CEO Contract Horizon and Innovation

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Abstract

We study the relation between innovation quality and managerial time horizon, measured by the time remaining until the end of fixed-term CEO employment contracts. Firms with longer CEO horizons produce more important innovation on average: one additional year in horizon is associated with 8% more patent citations. Long-horizon CEOs also increase R&D, design more exploratory innovation strategies, hire more inventors, and set longer-term incentives for researchers. We provide suggestive evidence that CEO contracts affect innovation decisions, using exogenous restrictions to contract length resulting from a governance reform. Our results support the view that long-term managerial capital is important for innovation.

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1. Introduction

This paper investigates the relation between the length of managerial time horizon and innovation quality. Understanding this relation is important in light of the widely held view that short-term managerial horizons are detrimental to long-term investment[1]—and given the crucial role of these investments in promoting economic growth and especially innovation (Solow, 1957).

Theoretical predictions concerning this relation, however, are mixed. On the one hand, longer time horizons and the lack of short-term pressure may induce less innovation effort (see e.g. Jensen and Meckling, 1976; Weisbach, 1988; Roe, 2013). On the other hand, longer horizons can improve innovation quality by mitigating "short-termism" (e.g., Stein, 1988, 1989), curtailing risk avoidance (e.g., Holmstrom, 1982; Manso, 2011), and enabling commitment to long-term plans that can attract and motivate employees (e.g., Van den Steen, 2005, 2016). Following the latter arguments, practitioners have long sought increased time horizons to insulate managers from short-term pressure (Hayes and Abernathy, 2007; Phelps, 2010). Ultimately, the relation between managerial time horizon and innovation quality is an empirical question.

We provide statistical evidence of a positive association between managerial time horizon and innovation quality as measured by, respectively, CEO contract horizon and patent citations. We discuss possible interpretations of this association, including the potential incentive effects of contracts discussed by regulators, as well as an alternative interpretation—namely, that boards of directors may deliberately time CEO horizons to coincide with innovation cycles. Although these explanations are probably not mutually exclusive, we provide suggestive evidence of incentive effects by exploiting exogenous restrictions to executive contract length after a governance reform. Our findings contribute to the growing body of research that underscores the importance to innovative firms of managerial incentives and especially of managerial time horizons (Aghion, Van Reenen, and Zingales, 2013; Budish, Roin, and Williams, 2015).

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[1] See, for example, BlackRock CEO Larry Fink's 2016 letter for CEOs, available at https://www.blackrock.com/corporate/en-no/literature/press-release/2016-larry-fink-ceo-letter.pdf; W. A. Galston and E. C. Kamarck, "More builders and fewer traders: A growth strategy for the American economy," available at www.brookings.edu/~/media/research/files/papers/2015/06/30-american-economy-growth-strategy-galston-kamarck/cepmglastonkarmarck4.pdf; Jacobs (1991); and Kay (2012).

We introduce *contract* horizon to the innovation literature. Fixed-term employment is the most direct way to set an employment time horizon and shield employees from short-term pressures. It is a common feature of working life and careers in research institutions (Bryson, 2004; Ackers and Oliver, 2007; Oliver and Hooley, 2010), where outcomes are so complex and/or difficult to verify that they cannot be linked to compensation incentives. Also common among executives, fixed-term contracts can prolong managerial time horizon by offering dismissal protection during a contract term that is set ex ante (Schwab and Thomas, 2006; Gillan and Nguyen, 2016; Cziraki and Groen-Xu, 2017). Contract horizon is easy to calculate—unlike previous measures of managerial time horizon based on compensation grants, which involve complex calculations and assumptions (Baranchuk, Kieschnick, and Moussawi, 2014; Gopalan, Milbourn, Song, and Thakor, 2014; Edmans, Fang, and Lewellen, 2017). For empirical research, contract horizon is especially convenient: it is set ex ante, declines predictably, and is reset in the case of a renewal. The last property produces discontinuous variation in incentives that does not coincide with most other potential innovation drivers.

The main challenge in establishing the association between CEO contract horizon and innovation quality is the availability of contract data. We overcome this challenge by using actual executive employment contracts filed with the US Securities and Exchange Commission (SEC) by firms that are publicly listed in the United States and filed at least one patent application with the US Patent Office (USTPO) during the period 1993–2008.

We first show that fixed-term contracts indeed protect CEOs from dismissal and thus set a managerial time horizon. From a legal perspective, contracts protect from premature dismissal without severance except for “good cause”. In line with this legal protection, CEO turnover rates are in fact significantly lower within the contract term (4%) than at its expiration (8%). Furthermore, CEO turnover is less related to performance before than at the end of a contract.

Our main contribution is to show that on average, the link between CEO contract horizon and patent citations is positive. We compare citations to the patents filed at different contract horizons for the same CEO-firm pair; in these regressions we control in several ways for aggregate trends in innovation, in particular by “scaling” citations and considering only citations within three (or five) years of the award. One additional year in contract horizon is associated with 8% more patent citations (relative to the mean). Moreover, consistent with theories of managerial myopia and career concerns and risk avoidance, this association is stronger in subsamples of CEOs who are likely more susceptible to these issues such as: those who are young and/or new to the firm. The association is robust: it holds for CEOs of different ages and tenures as well as across time, industries, states, and technologies, and for alternative measures of patent quality, such as the adjusted stock market reaction to the patent award (Kogan, Papanikolaou, Seru, and Stoffman, 2017).

We address a number of possible methodological concerns. The first is that the association we observe is instead driven by systematic lags between innovation decisions and patent applications; against this possibility, we show that results are strongest for industries in which we estimate (using the methodology of Hall, Griliches, and Hausman, 1986) *zero* lag between innovation decisions and patents. Second, the existence of omitted idiosyncratic trends that drive a spurious association would be inconsistent with our results showing a 36% increase (over the mean) in scaled citations after contract renewals. At contract renewals, contract horizon is reset, and incentives discontinuously change, whereas other innovation factors likely continue in the same trend. Third, the observed effects are more pronounced in better-governed firms, which makes it less likely that our results are driven by strategically timed patent filings by CEOs. Fourth, we establish that the association between CEO contract horizon and patent citations reflects neither omitted changes in compensation over the contract horizon (cf. Bereskin and Hsu, 2013) nor changes in the patenting environment over time or across states, industries, or technology classes (cf. Hall, Jaffe, and Trajtenberg, 2001; Lerner and Seru, 2016).

Next, we report differences in budgeting, personnel, and innovation direction across CEOs under long and short contract horizons; these differences highlight how long-horizon CEOs can change innovation. One additional year in CEO contract horizon is associated with 3% more annual R&D investments, a 9% greater fraction of patent filings involving a new inventor, a 10% greater fraction of patents in technologies new to the firm, a 7% greater variance in citations to patents, 3% more technological diversity (i.e., the Herfindahl index of the patents' technology classes), and an 0.4% increase in the breadth of innovation (measured as adjusted and scaled generality; Hall et al., 2001). Long-horizon CEOs also provide long-term incentives to research heads: one additional year in contract horizon is associated with 0.18 more years (an increase of 5%) in the length of new research head contracts. These results demonstrate the trade-offs in the innovation process: higher-quality innovation is associated with managerial practices—such as allocating valuable resources and adopting risky strategies—that can be costly for CEOs, especially in the short term.

Our results are consistent with theories of short-termism and CEO "myopia" (Stein, 1988, 1989), theories of risk avoidance and CEO career concerns (Holmstrom, 1982; Manso, 2011), and the argument that a firm's long-term plans require commitment (Van den Steen, 2005, 2016). Our findings also confirm the importance, for innovation, of managerial time horizon by documenting the positive association that exists (on average) between CEO contract horizon and innovation quality. This positive association could reflect a combination of two mechanisms. First, the dismissal protection provided by longer contract horizons can incentivize CEOs to direct more resources to innovation and to pursue more exploratory research strategies (the *incentive* hypothesis). At the same time, firms with predictable investment opportunities may also set CEO contract expiration dates to coincide with future declines in investment opportunities (the *timing* hypothesis); under this mechanism, the firm can time contracts so as to reinforce incentives when innovation opportunities are greatest.

Although these explanations are likely to coexist, a complementary test yields evidence suggestive of incentive effects. Distinguishing between these two hypotheses is challenging because there is hardly any exogenous variation in contract length. We therefore exploit a contract length reform—made in 2003 to the UK Corporate Governance Code—that restricted the length of executive contracts, for firms publicly listed in the United Kingdom, to one year. This reform exogenously decreased CEO contract lengths for a small but innovative group of firms in our sample that were cross-listed in the UK during 2003, but it had no direct effect on other publicly listed firms in the US. We use a difference-in-differences approach to show that, after 2003, contract length for the cross-listed firms decreased by an average of 2.4 years (54% over the sample mean) as compared to control firms, which were US-listed but not UK-listed. Innovation quality was also affected: a back-of-the-envelope calculation estimates a 7% decline in patent citations for a one-year *involuntary* reduction in CEO contract length.

The quasi-experiment affects cross-listed firms in which the potential incentive effects of contracts are most pronounced: the CEOs of these firms are subject to multiple sources of short-term pressure and dismissal concerns. In addition, the usual caveats for difference-in-differences estimation hold. In particular, it is possible that unobserved factors may have caused a differential shift in innovation at the time, even though our cross-listed and control firms exhibit similar trends in observables prior to 2003. With regard to external validity, a comparison between average cross-listed and non–cross-listed firms suggests that our findings hold especially for firms that are large, that have large patent portfolios of relatively low quality, and whose CEOs operate under contracts of longer duration than the industry average.

Our results contribute to the literature on the relation between corporate innovation and long-term CEO incentives. Most empirical work in this area links innovation or long-term investment to long-term equity compensation (Yanadori and Marler, 2006; Baranchuk et al., 2014; Chang, Fu, Low, and Zhang, 2015; Edmans et al., 2017), shareholder time horizon (Lerner, Sorensen, and Stromberg, 2011; Aghion et al., 2013; Tian and Wang, 2014; Asker, Farre-Mensa, and Ljungqvist, 2015; Bebchuk, Brav, and Jiang, 2015; Barrot, forthcoming), and employment laws (Acharya, Baghai, and Subramanian, 2014). We believe that our study is the first to explore long-term contracts as a measure of CEO long-term incentives. This measure is relevant because few private firms or non-corporates compensate managers or inventors with equity instruments; instead, fixed-term contracts predominate. The work closest to ours is that of Azoulay, Zivin, and Manso (2011), who show that long-term grant holders produce more important research. Relative to that paper, the novelty of our study lies in its focus on managers in public firms where time-related agency conflicts are most prominent: public investors expect continuous performance reports that generate short-term pressure.

2. Data

We combine information on patent filings and CEO employment contracts using the SEC–USPTO match published by Kogan et al. (2017). Our analysis is restricted to the 4,522 firms, publicly listed in the US that filed for at least one patent between 1993 and 2008 (inclusive). This approach yields a sample of 23,642 firm-year observations. We first describe the contract data and then the patent data.

2.1. CEO employment contract data

The SEC (Regulation S-K, Item 402) requires that companies disclose material employment contracts, or their details, with top executives. Contract filings are electronically available for all contracts entered after 1992; however, coverage is limited for the years 1993–1995.

We obtain copies of fixed-term CEO employment contracts thanks to this filing requirement. These contracts are comprehensive written agreements that specify employment terms—including the CEO's responsibilities, compensation, perquisites, termination conditions, and payments—as well as restrictions on outside activities. A typical fixed-term CEO contract has a fixed length of from one to five years and can be renewed, amended, or extended. The employment contract may, alternatively, cover only some aspects of the employment relation (e.g., nondisclosure agreements) and have no explicit termination date. In that case and in all cases with no written contract, the CEO is said to be employed "at will".

We use data from contract filings hand-collected by Cziraki and Groen-Xu (2017) for all firms publicly listed in the US during 1993–2008. Our information for fixed-term contracts includes the planned expiration date of the contract and, where available, the starting date of the employment agreement. We extract additional information on contract renewals and separation dates—as well as financial accounts and prices—from these standard respective sources: Boardex, Execucomp, Compustat, and the Center for Research in Security Prices (CRSP).

To illustrate the source of our contract data, Fig. 1 (Panel A) excerpts a contract between Advanced Micro Devices Inc. (AMD) and its CEO, Hector Ruiz, for 2002–2007. In this contract, the start year is 2002 and the length is five years: "You will be employed by AMD as its President and Chief Executive Officer commencing on April 26, 2002 [(the "Commencement Date")] […] This Agreement shall be effective upon the signing by both parties […], and shall expire five (5) years after the Commencement Date."

In Panel B of the figure, the last three column headings correspond to the main contract-based variables used in the analysis: start year, contract length, and contract horizon. At any time, contract horizon is defined as the number of years remaining before the contract ends.

2.2. Innovation data

We use information on R&D expenditures retrieved from Compustat to measure innovation *input*. To measure innovation *output*, we use data on patent filings. We retrieve these data from Kogan et al. (2017), who match public firms' patent records and return data from the USPTO and CRSP, respectively. Following standard practice in the literature, we time patents based on their application year (since it better reflects the time of the invention than does the grant year) and characterize patents in terms of their citation count. Information on the inventors of patents is from the Harvard Business School patent data (Lai, D'Amour, and Fleming, 2009).

We address the standard concerns related to patent data. These include heterogeneity in patenting and citation practices over time, technological classes, and regions as well as potential bias due to data truncation (since patents granted later in the sample have less time to be cited than patents granted earlier). The latter issue is magnified in our setting owing to the time-varying nature of managerial horizon data.

We take three standard and complementary approaches to addressing these concerns. First, we restrict the sample to citations made within three years of the grant date made by other firms (cf. Hall et al., 2001; Lerner et al., 2011). This approach renders data truncation less likely to bias the results (Hall et al., 2001; Lerner and Seru, 2016). In robustness checks, our findings hold also when we consider citations within five years of the award, which mitigates the concern that the first three years of citations represent only an irrelevant fraction of total citations.

Second, we look at citations relative to those awarded in the same technology class and year (or "cohort", as in Hall et al., 2001; Seru, 2014). Our main outcome variable, *cites*, is the ratio of (a) the number of citations per patent to (b) the average number of such citations within the focal patent's cohort. The aim of this scaling procedure is to control not only for data censoring and truncation problems but also for the shifts in citations engendered by changes in patent office policy or technological fluctuations (Lerner and Seru, 2016). Furthermore, we add multiple controls for these shifts in our multivariate analysis. We also check that our main results continue to hold for subsamples of patents in different technology classes, states, and years. In particular, we verify that our results continue to hold for the subsample of older patents filed through 2000, which gives us a better sense of how influential patents actually are.

Finally, we also use alternative quality metrics that are less likely to be affected by changes in the patenting environment: (adjusted) stock market reaction to the patent grant; and the likelihood of a patent being pledged as collateral. Information on market reactions comes from Kogan et al. (2017). Information about collateral pledges is from patent reassignment files at the USPTO (Mann, 2016).

3. CEO contracts in innovative firms

This section describes our sample of fixed-term CEO employment contracts in innovative firms, setting the stage for the main analysis (in Section 4) on contract horizon and innovation. Here we (i) describe the considerable heterogeneity in the incidence and length of fixed-term contracts over time, across industries, and across regions; (ii) provide evidence consistent with the notion that fixed-term contracts provide protection against dismissal; and (iii) analyze the determinants of fixed-term contracts and their length.

3.1. Distribution of CEO fixed-term employment contracts among innovative firms

CEO-firm pairs under a fixed-term contract account for 17% of our sample of innovative firms (Panel A in Table 1). This average incidence among innovative firms is comparable to Cziraki and Groen-Xu's (2017) estimate of a 23% for all firms publicly listed in the US.

Panel A also reveals a significant increase in the incidence of fixed-term contracts among innovative firms from 1993 until 2000. Although the limited coverage of contract data during the early 1990s might explain the lower incidence in the beginning of the sample, the growing popularity of CEO fixed-term employment may also reflect efforts to protect CEOs from increased shareholder pressure (Kaplan and Minton, 2012; Peters and Wagner, 2014). Yet the average fixed-term contract length decreases over time: from 3.88 years for contracts signed in 1993 to 2.75 years for 2008. This decline is consistent with the accelerating economic environment (e.g., more rapid information processing, high-frequency trading).

Panel B in Table 1 displays the distribution of contract length for new and renewal fixed-term contracts. In our sample of 603 new contracts, the most common lengths are three years (140 instances, or 23.22% of the sample) and two years (100 instances, 16.58%). One-year and five-year contracts follow with 80 instances (13.27%) and 68 instances (11.28%), respectively.[2] The distribution pattern for renewal contracts is similar. The average contract length is 3.56 years, and the median is 3.0 years.

Figure 2 plots the sample composition (in firm-years) across the 49 Fama–French industries (Panel A) and US states (Panel B). For ease of reference, the data are sorted in terms of decreasing frequency. Circles represent the fraction of firm-years in each industry or state out of all firm-years in the sample. As is typical for samples of patenting firms, the industries most represented are Measuring and Control Equipment, Pharmaceuticals, and Electronics while the most represented states are California, Massachusetts, and New York.

Figure 2 also shows similarity in the industry- and regional-level composition for the subsamples of innovative firms using fixed-term contracts and firms using long-term contracts (i.e., contracts longer than the sample average of 3.56 years). So not only are innovative firms better represented (than public firms) in biomedical and technological industries, innovative firms with fixed-term or long-term contracts are also better represented in those same industries. For example, in the full sample of innovative firms, firms in the Measuring and Control Equipment (resp. Trading) industry represent a similar fraction of observations as in the sample of companies with fixed-term contracts: 13.87% (resp. 0.06%) and 11.81% (resp. 0.16%).

Whereas the composition is similar across subsamples, contract incidence and contract length both exhibit substantial heterogeneity at the industry level (Fig. 3) and also at the state level (Fig. 4). For example, Panel A in Fig. 3 shows that roughly 50% of innovative companies in the Trading industry have fixed-term contracts—as compared with only 16% of innovative companies in Measuring and

[2] Details on contract length are not available for 212 contracts. For these contracts, we know the number of years left before expiration but not the initial date.

Control Equipment. These two industries also differ in terms of the average contract length: 3.81 and 3.50 years, respectively (see Panel B in Fig. 3).

It is unlikely that these industry and state differences in contracting are random. To the contrary, certain patterns in the data suggest that more and longer contracts are issued when frictions (e.g., CEO myopia, career concerns) are prominent. Because such frictions can make it harder for the firm to motivate innovation and to attract or retain CEOs, long-term contracts may be especially useful in these contexts. First, contracts are more common in the industries in which innovation efforts are difficult to monitor and evaluate (e.g., Entertainment, Recreation, and Pharmaceuticals); this finding is consistent with the longer vesting durations in such industries (Gopalan et al., 2014). Second, the industries with a long lag between innovation inputs (R&D) and patent outputs have longer-than-average fixed-term contracts; examples include Agriculture and Business Services.[3] Such lags complicate the measurement and hence the compensation of innovation effort. Third, fixed-term contracts are more prevalent and of longer length in riskier industries (e.g., Insurance, Banking; in line with Gillan, Hartzell, and Parrino, 2009). In these industries, managers may be more aware of career concerns related to downside risk and consequently more reluctant to undertake innovative projects. Fourth, in the environments where innovation is common, such as California, fixed-term contracts are less frequent. However, because (in part) California is the home state of companies pursuing risky innovations to which patents and profits typically accrue with long lags, its average fixed-term contract length exceeds the national average. The same holds true for Massachusetts, the home state of several companies in Electronic Equipment or Pharmaceuticals.

3.2. Contract horizon and turnover probability

Fixed-term contracts can set a contract horizon because they reduce the likelihood of dismissal: under the terms of most such contracts, CEOs cannot be dismissed without "good cause" (Schwab and Thomas, 2006). Good cause typically includes CEO felony and disability, but not poor performance or even incompetence (Gillan and Nguyen, 2016). Although firms retain the ability to terminate contracts

[3] We classify industries by the average lag between R&D and patents using the methodology of Hall et al. (1986); see Appendix A for details.

of, for example, *extremely* poor-performing CEOs, contract terms typically specify severance payments for early dismissal and compensation for non-vested equity foregone. Hence "premature turnover" (i.e., before contract expiration) is costly for the firm, and that cost is increasing in the contract horizon (Cziraki and Groen-Xu, 2017). Even though the cost of premature turnover may prove to be much lower than the benefits of hiring a new CEO, they can still be high enough to induce shareholder outrage.[4] At a minimum, these costs probably help employment contracts serve as a credible commitment to a given evaluation period.

Our evidence is consistent with a "turnover protection" role of fixed-term contracts. First, turnovers are more common in the expiration year than in the years before the contract expires (Table 2, Panel A). Average turnover is significantly higher at expiration than within the contract term (*t*-statistic ranging from 3.01 to 7.42, columns [1]–[5]), except for the last year before expiration ($t = 1.61$, column [6]). Whereas turnover rates at expiration average 8% (column [7]), average premature turnover rates range between 0% and 6% (columns [1]–[6]). This range translates into an annual premature turnover rate of 4% (column [9]), which is 3 to 4 times lower than the unconditional annual turnover rates of 12%–16% reported in the literature (Kaplan and Minton 2012; Jenter and Lewellen 2014). The relatively low premature turnover rates that we find are consistent with Holmes (2011) and Cziraki and Groen-Xu (2017), who show that turnover incidence decreases with contract horizon for, respectively, football coaches and the CEOs of all firms publicly listed in the US (not only innovative ones, as in our sample).

Second, the performance-turnover sensitivity appears to be higher at contract expiration (Panel B of the table). Before premature turnover, annual industry-adjusted returns average +0.7% and return on assets (ROA) averages −2.8%. In comparison, turnover at contract expiration follows significantly worse performance ($t = 2.90$ for stock returns; $t = 4.03$ for ROA), with an annual return of –1.7% and an ROA of −7.28%. The results in raw returns are qualitatively similar.

Third, only few premature turnovers in our sample seem to be performance related. Panel C of Table 2 shows that only 11 (out of 123) premature turnovers occurred explicitly because of performance

[4] See, for example, "Notebook – Reward for failure is alive and well: It's a boardroom culture that corroded Corus," *The Guardian* (15 March 2003).

issues and only 15 owing to external shareholder pressure. Most premature turnovers (78) were due to other reasons, including health and better outside positions. We retrieve the turnover reasons for the 123 premature turnovers in our sample using news announcements and SEC filings.

We also present evidence related to performance review at expiration—namely, evidence for explicit contract renegotiations *after* expiration. Statistics presented in Panel D of the figure are based on a subsample of 835 CEOs whose fates following contract expiration are observable through new contracts.[5] First, renewals are not automatic: only 84% (698) stay in the firm for over a year after contract expiration (within our sample period). Second, only a fraction of the surviving CEOs receive a follow-up fixed-term agreement: just 37% (307 of the 835) CEOs start a new contract with the same firm by 2008. Finally, the majority of these new contracts have different terms: only 15% (125 of 835) CEOs receive a follow-up contract of the same length.

3.3. Determinants of CEO fixed-term contracts and contract length

While fixed-term contracts provide CEOs with dismissal protection, long-term contracts also increase the firm's cost of renegotiation and dismissal. Yet their advantages to the CEO can benefit the firm if it can then more easily attract, retain, and incentivize CEOs, especially when retention rates are low and information asymmetry is high (Rotemberg and Saloner, 2000). At the same time, CEO candidates much prefer the security and stability of a long fixed-term contract—and even more so when the likelihood of being replaced is high and/or they have greater bargaining power (Schwab and Thomas, 2006). In this section, we compare the characteristics of firms and CEOs with different contract practices.

In line with prior research, Table 3 reports that employment contracts are more frequent in homogenous or riskier industries (cf. Gillan et al., 2009)[6] and with younger CEOs and CEOs with higher

[5] These CEOs survived until contract expiry and have contracts that expire by 2007. We impose the latter restriction because our sample of contracts ends by 2008, which means that we can observe subsequent contracts only for CEOs whose contracts expire by 2007. The number of CEOs that satisfy both of these restrictions is 835.

[6] Following Gillan et al. (2009), we calculate the *industry survival rate* as 1 minus the percentage of firms in the industry that were delisted during a focal year because of mergers and acquisitions (as identified by CRSP) and calculate *industry homogeneity* as the median, across all firms in an industry, of the percentage variation in monthly stock returns explained by an equally weighted industry.

salaries and incentive pay (measured as the ratio of the value of stock option grants to total pay over the same period). Employment contracts are also more frequent in companies with lower "all-round" governance (measured by the G-index or insider-dominated boards; see Chen, Cheng, Lo, and Wang, 2015) and in states with relatively low levels of protection for employees (as measured by Garmaise (2011) index of anti-takeover protection and enforcement of non-compete agreements).

Consistently with the theoretical importance of contractual protection when frictions (such as CEO career concerns) may obstruct innovation, employment contracts are more popular in environments where innovation is less certain. In particular, Table 3 shows that employment contracts are more prevalent and of longer length in industries with higher uncertainty over innovation outcomes (as measured by the industry-level dispersion of patent citations, or "industry citation variance"). This table also indicates that the prevalence of CEO fixed-term contracts is correlated to the ease with which innovation can be motivated or monitored. Thus fixed-term contracts are more popular in firms with tangible assets, high market-to-book ratios, high ratios of R&D investment to total assets, extensive patent stocks, and many citations to those patents.

However, Table 3 evidences no clear pattern of association between contract length and innovation inputs or outputs. That being said, cross-sectional comparisons of CEO-firm pairs with contracts of different lengths can mask substantial time-series heterogeneity. Indeed, for a given CEO-firm pair under a fixed-term contract, the effective contract horizon monotonically decreases over time. A comparison of firm innovation across CEO-firm pairs with long and short contract horizons (rather than with short and long contracts) reveals some of this heterogeneity. In contrast to the unclear patterns for contract *length*, the last column of Panel C in Table 3 shows a positive and strong association between contract *horizon* and firm-level cites. We next explore this correlation further in a multivariate setting by exploiting detailed data at the patent level and controlling for heterogeneity across CEO-firm pairs.

4. Contract horizon and innovation quality

In this section we describe our empirical strategy as well as our main results on the relationship between contract horizon and innovation.

4.1. Data structure

To explore the relation between contract horizon and patent cites, we restrict the sample to firm-year pairs under fixed-term contracts. For the contracts terminated prior to contractual expiration (27%), the sample includes only the firm-year observations prior to the early termination. Altogether, the resulting sample comprises 4,031 firm-year pairs and information on 100,370 patents filed with the USPTO by 1,262 firms (headed by 1,346 CEOs) during the 1993–2008 period.

We structure the data in "contract horizon event time". The main advantage of this structure is that it allows us to exploit within-contract variation in innovation and hence to control for heterogeneity across CEO-firm pairs. The sample structure is illustrated by Fig. 1 (in Section 2.1). We focus on the part of our data pertaining to the (single) contract between AMD and Ruiz. As Panel A shows, this contract started in 2002 and had a length of five years. In most of the analysis, observations are patents. Panel B describes a subset of the patents filed by AMD while Ruiz was CEO. For all patents in Fig. 1, the contract start year is 2002 and the contract length is 5.

The variation we exploit is the reduction in Ruiz's contract horizon from 2002 onwards—that is, as time progresses from the contract's start toward its expiration. For instance, contract horizon = 5 for Patent #6445174, which was filed during 2002 when five years still remained before the contract end; but for Patent #7183152, filed during 2004, contract horizon = 3 because at that time there remained only three years before the AMD–Ruiz contract expired.

Table 4 presents patent-level summary statistics for the main variables used in our empirical analysis. The average contract horizon is 2.47 and the average cites per patent are 1.16. The average CEO in our sample is 56.27 years old, has a tenure of 6.25 years, and works in a 12.74-year-old firm. The average contract's length is 4.49 years and was entered into by the CEO-firm pair in 1999.

4.2. Empirical strategy

We compare patent citations across innovations filed during different contract horizons for the same CEO-firm pair. We estimate the following regression:

$$y = \beta Horizon + Firm\ cross\ contract\ start\ year\ FE + \boldsymbol{X}'\theta + \varepsilon. \quad (1)$$

We include firm cross contract start year fixed effects, which control for heterogeneity in innovation and contract setting across different CEO-firm pairs. The vector of controls, $\boldsymbol{X}$, varies across specifications (see also Table 5 in Section 4.3). We account for serial correlation by clustering standard errors at the firm level. The variation in contract horizon that we estimate is of the within-contract type: changes in the horizon of a given firm's CEO as time passes and their contract approaches expiration.

A technical challenge encountered by this kind of empirical model is to distinguish contract horizon from time and other linear time-varying effects (e.g., CEO tenure). Yet including a set of time indicators to adjust for changing trends raises an identification problem; the reason is that within-contract variation in contract horizon is a deterministic function of calendar year (i.e., horizon equals expiration year minus calendar year). We overcome this problem by scaling patent citations (as described in Section 2.2), which absorbs aggregate calendar-year and technology effects. One other possible concern is that our scaling procedure does not fully absorb idiosyncratic firm- or CEO-level trends in innovation quality; in that event, we could be misled by a spurious relation between contract horizon and cites. In Section 4.4, we address this concern and present evidence that counters it.

4.3. Results

Table 5 reports ordinary least-squares (OLS) estimates of different specifications of Eq. (1). These results show the significant and positive relation between contract horizon and cites. The interpretation of the coefficient in column [1] of Panel A is that an extra year in the contract horizon of a company's CEO is associated with patents that receive 0.094 more cites within three years of being granted, which corresponds to an 8% increase over the sample mean of 1.16 (see Table 4). The difference in citations is relative to the patents filed by the same CEO later in the contract, when fewer years remain before expiration.

We expand Eq. (1) to explore in more detail the positive relation between contract horizon and innovation quality. In particular, we replace the continuous *horizon* variable with individual indicator variables for contract horizons between one and five years along with another indicator for contract horizons longer than five years. To avoid multicollinearity, we have no indicator for contract horizons shorter than one year. Figure 5 plots the coefficient estimates together with their 90th (thin vertical lines) and 50th (thick vertical lines) percentile confidence intervals. An increasing pattern between contract horizon and cites is clearly evident in the graph.

Table 5 (Panel A, columns [2]–[9]) shows that the positive relation between contract horizon and innovation quality is greater in subsamples where CEOs are likely to have more pronounced short-term orientation and career concerns. These subsamples include, most notably: low-tenure CEOs (columns [2] and [3], *p*-value difference of 0.783); younger CEOs (columns [4] and [5], *p*-value difference of 0.063); younger firms (columns [6] and [7], *p*-value difference of 0.567); and new CEOs (columns [8] and [9], *p*-value difference of 0.00). We split the samples by their medians. The only exception concerns tenure, for which we follow Pan et al. (2014) and adopt the "0–3 | >3" split.

The estimates in Table 5 (Panel A, columns [2]–[7]; Panel B, columns [1]–[2]) also establish that CEOs of different age and tenure, firms of different maturity, CEOs under both new and renewed contracts, as well as short or long contracts all exhibit a positive correlation between contract horizon and innovation quality. It follows that time-varying firm and CEO characteristics—the variables most closely related to contract horizon—cannot explain the average positive correlation between contract horizon and patent citations in the data. The positive estimate for high-tenure CEOs (in office more than two years) in column [3] suggests that we are not merely picking up the "below-3-tenure effect" reported by Pan, Wang, and Weisbach (2014). Similarly, the positive estimates for older CEOs and for contract renewals (in columns [5] and [8], resp.) suggest that we are not simply capturing a "new CEO" effect (or a "young CEO" effect; see Acemoglu, Akgicit, and Celik, 2014). Finally, the significantly positive estimates for both short (≤ 3.56 years, the sample average) and long contracts (> 3.56 years) in columns [1] and [2] of Panel B suggest the results are not driven by only the longer contacts in the sample.

Differences in the patenting and citing environment over time and across states, industries, and technology classes (Lerner and Seru, 2016) also do not appear to drive the positive association between CEO contract horizon and cites. Indeed, in Fig. 6, our results continue to hold when the sample is split into those filed before 2001 and after 2000 (see resp. the blue circles and red triangles in Panel A), across companies headquartered in different states (Panel B), across patents in different technology classes (Panel C), and across companies competing in different industries (Panel D). In each panel, we plot the coefficient estimates and the confidence intervals at the 50th and 90th percentile levels. The significant correlation holds for practically all subsamples. The only exceptions are the low-tenured CEOs (in the earlier sample) and the patents filed in Maryland, for which we have few observations. The effect is similar across industries except for a stronger effect in Machinery and a weaker effect in Consumer Goods (see Panel D). In addition, Column [3] in Panel B of Table 5 shows that differential trends across technology classes, captured here in start year × firm × technology class fixed effects, do not explain the correlation between contract horizon and innovation quality. Columns [4]–[6] in Panel B establish that the correlation holds also under different quality metrics that may be less affected by changes in the citing environment: scaled citations within five years of application (column [4]), the adjusted stock market reaction to the announcement of patent grants (column [5]), and a variable indicating if the patent was used as collateral (column [6]).

Furthermore, differences in compensation incentives across the contract do not appear to drive the positive association between contract horizon and cites. Indeed, in column [7] of Panel B (Table 5), 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, and the sensitivity of compensation to stock price increases of unvested equity (Baranchuk et al., 2014; calculated following Core and Guay, 2002); 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); clawback provisions (Babenko, Bennett, Bisjak, and Coles, 2012); and the value of vesting stocks and options (Edmans et al., 2017). To capture size effects, we control for the executive's level of compensation (log of TDC1). Information on vesting-related compensation measures comes from Equilar and hand-collection (see Edmans et al., 2016) and is available only for a subset (amounting to 9.1%) of our sample. Our regressions set each missing vesting-related compensation feature to zero and—to reduce selection bias—include a dummy indicating where this recoding of the variable was made.

Also consistent with the notion that differences in compensation incentives across the contract do not appear to drive the results, Fig. 7 shows these compensation measures do not seem to correlate at all with contract horizon. In the figure, we plot the results from estimating Eq. (1) with compensation measures as dependent variables; there is no significant relationship evident between compensation and horizon. The only significant coefficients are those for duration (with a five-year horizon) and for maximum years to vesting (with a horizon exceeding three years). These coefficients are negative, implying that the variables are more likely to offset than to explain the effect of a long contract horizon. Intuitively, the difference between contract horizon and "compensation horizon" arises because the former is set ex ante and shortens monotonically over time. In contrast, firms regularly "replenish" the compensation horizon to keep it stable over time (Carter and Lynch, 2001).

4.4. Robustness tests

This section addresses three potential methodological concerns and presents suggestive evidence against them. Although we cannot rule them out entirely (because econometricians have only partial information), the preponderance of evidence indicates that these concerns are not first-order.

4.4.1. Idiosyncratic CEO-firm–level trends. A spurious relation between contract horizon and cites can arise if the latter's scaling procedure does not fully absorb idiosyncratic CEO-firm–level trends in innovation quality, such as age or experience effects. The most stringent test for this concern is a regression in which contract renewals distinguish the horizon effect from a trend. If a CEO-firm–specific trend fully explains our results, then an "off-trend" change in contract horizon (e.g., a renewal) should *not* lead to an off-trend change in innovation.

We implement this test in a "multiple event time study" framework that uses variation in the timing of contract renewals across CEO-firm pairs. We estimate the following regression:

$$y = \alpha + \sum_j \beta_j Event_{t+j} + CEO\text{-}Firm\ FE + Contract\ start\ FE + Tenure\ FE + \varepsilon, \qquad (2)$$

where $Event_{t+j}$ is an indicator variable for the jth year around a contract renewal in year t. This framework is similar to a traditional event-study analysis, except that each patent observation can simultaneously relate to several renewals of the same CEO-firm pair. A single patent can therefore account for several observations in this sample, in each case relating to a different renewal event (as is typical for the daily returns included in return event studies). We may illustrate using a CEO-firm pair with a first renewal in 2000 and a second renewal in 2002. For this pair, year 2001 was not only the first year after the first renewal ($t + 1$) but also the last year before the next renewal ($t - 1$). We follow convention in assuming that the effects are the same for each of the renewal events (i.e., the first, second, and subsequent ones).[7] The series of coefficients β_j are the main estimates of interest. They are normalized relative to the year before contract renewal (β_{-1}), which is excluded from the regression. For $j > 0$, the term β_j is an estimate of the change in (scaled) citations j years *after* the contract renewal—for the 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 before or may have afterwards). The variation used to estimate the values of β_j is the within–CEO-firm pair changes in contract horizon around contract renewals. To control for aggregate trends in contracts and innovation and in CEO tenure, the regressions include (respectively) contract start year fixed effects and CEO tenure fixed effects (see Pan et al., 2014). In some specifications we include contract start year × technology class fixed effects to control for potential technology-specific trends. Standard errors are clustered at the CEO-firm level to adjust for heteroskedasticity and for within–CEO-firm pair correlation over time.

Trends specific to CEO-firm pairs seem not to drive the observed positive association between contract horizon and cites. Fig. 8 report evidence of a discontinuous increase in cites after contract renewal based on the regressions estimates in Table 6. Table 6 summarizes estimates of Eq. (2) for the period spanning eight years before the contract renewal date to six years after that date; for no (resp. one) renewal contract do we observe more than six (eight) years after (before) the renewal. The table reports β_j coefficients for the window consisting of four years before and three years after contract renewal as well as for (a) the entire sample (columns [1] and [4]) and (b) the subsample of patents filed by CEO-firm pairs with at least one contract renewal (columns [2], [3], [5], and [6]). We interpret the estimates in column [2] as follows. Relative to the year before the renewal, cites increase by 0.420 in the renewal year, which corresponds to a 36% increase over the sample mean (1.16; see Table 4). One year after the renewal, quality remains higher than the year before the renewal: patents receive an extra

[7] See Sandler and Sandler (2014) for further detail on this procedure. The authors use Monte Carlo simulations to show that other ways of dealing with multiple events (e.g., ignoring subsequent events, duplicating observations so that there is at least one observation per discrete event time) can themselves create trends in the outcome variable—before and after an event—and thereby lead to biased estimates.

0.312 cites, (i.e., 27% above the sample mean). Results are quantitatively similar if we control for technology trends (column [3]). The estimates in columns [4]–[6] exhibit a similar pattern for innovation quality as measured by the adjusted stock market reaction to the patent award.

Results in Panel A of Table 5 (see also Panel A of Fig. 6) are also against the concern that CEO-firm pair trends, specifically CEO experience, drive the association between contract horizon and cites. The results in the table confirm a stronger association between CEO contract length and innovation quality for less experienced CEOs (column [2]) and new CEOs (column [9]). Instead, if a CEO experience effect did in fact explain our results, then we would expect a positive association between CEO *experience* and innovation quality. Also against this concern, Pan et al. (2014) offer evidence against linearity in the investment–CEO tenure relation.

Finally, as complementary evidence we show in simulations that mean-reverting CEO-firm pair omitted factors are also unlikely to create our results. We reject the absence of a spurious relationship caused by the mean-reverting process with normally distributed shocks in only 3% of simulations (see Appendix B). We focus on mean reverting processes as any residual omitted variables that might explain our results must co-move with contract horizon—that is, change gradually as contracts approach expiration and then discontinuously *reverse* when the contract is renewed. Mean-reverting omitted variables (e.g., in technological opportunities) that overlap with the contract horizon are one such type of variable. However, asymmetry between the gradual decline in innovation quality as the contract period transpires and the sharp increase at renewals is difficult to reconcile with mean-reverting processes, which should rather exhibit either a sharper decline before renewal or a more gradual increase after renewal. This latter property of mean reverting processes helps explain the simulations' results.

4.4.2. Lags between innovation decisions and patent applications. Another concern is that systematic lags between innovation decisions and patent applications can result in a spurious relation between contract horizon and citations. Such lags would lead to an erroneous attribution of current innovation outcomes to the choices of an incumbent CEO—that is, despite their true origin with prior managers (or with the same CEO but in earlier years, possibly before the contract began). In that case, the findings would be more consistent with theories that predict CEO entrenchment under longer contracts. They might also reflect potential disciplinary effects (Weisbach, 1988; Morck, Shleifer, and Vishny, 1989; Denis, Denis, and Sarin, 1997; Mikkelson and Partch, 1997; Hartzell, 1998; Bertrand and Mullainathan, 2003) or risk-taking effects (Bebchuk and Stole, 1993) of impending contract expiration rather than a positive association between contract horizon and innovation.

Against this alternative interpretation, Panel A in Table 7 shows that our results are strongest for the industries with *no* lag between innovation decisions and patents. (To classify industries by the average lag between R&D and patents—as described in Appendix A—we use the methodology of Hall et al., 1986.) Consistent with the existence of measurement inaccuracies, the association between contract horizon and cites weakens as the lag between expenditures and filings increases across industries. Indeed, for the industries characterized by lags of two or more years (columns [3]-[4]), the estimates of the contract horizon dummies are often negative and seldom significant.

4.4.3. Strategic patent filings by CEOs. The third concern is that CEOs may strategically *time* their patent filings, leading to a spurious relation between CEO contract horizon and cites. For example, CEOs may time filings to maximize their bargaining power in contract renegotiations (cf. the similar cases of rigging discussed by Baker, 1992; Morse, Nanda, and Seru, 2011). Such behavior can be costly given the legal incentives to claim priority by filing patents immediately after even incremental steps in the development process (Branstetter, 2006). Contra the strategic timing of patents for private benefits, Panel B in Table 7 shows that our results are actually stronger in better-governed firms. In agreement with the notion that powerful CEOs are less concerned about their careers, citations of the patents shepherded by CEOs with more power—as indicated by an insider-dominated board, a Chairman-CEO position, or a higher G-index—are less related to contract horizon: the coefficient for the interaction term between the contract horizon and the respective governance measures is negative (though not significantly so for Chairman-CEOs). Note that, in line with previous research (e.g., Kang, Liu, Low, and Zhang, 2014) showing how CEO-connected board members protect managers from market pressure, we find a positive relation between innovation and insider-dominated boards.

5. Management of innovation

5.1. Managerial practices and innovation

Taken together, the results presented so far imply that CEO contract horizon is positively associated with innovation quality. However, CEOs of public firms are hierarchically distant from the inventors who actually implement innovation (Burgelman, 1994; Lerner and Wulf, 2007; Chang et al., 2015). How can the CEO's contract horizon be related to innovation outcomes at all? Here we discuss this question and present evidence of practices—among CEOs with longer horizons—that can affect innovation.

Theory and also practitioner surveys (e.g., Barsh, Capozzi, and Davidson, 2008) suggest three broad avenues by which CEOs affect innovation. First, they can allocate more resources—such as funds and personnel—to innovation projects (Noda and Bower; 1996; Bertrand and Schoar, 2003; Bennedsen, Nielsen, Perez-Gonzalez, and Wolfenzon, 2007; Bloom, Sadun, and Van Reenen, 2016). Second, CEOs can direct their firm's strategy toward innovation efforts (Chandler 1962; Van den Steen, 2016). Finally, they can set incentive contracts for other managers, including R&D heads, that have been shown to facilitate innovation (Yanadori and Marler 2006; Lerner and Wulf, 2007). We next explore these channels in turn.

5.2. Evidence of managerial practices

To investigate the practices followed by long-horizon CEOs associated with higher-quality innovation, we estimate Eq. (1) using different firm-year and patent-level indicators of innovation. Table 8 reports the results.

In accord with the idea that CEOs can affect innovation by allocating resources strategically, column [1] in Panel A of the table shows that long-horizon CEOs allocate more capital resources to long-term innovation: one additional year in contract horizon is associated with 3% more annual R&D investments (normalized by the size of book assets) relative to the sample mean (0.07; see Table 4). These greater resources are more likely to reflect a change in strategy than a simple expansion in innovative scale: there is no significant association between contract horizon and *quantity* of patent filings (column [2]). Furthermore, CEOs with a longer horizon can evidently attract (or motivate) more inventors to patent for the first time in the focal firm (column [3]): relative to the sample average (0.65; see Table 4), an additional year in contract horizon results in a 9% increase of the fraction of the patents filed by new inventors. So apparently, long-horizon CEOs also allocate more *human* capital resources to innovation (than do other CEOs).

Supporting the notion that a CEO can alter the firm's innovation strategy, Table 8 (Panel A) reports a positive association between longer contract horizons and explorative innovation. Relative to the sample means, an additional year of contract horizon translates into patents with 7% greater citation variance (column [4], sample mean of 6.11; see Table 4) and 3% greater technological diversity (column [5]—as measured by the Herfindahl index of the patents' technology classes, which averages 0.58 for the sample; see Table 4). An additional year also corresponds to a 10% increase of the fraction of patents in technology classes new to the firm (column [6], sample mean of 0.05; see Table 4). Moreover, column [7] shows that the positive effect of CEO horizon on influential innovation is greatest for patents in technological areas new to the firm. The "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 horizon. Finally, column [8] shows that an additional year in contract horizon is associated with 0.4% more general patents (relative to the sample mean of 1.02; see Table 4) as measured by the *generality* of patents (i.e., dispersion across technology classes of forward citations, which we scale relative to other patents in the same cohort and technology class and then adjust following the procedure outlined by Hall et al., 2001).

In contrast, the values reported in column [9] of Panel A indicate that differences in project pipeline across long and short horizons do not reflect complete overhauls to the nature of the company's technology. We find no differences in the *originality* of patents (i.e., dispersion across technology classes of backward citations; Jaffe and Trajtenberg, 2002). This finding is consistent with the idea that managers influence the strategy and resources devoted to innovation rather than the scientific input.

Finally, in agreement with the idea that CEOs can set incentive contracts for other managers, Panel B of Table 8 shows that contracts of Chief Research Officers (CROs) and Chief Financial Officers (CFOs) are longer when their contract is set under a long-horizon CEO. These C-level officers can be crucial for innovation: CROs execute research strategies and CFOs execute resource allocation (Lerner and Wulf, 2007). We use a novel sample consisting of 157 CROs (including Chief Research, Scientific, and Medical Officers), 561 CFOs, and 123 Chairmen for whom firms disclose material employment contracts. We then compare executive contract length across contracts set under CEOs with different horizons. To absorb any effects of contract length coordination across executives by the board, our regressions control for CEO contract length and contract start year. The results suggest that an additional year in CEO contract horizon is associated with 0.18 additional years in CROs' contracts (column [1]), which corresponds to a 5% increase relative to the sample mean (3.34; see the bottom of that column). Similarly, CFO contracts are associated with 0.17 more years (5% increase relative to the sample mean of 3.52) for each additional year in the CEO contract horizon. The coincidence in contract terms for CEOs and CROs does not seem to reflect a company-wide initiative to elongate employment contracts: column [9] shows that no such association emerges with Chairmen contracts that are under the control of the board and *not* of the CEO.

In sum, our results suggest that firms with a longer CEO horizon allocate more resources to R&D, are more active in exploratory innovation, and set longer-term incentives for research managers. These findings point to pathways CEOs could follow to achieve better innovation outcomes, but they also highlight the costs and trade-offs of the innovation process. Resources allocated to innovation are costly, exploratory research in new technologies can be risky, and long-term contracts to middle managers makes it more difficult for the firm to replace them.

6. Interpretation of results

The results reported so far show a significant association between CEO contract horizon and innovation quality (Tables 5–7) that coincides with specific managerial practices such as allocation of resources to innovation (Table 8).

One interpretation of these results, which is consistent with our theoretical motivation for a positive association between managerial time horizon and innovation, is that the dismissal protection provided by longer contract horizons incentivizes CEOs to engage in costly yet innovation-enhancing managerial practices (the *incentive* hypothesis). For example, longer contract horizons can help to align managers' and shareholders' respective time horizons (Jensen and Meckling, 1976; Holmstrom, 1982;

Narayanan, 1985; Stein, 1988, 1989). They also can provide the tolerance for early failure—and the reward for long-term success—that motivate CEOs to commit to uncertain research strategies (Manso, 2011).

Another possible explanation is that boards set CEO contracts so that expiration dates coincide with declines in investment opportunities (the *timing* hypothesis). In this way, innovating firms can set stronger incentives to coincide with greater investment opportunities and thereby better sort and retain CEOs. Personnel continuity may be needed when projects involve complex sequential tasks, as is typical for innovation (Rotemberg and Saloner, 2000; Van den Steen, 2016). This explanation is also consistent with theories of CEO myopia, career concerns, and commitment, if boards set long-term contracts to protect innovation incentives when innovation opportunities are strong. Note, however, that the timing hypothesis requires that firms be able to predict the rise and fall of investment opportunities several years in advance.

The two explanations are likely to coexist and might even reinforce each other: firms may wish to time contracts precisely to set better incentives. In equilibrium and absent exogenous restrictions on contract length, firms are likely to use the contract length best suited for their needs. Our results in Section 2 are consistent with this notion: firms with more apparent frictions in monitoring and motivating innovation are more likely to use long-term contracts.

Notwithstanding the likely coexistence of these explanations, in this section we describe a complementary test for the incentive hypothesis using a subsample of firms subject to an exogenous restriction on the length of their CEOs' contracts. In particular, we exploit a 2003 reform in the UK Corporate Governance Code that restricted the length of executive contracts to one year for firms publicly listed in the United Kingdom. This reform is useful because it affected all US firms in our sample that were cross-listed in the United Kingdom during 2003 but did *not* directly affect any other firms listed publicly in the US. After giving some background information on the reform, we describe compliance and innovation outcomes for the subsample of firms cross-listed in the United Kingdom (relative to the control group of US firms *not* so cross-listed). We then discuss the magnitudes of our estimates and the external validity of our findings.

6.1. The 2003 contract length reform of the UK Corporate Governance Code

Discussions on UK reforms to director contract length date to the mid-1990s. These discussions center on severance pay. Executive severance packages were deemed "pay for failure" and an especially egregious instance of executive–blue-collar inequality. Because the amount of severance pay is a multiple of contract duration, proponents of reforming contract length argued that shorter contracts would reduce severance pay. The business press commented on shareholder protests against long contracts as follows: "Investors dislike contracts that last longer than 12 months because they increase the size of any payoff when executives are axed." As more dramatically (though less succinctly) put by another journalist, "two-year-contracts […] are now regarded as unacceptable by shareholders and government alike for the simple reason that they are route one to the nauseating rewards for failure that we have seen rather too many of in recent months."[8]

The justifications for long executive contracts that are given by firms focus on risk-taking incentives and the downside of failing to attract top talent. These concerns were commonly dismissed by shareholders and government alike. The tone of this debate is exemplified by the following text from an article in *The Guardian* on the 2003 reform. Innovation is important to the writer but not connected to the mentioned incentives on contract length:

> Those managers who often view their vice as virtue claim that without such incentives [long contracts] entrepreneurs would not take risks. But the only risks being taken are with share prices, which fund our pensions. The notion that globalization sees Wall St salaries in the Square Mile is also bogus. It should be innovation and success – difficult quantities to measure – that merit pay rises, not the fact that a company's interest span borders. Even then riches should be shared.[9]

Before 2003, the UK Corporate Governance Code (formerly known as the "Combined Code"; here, simply the Code) had already set a *target* contract duration of one year. However, this provision was not mandatory. Provision B.1.7 of the pre-2003 Code reads as follows: "There is a strong case for setting notice or contract periods at, or reducing them to, one year or less. Boards should set this as an objective; but they should recognize that it may not be possible to achieve it immediately" (Financial Reporting Council, 1998).

[8] "Chairman may capitulate over contract," *The Guardian* (23 November 2003); "Fear of failure drives Tesco's two-year deals," *The Guardian* (7 June 2003).

[9] "Wages of failure – The City just voted against greed," *The Guardian* (21 May 2003).

In 2003, following public pressure from a number of high-profile cases of CEO turnovers involving large severance packages,[10] the UK government removed the flexibility of Provision B.1.7 and adopted a more stringent regulation. The Code's new Provision D.1.5, which has been in effect since 2003, reads: "Notice or contract periods should be set at one year or less" (Financial Reporting Council, 2016).

Compliance with the reform appeared to be prompt and nearly universal. A compliance report commissioned by the government shows that 94.9% of FTSE350-listed companies complied with the one-year-contract provision in 2004, with that percentage rising to 96.6% in 2005 (Grant Thornton, 2005). Among FTSE100 companies, compliance levels were at 97% and 98% for (respectively) 2004 and 2005. In contrast, before the reform in 2002, 22% of FTSE100 and 16% of FTSE250 firms had executive contracts longer than two years (Deloitte, 2004).

It is worth mentioning that noncompliance need not lead to legal sanctions, since the Code operates on the "comply or explain" principle. Unsatisfactory explanations or wrongful reporting do lead to formal sanctions—in particular, banishment from Premium Listing at the London Stock Exchange (Conyon, 1994; Financial Conduct Authority, 2016)—and also to informal sanctions, such as failing of the advisory "say on pay" votes (Ferri and Maber, 2013).

6.2. Contract length in the subsample of cross-listed firms

The 2003 reform is useful for isolating the potential incentive effect of contracts because it applied to a small group of US firms in our sample that were also listed in the United Kingdom but did not directly affect other US firms in the sample. Panel A in Table 9 compares summary statistics on the sample's 30 cross-listed UK firms, and other US-listed firms, for the eight years around the reform (2000–2008). The cross-listed firms constitute the sample for which contract length is perhaps most relevant: as compared with the industry average, these firms are less innovative in terms of citation count (1.87 per patent, versus 2.38 for patents by other US firms) and have longer contracts (4.41, versus 4.14 for other US firms). Although small in number, our cross-listed firms produced 21% of the total patent output by

[10] See "Financial fat cats or tigers," *Financial Times* (28 January 1995); "The fat cats keep getting fatter," *Financial Times* (1 August 1998); ABI-NAPF (2002); Trade and Industry Committee (2003).

public firms in the US during the eight years surrounding the reform. Figure 9 shows the industry distribution of this subsample (Panel A) and of all US patents (Panel B) for the relevant years. Comparing the two panels reveals that a larger-than-average proportion of the cross-listed firms are in industries, such as Aircraft and Pharma, where long-term contracts are the norm.

To estimate the reform's effect on contract length, we use a difference-in-differences approach that compares average changes in contract length made after the reform by cross-listed firms and by other US firms in our sample. For each firm-year between 2000 and 2008, we link contract length to the cross-listed status and its interaction with a dummy for the post-reform period. We include company and year fixed effects in the estimation to capture the potential effect of the regulation, (i.e., rather than firm heterogeneity or time trends); standard errors are clustered at the firm level. In line with the compliance reports, Panel B of Table 9 shows that relative contract length decreased significantly for cross-listed firms after 2003. The average relative decrease amounted to 2.4 years in contract length as compared with US-listed firms (when we control also for CEO tenure), which corresponds to a 54% decrease from the sample mean (4.49; see Table 4).

6.3. Innovation in cross-listed firms

Next, we link the reform to innovation quality using the same difference-in-differences approach. Much as in the regression described by Eq. (1), this test links citations to cross-listing status and the reform for each patent issued between 2000 and 2008. We include fixed effects for year, technology, firm, and tenure in order to capture other contemporaneous effects; standard errors are clustered at the firm level. We use "raw" citations (within three years of grant and excluding self-citations) as our dependent variable, rather than cites as in the analysis of Section 4, because the year and technology fixed effects absorb any aggregate time and technology variation in citations.

Our evidence suggests that, after the reform, raw citations decrease on average by 0.74 (column [3]) for patents in cross-listed firms. This decrease is relative to the control group of patents in firms publicly listed in the United States. The decline is economically significant: it corresponds to 26% of the pre-2003 average annual raw citations to patents by the cross-listed firms (2.83) and 17% of the unconditional mean (4.16; see Table 4). On a per-contract-year basis (divided by the 2.4-year decrease in contract length), these citations translate into an effect of 11% of the cross-listed firms' sample mean and 7% of the unconditional mean. The results persist irrespective of controlling for CEO age fixed effects (column [2]). In Panel B of Table 9, column [4] reports similar results from regressing raw citations directly on contract length. Prior to 2003, in contrast, the cross-listed and control firms exhibited similar trends (see Fig. 10).

6.4. Discussion

Taken at face value, the relative decline in innovation quality after the UK reform is consistent with the incentive hypothesis. If one assumes that innovation opportunities for both cross-listed and control firms would have evolved similarly in the absence of such a reform, then a back-of-the-envelope calculation (based on the results in Table 9; see Section 6.3) estimates a 7% decrease in average innovation quality for every one-year decrease in CEO contract length (as compared with the unconditional sample mean).

However, this evidence is not without limitations: the usual caveats for difference-in-differences estimation hold. Thus, we cannot rule out that factors we do not observe as econometricians caused a differential shift in contract length and innovation—that is, even though no differential trends with respect to observables between cross-listed and control firms are visible prior to 2003 (Fig. 10, Panel A).

To assess the external validity of these findings, we compare the average cross-listed and non–cross-listed firms in our sample (see Panel A in Table 9). This comparison reveals that our findings are likely most applicable to large firms with large patent portfolios (but low citation counts) and operating under contracts of length longer than the industry average.

7. Conclusions

Different theories make opposing predictions about the relationship between contract horizon and innovation. In this study we find that relationship to be, on average, positive. In short: one additional year of CEO contract horizon is associated with innovation of 8% higher quality (relative to the sample mean). Our results support the growing body of evidence on the importance, to innovative firms, of managerial capital and long-term incentives.

More specifically, this study is the first to explore the role of contract horizon in innovation. Most research on long-term incentives focuses on the structure of equity compensation in public firms, but equity incentives are not applicable in the nonprofit sector or to research institutions not listed on any stock exchange. Hence one advantage of using contract horizon to set long-term innovation incentives is that this approach can be applied also in government and university settings.

We describe several managerial practices associated with higher-quality innovation. Chief Executive Officers with relatively longer contract horizons budget more resources to R&D, design more exploratory research strategies, and motivate more inventors to patent in their firm for the first time. In support of the notion that managers can influence the productivity of other inputs, we find that longer-horizon CEOs give their Chief Research Officers longer contracts. These results contribute to the literature showing that C-level officers, despite their hierarchical distance from inventors, do matter for innovation.

A subsample of firms affected by a UK reform that limited the length of executive contracts to one year provides evidence that supports incentive effects of contracts. Innovation quality is estimated to fall by 7% (relative to the sample mean) for each year of contract length reduced after the reform. These results underscore the possibility of negative consequences for innovation due to policies restricting CEO contract length.

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Fig. 1. Example of the main sample variables

Panel A. 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").

[…]

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.

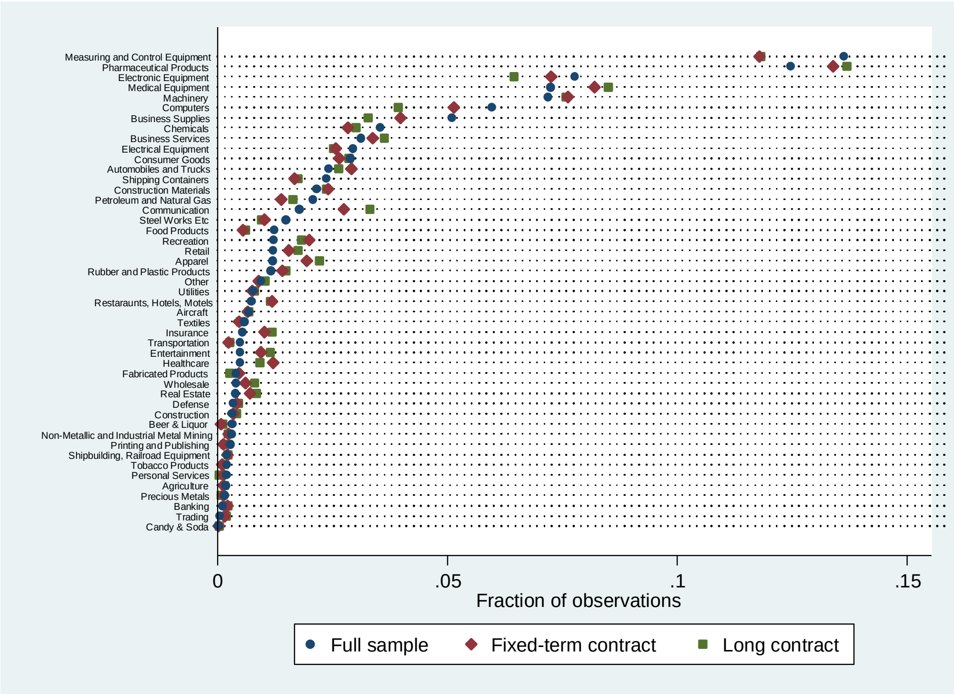
Panel B. Corresponding excerpt from the data set

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

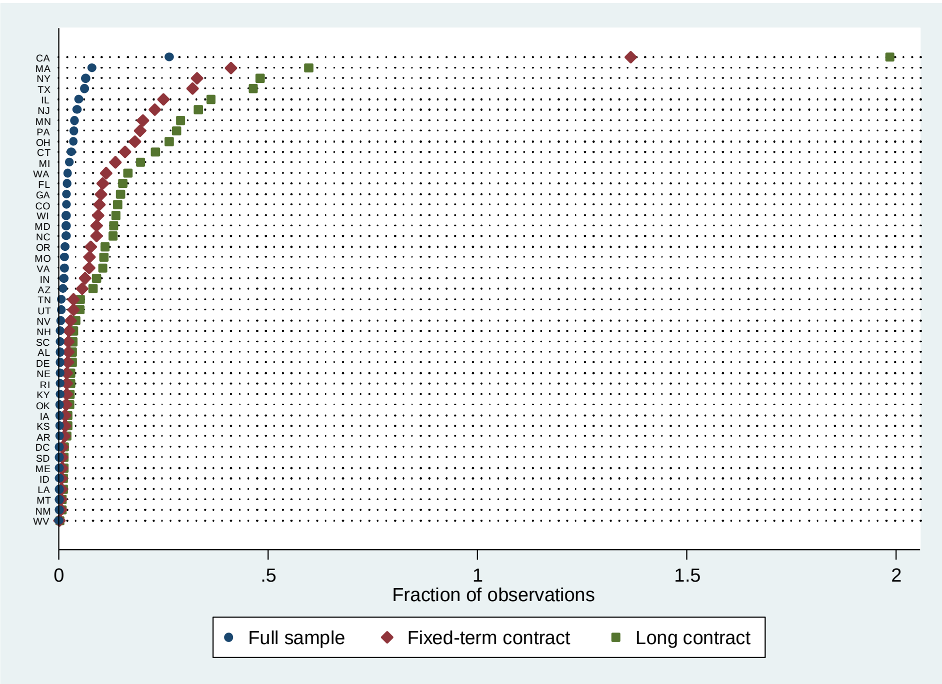
This figure shows an example of our main sample variables. Panel A shows an excerpt of the employment agreement between Hector Ruiz and Advanced Micro Devices, Inc. Panel B illustrates, for a subsample of patents with application year between 2002 and 2006 and given cites, how we calculate contract length, contract horizon, and contract age. Contract length is the length of the contract valid at the time of application (five, as shown in Panel A). Contract horizon is the number of years between application year and expiration of the contract. Contract age is the difference between application year and contract start year.

Fig. 2. Sample composition

Panel A. Industry



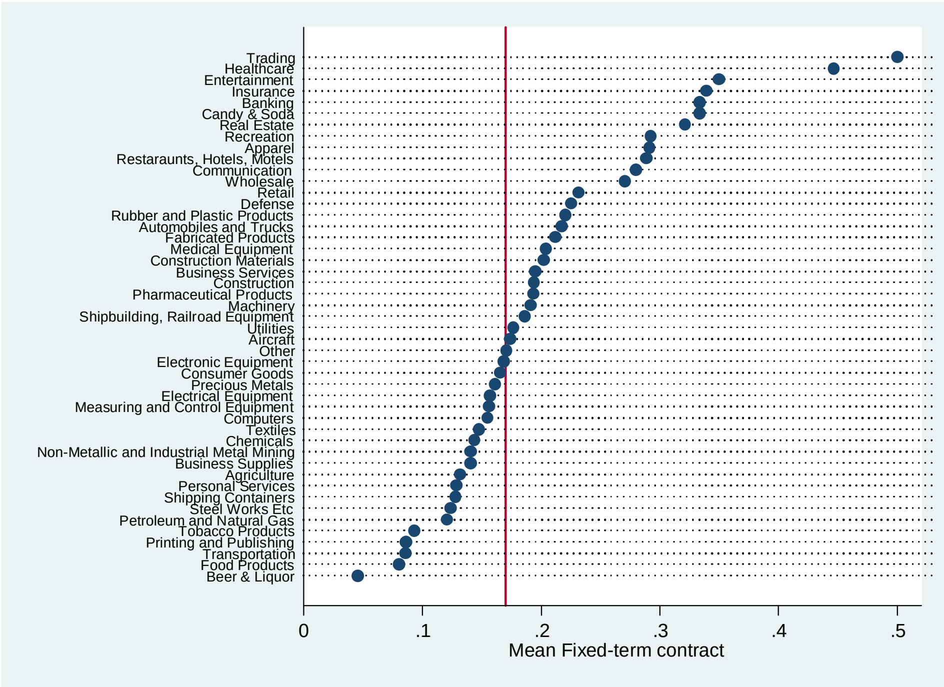
Panel B. State



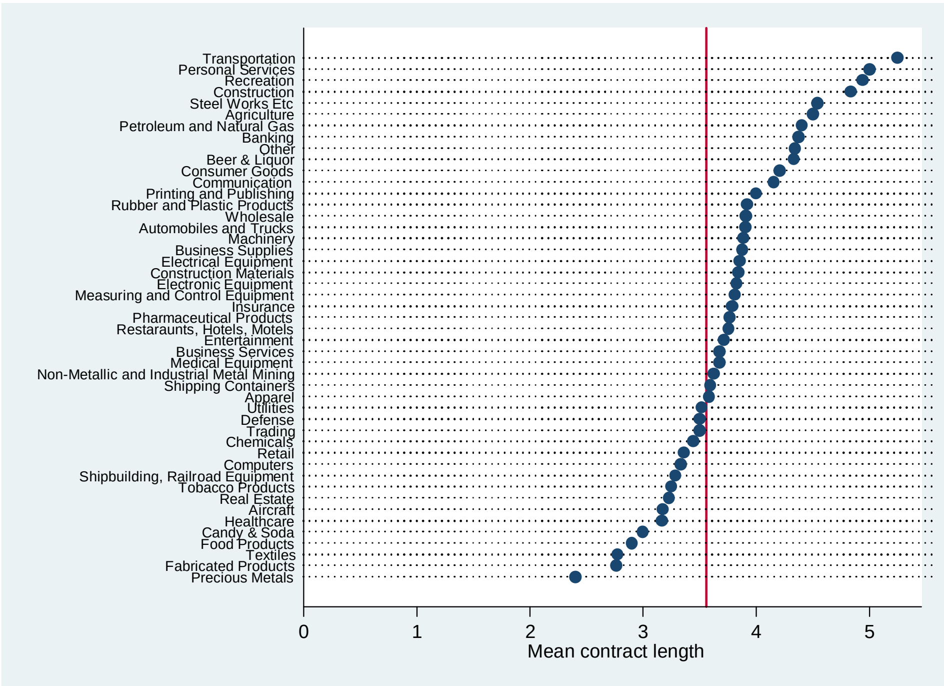
This figure plots the fraction of firm-year observations across industries (Panel A) and states (Panel B) for different samples: full sample (blue dots), firm-years under fixed-term contracts (red diamonds), and firm-year under long contracts (green squares). Long contracts are those longer than the sample mean length of 3.56. Panel A uses the Fama-French 49 industry classification.

Fig. 3. Frequency and length of fixed-term contracts by Fama–French industries

Panel A. Frequency of fixed-term contracts



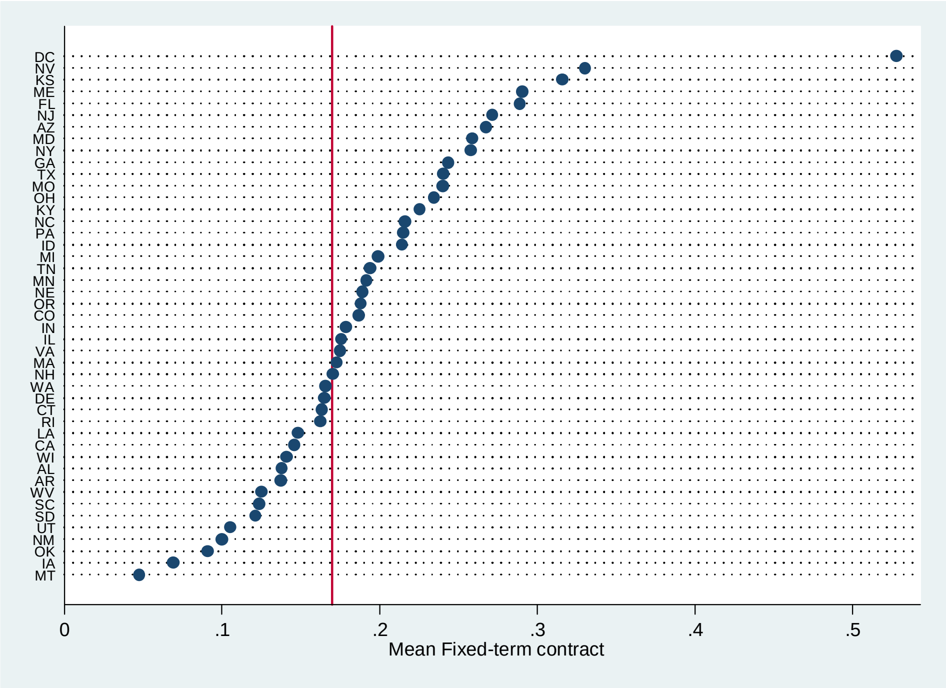
Panel B. Contract length



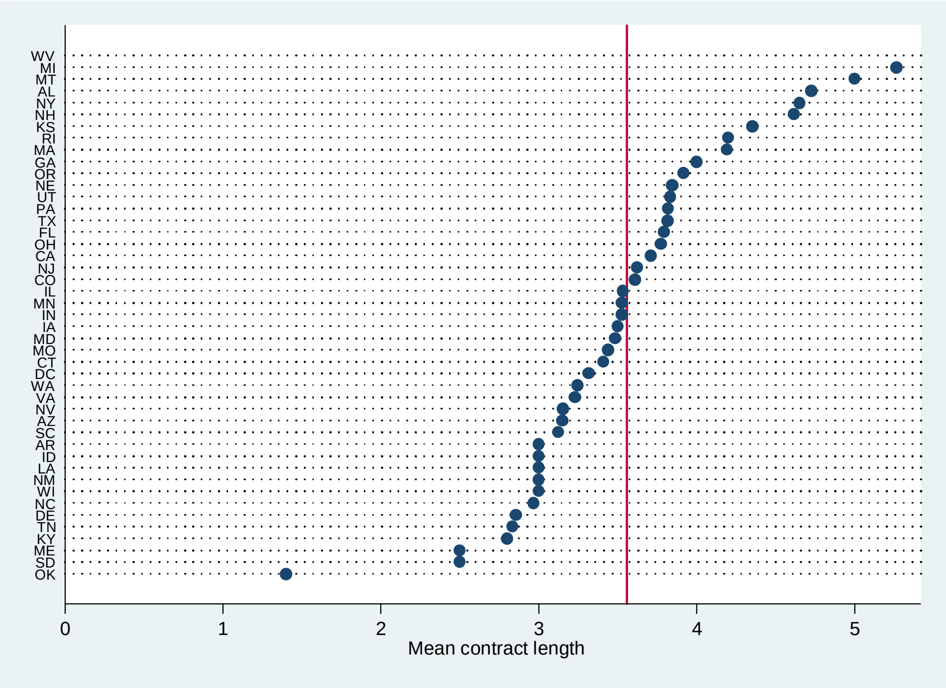
This figure plots the average frequency and length of fixed-term contracts across Fama-French industries. In Panel A, each circle represents the fraction of total firm-year observations in an industry under a fixed-term contract. In Panel B, each circle represents the average contract length in an industry, calculated over all firm-year observations under a fixed-term contract for firms in that industry. The red lines represent the respective sample means.

Fig. 4. Frequency and length of fixed-term contracts by state

Panel A. Frequency of fixed-term contracts

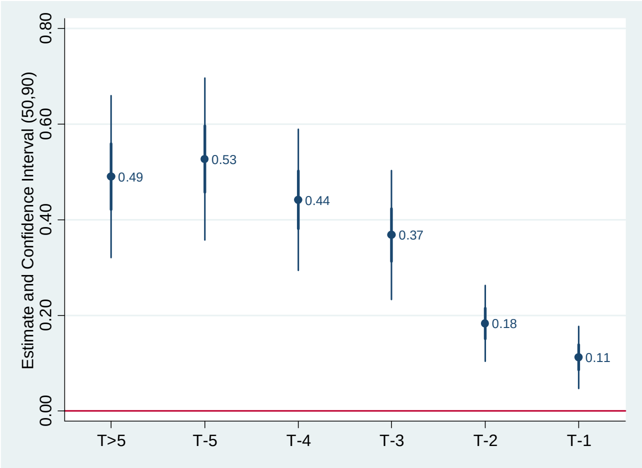


Panel B. Contract length



This figure plots the average frequency and length of fixed-term contracts across states. In Panel A, each circle represents the fraction of total firm-year observations in a state under a fixed-term contract. In Panel B, each circle represents the average contract length in a state, calculated over all firm-year observations under a fixed-term contract for firms in that state. The red lines represent the respective sample means.

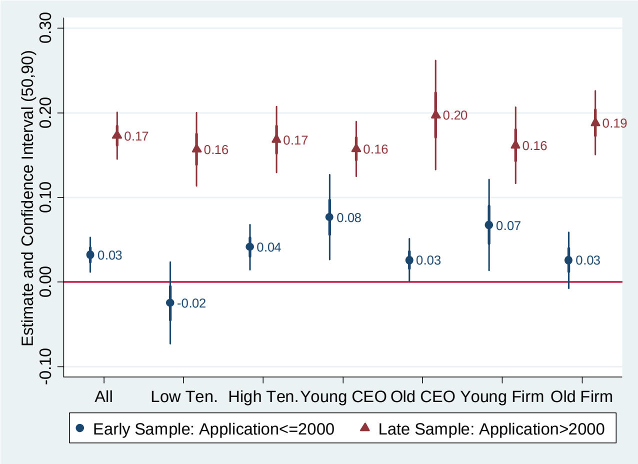
Fig. 5. Contract horizon and citations



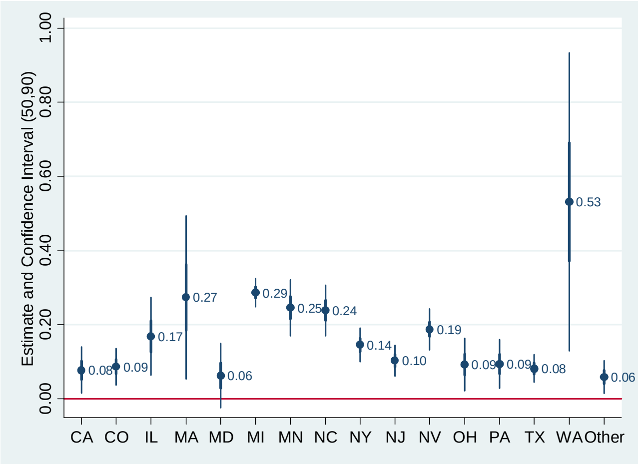
This figure plots the coefficient estimates and the 50th and 90th confidence intervals of an expanded version of equation (1) where horizon is replaced by a full set of contract horizon dummies (e.g., T-1 indicates patents filed when the CEO-firm pair have one year remaining in the contract) for contract horizons between one and five years, and an additional indicator variable for contract horizons longer than five years. To avoid multicollinearity, we exclude from the estimation the dummy that indicates contract horizons shorter than one year (0-years horizon).

Fig. 6. Contract horizon and citations

Panel A. Tenure and age in early and late samples

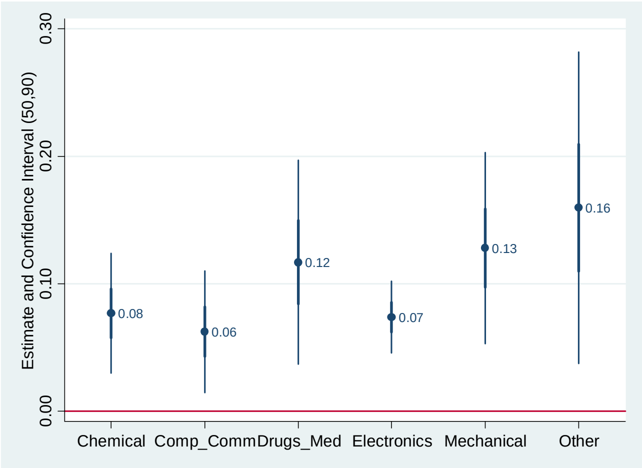


Panel B. Top 15 patenting states

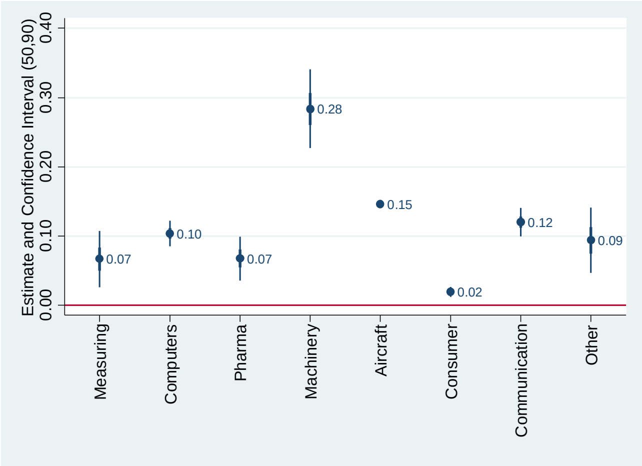


(more…)

Panel C. Top patenting technology classes

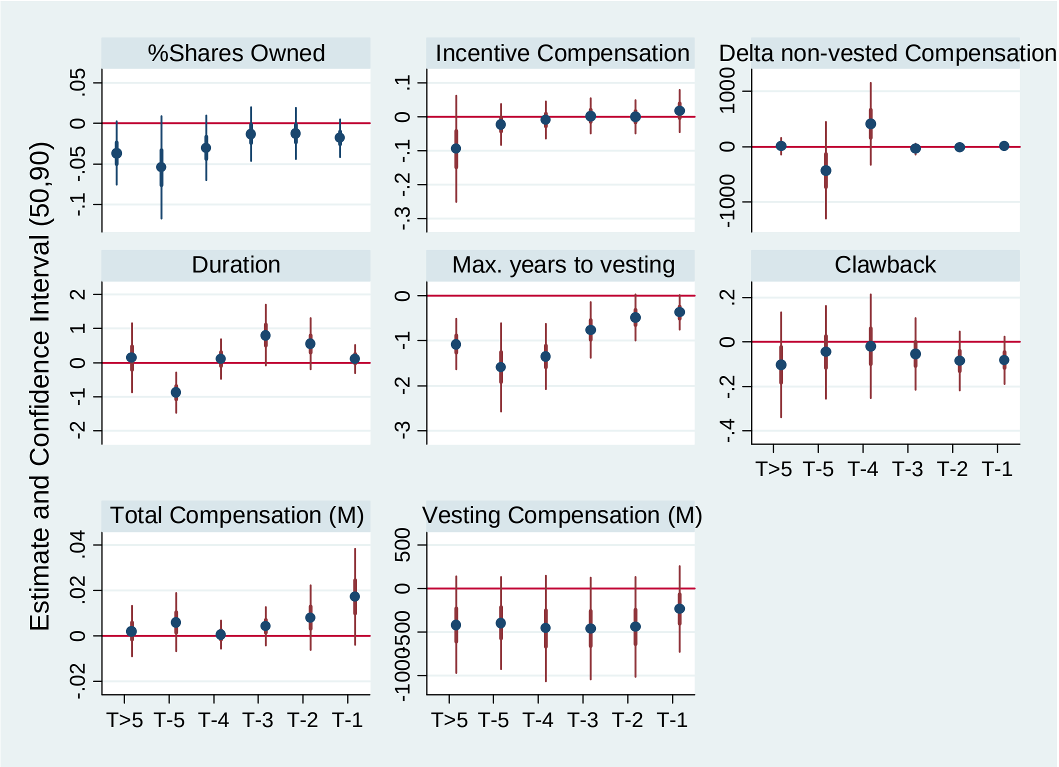


Panel D. Top patenting industries



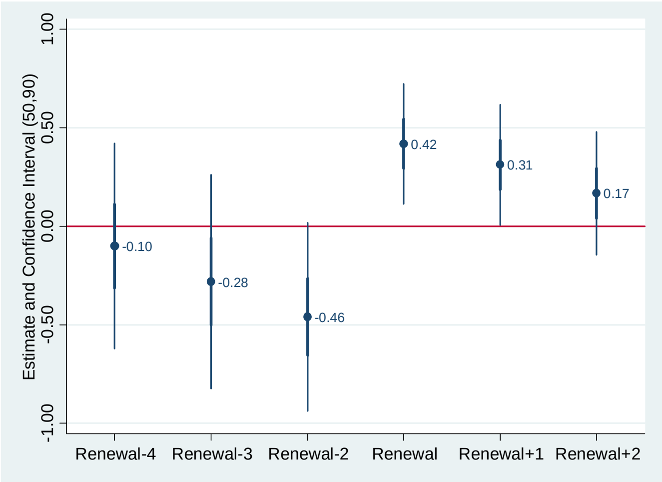
This figure plots the coefficient estimates of horizon in equation (1) and the 50th and 90th confidence intervals across different subsamples. The dependent variable is cites. In Panel A, the subsample Low (High) Tenure corresponds to CEOs with tenure below (above) three (two) years. The subsample Young (Old) CEO corresponds to CEOs with less (more) than 57 (56) years of age. The subsample Young (mature) firm corresponds to firms with less (more) than 12 (11) years since foundation. Red marks applications after 2000 and blue before or in 2000. In Panel B, Other indicates the subsample of patents filed in the states that individually account for less than 1% of the sample. In Panel C, patents are classified according to the "HJT 1-digit technological classification" (Hall et al., 2001). In Panel D, Other indicates the subsample of patent filed in the industries that individually account for less than 5% of the sample.

Fig. 7. Contract horizon and compensation



This figure plots the coefficient estimates and the 50th and 90th confidence intervals of an expanded version of Eq. (1) in which horizon is replaced by a full set of contract horizon dummies (e.g., T-1 indicates patents filed when the CEO-firm pair have one year remaining in the contract) for contract horizons between one and five years, and an additional indicator variable for contract horizons longer than five years. To avoid multicollinearity, we exclude from the estimation the dummy that indicates contract horizons shorter than one year (0-year horizon). The dependent variable is indicated in the sub-plot title.

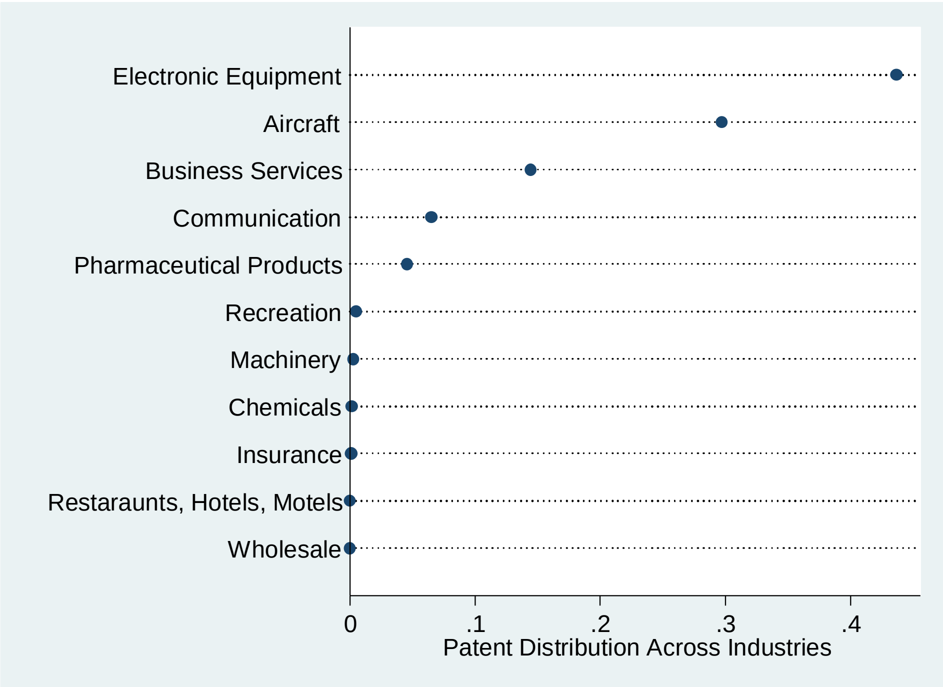
Fig. 8. Citations at contract renewals



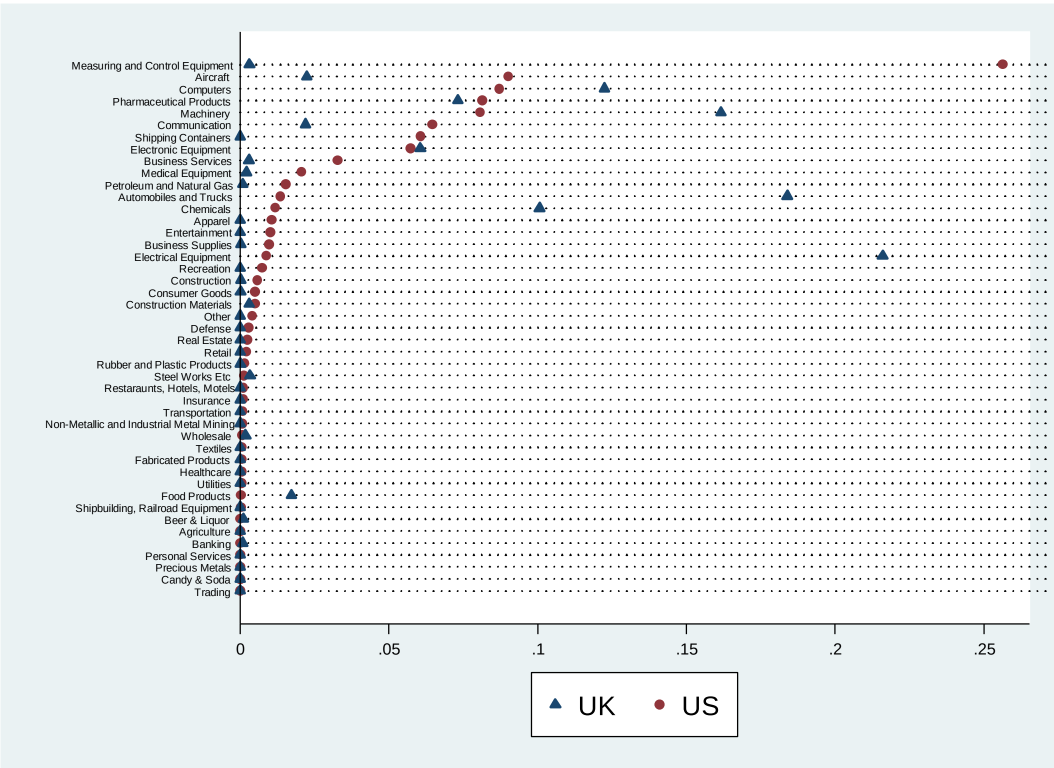
This figure plots the coefficient estimates and the 50th and 90th confidence intervals of Eq. (2) as reported in Column 2 of Table 6. The main explanatory variables are event-time indicators of contract renewal. In the estimation, we include a separate indicator for each year of the span from eight years before to six years after the contract renewal date—the indicator variable corresponding to one year before the renewal is the excluded dummy. We report values for the window from four years before to three years after contract renewal.

Fig. 9. Patent distribution across industries 2000–2008

Panel A. US firms cross-listed in the United Kingdom

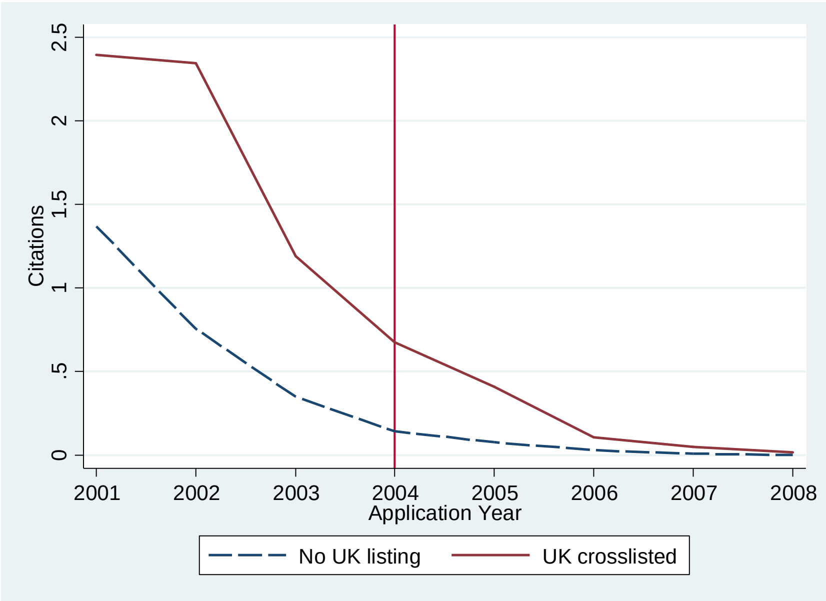


Panel B. All publicly listed firms in the United States and the United Kingdom



This figure plots the average fraction of patents across Fama-French industries for the US firms that were cross-listed in the UK in Panel A, and all firms listed in the UK (blue diamonds) or in the US (red circles) in Panel B.

Fig. 10. Trends in raw cites prior to the UK reform



This figure plots average raw cites from 2000-2008. The solid red line depicts the firms that were also listed in the UK in 2003, while the dashed blue line depicts those that were not.

Table 1. Univariate statistics of CEO fixed-term contracts (contract level data)

Panel A. Distribution of CEO fixed-term contracts over time

Year	Observations		Fraction fixed-term	Average contract length			Average contract horizon	No. of contracts	
	All	Under fixed-term contract		New	Renew.	All		New	Renew.
1993	1,216	11	0.01	4.00	3.83	3.88	2.82	4	7
1994	1,418	50	0.04	5.32	3.50	4.97	5.26	26	6
1995	1,514	99	0.07	5.64	4.00	5.12	4.21	28	17
1996	1,575	165	0.10	5.57	3.78	4.61	3.65	35	46
1997	1,731	274	0.16	3.93	3.69	3.80	3.11	48	64
1998	1,719	328	0.19	4.15	3.11	3.69	2.83	49	46
1999	1,725	351	0.20	3.71	3.43	3.55	2.30	45	59
2000	1,736	404	0.23	3.65	3.15	3.41	2.29	45	48
2001	1,794	433	0.24	3.90	3.08	3.51	2.10	51	59
2002	1,690	382	0.23	3.13	3.53	3.24	1.95	60	32
2003	1,570	334	0.21	3.57	2.82	3.24	1.87	40	38
2004	1,447	289	0.20	2.87	3.40	3.11	1.86	41	35
2005	1,349	268	0.20	3.14	3.14	3.14	1.90	44	29
2006	1,253	254	0.20	2.42	3.14	2.76	1.94	44	39
2007	1,090	248	0.23	2.58	2.94	2.78	1.83	32	41
2008	815	141	0.17	3.10	2.50	2.75	1.72	11	19
Tot.	23,642	4,031	0.17	3.78	3.30	3.56	2.82	603	585

Panel B. Distribution of CEO fixed-term contracts by contract length

Contract length	New		Renewed		Total	
	Count	%	Count	%	Count	%
1	80	13.27	46	7.86	126	10.61
2	100	16.58	92	15.73	192	16.16
3	140	23.22	177	30.26	317	26.68
4	53	8.79	53	9.06	106	8.92
5	68	11.28	50	8.55	118	9.93
6	28	4.64	14	2.39	42	3.54
7	12	1.99	2	0.34	14	1.18
8	10	1.66	7	1.20	17	1.43
>9	34	5.64	10	1.71	33	2.78
missing	78	12.94	134	22.91	212	17.85
Total	603		585		1,177	

This table presents summary statistics across years (Panel A) and contract length (Panel B). The sample comprises all patenting firms that were listed in the United States between 1993 and 2008.

Table 2. CEO turnover and contractual protection

Panel A. Premature turnover by contract length and remaining years

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
	Turnover incidence							# of relevant contracts	Premature turnover (annualized)
Length/ Horizon	>5	5	4	3	2	1	0		
1						10	8	126	10
						6%	9%		8%
2					14	11	8	178	25
					6%	6%	6%		7%
3				14	10	16	20	274	40
				4%	3%	6%	9%		5%
4			3	4	3	7	4	95	17
			2%	4%	3%	10%	8%		4%
5		3	2	6	4	2	7	100	17
		2%	2%	5%	4%	2%	10%		3%
All	1	3	10	27	35	47	48	1,054	123
	0%	1%	3%	4%	4%	6%	8%		4%
t-stat of difference to (7)	7.42	3.53	3.69	3.13	3.01	1.60			5.36

Panel B. Performance before turnover

Turnover	Premature	Around expiration year	t-stat of difference
Industry-adjusted returns	0.65%	-1.68%	2.90
Raw returns	2.43%	1.56%	0.33
ROA	-2.79%	-7.28%	4.03

Panel C. Reasons for premature turnover

	Number	% of all premature turnovers	% of premature turnovers with information
Health/mandatory retirement age	30	29%	25%
Acquisitions	24	23%	20%
Shareholder pressure	15	14%	13%
New position	13	13%	11%
Cause	11	11%	9%
Performance	11	11%	9%
No information	16		13%

Panel D. Outcomes

	Incidence	Relevant comparison group	%
Survival beyond expiration	698	835	84%
Subsequent contract	307	835	37%
Subsequent contract of equal length	125	835	15%

This table presents statistics on turnover and renewals for contracts starting and ending between 1993 and 2008. Panel A (columns [1]–]7]) reports the number of turnovers in a given pair of number of remaining years (columns) and contract length (rows). The number of relevant contracts (column [8] for each contract length/row) is the denominator for the fractions underneath and equal to the number of all contracts that expire before the end of 2008. Column [9] reports the annualized ratio between the number of premature turnovers (before the last year prior to expiration) and relevant contracts. Panel B reports the average annual stock returns and ROA in the year prior to turnovers. Panel C reports the number of premature turnovers sorted by the reason for turnover. Panel D reports the number of CEOs who survive in the sample beyond the first year after expiration as a fraction of those who survive to the expiration year, observed until 2008, and, as a fraction of the same, the number of CEOs who receive another contract, and those new contracts of equal length.

Table 3. Determinants of CEO fixed-term contracts and contract length (firm-year level data)

	Total Sample			At-will		Fixed-term contract		Contract length		Contract horizon		Tests for differences in means		
								Long	Short	Long	Short	Fixed – At-will	Long – short contract	Long – short horizon
	Obs.	Mean	SD	Obs.	Mean	Obs.	Mean	Obs.	Mean	Obs.	Mean	*t*	*t*	*t*
Panel A. Characteristics of employment agreements														
Fixed-term contract	23,642	0.171	0.376	19,611	0.000	4,031	1.000	1.000	1.000	1.000	1.000			
Contract length	23,262	0.594	1.628	19,611	0.000	3,651	3.785	5.575	2.458	5.982	3.087	241.8	59.7	41.3
Panel B. CEO characteristics														
CEO age	10,506	55.086	7.525	7,431	55.581	3,075	53.889	54.898	53.465	55.089	53.532	-10.5	4.9	4.7
CEO tenure	23,642	2.787	5.489	19,611	2.514	4,031	4.120	4.952	3.670	4.963	3.885	17.0	6.6	4.9
CEO ownership	23,642	0.123	1.245	19,611	0.116	4,031	0.154	0.151	0.159	0.102	0.169	1.8	-0.2	-1.1
Salary	9,360	674.15	395.82	7,282	660.00	2,078	723.71	821.45	661.60	755.60	713.09	6.5	8.6	2.0
Incentive to total comp.	23,642	0.282	0.377	19,611	0.263	4,031	0.377	0.403	0.365	0.433	0.362	17.6	2.8	4.7
Panel C: Firm characteristics														
Market-to-book ratio	21,020	3.176	2.518	17,379	3.197	3,641	3.075	3.081	3.110	3.106	3.066	-2.7	-0.3	0.4
Return on assets	21,056	0.001	0.182	17,412	0.002	3,644	-0.003	0.006	-0.008	0.026	-0.012	-1.6	2.1	5.0
Sales growth	20,603	0.154	0.523	17,041	0.155	3,562	0.146	0.139	0.162	0.153	0.144	-1.0	-1.2	0.4
Leverage	21,112	0.132	0.340	17,461	0.126	3,651	0.161	0.181	0.160	0.202	0.150	5.6	1.7	3.7
Total assets ($M)	21,120	3263.5	6180.1	17,468	3263.0	3,652	3265.8	4043.6	2910.1	3613.8	3167.1	0.0	4.9	1.7
Tangibility	18,493	0.903	0.128	15,244	0.908	3,249	0.881	0.885	0.878	0.879	0.881	-11.2	1.3	-0.4
R&D/Total assets	21,158	0.079	0.116	17,498	0.080	3,660	0.072	0.069	0.074	0.063	0.075	-3.9	-1.4	-2.8
Patents/Total assets	21,120	0.039	0.078	17,468	0.040	3,652	0.033	0.035	0.032	0.030	0.033	-5.0	1.2	-1.2
Cites	23,642	1.996	2.679	19,611	2.029	4,031	1.836	1.898	1.811	2.144	1.749	-4.2	1.0	4.2
G-Index	2,622	9.342	2.635	1,919	9.248	703	9.599	9.498	9.722	9.541	9.618	3.0	-1.1	-0.3
Insider board	23,642	0.015	0.122	19,611	0.013	4,031	0.024	0.033	0.020	0.039	0.020	5.1	2.4	3.2
Panel D. Industry and state characteristics														
Industry homogeneity value	23,642	0.479	0.500	19,611	0.491	4,031	0.421	0.409	0.444	0.441	0.416	-8.0	-2.1	1.3
Industry survival rate	21,158	1.014	0.098	17,498	1.018	3,660	0.994	1.000	0.992	1.011	0.989	-13.5	2.3	5.7
Industry median volume of sales	21,158	0.241	0.991	17,498	0.276	3,660	0.074	0.101	0.060	0.162	0.049	-11.3	2.0	5.0
Industry citation variance	3,219	5.778	5.322	373	4.115	2,846	5.996	6.327	5.694	6.773	5.793	6.5	3.0	3.9
Garmaise index	19,558	3.399	2.403	15,786	3.327	3,772	3.699	3.807	3.723	3.755	3.683	8.6	1.1	0.8
Anti-takeover Index	19,558	0.566	0.496	15,786	0.556	3,772	0.609	0.636	0.611	0.624	0.605	6.0	1.5	1.0

This table presents firm-year level summary statistics for the entire sample and for the subsamples of firm-years under fixed-term contracts, at-will employment, and under above- and below-mean (3.56) fixed-term contracts, as well t-statistics of differences in means between the subsamples. The sample comprises all patenting firms that were listed in the United States between 1993 and 2008. Cites corresponds to average citations for all patents filed by the firm.

Table 4. Summary statistics for the analysis sample

Panel A. Patent-level data

	[1] Observations	[2] Mean	[3] SD
Horizon	100,370	2.47	2.32
Start year	99,744	1,999	3.64
Contract length	89,591	4.49	2.03
Contract renewal	100,370	0.56	0.49
CEO age	97,096	56.27	6.25
CEO tenure	97,645	6.25	7.99
Firm age	88,323	12.74	7.53
Raw cites	100,370	4.16	9.06
Cites	100,370	1.16	2.22
Scaled originality	94,298	1.04	0.22
Scaled generality	57,935	1.02	0.36
% shares owned	86,886	0.03	0.51
Total compensation (TDC1)	86,757	18,734.06	25,583.30
Delta of non-vested compensation	73,598	155.87	1032.04
Vesting compensation (£M)	85,848	28.30	895.00
Incentive compensation	100,370	0.72	0.31
Duration	15,792	2.30	2.20
Maximum years to vesting	48,604	0.96	1.59
Clawback	55,222	0.06	0.24
Vesting data dummy	100,370	0.91	0.29
Insider board	100,370	0.07	0.26
Institutional ownership	100,370	2.50	3.63
Chairman CEO	100,370	0.72	0.45
G-index	31,059	9.06	2.18
Market response	100,370	0.01	0.03
Collateral	100,370	0.02	0.13
New inventor	72,428	0.65	0.48
Self-cites	100,370	0.42	1.23
New technology class	100,370	0.05	0.22

Panel B. Firm-year-level data

	[1] Observations	[2] Mean	[3] SD
Horizon	4,031	2.32	2.33
R&D/Assets	4,031	0.07	0.12
Patents	4,031	27.56	171.33
Technological focus	4,031	0.58	0.35
Variance citations	2,325	6.11	7.76

This table presents summary statistics. The sample comprises all patenting firms listed in the United States between 1993 and 2008 under a fixed-term CEO contract.

Table 5. Contract horizon and citations

Panel A. Main analysis

Subsample	[1] Full	[2] Low tenure	[3] High tenure	[4] Young CEO	[5] Old CEO	[6] Young firm	[7] Mature firm	[8] Contract renewals	[9] New CEO
Horizon	0.094***	0.077**	0.067**	0.129***	0.073***	0.119***	0.101***	0.061***	0.166***
	(0.018)	(0.026)	(0.026)	(0.020)	(0.022)	(0.020)	(0.024)	(0.021)	(0.022)
Observations	99,295	40,897	55,612	47,521	48,502	34,225	53,105	59,382	39,896
R-squared	0.133	0.155	0.116	0.158	0.101	0.187	0.101	0.133	0.133
p-value difference			0.783		0.063		0.567		0.00

Panel B. Complementary tests

	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Dep. Var.	Cites	Cites	Cites	Cites_5	Market response	Collateral	Cites
Subsample	Short Contract	Long Contract					
Horizon	0.120***	0.089***	0.099***	0.089***	0.002*	0.003**	0.085***
	(0.025)	(0.020)	(0.017)	(0.018)	(0.001)	(0.001)	(0.018)
% shares owned							-0.123***
							(0.039)
Incentive compensation							0.040
							(0.095)
Delta of non-vested compensation							0.000***
							(0.000)
Duration							0.049
							(0.060)
Maximum years to vesting							-0.069
							(0.053)
Clawback							-0.028
							(0.018)
Total compensation (TDC1)							0.000
							(0.000)
Vesting compensation							-0.000
							(0.000)
Vesting data dummy							0.076
							(0.061)
Observations	31,985	67,304	96,604	99,295	92,635	92,635	99,295
R-squared	0.177	0.107	0.224	0.133	0.566	0.328	0.133
Start-year×Firm×Tech.-class FE	No	No	Yes	No	No	No	No

This table reports the OLS estimates of Eq. (1), including contract start year cross firm fixed effects. Standard errors clustered on the firm level are reported in parentheses. The dependent variable in Panel A is cites. The main explanatory variable is horizon*:* the number of years remaining in the contract between a firm and a CEO before expiration. Columns [2]–[9] of Panel A report estimates for subsamples as indicated in the column title. The subsample Low (High) tenure corresponds to contracts for CEOs with tenure below (above) thee (two) years. The subsample Young (Old) CEO corresponds to contracts for CEOs with less (more) than 57 (56) years of age (sample median). The subsample Young (Mature) firm corresponds to contracts for firms with less (more) than 12 (11) years since foundation (sample median). In Panel B, the dependent variable is reported at the top of each column. Cites_5 are scaled citations measured up to five years after granting. The vesting data dummy is an indicator variable set to 1 for observations with no information on average duration and maximum vesting years. Column [1] (resp., [2]) uses a subsample of patents applied for under a contract shorter than or equal to (resp., longer than) 3.56 years. ***, **, and *indicate that the estimates are significantly different from zero at the1%, 5%, and 10% levels, respectively.

Table 6. Citations near contract renewals

	[1]	[2]	[3]	[4]	[5]	[6]
Dependent variable		Cites			Market response	
Sample	All	With renewal	With renewal	All	With renewal	With renewal
4 years before renewal	0.092	-0.100	-0.096	-0.002	-0.005	-0.005
	(0.227)	(0.315)	(0.315)	(0.004)	(0.006)	(0.007)
3 years before renewal	-0.045	-0.281	-0.276	0.002	0.000	0.000
	(0.245)	(0.329)	(0.328)	(0.005)	(0.004)	(0.004)
2 years before renewal	-0.333	-0.460	-0.455	0.005	0.000	0.000
	(0.219)	(0.289)	(0.288)	(0.006)	(0.005)	(0.005)
Renewal	0.223	0.420**	0.422**	0.011**	0.013**	0.014**
	(0.174)	(0.185)	(0.185)	(0.005)	(0.006)	(0.006)
1 year after renewal	0.141	0.312*	0.316*	0.005	0.012*	0.012*
	(0.174)	(0.185)	(0.186)	(0.005)	(0.007)	(0.007)
2 years after renewal	0.085	0.168	0.170	-0.008	-0.001	-0.001
	(0.171)	(0.189)	(0.189)	(0.007)	(0.004)	(0.004)
3 years after renewal	0.012	0.114	0.118	-0.017*	-0.008	-0.008
	(0.176)	(0.215)	(0.214)	(0.009)	(0.005)	(0.005)
Observations	95,878	31,620	31,620	95,878	31,620	31,620
R-squared	0.236	0.238	0.238	0.543	0.469	0.469
Contract-start year cross technology-class FE	No	No	Yes	No	No	Yes

This table reports the OLS estimates of different versions of Eq. (2). Standard errors clustered on the firm level are reported in parentheses. The dependent variable is reported at the top of the column. 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 eight years before to six years after the contract renewal date—the indicator variable corresponding to one year before the renewal is the excluded dummy. We report values for the window from four years before to three years after contract renewal. All regressions include tenure fixed effects. Columns [1] and [2] as well as columns [4] and [5] include contract start year fixed effects. Columns [3] and [6] include contract start year cross technology-class fixed effects. ** and * indicate that the estimates are significantly different from zero at the 5% and 10% levels, respectively.

Table 7. Alternative explanations

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	0.102***	0.084***	0.167***	0.154
	(0.017)	(0.020)	(0.020)	(0.111)
Governance measure	0.203**	-0.019	0.326***	0.042
	(0.103)	(0.015)	(0.075)	(0.072)
Horizon × Governance measure	-0.066***	0.005	-0.091***	-0.001
	(0.021)	(0.004)	(0.023)	(0.010)
Observations	99,295	99,295	99,295	30,756
R-squared	0.133	0.133	0.133	0.109

This table presents the results of the OLS regressions, reporting coefficients along with (heteroskedasticity-robust and firm-clustered) standard errors in parentheses. The dependent variable is cites. The main explanatory variable is horizon: the number of years remaining on the CEO's contract. All specifications include firm and contract start-year 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 A for further detail). 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. ***, **, and * indicate that the estimates are significantly different from zero at the 1%, 5%, and 10% levels, respectively.

Table 8. Management of innovation

Panel A. Pathways to innovation

Dependent variable:	[1] R&D / Total assets	[2] Patent filings	[3] New inventor	[4] Variance citations	[5] Technological focus	[6] New technology class	[7] Cites	[8] Scaled generality	[9] Scaled originality
Horizon	0.002***	-1.164	0.060***	0.428***	-0.017***	0.005***	0.091***	0.004**	-0.001
	(0.001)	(0.998)	(0.011)	(0.140)	(0.003)	(0.001)	(0.017)	(0.002)	(0.002)
New tech. class							-0.029		
							(0.095)		
Horizon × New tech. class							0.063**		
							(0.032)		
Observations	3,217	3,704	71,467	1,904	3,317	99,295	99,295	57,132	93,220
R-squared	0.768	0.749	0.170	0.561	0.668	0.266	0.133	0.050	0.060

Panel B. Long-term incentives for chief research officers (CROs)

Dependent variable: Contract length of…	[1] CROs	[2] CFOs	[3] 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
Average contract length	3.34	3.52	5.19

This table presents the results of the 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. The last row in Panel B reports the average contract length for CRO, CFO, and chairmen contracts, respectively. All columns in Panels A include contract start-year cross firm fixed effects. ***, **, and *indicate that the estimates are significantly different from zero at the 1%, 5%, and 10% levels, respectively.

Table 9. Cross-listed firms during the UK code change

Panel A. Summary statistics for cross-listed firms

	Cross-listed firms			Other US firms			Test of difference in means
	Obs.	Mean	Median	Obs.	Mean	Median	T-value
Raw cites	12,977	1.87	0.00	48,782	2.38	0.00	-53.27
Contract length	12,977	4.41	5.00	48,782	4.14	4.00	-11.57
Industry avg. US contract length	12,977	4.45	4.68	48,782	4.16	3.83	-6.46
Post	12,977	0.39	0.00	48,782	0.42	0.00	52.61

Panel B. Contract length and innovation

	[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 A reports univariate statistics for the patents applied for by the US firms cross-listed in the UK and those that were not, with the t-statistics for the difference in means. Panel B presents the results of the OLS regressions, reporting coefficients along with (heteroskedasticity-robust) standard errors in parentheses. The dependent variables are those indicated by the column heading. The data used for both panels are restricted to the patents with application year between 2000 and 2008. The “Cross-listed” dummy is set to 1 for the firms that were cross-listed in the UK in 2003; the “Post” dummy is set to 1 for years after 2003. ***, **, and * indicate that the estimates are significantly different from zero at the 1%, 5%, and 10% levels, respectively.

Appendix A. 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 (see Eq. (A.1)):

$$Filings_{it} = \alpha^f + \sum_{j=0}^{3} \beta_j^f \log R\&D_{i,t-j} + \varphi^f \log \sum_{j=0}^{3} R\&D_{i,t-j} + \theta^f \log Assets_{i,t} + \mu_t^f + \varepsilon_{it}^f \quad . \tag{A.1}$$

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 the results derived from the Poisson model, similar results are obtained when we use 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)

In accordance with Hall et al. (1986) and 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, Edler, Frietsch, and Schmoch, 2006; Mihm, Sting, and Wang, 2015). Agriculture, food, and business services are the industries with the longest lags—possibly due to intermediate development in such industries.

Appendix B. Mean-reverting variables: Simulations

To illustrate that a mean-reverting variable cannot generate our results, in Table A.1, we report the results of Monte Carlo simulations using artificial data that are 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) CEO-firm 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} that exclusively explains an outcome variable y_{it}. So that c_{it} gets more correlated with the contract horizon, we fix contract lengths 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 is as follows (see Eq. (B.1)):

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

We then generate an outcome variable as a function of c_{it} (see Eq. (B.2)):

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

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 (see Eq. (B.3)-(B.4)):

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

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

Table A.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 A.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