Internet Appendix to
"CEO turnover and volatility under long-term employment contracts"

This Appendix provides additional analyses and results for our paper "CEO turnover and volatility under long-term employment contracts". The discussion can be found in the main text of the paper; the tables are referred to as A-#, where # is the table number in Appendix.

Appendix B: CEO turnover - robustness

This section contains and describes robustness tests for our turnover regressions.

Logit. Most literature on CEO turnover employs models using logit regressions (Denis, Denis, and Sarin 1997; Mikkelson and Partch 1997; Perry 1999; Huson, Parrino, and Starks 2001) or probit regressions (Jenter and Lewellen 2014).[13] In Table A-1, Column 1 shows that the estimates obtained from a logit model of CEO turnover are similar to those of the linear probability model and the Cox hazard model in Table 4 of the paper: specifically, CEOs with more years remaining to contract expiration are less likely to leave the firm.

Fixed effects. Column 2 of Table A-1 shows that the results of our turnover models remain similar if we use CEO fixed effects instead of firm fixed effects. Our results hold similarly if we use industry fixed effects. These models are shown in Table A-5, Columns 1 and 3 (Internet Appendix C).

Tenure and age. In Column 3 of Table A-1, we also control for CEO tenure and age. Since tenure and age cannot be identified separately in a linear model that includes executive- and year fixed effects, we follow Berndt and Griliches (1993) and drop a subset of dummy variables. As shown by Pan, Wang and Weisbach (2013) who use dummy variables for tenure below three and over six, tenure effects are not linear. Dummy variables also allow us to capture potentially linear effects and non-linear effects. Our results remain similar.

Voluntary turnover. Executives may leave voluntarily, either because they receive unsolicited external offers, or because they had been scouting for offers when their turnover probabilities were high. Unsolicited external offers may reflect the performance in comparable firms, rather than contractual protection. We show that voluntary turnover events are not driving the result that remaining time under contract predicts CEO turnover. Our data include 187 voluntary CEO turnovers, identified following the Parrino (1997) algorithm by Jenter and Kanaan (2015), Peters and Wagner (2013), Eisfeldt and Kuhnen (2013), and in our own search. Table A-2 contains a break-down of the number of CEO turnover observations. As in Jenter and Kanaan (2015), we now treat voluntary turnovers as right-censored

[13] For a review of the literature on CEO turnover, see Brickley (2003).

observations and re-estimate the OLS models in Table 4 Column 1 and Table A-1 Column 3. Columns 4-5 in Table A-1 show the results. We find that CEOs with 1-2 years remaining are not less likely to be dismissed than CEOs with less than one year; however, CEOs with more than 2 years are significantly more likely to be dismissed, in line with the baseline results. While the economic magnitude of the estimates becomes smaller, we still reject at the 1% level the null that all of the coefficients of the dummy variables are equal to each other. Columns 6-7 in Table A-1 show that excluding voluntary turnover firm-years from the sample yields qualitatively and quantitatively similar results. Finally, we also obtain similar results if we exclude all observations of a CEO that leaves voluntarily—these results are not tabulated. Overall, the results of these tests suggest that our treatment of voluntary turnovers does not affect our results.

Additional control variables. The relation between CEO contract horizon and turnover probability is robust to the inclusion of additional independent variables. In Table A-3, we estimate models of CEO turnover controlling for CEO contract horizon dummies as well as a several control variables motivated by prior literature. We control for ROA, tenure performance, size, whether the firm pays a dividend, the B/M ratio (Jenter and Lewellen, 2014), institutional ownership (Denis, Denis, and Sarin, 1997; Parrino, Sias, and Starks, 2003), CEO age (Parrino, 1997; Jenter and Lewellen, 2015), the percentage of independent directors (Weisbach, 1988; Guo and Masulis, 2015), and the percentage of directors on the board who are "busy" by the definition of Fich and Shivdasani (2006) – which is that they serve on more than 3 boards. We estimate the turnover models in three functional forms: OLS (linear probability model), logit, and Cox hazard model. Because the additional variables on board characteristics reduce our sample size, we estimated two separate specifications with and without them. All of our results hold across functional forms and with these additional control variables.

The performance sensitivity of CEO turnover. The main analysis of the paper shows the sensitivity of CEO turnover to industry-adjusted performance measures. We examine the robustness of these findings by using raw measures of performance (past-year returns or tenure performance, as in Table 4). These models are shown in Table A-4 (Columns 1 and 4). Furthermore, we also verify that the choice of functional form does not alter our results on how contracts affect the performance-sensitivity of CEO turnover. Columns 2 and 5 in Table A-4 show that our results hold when using logit models, while Columns 3 and 6 in Table A-4 show that our results are also similar using Cox proportional hazard models.

Table A-1: Remaining years under contract and turnover probability – robustness

This table summarizes estimates from models of CEO turnover. Column 1 shows estimates from a logit regression, while Columns 2-7 show estimates from linear probability models of CEO turnover, reporting coefficient estimates with standard errors given underneath. The dependent variable is a dummy set to 1 only for the years in which a firm's CEO leaves. In Columns 4 and 5, following Jenter and Kanaan (2015), we treat involuntary CEO turnover events as right-censored observations. In Columns 6 and 7, we remove firm-year observations with involuntary turnover. Independent variables are defined in Appendix A of the paper. Standard errors (in parentheses) are robust to heteroskedasticity and are clustered at the CEO level. The data span the years 1992–2008. Asterisks indicate that the estimates are significantly different from zero at the *** 1% level, ** 5% level, and * 10% level.

Sample	All	All	All	Involuntary	Involuntary	Involuntary	Involuntary
Model	Logit	OLS	OLS	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1-2 years remaining	-0.206*	-0.036***	-0.037***	-0.017**	-0.019**	-0.018**	-0.020**
	(0.118)	(0.009)	(0.009)	(0.008)	(0.008)	-(0.008)	-(0.008)
2-3 years remaining	-0.596***	-0.074***	-0.086***	-0.051***	-0.054***	-0.053***	-0.056***
	(0.125)	(0.009)	(0.01)	(0.008)	(0.008)	-(0.008)	-(0.008)
3-4 years remaining	-0.879***	-0.094***	-0.112***	-0.066***	-0.075***	-0.068***	-0.078***
	(0.141)	(0.010)	(0.01)	(0.009)	(0.009)	-(0.009)	-(0.009)
4-5 years remaining	-1.058***	-0.099***	-0.116***	-0.065***	-0.070***	-0.066***	-0.071***
	(0.210)	(0.013)	(0.013)	(0.011)	(0.011)	-(0.011)	-(0.011)
> 4-5 years remaining	-1.375***	-0.093***	-0.122***	-0.064***	-0.071***	-0.066***	-0.073***
	(0.224)	(0.013)	(0.014)	(0.013)	(0.013)	-(0.013)	-(0.013)
Tenure		-0.004***		-0.005***		-0.005***	
		(0.001)		(0.001)		(0.001)	
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry F.E.	Yes						
CEO F.E.		Yes					
Firm F.E.			Yes	Yes	Yes	Yes	Yes
Tenure dummies			Yes		Yes		Yes
Age dummies			Yes		Yes		Yes
F-test of weak instruments: coefficients of remaining years all = 0							
F-test		23.08**	19.52**	16.141***	18.984***	16.737***	19.864***
F-test: coefficients of remaining years all equal							
F-test		15.53***	13.68***	10.052***	12.100***	10.532***	12.67***
N	7,456	7,456	7,456	7,456	7,456	7,269	7,269

Table A-2: Turnover data

This table presents univariate statistics of turnover events. We obtain information on the nature of CEO turnovers in our sample from Eisfeldt and Kuhnen (2013), Peters and Wagner (2013), and Jenter and Kanaan (2015). Finally, for the remaining turnover events, we conduct our own search (see Column 3). The data span the years 1992–2008.

Data source	Eisfeldt and Kuhnen (2013)	Peters-Wagner (2013) and Jenter and Kanaan (2015)	Our own search	Total
Voluntary ("exogenous") turnover	135		52	
Forced turnover	69	140	207	
Unclassified turnover	254			
Total	**458**	**140**	**259**	**857**

Table A-3: Additional control variables in CEO turnover models

Columns 1–2 present estimates from linear probability models of CEO turnover, and Columns 3–4 contain estimates from logit models of CEO turnover, reporting coefficient estimates with standard errors underneath. Columns 5–6 show the results from Cox proportional hazard models, reporting hazard ratios for CEO turnover with standard errors underneath. Independent variables are defined in Appendix A. Standard errors are clustered at the CEO level. The data span the years 1992–2008. Asterisks indicate that the estimates are significantly different from zero at the *** 1% level, ** 5% level, and * 10% level.

	Models of CEO turnover					
Model	OLS	OLS	Logit	Logit	Cox	Cox
	(1)	(2)	(3)	(4)	(5)	(6)
Tenure	-0.002***	-0.002**	-0.033**	-0.041**		
	(0.001)	(0.001)	(0.013)	(0.019)		
1-2 years remaining	-0.026**	-0.079***	-0.273***	-0.830***	0.844	0.489***
	(0.010)	(0.021)	(0.105)	(0.232)	(0.102)	(0.104)
2-3 years remaining	-0.054***	-0.088***	-0.658***	-1.015***	0.656***	0.465***
	(0.013)	(0.025)	(0.163)	(0.275)	(0.087)	(0.099)
3-4 years remaining	-0.073***	-0.125***	-0.996***	-1.810***	0.470***	0.193***
	(0.010)	(0.022)	(0.150)	(0.287)	(0.073)	(0.059)
4-5 years remaining	-0.081***	-0.115***	-1.209***	-1.501***	0.339***	0.258***
	(0.013)	(0.024)	(0.261)	(0.354)	(0.079)	(0.090)
> 5 years remaining	-0.090***	-0.131***	-1.745***	-2.345***	0.208***	0.124***
	(0.011)	(0.023)	(0.283)	(0.407)	(0.058)	(0.055)
ROA	-0.028	-0.066	-0.361	-0.952	0.476***	0.195
	(0.021)	(0.069)	(0.247)	(1.119)	(0.133)	(0.231)
Tenure performance	-0.006**	-0.004	-0.134	-0.080	0.794***	0.842
	(0.003)	(0.005)	(0.085)	(0.142)	(0.062)	(0.119)
Ln(assets)	0.005**	0.000	0.079**	0.012	1.143***	1.065
	(0.002)	(0.006)	(0.032)	(0.086)	(0.040)	(0.088)
Dividend	-0.007	0.001	-0.114	0.036	0.943	1.161
	(0.009)	(0.011)	(0.138)	(0.181)	(0.108)	(0.239)
B/M	0.002	0.022	0.029	0.362*	0.962	1.388
	(0.004)	(0.018)	(0.048)	(0.216)	(0.086)	(0.343)
Institutional ownerhsip (%)	-0.051***	-0.010	-0.751***	-0.057	0.490***	0.972
	(0.010)	(0.016)	(0.151)	(0.246)	(0.076)	(0.244)
CEO Age	0.001***	0.001	0.022***	0.009	0.984***	0.968***
	(0.000)	(0.001)	(0.007)	(0.013)	(0.006)	(0.011)
Independent directors (%)		-0.064**		-1.227***		0.534
		(0.028)		(0.416)		(0.239)
Busy directors (%)		0.041		0.674		2.490
		(0.041)		(0.638)		(1.611)
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Industry F.E.	Yes	Yes	Yes	Yes	Yes	Yes
N	6,701	2,457	6,701	2,457	6,701	2,457

Table A-4: Performance adjustment in turnover models

This table summarizes models of CEO turnover. Columns 1 and 3 report the results from a Cox proportional hazard model; the values shown are hazard ratios for CEO turnover with standard errors given underneath. Columns 2 and 4 report a logit model where the dependent variable is a dummy for CEO turnover. These columns report coefficient estimates with standard errors given underneath. *Tenure performance* is stock return measured over the preceding 5 years or since the start of the CEO's tenure, whichever is shorter, scaled by its standard deviation (Jenter and Lewellen 2014). All regressions include industry and year fixed effects. Standard errors (in parentheses) are robust to heteroskedasticity and are clustered at the CEO level. The data span the years 1992–2008. Asterisks indicate that the estimates are significantly different from zero at the *** 1% level, ** 5% level, and * 10% level.

Model	Logit	Cox	Logit	Cox
Performance measure	Return	Return	Tenure performance	Tenure performance
	(1)	(2)	(3)	(4)
1-2 years remaining	-0.2152*	0.8781	-0.2021	0.9100
	(0.130)	(0.106)	(0.134)	(0.114)
2-3 years remaining	-0.5769***	0.6892***	-0.5410***	0.7400**
	(0.139)	(0.090)	(0.142)	(0.099)
3-4 years remaining	-0.9257***	0.4883***	-0.9193***	0.4974***
	(0.161)	(0.075)	(0.166)	(0.079)
4-5 years remaining	-1.1808***	0.3357***	-1.1243***	0.3600***
	(0.245)	(0.080)	(0.246)	(0.087)
> 5 years remaining	-1.6566***	0.2141***	-1.6389***	0.2173***
	(0.285)	(0.059)	(0.291)	(0.063)
< 1 year remaining × performance	-0.1991	0.7564*	-0.0893	0.8417
	(0.170)	(0.123)	(0.144)	(0.117)
1-2 years remaining × performance	-0.6583***	0.5527***	-0.3489**	0.6451***
	(0.189)	(0.100)	(0.153)	(0.102)
2-3 years remaining × performance	-0.7880***	0.4300***	-0.5597***	0.4401***
	(0.222)	(0.098)	(0.191)	(0.096)
3-4 years remaining × performance	-0.0518	0.9280	-0.0338	0.8878
	(0.219)	(0.205)	(0.167)	(0.164)
4-5 years remaining × performance	0.1236	1.1305	-0.0866	0.8587
	(0.319)	(0.374)	(0.294)	(0.288)
> 5 years remaining × performance	0.0776	1.0134	0.0160	0.9739
	(0.340)	(0.334)	(0.260)	(0.277)
Tenure	-0.0244***		-0.0227***	
	(0.009)		(0.008)	
Industry F.E.	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes
N	7,113	7,113	7,113	7,113

Appendix C: Model Fit

Table A-5 compares the fit of our model to alternative models of CEO turnover. Column 1 shows that our baseline OLS model of CEO turnover using contract information, in addition to industry and calendar year FE, has an adjusted R^2 of 2.74%. The model uses the same covariates as Jenter and Lewellen (2014), but no contract information has an adjusted R^2 of 1.67%. Finally, when we add contract information to the model of Jenter and Lewellen (2014) in Column 3, the adjusted R^2 rises substantially (to 2.8%). These statistics suggest that using contract information alone provides more accurate predictions of CEO turnover than using firm size, profitability, dividend payer status, tenure performance, and B/M. Moreover, adding contract information to traditional models of CEO turnover results in a meaningful increase of model precision.

To assess the fit of the hazard models, we use two measures (Harrell et al., 1982). The Harrell's C statistic of 68.3% means that, when one of two CEOs stays in office ("survives") longer than the other one, the probability that the CEO staying in office is assigned a lower hazard ratio by the model plus half the probability that the two have an equal hazard ratio amounts to 68.3%. The Somers' D statistic of 36.7% means that, when one of two CEOs is observed to stay in office longer than another, the model predicts that the CEO staying in office longer is 36.7% more likely to have a lower hazard ratio than the dismissed CEO. Hence, a higher value on both numbers indicates a better model fit.

After assessing the fit of our baseline specification in Column 4 of Table A-5, we compare the fit to a hazard model that uses the same covariates as Jenter and Lewellen (2014), but no contract information. Similarly to the case of the OLS models, both goodness-of-fit measures are lower for the model that does not contain contract information as compared the model in Column 4. Finally, in Column 6, we explore how much more accurately we can predict CEO turnover by including contract information. When comparing the fit to the model in Column 5, Harrell's C increases from 67.6% to 70.8%, i.e. by 3.2 percentage points (or 4.7% in relative terms). Somers' D increases from 35.2% to 41.5%, i.e. by 6.3 percentage points (or 17.9% in relative terms).

Table A-5: Predictive power of CEO turnover models with and without contract terms

Columns 1–3 present estimates from linear probability models of CEO turnover, reporting coefficient estimates with standard errors underneath. Columns 4–6 show the results from Cox proportional hazard models, reporting hazard ratios for CEO turnover with standard errors underneath. Independent variables are defined in Appendix A. Standard errors are clustered at the CEO level. When one of two CEOs stays in office longer than another, Harrell's C-statistic measures the probability that the CEO staying in office is assigned a lower hazard ratio by the model plus half the probability that the two have an equal hazard ratio. When one of two CEOs is observed to stay in office longer than the other, the Somers' D-statistic measures how much more likely it is that the hazard model estimates a lower hazard ratio for the surviving CEO than the dismissed CEO. The data span the years 1992–2008. Asterisks indicate that the estimates are significantly different from zero at the *** 1% level, ** 5% level, and * 10% level.

Model	OLS	OLS	OLS	Cox	Cox	Cox
	(1)	(2)	(3)	(4)	(5)	(6)
Tenure	-0.0012*	-0.0015**	-0.0015**			
	(0.001)	(0.001)	(0.001)			
1-2 years remaining	-0.0186**		-0.0259**	0.9058		0.8419
	(0.008)		(0.010)	(0.100)		(0.102)
2-3 years remaining	-0.0525***		-0.0538***	0.6981***		0.6640***
	(0.011)		(0.013)	(0.083)		(0.087)
3-4 years remaining	-0.0649***		-0.0726***	0.5249***		0.4704***
	(0.008)		(0.010)	(0.071)		(0.072)
4-5 years remaining	-0.0731***		-0.0807***	0.3800***		0.3399***
	(0.010)		(0.013)	(0.077)		(0.079)
> 5 years remaining	-0.0764***		-0.0880***	0.2729***		0.2049***
	(0.010)		(0.011)	(0.060)		(0.057)
ROA		-0.0079	-0.0062		0.4000***	0.4326***
		(0.009)	(0.009)		(0.109)	(0.119)
Tenure performance		0.0079	0.0079		0.7869***	0.7939***
		(0.005)	(0.005)		(0.061)	(0.061)
Ln(assets)		0.0027	0.0028		1.0840**	1.0964***
		(0.002)	(0.002)		(0.037)	(0.037)
Dividend		-0.0365	-0.0343		0.9044	0.9096
		(0.023)	(0.021)		(0.102)	(0.103)
B/M		-0.0066**	-0.0067**		1.0561	1.0365
		(0.003)	(0.003)		(0.068)	(0.072)
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Industry F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	2.74%	1.67%	2.80%			
Harrell's C				0.683	0.676	0.708
Somers' D				0.367	0.352	0.415
N	7,456	6,709	6,709	7,456	6,709	6,709

Appendix D: Contract horizon and risk – robustness

This section contains and describes robustness tests for our regressions of risk on contract horizon.

Compensation. Starting with Holmstrom (1982), the literature has argued that firms need to provide compensation packages that incentivize risk taking and thereby offset the effect of career concerns. Several papers provide evidence that certain types of compensation (e.g. options) are indeed able to induce risk taking (e.g. Agrawal and Mandelker 1987; DeFusco, Johnson, and Zorn 1990; Guay 1999; Coles, Daniel, and Naveen 2006; Low 2009; Chava and Purnanandam 2010; Hayes, Lemmon, and Qiu 2012; Gormley, Matsa, and Milbourn 2013; Shue and Townsend 2017). Option vesting periods induce incentives that vary over time (Ladika and Sautner 2018; Edmans, Fang, and Lewellen 2017; Gopalan et al. 2014). Such time-varying compensation incentives may explain our results.

To disentangle the effects of career concerns and compensation, we add the following control variables to our main regression: the level of compensation (log of TDC1, the sum of cash compensation and equity compensation granted in that year); the sensitivity of the CEO's unvested and vested portfolio of stock and options to stock returns (stock price sensitivity); and the sensitivity of that portfolio to stock return volatility (vega). These sensitivities are computed using Core and Guay's (2002) methodology. These variables are from ExecuComp, which has data for 3,348 observations in our sample. To retain observations with missing values, we add a dummy for missing compensation and set the compensation values to the sample average.

The results are shown in Table A-6. Our results hold when we control for compensation. The magnitude of the coefficient estimate is slightly reduced (0.029 compared to 0.033 in Table 6 Panel A), but it is significant across all specifications. To ensure that our tests are comparable to previous literature, we estimate the model specification of Coles, Daniel, and Naveen (2006). Controlling for Coles, Daniel, and Naveen's (2006) set of variables yields similar results. Overall, we conclude that compensation cannot explain our baseline results.

Excluding the first year of each CEO. Including each CEO's first year in the sample may be problematic because a CEO's starting date rarely coincides with the fiscal year end. Hence, the first fiscal year of a new CEO is likely to contain days under the former CEO. To ensure that our results are not driven by CEOs' first years in office, we discard all firm-years that correspond to a CEO's first year of employment in that position (1,398 observations, or 19%). The results, reported in Table A-6 Column 4, are actually stronger than the baseline regression.

CEO and firm age. Because we use CEO fixed effects in all our main regressions, controlling for tenure (see Column 6 in Table 6 Panel A, and Columns 2, 4, 6 and 8 in Table 7) yields similar results to controlling for any variables that change linearly over time, including age. However, the effect of CEO age on firm outcomes may decrease over time. In addition, the firm's history and position in its life cycle may exhibit a predictable time trend. For example, firms may become less risky over time as they accumulate assets, equity, and expertise. As renewals constitute an "off-trend" jump in the remaining years under contract that does not commove with time, we can use this setting to distinguish the effects of CEO age from the contract. In Table A-6 Column 5 we use the renewal setting and include the natural logarithm of CEO age as a control variable. In Column 6, we control for firm age ranges based on recent work by Arikan and Stulz (2016) Controlling for age does not explain our results: volatility after contract renewals is still significantly higher than in renewal years. In unreported analyses, we also find that the results remain unchanged for capital expenditures and leverage as well.

Lags between decisions and volatility. A spurious relation between contract horizon and risk can emerge if there are systematic lags between investment decisions and volatility increases. It is unlikely that the existence of such lags biases our results for two reasons. First, the relation between contract horizon and both volatility (Table 6), and capital expenditures and leverage (Table 7), shows the same pattern both in the linear specification and for renewals. If capital expenditures or leverage take longer to be incorporated into stock prices, they should follow different patterns. Second, we follow the methodology of Hall et al. (1986) and estimate the average lag per Fama-French 49 industry between R&D expenditures and information availability about innovation output as measured by patent applications. Using this information, we rerun our main regressions from Table 6, Panel A discarding industries that have a lag of 3 years (the longest lag), 2-3 years, or 1-3 years. Both the statistical and the economic significance of our results increases as we discard observations with lags. These results are untabulated.

Renewals prior to contract expiration. Table 2 Panels C and D show that most contracts in our sample are renewed at or close to their expiration. Renewing contracts much before their expiration implies that at any point in time, there are not two, but three options that the board may consider: (i) firing the CEO, (ii) retaining the CEO without offering a renewal, and (iii) retaining the CEO and offering a renewal. If investors are aware that both the second and the third options are available to the board, they should update their information when observing CEO retention *without* renewal for an extended period of time. This most likely increases uncertainty about CEO turnover and therefore corporate policies, leading to an increase in volatility over the course of the contract in cases where the CEO is retained without renewal. This effect would work in the opposite direction compared to the patterns we show in the paper.

CEO and firm survival. Although CEO contract length is predetermined, the decision to replace (or not) the CEO at the end of the contract cycle is endogenous and, therefore, could be related to the investment and volatility patterns that we document. To ease the concern that our results are driven only by CEOs whose contracts are (later) renewed and not by those whose are not, we re-estimate our baseline specifications for the subsample of CEO contracts that are not (eventually) renewed. The results of these regressions, shown in Columns 1-2 in Table A-7, are similar to our baseline results. Further, endogenous firm (non-)survival may also bias our results if some CEOs pursue a particular strategy to maximize the probability of an acquisition, and this strategy produces a volatility pattern different from what we observe. We verify that our results hold if we exclude firms that exit the sample because they are acquired, or for other reasons related to firm performance. These results are unreported for brevity. We conclude that neither CEO nor firm survival has a major effect on our results.

Long vs. short contracts. Another potential concern is that our results may be driven by firms with longer CEO contracts, which are the only ones that have observations with a higher number of years remaining under contract. To alleviate this concern, in Columns 3-6 in Table A-7, we split the sample into CEOs with contracts longer than three years and those with contracts of at most three years. Our results hold in both subsamples. The two coefficients are not statistically different from each other, suggesting that our results are not driven by differences between these two samples. In unreported tests we find similar results for idiosyncratic risk.

Selection into the sample. Because all of our results are estimated with CEO fixed effects, they are valid within the sample of CEOs with fixed-term contracts, a non-trivial percentage of firms within the S&P 500 (see also Gillan et al., 2009). In additional tests, we use variation in the legal treatment of employment contracts to show that selection into the sample of fixed-term contracts does not have a material effect on our results (Internet Appendix G).

Table A-6: Robustness: compensation, first year in office, CEO age, and firm age

This table presents the results of OLS regressions, reporting coefficients with standard errors underneath. The dependent variable is volatility. Independent variables are defined in Appendix A. All regressions contain CEO fixed effects. Standard errors are robust to heteroskedasticity and are clustered at the CEO level. The data span the years 1992–2008. Asterisks indicate that the estimates are significantly different from zero at the *** 1% level, ** 5% level, and * 10% level.

	Dependent variable: volatility					
	Compensation	Coles et al.	Coles et al.	No first year	CEO age	Firm age
	(1)	(2)	(3)	(4)	(5)	(6)
Remaining years	0.029**	0.030**	0.029**	0.037***		
	(0.014)	(0.014)	(0.014)	(0.012)		
Sensitivity of unvested equity grants	1.851	1.631	1.639			
	(1.175)	(1.103)	(1.107)			
Sensitivity of vested equity grants	-0.159	-0.212	-0.209			
	(0.241)	(0.230)	(0.231)			
Vega of unvested equity grants	0.101***	0.112***	0.112***			
	(0.027)	(0.028)	(0.028)			
Vega of vested equity grants	-0.052**	-0.056**	-0.055**			
	(0.021)	(0.022)	(0.021)			
Ln(total compensation)	-0.026	-0.020	-0.017			
	(0.023)	(0.020)	(0.022)			
Cash compensation	-0.009		-0.013			
	(0.028)		(0.028)			
Total assets		1.942***	1.979***			
		(0.731)	(0.728)			
Market-to-book		-0.056**	-0.056**			
		(0.022)	(0.022)			
CAPEX/assets		-0.277	-0.282			
		(0.544)	(0.543)			
R&D/assets		1.110**	1.112**			
		(0.463)	(0.463)			
Leverage		0.483**	0.483**			
		(0.221)	(0.221)			
Compensation data missing (dummy)	0.081	0.093	0.100			
	(0.194)	(0.187)	(0.191)			
Renewal year					-0.058	-0.048
					(0.067)	(0.067)
Year after renewal					0.146**	0.127**
					(0.066)	(0.062)
Log CEO age					-0.944***	
					(0.290)	
Firm age 4-9 years						-0.136
						(0.128)
Firm age > 9 years						-0.712***
						(0.158)
Constant	3.033***	2.996***	2.989***	2.985***	6.437***	3.140***
	(0.200)	(0.198)	(0.201)	(0.031)	(1.165)	(0.135)
CEO F.E.	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.620	0.625	0.625	0.609	0.010	0.036
N	7,456	7,456	7,456	6,058	3,986	3,986
	F-test: renewal = after renewal					
F-test					5.013**	4.045**
p-value					(0.001)	(0.045)

Table A-7: Sample composition effects: robustness to CEO survival and to contract length

This table presents the results of OLS regressions, reporting coefficients with standard errors underneath. The dependent variable is volatility. Columns 1-2 repeat the result of selected models from Table 6 for CEOs whose contracts are not renewed. Each of Columns 3-6 presents regression results for the subsample indicated in the column heading. Short contracts are defined as those with a length of maximum three years; long contracts are those with a length exceeding three years. Variables are defined in Appendix A. Standard errors are robust to heteroskedasticity and are clustered at the CEO level. Asterisks indicate that the estimates are significantly different from zero at the *** 1% level, ** 5% level, and * 10% level.

Subsample	Non-renewed CEOs		Short contracts	Long contracts	Short contracts	Long contracts
	(1)	(2)	(3)	(4)	(5)	(6)
Remaining years	0.032**		0.060**	0.029*		
	(0.016)		(0.027)	(0.017)		
Fraction remaining		0.272***			0.225***	0.300**
		(0.073)			(0.071)	(0.119)
Constant	3.007***	2.927***	2.899***	2.999***	2.806***	2.963***
	(0.036)	(0.043)	(0.062)	(0.039)	(0.07)	(0.039)
CEO F.E.	Yes	Yes	Yes	Yes	Yes	Yes
R^2	63%	63%	66%	69%	67%	69%
N	3,470	3,470	4,279	3,177	4,279	3,177
F-test: equality of slope coefficients for short and long contracts						
F-test			1.040		0.580	
p-value			(0.307)		(0.448)	

Appendix E: Volatility patterns in firms without fixed-term contracts

The results in the paper are consistent with the prediction that CEOs adopt corporate policies in response to contractual protection. Yet, it is possible that CEO contract cycles are correlated with investment opportunities. New CEOs may be hired at times of unusually good or bad investment opportunities, which later revert to the mean. Boards may choose to coordinate the length of CEO employment contracts with natural technological cycles to set a planning horizon for the entire firm. In turn, such a commitment to the CEO and her projects may incentivize employees to work on uncertain projects without fearing a reversal by the next CEO and help the firm attract talent for the new projects (Rotemberg and Saloner 2000; Van der Steen 2005, 2018). Even in the absence of technological cycles, hiring or renewal decisions can create a sudden increase in volatility. If volatility gradually declines over time as the market learns about the CEO's new plans, these "managerial life cycles" may produce the patterns we report.

If it is not the contract that causes the relation between contract horizon and volatility, but rather technological cycles or the board's decision to evaluate and keep the CEO precisely when a volatility cycle has completed, we should see similar volatility cycles also in the *absence* of fixed-term contracts. Therefore, we use CEOs without fixed-term contracts to identify non-causal cycles related to hiring and renewal decisions. We use three different tests in this setting. This Appendix provides supplementary material on the identification of "renewals" from compensation raises (E.1), the matching procedure between firms without fixed-term contracts but significant raises (E.2) or actual jumps in volatility (E.3), and the quality of the match (E.4).

E.1. *Technological cycles do not predict volatility for CEOs employed under at-will contracts*

First, we show that plausible sources of risk cycles (cycles in investment, demand, or asset duration) are not correlated with volatility in firms whose CEOs are employed at will. As described in Section III.B, the average cycles are longer than the average CEO contract. For each firm-year, we compute the number of years left until the end of the cycle. As Panel A in Table A-9 shows, the average difference between this variable and the number of years remaining to the expiration of the CEO's contract is 1.01 for

CAPEX cycles, 1.27 for sales cycles, and 1.20 for depreciation cycles. The large difference between these cycles and the contract horizon indicates that these cycles do not always coincide with contract cycles.

Panel B in Table A-9 shows a regression of volatility on the number of years remaining in the technological cycle and CEO fixed effects.[14] The number of years remaining in a technological cycle is either negatively correlated with volatility, or shows no significant relation to volatility. Thus, the relation between technological cycles and volatility is different for firms without contracts, contradicting the prediction that technological cycles cause volatility, which should be true even in the absence of contracts.

E.2. *Volatility around CEO evaluations*

Regardless of whether or not CEOs have fixed-term contracts, boards evaluate their performance at appropriate intervals and decide whether to keep them. We therefore test for abrupt increases in volatility around the end of firm-specific evaluation cycles. Since we do not observe performance evaluations similar to "renewals" in the absence of contracts, we use two methods to identify them in the sample of firms without fixed-term contracts: an implicit measure based on compensation and *actual* jumps in volatility (in the next section, E.3). Our first attempt at identifying performance evaluations is to look at a noisy outside measure: significant raises in compensation. This logic follows Gao, Harford, and Li (2012) who show that *cuts* in CEO compensation predict turnover and interpret it as a vote of no confidence. If significant pay changes indicate board evaluation events, compensation raises should also indicate a vote of confidence. In unreported results we verify that in our baseline sample of CEOs with fixed-term contracts, contract renewals are indeed followed by a raise of on average 15%, indicating that evaluations coincide with these raises. Indeed, 44% of compensation raises over 15% in the firms with fixed-term contracts take place just after renewals.

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[14] To guard against the concern that we correctly measure the length of technological cycles, but miss their timing by a year, we also use the lag or the lead of the number of remaining years in the cycle, and find similar results. These tests are unreported.

If purely technological reasons drive our results, we should observe similar patterns around evaluations of CEOs without fixed-term contracts. However, Panel C in Table A-9 shows that this is not the case. On the contrary, CEOs without fixed-term contracts are associated with more volatility (Columns 1 and 2), idiosyncratic risk (Columns 5 and 6), but not systematic risk (Columns 3 and 4), and investment just before, not after evaluations. The difference between the year before and after the evaluation is not significant. These results are robust to controlling for tenure and age. Hence, to the extent that, similarly to the contract renewal for fixed-term CEOs, compensation raises to CEOs without such contracts are a good measure of evaluations, we do not observe abrupt increases in volatility around these events. These tests suggest that our results are not driven by CEOs being reappointed based on new ideas that they may implement in a new technological cycle.

E.3. *Matching on volatility peaks*

Raises in compensation may happen for reasons other than evaluations for new ideas, so they may not provide an accurate enough measure of the timing of a firm-specific cycle. For example, some firms may reward the CEO for carrying out those ideas, netting out the volatility-effects of compensation. If a large increase in volatility indicates the start of a firm-specific cycle, then also firms *without* fixed-term contracts should exhibit these cyclical volatility patterns. To see whether abrupt increases in volatility decrease gradually in conjunction with evaluation cycles, we also explicitly choose firms (without fixed-term contract) with large volatility increases. As a cutoff, we use the 20bps rise in volatility around renewals from our baseline results in Table 6.

To find the most likely cycle length for a firm without a fixed-term contract, we employ a matching procedure: we match each firm without a fixed-term contract and with a volatility peak to another firm whose CEO is employed under a fixed-term contract and has similar characteristics. We then use the contract length of the fixed-term CEO as the predicted length of the technological cycle for the firm without a fixed-term CEO contract. Section E.4 below provides more detail on the matching procedure. We then test whether volatility follows a predictable pattern of decline after a spike, similarly to our results for fixed-term contracts.

Out of a total of 4,588 observations, there are 944 such “volatility-peak” observations. We discard 74 observations in the year of a CEO turnover event, because the rise in volatility can be related to speculation about succession and/or learning about CEO ability as in Pan, Wang, and Weisbach (2015). We match the remaining 871 observations to observations under fixed-term contracts where (i) the CEO was renewed and thus was allowed to start a new cycle; (ii) the firm is in the same (FF 48) industry; (iii) in the remaining set of observations, the firm-year is closest to the at-will observation in size (total assets) and market-to-book. We are able to match 850 observations, with 21 (2.4%) unmatched because there is no renewed fixed-term CEO in the same industry. Section E.4 discusses match quality in detail.

Once the matching is complete, we use the contract length of the matched fixed-term CEO to measure the technological cycle of the at-will CEO. If firms choose contracts to have the same length as technological cycles, then the technological cycle of the CEO employed at-will should be similar as the contract length of a similar fixed-term CEO. We then regress volatility on the remaining time in the imputed technological cycle. In this analysis, we exclude the initial observations with the volatility peak, as, by construction, this would hard-wire a correlation between remaining years and volatility.

Panel D in Table A-9 shows the results. Column 1 contains results after matching without requiring an exact match on industry, and Column 2 shows the results if we require an exact match on industry. We find no significant relation between the number of remaining years in the technological cycle estimated using similar firms with fixed-term contracts in any of our specifications. Using this alternative method to infer the timing of technological cycles, we again conclude that, unlike for CEOs employed under fixed-term contracts, volatility does not exhibit the same significant positive correlation with time remaining under contract (time remaining in the cycle) for CEOs employed at will.

In Columns 3-4, we combine the matching procedure with the compensation raises. We relate volatility of firms without fixed-term contracts to a “contract horizon” obtained from matched firms where a renewal contract started in the same year as the compensation peak in the firm without a contract (our proxy for an evaluation year). We perform a nearest neighbor matching to find, for each at-will firm-year observation with a raise of over 15%, a matching fixed-term contract renewal year using two different sets of requirements. First, we match observations on size (measured as total assets) and Tobin's Q. Second, we use nearest neighbor matching for these two variables, but *also* require that the matched fixed-term observation should be in the same (Fama-French 48) industry as the at-will observation. Finally, after matching the observations, we use the contract length of the fixed-term observation to measure contract cycles for the CEO employed at will. The increase in compensation pins down the start of the cycle, making it possible to obtain the number of years remaining in the cycle. Again, we find that the number of years remaining in the cycle is not significantly related to volatility.

E.4. *Match quality*

Table A-8 provides detailed information on the quality of the match between the at-will observations. Panels A and C of Table A-8 show our tests on match quality for volatility peaks. Panel A shows descriptive statistics of selected variables, as well as paired t-tests of equality between the at-will and matched fixed-term observations for the matching sample where we do not require an exact industry match. First, we examine the quality of the match for the matching variables: total assets and Tobin's Q. Total assets average $2,115 million for at-will observations and $2,125 million for fixed-term observations. Although the difference is statistically significant, on average, it is economically small at 0.46% of the mean value. Tobin's Q is very closely matched at 2.44 for at-will and 2.42 for fixed-term observations. The results of t-test suggest that we cannot reject the null that the average difference within matched pairs is zero.

To have a more precise sense of match quality, we also examine differences along a number of variables that we *do not* match on. For example, even though we do not match observations on tenure, the tenure of at-will and fixed-term CEOs is quite similar (7.31 years vs. 6.91 years, respectively). The difference is not statistically significant. CEOs of at-will firms are significantly (1.93 years) younger than their matched counterparts. At-will observations have higher volatility, higher idiosyncratic risk, and higher beta than matched fixed-term observations. Although capital expenditures are not significantly different across the two groups, at-will firms have higher R&D. Finally, we also examine the length of the technological cycles for the at-will and matched fixed-term firms. At-will firms appear to have longer cycles when we use peaks of capital expenditures (sales, or depreciation) to infer cycle length. However, when using troughs of the respective variables, we find that sales cycles of at-will firms are significantly longer (8.52 years vs. 6.45years, respectively). With this one exception, most technological cycle variables have higher means in the fixed-term sample, though the difference is not always statistically significant.

Panel C in Table A-8 shows similar statistics for the case when we perform the matching requiring an exact match on industry. (When performing this match, we drop 2 at-will firm-year observations from the sample, because there is no exact industry match that has a contract renewal. Hence, the averages for the at-will sample may also differ slightly from the values in the first matching exercise.) Not surprisingly, the industry match requirement renders the match on the two other match variables—size and Tobin's Q—less precise. The average matched firm with fixed-term contracts is now smaller, with $1,977 in total assets. The difference is statistically significant; however, in economic terms, it is only 6.54% of the sample mean. Tobin's Q is now significantly lower for matched fixed-term observations at 2.14 (vs. 2.44).

Turning to other variables that we do not include in the matching, we see that tenure is still quite similar across observations. As before, age, volatility, idiosyncratic risk, beta, and R&D are higher for at-will firms. Capital expenditures are now also significantly higher (by the margin of 12.94%). Finally, there is no clear pattern regarding the length of technological cycles: at-will firms have longer cycles according to some measures, but shorter cycles according to others.

We also evaluate the quality of the match when we use compensation increases, rather than volatility peaks, to infer CEO renewal cycles at at-will firms. Panel B in Table A-8 shows statistics of match quality for matches formed based on total assets and Tobin's Q, not requiring that the firms be in the same industry. First, from the first two rows in Table A-6, it is evident that the matching is almost perfect along the dimensions that we impose. Total assets average $1,305 million for at-will observations and $1,299 million for fixed-term observations. The difference is not statistically significant and is economically small (0.46% of the mean value). Tobin's Q is very closely matched at 2.32 for both groups. The results of t-test do not reject the null that the average difference within matched pairs is zero.

Second, turning to characteristics that we do *not* match on, it appears that CEO age and tenure are also closely matched. Specifically, by imposing the match on total assets and Tobin's Q, we actually end up with CEO tenure values that are quite similar at 4.46 years for at-will and 4.62 for matched fixed-term CEOs. CEO age is also remarkably similar at 53.13 and 53.46 years. Neither of these differences is statistically significant. Similarly to the matching exercise based on volatility peaks, we once again find that volatility, idiosyncratic risk and beta, as well as capital expenditures and R&D are higher for at-will firms than for matched fixed-term firms. These differences underscore the importance of controlling for CEO (or firm) fixed effects in our regressions. Finally, measures of capital expenditure, sales, and depreciation cycles tend to be somewhat higher for fixed-term observations; however, this result reverses if we use troughs in sales to measure cycles (similarly to the matching exercise using volatility peaks) and are not statistically significant for most variables.

In Panel D of Table A-8, statistics are similar to those of the case when we perform the matching requiring an exact match on industry. Once again, the industry match requirement renders the match on the two other match variables—size and Tobin's Q—less precise. The average matched firm is now smaller, with $1,287 million in total assets vs. the $1,305 million of the at-will firms. While the difference is statistically significant, in economic terms, it is only 1.37% of the sample mean. Tobin's Q is now significantly lower for matched fixed-term observations (2.17 vs. 2.34, respectively). However, the difference is small in economic terms (6.5% of the mean).

Turning to other variables that we do not include in the matching, tenure is slightly higher for matched fixed-term observations than for at-will observations (4.71 vs. 4.37 years). In economic terms, the difference of 1/3 year is still moderate. CEO age is not significantly different across at-will and matched observations. As before, volatility, idiosyncratic risk, beta, and R&D are higher for at-will firms.

Capital expenditures are not significantly different, however. Finally, there is no clear pattern regarding the length of technological cycles: at-will firms have longer cycles according to some measures, but shorter cycles according to others.

Table A-8: Match quality

This table presents summary statistics for observations of CEOs employed at will and matched observations of CEOs employed under fixed-term contracts, using the nearest neighbor matching method. In each panel, we show the means of the two match variables, total assets and Tobin's Q, as well as a number of additional variables that were *not* used to create the match. First, we show the means of the variables for the at-will group and the matched fixed-term observations. The asterisks indicate the result of a paired t-test examining whether the average difference across matched pairs is zero. Next, we show the average difference in terms of the levels of the variable and finally, we show the average difference as a percentage. Panels A and C, we identify evaluation points in at-will contracts based on volatility peaks, defined as increases of over 20% in volatility. In Panels B and D, we identify evaluation points based in at-will contracts based on compensation peaks, defined as increase of over 15% in total compensation, the average raise in compensation following renewals in our sample of fixed-term contracts. In the Panels A and B, we match on total assets and Tobin's Q. In Panels C and D, we match on total assets and Tobin's Q and additionally require the matched observation to be in the same Fama-French 49 industry as the at-will observation.

	Exact match on industry: No									
	Panel A: At-will cycle: volatility peak					**Panel B: At-will cycle: compensation peak**				
	At-will	Fixed-term	Diff.	Levels	as %	At-will	Fixed-term	Diff.	Levels	as %
Test of equality for the match variables										
Total assets	2115.38	2125.15	***	-9.76	-0.46%	1304.77	1298.78		5.99	0.46%
Tobin's Q	2.44	2.42		0.02	0.87%	2.32	2.32		-0.01	-0.22%
Test of equality for selected other variables										
Tenure	7.31	6.91		0.40	5.46%	4.46	4.62		-0.15	-3.43%
Age	54.37	56.29	***	-1.93	-3.54%	53.13	53.46		-0.32	-0.61%
Volatility	3.74	2.60	***	1.14	30.48%	3.48	3.04	***	0.45	12.86%
Idiosyncratic risk	3.37	2.36	***	1.01	29.93%	3.24	2.83	***	0.41	12.57%
Beta	1.09	0.99	***	0.09	8.68%	0.96	0.90	**	0.06	6.71%
Capital expenditures	0.07	0.07		0.00	5.97%	0.08	0.06	***	0.01	13.89%
R&D	0.06	0.02	***	0.04	74.58%	0.08	0.04	***	0.05	54.80%
Capex cycle - peaks	4.67	5.45	***	-0.78	-16.74%	4.67	5.17	***	-0.50	-10.71%
Sales cycle - peaks	5.15	5.46		-0.31	-5.97%	4.71	5.31		-0.60	-12.73%
Depreciation cycle - peaks	4.67	6.08	*	-1.42	-30.36%	4.66	4.87		-0.21	-4.52%
Capex cycle - troughs	6.01	6.92	***	-0.90	-15.01%	5.75	6.03	*	-0.28	-4.87%
Sales cycle - troughs	8.52	6.45	**	2.07	24.29%	6.51	6.05		0.46	7.05%
Depreciation cycle - troughs	6.28	7.21		-0.93	-14.81%	5.98	6.08		-0.10	-1.62%

	Exact match on industry: Yes									
	Panel C: At-will cycle: volatility peak					**Panel D: At-will cycle: compensation peak**				
	At-will	Fixed-term	Diff.	Levels	as %	At-will	Fixed-term	Diff.	Levels	as %
Test of equality for the match variables										
Total assets	2115.38	1977.01	***	138.37	6.54%	1304.77	1286.89	**	17.88	1.37%
Tobin's Q	2.44	2.14	**	0.29	12.10%	2.32	2.17	***	0.15	6.50%
Test of equality for selected other variables										
Tenure	7.29	6.79		0.50	6.81%	4.37	4.71	***	-0.33	-7.62%
Age	54.31	56.33	***	-2.02	-3.71%	53.05	53.66		-0.61	-1.14%
Volatility	3.69	2.70	***	1.00	27.00%	3.46	3.12	***	0.34	9.95%
Idiosyncratic risk	3.33	2.46	***	0.87	26.07%	3.22	2.92	***	0.30	9.35%
Beta	1.10	0.93	***	0.17	15.36%	0.94	0.88	***	0.06	6.45%
Capital expenditures	0.07	0.06	**	0.01	12.94%	0.08	0.07		0.01	13.38%
R&D	0.06	0.02	***	0.04	70.46%	0.09	0.03	***	0.05	61.74%
Capex cycle - peaks	4.88	5.39	**	-0.52	-10.61%	4.70	5.13	***	-0.43	-9.14%
Sales cycle - peaks	5.25	5.50		-0.25	-4.76%	4.78	5.53		-0.75	-15.70%
Depreciation cycle - peaks	5.58	4.58		1.00	17.92%	3.88	3.56		0.31	8.06%
Capex cycle - troughs	5.92	6.77	***	-0.86	-14.45%	5.67	6.07		-0.39	-6.95%
Sales cycle - troughs	8.61	5.74	**	2.87	33.33%	7.20	6.24	**	0.96	13.35%
Depreciation cycle - troughs	6.94	6.67		0.27	3.88%	6.16	6.41		-0.25	-4.06%

Table A-9: Alternative cycles

Panel A presents descriptive statistics of alternative cycle and contract horizon measures. Panel B reports the coefficients of years remaining (and standard errors underneath) for 18 regressions where the dependent variable is volatility and the independent variables are the number of years remaining in technological cycle (or its lead, or its lag, respectively, as indicated by the row caption) and CEO fixed effects, and where the sample is CEOs employed under at-will contracts. We infer the length of technological cycles from peaks (troughs) in capital expenditures, sales, and depreciation (indicated in the column caption). Panel C shows regressions of volatility on dummy variables indicating the year of a 15% or larger raise in compensation and the year after. Panel D shows estimates from regressions of volatility on the number of years remaining in matched contract cycles for CEOs employed at will. In Panel D, we infer cycle peaks from peaks in volatility in Columns 1-2, and from increases in compensation in Columns 3-4. We match firms based on size and Tobin's Q in all columns. In Columns 2 and 4 in addition, we require an exact match on industry. For firms with multiple peaks, we estimate cycle length as the average over time. All regressions in Panels B, C and D include CEO fixed effects. Standard errors in Panels B, C, and D are robust to heteroskedasticity and are clustered at the CEO level. Asterisks indicate that the estimates are significantly different from zero at the *** 1% level, ** 5% level, and * 10% level.

Panel A: Descriptive statistics of alternative cycle and predicted contracts

	Length (in years)	Within-firm st.dev.	Difference to actual contract (in remaining years)
Cycle length - peaks			
CAPEX	4.98	0.51	1.01
Sales	5.50	0.28	1.27
Depreciation	4.84	0.23	1.20
Cycle length - troughs			
CAPEX	5.91	0.78	1.11
Sales	6.49	0.53	1.49
Depreciation	6.29	0.49	1.33

Panel B: Remaining years in technological cycles and volatility for CEOs under at-will contracts

Cycle	Peak-to-Peak			Trough-to-Trough		
	CAPEX	Sales	Depreciation	CAPEX	Sales	Depreciation
	(1)	(2)	(3)	(4)	(5)	(6)
Remaining years	-0.043**	-0.092***	-0.100***	-0.084***	-0.086	-0.419*
	(0.021)	(0.031)	(0.032)	(0.028)	(0.102)	(0.241)
CEO F.E.	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: Risk-taking in firms with no fixed-term contracts						
	Volatility	Volatility	Beta	Beta	Idiosyncratic risk	Idiosyncratic risk
Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)
Year before raise	0.215***	0.183***	0.000	0.000	0.314***	0.274***
	(0.062)	(0.070)	(0.000)	(0.000)	(0.058)	(0.064)
Year after raise	0.1	-0.004	0.000	0.000	0.184*	0.063
	(0.111)	(0.130)	(0.001)	(0.001)	(0.102)	(0.121)
Tenure		-0.024***		0.000		-0.024***
		(0.007)		(0.000)		(0.007)
CEO F.E.	Yes	Yes	Yes	Yes	Yes	Yes
F-test (before=after)	0.75	1.47	0.80	0.43	1.13	2.12
p-value	(0.388)	(0.226)	(0.371)	(0.511)	(0.289)	(0.146)
R^2	0.632	0.633	0.279	0.276	0.627	0.633
N	1,917	1,863	1,917	1,863	1,917	1,863

Panel D: Remaining years in cycles and volatility for CEOs without fixed-term contracts				
Cycle length matched on	Volatility peaks		Compensation increases	
	(1)	(2)	(3)	(4)
Remaining years - matched cycle	0.009	-0.007	-0.027	-0.005
	(0.031)	(0.034)	(0.027)	(0.047)
Constant	2.856***	2.890***	3.071***	3.022***
	(0.072)	(0.072)	(0.064)	(0.109)
CEO F.E.	Yes	Yes	Yes	Yes
R-squared	59.6%	59.1%	61.5%	61.3%
N	1,432	1,411	1,331	1,327
Exact industry match?	No	Yes	No	Yes

Appendix F: Magnitudes in terms of turnover probability

Our results suggest a causal effect of career concerns on volatility. In this section, we take this channel more seriously and estimate magnitudes in a two-stage least squares (2SLS) regression that uses contract horizon as a predictor for turnover probability in the first stage.

Column 1 in Table A-10, Panel A shows our baseline model, estimated using an instrumental variables (IV) approach: the endogenous regressor in the second stage is the dummy for CEO turnover, which we instrument using the remaining years on the CEO's contract. The first stage is estimated using the specification shown in Column 1 of Table 4. The estimated model reveals a strong negative correlation between predicted turnover probability and return volatility. Lower CEO turnover probability is associated with significantly greater volatility. The values in Column 1 indicate that an increase of one standard deviation in turnover probability corresponds to a reduction of 31 basis points (bp) in return volatility. The 31 basis points correspond to 18% of one standard deviation in return volatility. Columns 2 and 3 show that these results also hold when we use our alternative measures of volatility: the mean of the absolute value of daily returns and the median of the absolute value of daily returns.

Similarly to our analysis in Table 6, we decompose volatility into systematic and idiosyncratic risk. Column 2 in Panel A of Table A-10 shows that the negative relation between return volatility and turnover probability is driven by idiosyncratic risk. An increase of one standard deviation in the likelihood of turnover corresponds to a 36-bp decrease in idiosyncratic risk (which is 20% of one standard deviation of idiosyncratic risk). The contract horizon variables are sufficiently strong instruments. We report the first-stage F-statistic at the bottom of Panel A. The value of 32.11 far exceeds the conventional cutoff of 10, and also exceeds the 5% critical values given by Stock and Yogo (2005) for various levels of 2SLS bias relative to OLS. Thus, the bias of our estimates is below 5%. Column 3 of Table A-10, Panel A shows that an increase in turnover probability is associated with no (or very minor) reductions in systematic risk.

We also replicate our evidence on the sources of risk. Column 4 in Panel A of Table A-10 shows the negative association between turnover probability and capital expenditures (normalized by lagged total assets). An increase of one standard deviation in the likelihood of turnover corresponds to a 1.4-bp decrease in capital expenditures (or 15% of one standard deviation). The results in Column 5 (Table A-10, Panel A) show a significant negative correlation between distance to contract expiration and two leverage factors. The economic significance of this effect is smaller than for investment. An increase of one standard deviation in turnover probability corresponds to a decrease of 0.017 in leverage, which amounts to 5% of the standard deviation of leverage.

The 2SLS specification uses a linear model to predict turnover. In Panel B we show that the estimates are similar when we estimate the turnover probability using a Cox hazard model. Here, we estimate the standard errors using the method of Murphy and Topel (1985).

In Panel C, we address another possibility of a nonlinear relation between turnover likelihood and return volatility. The career outcome for a CEO with extremely high or low turnover probability can be so certain that there is no compelling reason for to change behavior. We regress volatility on dummy variables for CEOs with turnover probability in the lowest (“low”), highest (“high”), and third and fourth (“medium”) quintile. That is, our baseline comparison group is the second quintile. We find the same pattern when using a variety of other classifications. Consistent with our baseline results, for CEOs with high turnover probability, volatility is lower (than the baseline) by 36 bp. Medium turnover probability leads to a 23-bp decrease in volatility. Low turnover probability is not significantly related to volatility.

Table A-10: Estimates from two-stage models

This table presents the results of 2SLS regressions (Panel A) and of OLS regressions (Panels B and C), reporting coefficients with standard errors underneath. All models are estimated with CEO fixed effects, and a constant which we do not report. The dependent variable is stated in the column heading. Variables are defined in Appendix A. Standard errors are robust to heteroskedasticity and are clustered at the CEO level. The standard errors in Panels B and C are estimated using the method of Murphy and Topel (1985). The data span the years 1992–2008. Asterisks indicate that the estimates are significantly different from zero at the *** 1% level, ** 5% level, and * 10% level.

Panel A: 2SLS					
Dependent variable	Volatility	Idiosyncratic risk	Beta	Investment	Leverage
Turnover estimation	IV	IV	IV	IV	IV
	(1)	(2)	(3)	(4)	(5)
Turnover probability	-2.349***	-2.825***	0.001	-0.113***	-0.127***
	(0.243)	(0.242)	(0.001)	(0.014)	(0.037)
CEO F.E.	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	62%	63%	39%	54%	71%
N	7,456	7,456	7,456	7,456	7,456
F-test of weak instruments: coefficients of remaining years all = 0					
F-test			32.11**		

Panel B: Cox hazard model, linear specification					
Dependent variable	Volatility	Idiosyncratic risk	Beta	Investment	Leverage
Turnover estimation	Cox	Cox	Cox	Cox	Cox
	(1)	(2)	(3)	(4)	(5)
Turnover probability	-0.517***	-0.634***	0.001	-0.029***	-0.03
	(0.118)	(0.111)	(0.0004)	(0.006)	(0.019)
CEO F.E.	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	62%	63%	39%	54%	71%
N	7,456	7,456	7,456	7,456	7,456

Panel C: Cox hazard model, nonlinear effects					
Dependent variable	Volatility	Idiosyncratic risk	Beta	Investment	Leverage
Turnover estimation	Cox	Cox	Cox	Cox	Cox
	(1)	(2)	(3)	(4)	(5)
Low turnover probability	-0.037	0.016	-0.0003	0.005	0.013
	(0.057)	(0.054)	(0.0002)	(0.003)	(0.01)
Medium turnover probability	-0.193***	-0.242***	-0.0004**	-0.012***	-0.016**
	(0.068)	(0.064)	(0.0002)	(0.003)	(0.007)
High turnover probability	-0.246***	-0.244***	0.00030	-0.013***	0.002
	(0.047)	(0.045)	(0.0003)	(0.004)	(0.01)
CEO F.E.	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	62%	63%	39%	54%	71%
N	7,456	7,456	7,456	7,456	7,456

Appendix G: Selection into the Sample

Although most companies disclose the length of their CEO's employment contract, some may omit this legally required disclosure, even though their CEO is under a fixed-term contract. To put the number of our sample contracts into perspective: Gillan, Hartzell, and Parrino (2009) survey all S&P 500 firms in 2000 and find that 255 (or 45%) of their CEOs had employment contracts. Our sample contains 236 contracts that were in place with S&P 500 firms in 2000; therefore, 19 (or 3.8%) are missing. As S&P 500 firms tend to be large, they are likely to have better disclosure quality. For this reason, a higher percentage of omitted contracts in the rest of our sample is likely.

Since our analysis links the number of years remaining on a CEO's contract to risk, we necessarily focus on CEOs with fixed-term contracts. However, CEOs with fixed-term contracts may differ from other CEOs. Likewise, there could be differences among the firms that offer these various contract types. To control for the selection bias that could emerge from using a non-random sample, we follow the approach of Heckman (1979) and use the choice regression described next to compute the inverse Mills ratio. We use a state law characteristic for the identifying restriction: the at-will exception rule of good faith and fair dealing (henceforth referred to as "the exception rule"). This state-wide rule prohibits terminations made in bad faith or motivated by malice.[15] This rule protects rank-and-file employees with relatively shorter contracts (or even without contracts), which makes such forms of employment more attractive. The ensuing popularity of shorter contracts makes it difficult for executives to negotiate longer contracts for themselves. However, direct judicial consequences of this rule for CEOs are probably limited because they are already protected by individual contracts. The applicability of at-will exceptions is listed by state in Table A-11 (cf. Walsh and Schwarz 1996; Muhl 2001). In most states, these rules were adopted between 1960 and 1980 (i.e. before our sample's time frame) in response to debates driven both by that era's political sentiments and the particularities of some precedent cases.

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[15] There are two other exceptions that are less relevant for our purposes. Under the *public policy* exception, dismissal is not allowed if it violates the public policy (or a statute) of the state. Under the *implied contract* exception, an employee can dispute dismissal by proving the existence of an implicit (i.e. not written) contract.

To identify the firms that do not disclose their CEO contracts, we use the following determinants of disclosure quality: firm size, number of equity issuances, and standard deviation of analyst forecasts. Lang and Lundholm (1993) and Brown and Hillegeist (2007) show that these variables affect disclosure quality as measured by (the since discontinued) AIMR scores. As the determinants are fairly generic firm characteristics, we also include a variable that indicates whether the firm made any earnings restatements in the relevant year (as reported by Audit Dynamics).

We follow Gillan et al. (2009) in choosing other determinants of long-term contracts. These authors argue that labor market risk should be relevant for choosing contract terms; that is, firms operating in riskier industries must more frequently renegotiate contracts. Based on Gillan et al. (2009), we use the following indicators of industry risk: homogeneity of stock returns, volatility of median sales, and annual rate of survival. Both CEO and board characteristics should also affect contract negotiations. In particular, there is less uncertainty about incumbent CEOs, especially when they have been in their position for a long time. A similar argument can be made for older CEOs with a lengthy track record. We control for CEO incumbency, age, and tenure, and use the governance index of Gompers, Ishii, and Metrick (2003) to control for the board's power. To ensure that geographical effects are in fact due to at-will exceptions and not to other legal differences across states, we control—with respect to the state of incorporation—for such geographical indices as the anti-takeover index of Bertrand and Mullainathan (1999) and the anti-competition enforceability index of Garmaise (2011). All regressions contain industry and year fixed effects to control for exogenous shocks to the labor market.

Table A-12 presents the results. Column 1 reports the values for a probit specification that predicts the choice of entering into a fixed-term contract in terms of all the aforementioned variables. Column 2 uses the variables that are found to be significantly associated with contract choice in Column 1 to predict the choice of accepting a fixed-term contract. This regression is used to compute inverse Mills ratios for the regressions reported in Section V.

In line with Miles (2000), the states with the exception rule are significantly less likely to issue fixed-term contracts. As for the two other geographical variables, the anti-takeover (resp., anti-competition enforcement) index is significantly (resp., marginally) related to fixed-term contracts. Therefore, we find that CEOs are more likely to enter fixed-term and longer contracts if anti-takeover laws are in force, which is consistent with the complementarity of external and internal governance (Cremers and Nair 2005).

We find little evidence that firms with lower disclosure quality are less likely to disclose a contract. In defense of the disclosure bias hypothesis, the firms with more equity issuances are more likely to be in the sample, and such firms face more disclosure requirements. That said, smaller firms, as well as firms with more earnings restatements, are less likely to be in the sample of CEOs with a (disclosed) fixed-term contract. That these variables are related to the incidence of such contracts indicates that they measure firm characteristics unrelated to disclosure. The standard deviation of analyst forecasts is not significantly related to contract choice, which also suggests that information asymmetry is of little relevance to sample selection.

Industry homogeneity is associated with fewer contracts. In homogeneous industries, both CEO and firm have more outside options and, accordingly, an employment contract is less important. Our industry risk variables are not significantly related to contract choice. Incumbent CEOs are more likely to receive a fixed-term contract. Older and longer-tenured CEOs are more likely to have no contract, possibly because firms are less uncertain about their potential. The Gompers, Ishii, and Metrick (2003) governance index is positively associated with a firm's use of contracts. This measure is lower for firms with high shareholder orientation. The positive association suggests that a board of directors with less bargaining power is more likely to offer a fixed-term contract.

Table A-13 repeats the regressions from Column 1 of Panel A in Table 6 and Columns 1, 3, 5, and 7 in Table 7 of the paper controlling in addition for the inverse Mills ratio. Our results continue to hold after we control for sample selection.

Table A-11: At-will exceptions

This table summarizes the at-will exceptions by state, reporting the laws that prevail in each of the US states as of 2001. The data are from Muhl (2001) and Walsh and Schwartz (1996).

		At-will exceptions		
Code	**State**	**Public policy**	**Implied contract**	**Good faith and fair dealing**
AL	Alabama	0	1	1
AK	Alaska	1	1	1
AZ	Arizona	1	1	1
AR	Arkansas	1	1	0
CA	California	1	1	1
CO	Colorado	1	1	0
CT	Connecticut	1	1	0
DC	District of Columbia	1	1	0
DE	Delaware	1	0	1
FL	Florida	0	0	0
GA	Georgia	0	0	0
HI	Hawaii	1	1	0
ID	Idaho	1	1	1
IL	Illinois	1	1	0
IN	Indiana	1	0	0
IA	Iowa	1	1	0
KS	Kansas	1	1	0
KY	Kentucky	0	1	0
LA	Louisiana	0	0	0
ME	Maine	0	1	0
MD	Maryland	1	1	0
MA	Massachusetts	1	0	1
MI	Michigan	1	1	0
MN	Minnesota	1	1	0
MS	Mississippi	1	1	0
MO	Missouri	1	0	0
MT	Montana	1	0	1
NE	Nebraska	0	1	0
NV	Nevada	1	1	1
NH	New Hampshire	1	1	0
NJ	New Jersey	1	1	0
NM	New Mexico	1	1	0
NY	New York	0	1	0
NC	North Carolina	1	0	0
ND	North Dakota	1	1	0
OH	Ohio	1	1	0
OK	Oklahoma	1	1	0
OR	Oregon	1	1	0
PA	Pennsylvania	1	0	0
RI	Rhode Island	0	0	0
SC	South Carolina	1	1	0
SD	South Dakota	1	1	0
TN	Tennessee	1	1	0
TX	Texas	0	0	0
UT	Utah	1	1	1
VT	Vermont	1	1	0
VA	Virginia	1	0	0
WA	Washington	1	1	0
WV	West Virginia	1	1	0
WI	Wisconsin	1	1	0
WY	Wyoming	1	1	1

Table A-12: Choice of contract type

This table presents marginal effects from probit regressions and standard errors (in parentheses) that are robust to 35eteroscedasticity and clustered by year. The unit of observation is a firm-year. Models are estimated using 7,456 firm-years of CEOs with fixed-term contracts and 23,182 firm-years of CEOs without fixed-term contracts. The dependent variable is a dummy set to 1 if the firm and the CEO have a fixed-term contract (and to 0 otherwise). *Exception rule* is a dummy variable equal to 1 if the contract is governed by the law of a state with a good faith & fair dealing at-will exception. *Anti-takeover* is a dummy variable for a state with "business combination laws" according to Bertrand and Mullainathan (1999). *Garmaise* is the index of Garmaise (2009). *Restatement* is a dummy variable equal to 1 if the firm files an earnings restatement in the current year. *Industry homogeneity* is the median (across all firms of one of the 49 Fama-French industries) of the percentage variation in monthly stock returns that is explained by an equally weighted industry index; market-adjusted returns are annual stock returns adjusted by the value-weighted CRSP index. *Industry sales volatility* is the (FF 49) industry average of variance in sales over the past seven years. *Industry survival rate* is the industry rate of year-to-year survival within Compustat. *Governance index* is the index developed by Gompers, Ishii and Metrick (2003). All other variables are defined in Appendix A. The data span the years 1992–2008. Asterisks indicate that the estimates are significantly different from zero at the *** 1% level, ** 5% level, and * 10% level.

	Dependent variable: Fixed-term contract		
		(1)	(2)
Geography	Exception rule	-0.248***	-0.240***
		(0.01)	(0.01)
	Anti-takeover	0.108***	0.102***
		(0.01)	(0.01)
	Garmaise	-0.004*	
		(0.003)	
Disclosure quality	Assets	-0.275***	-0.275***
		(0.02)	(0.02)
	Log number of SEOs	0.581***	0.574***
		(0.01)	(0.01)
	Restatement	0.175***	0.176***
		(0.05)	(0.05)
	Analyst forecast SD	-0.001	
		(0.0005)	
Risk	Industry homogeneity	-0.788**	-1.353***
		(0.34)	(0.23)
	Industry sales volatility	0.041	
		(0.14)	
	Industry survival rate	0.191	
		(0.34)	
Governance	Renewal	0.317***	0.317***
		(0.09)	(0.09)
	Age	-0.006***	-0.006***
		(0.002)	(0.002)
	Tenure	-0.040***	-0.040***
		(0.003)	(0.003)
	Governance index	0.110***	0.110***
		(0.007)	(0.007)
Fixed effects	Industry	Yes	Yes
	Year	Yes	Yes
Constant		-3.157***	-2.937***
		(0.36)	(0.18)
N		30,638	30,638

Table A-13: Robustness of main results to selection into the sample

This table presents the results of OLS regressions, reporting coefficients with standard errors underneath. The dependent variable is listed in the column heading. The Inverse Mills' ratio is estimated from the model presented in Table A-12, Column A. All regressions include CEO fixed effects. Standard errors are robust to heteroskedasticity and are clustered at the firm level. The data span the years 1992–2008. Asterisks indicate that the estimates are significantly different from zero at the *** 1% level, ** 5% level, and * 10% level.

Dependent variable	Volatility	Beta	Idiosyncratic risk	Investment	Leverage
	(1)	(2)	(3)	(4)	(5)
Remaining years	0.035**	-0.00005	0.047***	0.003***	0.006***
	(0.014)	(0.00004)	(0.013)	(0.000)	(0.002)
Inverse Mills' ratio	6.126***	0.017	5.529***	0.019	-0.362
	(2.158)	(0.030)	(1.755)	(0.260)	(0.754)
Constant	1.066	0.004	0.996*	0.053	0.331
	(0.689)	(0.010)	(0.561)	(0.083)	(0.241)
CEO F.E.	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	61%	39%	63%	55%	70%
N	7,456	7,456	7,456	7,456	7,456

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