis that the difference between contracted and not-contracted CEOs is zero (last column). While all characteristics differ significantly between contracted and non-contracted CEOs in the non-matched samples, the matching procedure is able to reduce the differences. Among the variables included in the first stage, only tenure is significantly different between CEOs with and without contracts after matching, albeit with a much smaller difference of 0.45 years (compared to 1.80 before the match). Since we matched with tenure dummies instead of a continuous measure of tenure, the large difference may reflect non-linearity of the propensity to obtain a contract for CEOs with different tenure, a factor for which we account in the matching process.

In addition to the matching variables, we also show a comparison between the matched subsamples by the number of patents filed. There are no significant differences between CEOs with contracts and their noncontracted matched counterparts. These results corroborate our choice of match variables and confirm that the match procedure selects CEOs and firms that are similar to the treatment sample.

Table A.1 – Propensity score matching diagnostics

Panel A: Logit regression results

Dependent variable	Contract
Market/book	–0.045***
	(0.009)
Log of assets	–0.187***
	(0.014)
ROA	–0.217
	(0.148)
Constant	–1.951*
	(1.072)
Observations	20,237
Industry FE	Yes
Year FE	Yes
Tenure FE	Yes
Pseudo R-squared	0.1906
Chi-squared *p*-value	0.0000

Panel B: Pairwise comparisons

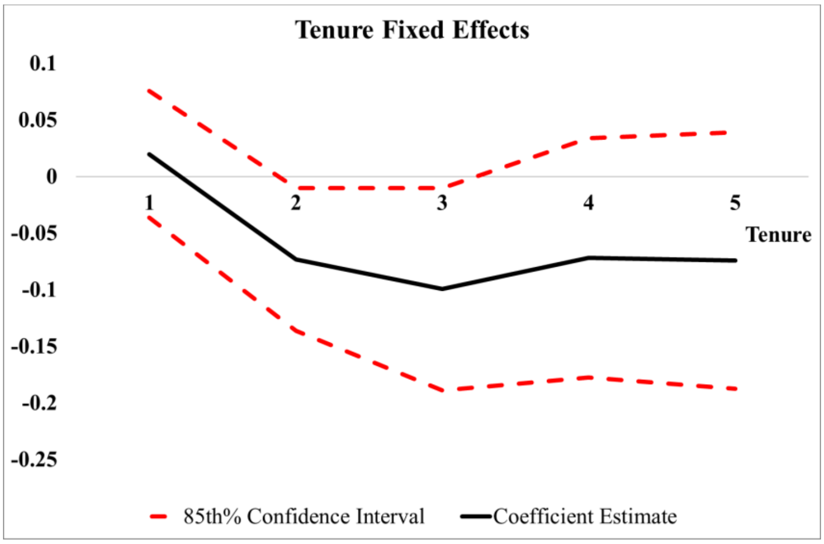
	Unmatched sample			Matched sample		
Variable	Difference	S.E.	T-stat.	Difference	S.E.	T-stat.
Market/book	–0.14	0.05	–2.89	0.12	0.06	1.85
Assets	254.25	117.20	2.17	–182.57	169.84	–1.07
ROA	–0.02	0.00	–4.10	0.00	0.00	–0.47
Tenure	1.80	0.11	16.88	0.45	0.15	2.92
No. of patents	–5.09	3.68	–1.38	0.66	5.00	0.13

This table reports diagnostics for a propensity score matching procedure between firm-years with fixed-term CEO contracts (treatment) and those without (control). Panel A reports the coefficients for a logit regression in which the dependent variable is an indicator set equal to 1 if the firm's CEO has a fixed-term contract in that year. The logit is run at the firm-year level, and this regression's results are used to estimate propensity scores for the treatment and control groups. Panel B reports differences between means for firm-years with fixed-term CEO contracts and for those without such contracts—in addition to standard errors and t-statistics for a test that such differences equal zero. The first (resp. second) group of columns reports statistics for the unmatched (resp. matched) sample. Asterisks indicate that the estimates are significantly different from zero at the ***1% level or *10% level.

Appendix B. Life-cycle fixed effects

Our regressions enable us to distinguish the effect of contract horizon on innovation from the potential influence of *Tenure* (i.e., we compare innovation quality across CEOs with the same tenure but differing contract horizons). The possibility of tenure effects on innovation is of interest in light of reported evidence on such effects in tangible investments (see Pan et al., 2014). To explore these potential effects, we looked for patterns in the estimated tenure fixed effects of our main results in column (3) of Table 3. Consistently with Pan et al. (2014), we find evidence suggesting that CEOs behave differently in the first two years of a contract. In particular, they file lower quality innovations; this dynamic is indicated by the respective estimated coefficients and is graphed in Figure B.1.

Figure B.1 Tenure fixed effects



Plotted coefficient estimates of equation (1), where *Cites* is regressed on contract horizon dummies, contract start year fixed effects, firm fixed effects, and tenure fixed effects. The figure plots the estimated tenure fixed effects. The solid line corresponds to the estimated coefficients for tenure fixed effects, and the dashed lines mark the 85% confidence intervals around those estimates.

Appendix C. Mean-reverting variables: Simulations

To illustrate that a mean-reverting variable cannot generate our results, we report in Table C.1 the results of Monte Carlo simulations using artificial data that are strongly similar to our real data. For each simulation, we generate 10,000 samples of 1,000 CEO–firm pairs. Then, for each sample of CEO–firm pairs $i \in \{1, \dots, 1000\}$, we draw (from uniform distributions) firm-CEO fixed effects $u_i \in \{0, \dots, 1\}$ and a number of contracts $J_i \in \{1, \dots, 5\}$ with length $T_{ij} \in \{1, \dots, 5\}$.

For each CEO–firm pair, we generate a mean-reverting process c_{it} which exclusively explains an outcome variable y_{it}. So that c_{it} will be more correlated with the contract horizon, we fix contract lengths to be equal to the average cycle length T of the mean-reverting process. For each CEO–firm pair i, we randomly generate (i) a (0, 1) normally distributed process mean $\bar{c}$, (ii) a reversion factor η that is uniformly distributed between 0 and 1, and (iii) a starting value c_{i0} that is normally distributed with mean $\bar{c}$ and variance $(1 - e^{-2\eta T})/2\eta$. Every subsequent year, the process equals

$$c_{it} = \eta(c_{i,t-1} - \bar{c}). \tag{C.1}$$

We then generate an outcome variable as a function of c_{it}

$$y_{it} = u_i + c_{it} + \varepsilon_{it}\,, \tag{C.2}$$

where ε_{it} is a normally distributed error term. To show that c_{it} cannot generate spurious correlation between y_{it} and our main variables, we regress y_{it} on contract horizon and on the discontinuity dummies around the renewal:

$$y_{it} = u_i + \beta h_{it} + \varepsilon_{it}\,; \tag{C.3}$$

$$y_{it} = u_i + \beta^- D_j^{h=1} + \beta^+ D_{j+1}^{h=T_{j+1}-1} + \varepsilon_{it}\,. \tag{C.4}$$

Table C.1 reports the outcomes: Panel A for the continuous regression and Panel B for the discontinuity. We can see that the mean-reverting process does not create a spurious correlation between contract horizon and the outcome. The average estimated beta is 0.2481 and is not significantly different from zero. We obtain spurious significance in only 3.9% of all cases. The average p-value of the test of difference between the two sites of the discontinuity at renewals is 0.51 and with a probability of 1 greater than 5%. Here spurious significance is obtained in only 4% of all cases.

Table C.1 – Mean-reverting processes

Panel A: Contract horizon

y_{it}	Estimation	Average β	t ($\beta = 0$)	% ($p < 0.05$)
$= u_i + c_{it} + \varepsilon_{it}$	$y_{it} = u_i + \beta h_{it} + \varepsilon_{it}$	0.002735	0.2481	0.0393

Panel B: Discontinuity at renewal

y_{it}	Estimation	Average p ($\beta^+ = \beta^-$)	$p[p(\beta^+ = \beta^-) > 0.05]$	% ($p < 0.05$)
$= u_i + c_{it} + \varepsilon_{it}$	$y_{it} = u_i + \beta^- D_j^{h=1} + \beta^+ D_{j+1}^{h=T_{j+1}-1} + \varepsilon_{it}$	0.51219	0.00	0.0423

The simulations show that mean reversion cannot explain our results. Nonetheless, we make a guess at possible mean-reverting variables in order to establish that our results are robust to controlling for them.

Appendix D. Classification of Fama–French industries by R&D–patent filing lag

To classify industries, we use the Poisson model described in Hall et al. (1986) and run it for each Fama–French industry f:

$$Filings_{it} = \alpha^f + \sum_{j=0}^{3} \beta_j^f \log R\&D_{i,t-j} + \varphi^f \log \sum_{j=0}^{3} R\&D_{i,t-j} + \theta^f \log Assets_{i,t} + \mu_t^f + \varepsilon_{it}^f ; \qquad \text{(D.1)}$$

here, at time t, the $R\&D_{i,t-j}$ are the R&D expenses lagged by j years and the μ_t^f are year fixed effects. We classify an industry as a lag j type if β_j^f is the largest of the coefficients that are significantly different from zero at the 5% level. Although our classification is based on results derived from a Poisson model, similar results are obtained when using a negative binomial specification.

The various industries (with Fama–French numbers in parentheses) are thus classified as follows in terms of their lag between R&D expenditures and patent filings.

- **Lag = 0:** Apparel (10), Medical Equipment (12), Rubber & Plastic Products (15), Construction (18), Steel Works (19), Machinery (21), Electrical Equipment (22), Aircraft (24), Shipbuilding & Railroad Equipment (25), Defense (26), Precious Metals (27), Petroleum and Natural Gas (30), Hardware (35), Measuring and Control Equipment (38), Business Supplies (39), Shipping Containers (40)
- **Lag = 1:** Entertainment (7), Consumer Goods (9), Healthcare (11), Textiles (16), Telecommunication (32), Electronic Equipment (37)
- **Lag = 2:** Tobacco Products (5), Chemicals (14), Fabricated Products (20)
- **Lag = 3:** Agriculture (1), Food Products (2), Business Services (34)