CEO Stress and Life Expectancy: The Role of Corporate Governance and Financial Distress*

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Abstract

Optimal pay-for-performance aligns managerial incentives with shareholder interests in the presence of private benefits. We focus on one source of private benefits—CEOs' health and longevity—and show that stricter monitoring regimes have significant adverse consequences for managers' long-term health. Our identification exploits the introduction of anti-takeover laws in the mid-1980s, as well as exposure to industry-wide downturns. Using hand-collected data on the dates of birth and death for more than 1,600 CEOs of large, publicly listed U.S. firms, we estimate that CEOs' lifespan increases by around two years when insulated from market discipline via anti-takeover laws. CEOs also stay on the job longer, with no evidence of a compensating differential in the form of lower pay. We estimate similar effects on longevity from exposure to industry-wide downturns during a CEO's tenure. Finally, we utilize machine-learning based age-estimation software to detect visible signs of aging in pictures of CEOs who experience distress shocks. Using a difference-in-differences design, we estimate that exposure to a distress shock during the Great Recession increases CEOs' apparent age by roughly 1 year over the next decade.

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1 Introduction

Much of the academic and policy discussion about high-profile jobs in business and other arenas revolves around their pay, performance, and incentives. Ever since the seminal work of Jensen and Meckling (1976), research has focused on the design of managerial incentives and corporate governance systems: How can shareholders minimize the moral-hazard issues arising from the separation of ownership and control, and ensure that the CEO maximizes the value of the firm? The key ingredient in this classical agency problem is the presence of so-called "private benefits" that the CEO, but not shareholders can extract. In a typical incentive-design problem, private benefits are a proxy for both direct monetary benefits (additional pay, loans) and a wide range of perks a manager can extract from the firm.

Less attention has been paid to another type of benefits that accrues only to the managers—their personal health and well-being. CEOs work long hours, frequently make stressful high-stakes decisions, such as decisions about firing or plant closures, and face uncertainty in times of crisis (Bandiera et al. 2017; Porter and Nohria 2018). They are subject to heightened monitoring and criticism when the company is underperforming. While the news media occasionally discusses the stress associated with C-suite positions and covers unexpected deaths in office, there is little research that assesses the implications of such pressures for CEOs' health, ability to stay on the job, and ultimately willingness to select into the CEO position.

One reason such costs have not been quantified is that it is difficult to find credible variation in job demands and stress.[1] While work-related stress is frequently cited as a powerful force affecting population health (e.g. Marmot (2004), Ganster and Rosen (2013), Kuka (2018)), quasi-experimental evidence on long-run effects is scant in any setting, and especially for the broader workforce as it is difficult to disentangle stress experienced on the job with the closely-related

[1] As Smith (1999) put it in his take on the existing literature: "After all, stress is a constant feature of life and there are multiple reasons in a day, week or year why people experience its symptoms. The empirical challenge is to reduce this complexity to a relatively few salient family and job life events that are the principal markers of excessive toll."

effects of financial hardship and poverty.

In this paper, we focus on the top managerial position, CEOs, and assess the trade-off between incentives to exert effort on the job and private benefits in the form of the CEOs' health. While the CEO position may be quite stressful in general, it also comes with high remuneration. Individuals in our sample are wealthy and unlikely to be affected by financial hardships or poverty even if they lose their job, ruling out the usual confound of financial hardship in explaining health consequences.

In order to identify the role of monitoring and stress, we exploit variation in monitoring due to governance legislation or during periods of crisis. We test whether such variation causally affects CEO longevity and aging. To perform our analysis we augment a version of the Gibbons and Murphy (1992) data with hand-collected information on the exact dates of birth and death (or survival to the present day) for more than 1,600 former CEOs of large U.S. firms included in the *Forbes* Executive Compensation Survey between 1970 and 1991—a period when several plausible exogenous changes to firms' governance systems as well as industry downturns occurred.

We exploit two sources of variation to estimate the causal effect of service as CEO under different governance regimes and degrees of distress, both of which have been defined and employed in a large volume of prior literature. The first analysis focuses on variation in the intensity of CEO monitoring due to the passage of anti-takeover laws across U.S. states in the mid-1980s. These laws shield CEOs from market discipline by making hostile takeovers by corporate raiders more difficult. Prior research has documented some of the associated private benefits: after the passage of the laws, CEOs became less tough in wage negotiations, decreased their rate of both plant creation and plant closures, and firm returns fell (Bertrand and Mullainathan 2003). The authors coined the expression that managers were "enjoying the quiet life," implying that the anti-takeover laws made life easier for CEOs, and lowered job-related stress. Here, we estimate the relationship between CEOs' exposure to more lenient governance laws and their lifespan in a hazard regression model with controls for CEO age, time trends, industry affiliation, and firm location. We restrict all analyses to CEOs appointed before the enactment of the laws to address the concern that anti-takeover laws alter the selection of CEOs.

Our estimates indicate that anti-takeover laws led to significant improvements in the life expectancy of incumbent CEOs. One additional year under lenient corporate governance lowers mortality rates by four to five percent for the average CEO in the sample. Non-linear specifications indicate that life expectancy gains from lenient governance accrue in the initial years of exposure, with gains as large as 9 percent per year, and incremental effects falling to zero within 5 years of initial exposure.

The estimated effect sizes are large by comparison to general health trends. For example, our estimates imply that being one year older (i.e. the effect of age) increases the mortality hazard of a CEO by roughly 12 percent. We can then calculate how much younger a CEO needs to be to compensate the effect of experiencing the anti-takeover laws. This calculation equates experiencing the anti-takeover laws to reducing a CEO's age by around 2 years.[2] An increase in lifespan of two years is sizable and comparable to known health threats. For example, smoking until age 30 is associated with a reduction in longevity by roughly one year; lifelong smoking, on the other hand, is estimated to reduce life expectancy by ten years or more (HHS 2014; Jha et al. 2013).

To shed light on intermediate outcomes preceding the changes in life expectancy, we examine how anti-takeover laws affect the pay and length of service (completed tenure) of exposed CEOs. We find no evidence of a compensating differential in the form of lower pay for CEOs who are protected from hostile takeover.[3] Estimates indicate positive but statistically insignificant effects on pay in our preferred specifications. Protected CEOs do, however, remain on the job for longer. These results suggest that one response of CEOs to a decrease in stress is to delay retirement,

[2]Results would be slightly larger if we use lifetables instead of our own estimates of CEO age effects. These complete calculation are provided in Section 4.2.

[3]The analysis of pay builds upon the results in an unpublished paper by the Quiet Life authors Bertrand, Mullainathan, et al. (1999). The labor literature has generally struggled to find evidence of compensating differentials outside of select settings and carefully-design experiments; for example, see the discussion and findings in Mas and Pallais (2017) and Lavetti (2018).

consistent with an expansion of private benefits.

Two important robustness checks support the main results. First, we account for the possibility that CEOs' length of tenure (and therefore length of exposure to BC laws) might be affected by the introduction of the laws. To do so, we estimate a model in which we relate a CEO's *predicted* BC exposure as of the year in which the BC law is introduced – based on a prediction model for remaining tenure in that year – to survival rates. The prediction model depends only on variables determined before the passage of the laws, such as the age of the CEO and pre-BC tenure, thereby purging the prediction of any endogeneity due to the BC laws themselves. Our estimates are robust to the use of this prediction to form the length of exposure. Second, we conduct a series of robustness checks to address the concerns regarding anti-takeover laws raised in Cain, McKeon, and Solomon (2017) and Karpoff and Wittry 2018. The results are unchanged when we consider alternative definitions of the anti-takeover laws, account for other firm or state anti-takeover provisions, exclude lobbying and opt-out firms, and when we perform different data cuts based on firms' industry affiliation or state of incorporation.

We next provide complementary evidence on the effect of stressful periods on a CEO's health exploiting industry-level distress shocks as a measure of an exogenous increase in a CEO's stress levels. We find that the experience of an industry-wide downturn – a separate and directionally opposite change in stress levels compared to BC law passage – increases a CEO's mortality risk by a similar magnitude as one year of exposure to the anti-takeover laws, approximately four to six percent.

In the final part of the paper, we document more immediate health implications of experiencing financial distress on CEOs' health. To do so, we utilize machine-learning algorithms designed to estimate a person's "apparent" age in order to detect visible signs of aging in the faces of CEOs.[4] To the best of our knowledge, this represents the first application of visual machine learning to a

[4]Apparent age reflects how old a person look. By definition, a person's apparent age may be different from a person's biological age. Here we focus on apparent age.

quasi-experimental research design. With this in mind, we include a detailed description of the procedure, as well as extensive robustness checks for issues that have been shown to impact the use of visual machine learning in non-causal settings. Our application illustrates the potential use of this technology in the study of health and aging without reliance on currently-standard measures based on mortality, hospital admissions, or survey responses. Specifically, we obtain access to the software of Antipov, Baccouche, Berrani, and Dugelay (2016), which was trained on more than 250,000 pictures and is the winner of the *ChaLearn Looking At People 2016* competition in the *Apparent Age Estimation* track.[5] We collect a sample of 3,086 pictures of the 2006 *Fortune 500* CEOs at firms included in the 2006 *Fortune 500* list from different points during their tenure, to estimate differential aging in response to industry-level exposure to the financial crisis. Using a difference-in-differences design, we estimate that CEOs look about one year older in post-crisis years if their industry experienced a severe decline in 2007-2008 relative to if it did not. The estimated difference between distressed and non-distressed CEOs increases over time, reaching about 1.18 years for pictures taken five years and more after the onset of the crisis.

In sum, our results indicate that stricter corporate governance regimes – which are generally viewed as desirable and welfare-improving – imply significant personal health costs to CEOs. While we lack direct measures of mechanisms that relate the business environment to health outcomes, the weight of the evidence suggests heightened stress from experiencing stricter governance and economic downturns constitutes a substantial personal cost for CEOs in terms of their health and life expectancy. An open question is whether managers fully account for these costs as they are progress through their career paths, and how these costs affect selection into service as a CEO.

Our paper adds to several strands of literature. First, it connects to a recent literature that sheds light on CEOs' demanding job and time requirements. Bandiera et al. (2017) obtain weekly

[5]In the field of Computer Science, winning important programming contests is, loosely speaking, comparable to being accepted at a top-five Economics or top-three Finance journal. The software we use was the winner of the second edition of the competition. While we have no information on how many teams participated in 2016, more than 100 teams participated in the first edition, and our software improved on the solution of the first edition's winner.

diaries of 1,114 CEOs of manufacturing firms and document long hours that often include six- and seven-day workweeks. The schedule appears to be even more intense in large firms. Porter and Nohria (2018) track 27 CEOs of multi-billion dollar firms 24/7 over three months,and record CEOs working an average of 62.5 hours per week, 79% of weekend days, and 70% of vacation days. Our results imply that the daily rigors involved in service as a CEO impose costs that show up throughout these individuals' lives. To the best of our knowledge, the only other work that has looked at executives' lifespans is Yen and Benham (1986), distinguishing between CEOs in more and less competitive industries. However, their analysis is based on a much smaller sample of executives, does not take into account self-selection into job environments, and instead of a rigorous survival analysis merely provides a simple comparison of means of CEOs' age at death across industries.

Second, our paper links to the health and labor literature on worker job stress and insecurity, and resulting health effects. Stress experienced on the job is thought to explain an important share of the association between socioeconomic status and life expectancy (Chetty et al. (2016a)), but causal evidence has been rare (Anderson and Marmot (2012)). A vast literature in medicine and biology associates stress with changes in hormone levels, brain function, cardiovascular health, and other health outcomes (e.g., McEwen (1998) and Sapolsky (2005)). A recent literature in economics documents causal effects of stress on decision-making (Coates and Herbert (2008), Kandasamy et al. (2014), Zhong et al. (2018)). Engelberg and Parsons (2016) document a strong and nearly instantaneous relationship between stock market crashes and hospital admissions, especially for anxiety-related reasons and panic disorders. Examining effects of working conditions directly, Hummels et al. (2016) show that when workers' job demands increase due to demand shocks, they suffer from more stress, injury, and illness. Looking among high-status individuals, Borgschulte and Vogler (2019) attempts to disentangle the effects of prestige and stress among US governors who win of close elections, finding that prestige effects dominate any deleterious effects of stress. Our paper extends this literature by isolating a direct shock to working conditions among wealthy individuals, thereby shutting down other confounding effects of poverty, social status, and access to health care that are thought to generate stress in other settings (e.g. Kuka (2018), Koijen and Van Nieuwerburgh (2019)).

Finally, we add to the literature in corporate governance that began with Bertrand and Mullainathan (2003) on the impact of business combination laws and anti-takeover laws. Subsequent papers have continued to focus on firm-level outcomes. For example, Giroud and Mueller (2010) show that the effect of BC laws on managerial slack is concentrated among firms in non-competitive industries, consistent with the notion that competition maintains managerial effort. Gormley and Matsa (2016) show that after the adoption of BC laws, managers even undertake explicit, value-destroying actions that reduce their firms' risk of distress, presumably to make their job easier and safer. Atanassov (2013) shows that patent count and quality decreased with the introduction of anti-takeover laws. Cheng et al. (2004) find that managers reduce their stock ownership following the enactment of BC laws, consistent with managers viewing both stockholdings and BC laws as (substitute) channels through which managers increase the control of the firm. Our paper differs from the existing literature on anti-takeover laws in that we explore the *personal-level* consequences of changes in corporate governance. Many of the mechanisms and channels suggested by the above papers work through the specific incentives to the CEO, but we are the first to look into the long-term health consequences of such incentivization. We conclude that higher incentives can enhance productivity and shareholders value, but might come with a dark side in the form of adverse effects on managers' health.

The remainder of the paper is organized as follows. Section 2 describes our data and methodology. Section 4 contains our results pertaining to life expectancy and exposure to anti-takeover laws. Section 5 presents the results pertaining to life expectancy and exposure to industry-wide distress shocks. Section 6 presents our evidence on CEOs' aging patterns. Section 7 concludes.

2 Dataset Construction

2.1 Anti-Takeover Laws

Anti-takeover statutes were passed by states to increase the hurdles for hostile takeovers. The second-generation[6] anti-takeover laws were comprised of several types of statutes. Besides Business Combination laws, states also frequently passed Control Share Acquisition and Fair Price laws, as well as Poison Pill and Directors' Duties laws, since the mid-1980s. We defer the discussion of the latter types of laws to Section 4.4.2, and focus on BC laws here.

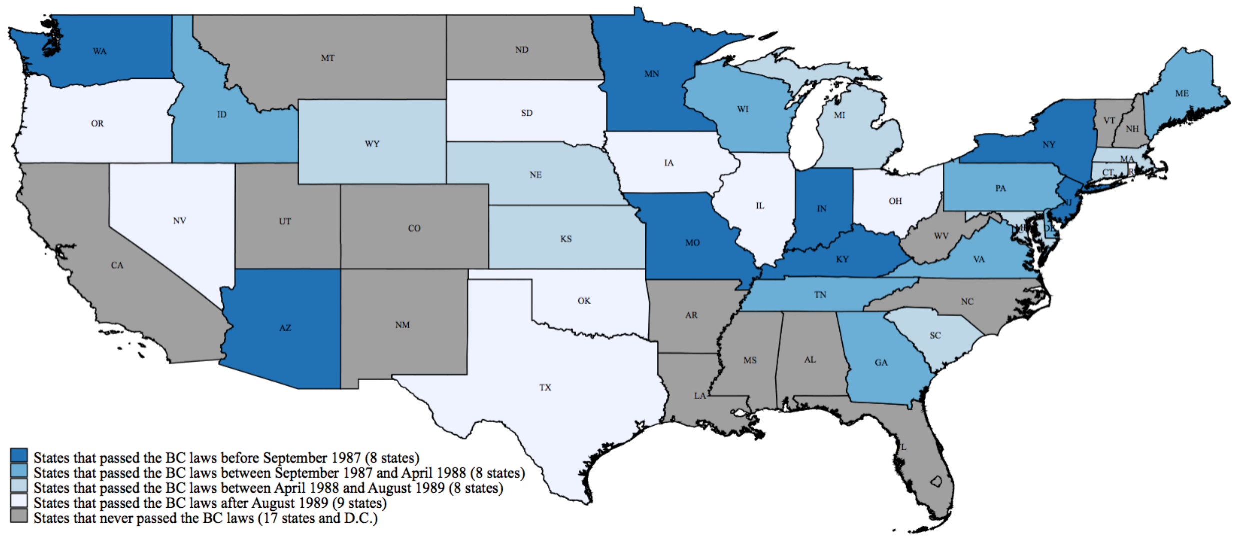
BC laws contributed to reducing the threat of hostile takeovers by imposing a moratorium that prohibits a large shareholder of a firm from conducting certain transactions with the firm, usually for a period of three to five years. Figure 1 visualizes the staggered introduction of BC laws across states. A total of 33 states passed a BC law between 1985 and 1997, with most laws being passed between 1987 and 1989. (Appendix-Figure B1 contains a similar map based on all five types of second-generation anti-takeover laws listed above.)

The fact that there is both variation across time and states in terms of the introduction of the BC laws is obviously very useful for identification, and one reason for the popularity of these laws in academic studies. Another advantage of using the passage of anti-takeover laws as identifying variation in corporate governance is that these laws applied to firms based on the state in which they were incorporated and not where they were headquartered or operated. The frequent discrepancy between firms' location and their state of incorporation enables us to assess the impact of the laws while concurrently controlling for shocks to the local economy.

[6]These laws are referred to as second-generation anti-takeover laws, since they were passed after the first-generation laws were struck down by courts. See also the discussion in Cheng, Nagar, and Rajan (2004) and Cain, McKeon, and Solomon (2017).

Figure 1: Introduction of Business Combination Laws Over Time

This figure visualizes the distribution of Business Combination law enactments over time. In total, thirty-three states passed a BC law between 1985 and 1997. The map omits the state of Hawaii, which never passed a BC law.



2.2 CEO Sample

The dataset begins with all CEOs listed in Executive Compensation Surveys published in *Forbes* between 1969 and 1991.[7] These surveys are derived from corporate proxy statements and include the executives serving in the largest U.S. firms. The full data set comprises more than 3,400 CEOs from over 1,600 firms; 102 CEOs serve in multiple firms. Our main sample restricts to 2,720 unique CEOs from 1,501 firms in 1975 or later and whose firm can be assigned a PERMNO from CRSP.

For these CEOs, we manually search for (i) their exact dates of birth to verify the birth year information provided in the original data set, (ii) whether the CEO has died or is still alive, and (iii) the date of death, if the CEO has passed away. The cutoff day for (ii) is October 1st, 2017; that is, all CEOs who did not pass away by this date are treated as alive in all of our analyses, even if they

[7]The data is an extended version of the dataset in Gibbons and Murphy (1992). We thank Kevin J. Murphy for providing the data.

have died since then. To obtain the information on birth and death, we use Google searches and online sources such as Ancestry.com. Ancestry links historical birth and death records, combining information from U.S. Census, Social Security Death Index, birth certificates, and other historical sources. Ancestry allows us to find the precise birth and death information even if we cannot find such information through newspaper searches, as long as we can be certain that we have uniquely identified the correct person on Ancestry. To do so, we would compare the Ancestry's information with that from Google searches and information in newspapers, such as birth place, elementary school, or city of residence. Identifying a person as alive turns out to be more difficult than identifying someone as dead. Even if we cannot locate the person on Ancestry, large newspapers such as the New York Times or Los Angeles Times will oftentimes report about the death of a famous former CEO; however, there tends to be less coverage of retired CEOs who are still alive. We classify a CEO as alive whenever we are able to find recent sources confirming their alive status. Most often, these sources are either newspaper articles or websites listing the CEO as a board member, sponsor, donor, or chairman/chairwoman of/for an organization or event.[8] We are able to obtain the birth and death information for 2,361 out of the 2,720 CEOs from our main sample, implying a finding rate of 87%, employed by 1,352 different firms.

In addition to these CEO-level variables, we collect information on historical states of incorporation. One major advantage of using anti-takeover laws as exogenous variation to corporate governance is that they apply to firms independently of their state of location, headquarters, and main business activities. We start from the information provided in CRSP/Compustat, but cannot simply use this information for the past since CRSP backfills the state of incorporation with the current one. In a first step, we compare the state of incorporation currently listed in CRSP to that listed in the SDC Mergers and Acquisitions and SDC New Issues databases. Whenever they deviate, we manually search for which information is correct and whether (and when) firms switched

[8]For sources that include a date, we only use sources from 01/2010 or after to conclude the person is still alive.

their state of incorporation.[9] We start from the current information provided in CRSP/Compustat and from Compustat Snapshot, firms' 10-Ks and other filings with the Securities and Exchange Commission, legal documents and newspaper articles. In total, we are able to identify the (historical) state of incorporation for 2,199 out of the 2,361 CEOs that remain in our main sample after the CEO birth and death information search phase.

In a final step, we return to CEO-related variables and collect two more pieces of information. First, we collect the actual tenure of all CEOs in the firms in which they appear in our data set.[10] Whenever possible we use Execucomp to fill in this information, and otherwise revert to Google searches. In this case, the New York Times *Business People* section proves especially useful, as it frequently reports on executive changes in our sample firms. We retain CEOs only if we can find at least yearly information on when they started and ended their tenure. When included in Execucomp or when we can find announcements of CEO changes in newspapers, we are oftentimes able to obtain the exact date or month of the CEO transition.[11] Second, we restrict our sample to CEOs whose firm was included in CRSP during the time of their tenure.[12] After these restrictions, we end up with a sample of 1,900 CEOs. To alleviate selection concerns, we present all results

[9]Prior literature has dealt in different ways with firms' deliberate choice to change their state of incorporation. For example, Bertrand and Mullainathan (2003) do not correct for state of incorporation changes in their main specification. They randomly check 200 firms in their sample and find that only three had changed their state of incorporation in the past. In robustness tests, they restrict to non-Delaware firms. Giroud and Mueller (2010) proceed accordingly. Cheng, Nagar, and Rajan (2004) report that none of their 587 *Forbes 500* firms changed their state of incorporation between 1984 and 1991. Gormley and Matsa (2016), instead, include historical information from SEC disclosure compact discs, Compustat back-tapes, SEC Analytics, as collected by Cohen (2012), and the legacy version of Compustat. In addition, they drop firms that changed their state of incorporation from treated to non-treated states or vice versa. Cain, McKeon, and Solomon (2017) rely on SEC Analytics, SDC and Compustat as well, and in addition on historical Moody's manuals.

[10]As explained above, our starting sample only contains years in which a firm appeared in the *Forbes* Executive Compensation Surveys. Oftentimes, there are gaps over time, or firms are only included in few years, which complicates the tenure classification. In addition, whenever we do not observe a predecessor or successor CEO in our data already, we cannot simply assume that the first (last) year in which we observe the CEO is also their first (last) year of tenure, and need to search for this information ourselves.

[11]Whenever we have yearly information only, we assume the CEO change happened in the middle of the year ("mid-year convention"). This is motivated by the notion that starting months of CEOs included in Execucomp are relatively uniformly distributed throughout the year (see Eisfeldt and Kuhnen (2013)).

[12]Note that this constraint is different from above where we required that we be able to assign a PERMNO to each firm in our sample. For instance, we would drop a person in this final step if he served as the CEO before the firm went public.

focusing on CEOs who were appointed in years prior to the enactment of the business combination laws, leaving us with a main sample of 1,605 CEOs.[13]

2.3 Variable Construction

Appendix A contains a description of all variables used in this study. Our first set of variables pertains to the birth and death information of the CEOs in our sample. *Birth Year* is the year of birth of a CEO. *DeadByOct2017* is an indicator that equals 1 if a CEO has passed away by the censoring date, October 1st, 2017, and 0 otherwise. *Death Year* captures the year of death of CEOs, is, however, calculated up to the monthly level. That is, *Death Year* would be 2000.5 for a person who died in June 2000. Besides year of birth, alive status and date of death (when applicable), we define several additional variables capturing various aspects of a CEO's tenure. *Tenure* captures a CEO's tenure, *BC* his exposure to a BC law, and *FL* his exposure to the first enacted of the second-generation anti-takeover law.

CEO-Year Level Structure and Intra-Year Data. We construct our data set in CEO-year level format. As a result, $Tenure_{i,t}$ is defined as a running variable, counting a CEO's cumulative tenure from appointment until year t. Similarly, $BC_{i,t}$ and $FL_{i,t}$ capture the cumulative exposure to BC and FL laws. For example, $BC_{i,t}$ would take the value 3 if the CEO has experienced three years under the BC regime until year t (including t). In addition, since we measure these three variables capturing a CEO's cumulative experience in years, we want to emphasize that they can take non-integer values. For example, Delaware's BC law was adopted on 2/2/1988. A CEO's BC exposure in 1988 would then be calculated as $BC_{i,1988} = \frac{365-\text{doy}(2/2/1988)}{365} = 0.92$. Similarly, CEOs not starting their tenure or stepping down at the beginning or end of the year can result in non-integer values for cumulative tenure. Finally, we also calculate squared cumulative tenure,

[13] We proceed accordingly for the analyses regarding the first-time enactment of any of the five second-generation anti-takeover laws; i.e., we require that the CEO is appointed before the first law is passed. This yields a sample of 1,510 CEOs. As an aside, throughout the paper, we use the pronoun "he" when referring to the CEOs in our sample since the vast majority of observations in our sample are male CEOs.

$Tenure^2_{i,t}$, as well as a CEO's age in year t ($Age_{i,t}$).

2.4 Summary Statistics

Table 1 presents the summary statistics for our main sample covering 1,605 CEOs, as well as sub-samples split by extent of BC exposure (zero, below-median, above-median).[14] Panel A on the upper left shows the statistics for the pooled sample of 1,605 CEOs. The median CEO in our sample was born in 1925, became CEO at age 52, and served for nine years. There is relatively large heterogeneity in tenure; moving from the 10th to the 90th percentile adds 17 years of tenure. 71% of our CEOs have passed away by October 1st, 2017. The median CEO died at age 83, and passed away in 2006.

Panel B at the bottom presents the summary statistics separately for CEOs with no BC exposure ($N = 980$), those with positive but below-median exposure ($N = 320$), and those with higher levels of exposure ($N = 305$). While some of the differences in variables across sub-groups are already suggestive of the effects we have in mind—e.g., 82% of CEOs without BC exposure have passed away, but only 68% (38%) of CEOs with below-median (higher) BC exposure—we need to be careful to not over-interpret them. In particular, BC laws were only introduced starting in 1985; consequently, CEOs who served towards the beginning of our sample are less often insulated from the laws during their tenure. This is, for example, reflected in the differences in starting years as CEOs. The median CEO without BC exposure began his tenure in 1974. In comparison, the median CEO with some (high) BC exposure is appointed in 1980 (1982). In turn, these differences in start years contribute, at least partially, to the stark discrepancies in death rates across groups until the censoring date. Another cross-group comparison that has to be considered carefully is that of age at death. A simple comparison between groups would overlook the fact that CEOs with BC

[14] We collapse variables that are defined in a cumulative way (i.e., in CEO-years), such as tenure and BC exposure, to the CEO-level. That is, we calculate a CEO's total tenure, total BC exposure etc. and base our summary statistics on these values.

Table 1: Summary Statistics

Panel A: Summary Statistics for All CEOs

	All CEOs (N=1,605)				
	Mean	SD	P10	P50	P90
Birth Year	1925	8.96	1914	1925	1937
DeadByOct2017	0.71	0.45	0	1	1
Death Year	2004	9.98	1989	2006	2016
Age of Death	81.95	9.92	67.58	83.42	93.50
AgeTak.Office	51.63	6.95	43	52	60
YearTak.Office	1977	7.21	1968	1977	1986
Tenure	10.62	6.86	3	9.08	20
BC | BC>0	5.68	5.05	0.54	4.41	12.37
FL | FL>0	5.90	5.13	0.77	4.45	12.82

Panel C: Top Industries and Incorporation States

	All	No BC	$\leq p50$ BC	$> p50$ BC
Top 5 FF49 Industries	Banking	Banking	Banking	Banking
	Utilities	Utilities	Utilities	Utilities
	Retail	Retail	Chem.	Retail
	Petrol.	Trans.	Retail	Insur.
	Trans.	Petrol.	Insur.	Petrol.
Top 3 States of Incorp.	DE	DE	DE	DE
	NY	NY	NY	NY
	OH	OH	NJ/OH	PA

Panel B: Summary Statistics for Different CEO Sub-Groups

	No BC Exposure (N=980)					Below-Median BC Exposure (N=320)					Above-Median BC Exposure (N=305)				
	Mean	SD	P10	P50	P90	Mean	SD	P10	P50	P90	Mean	SD	P10	P50	P90
Birth Year	1922	8.48	1913	1921	1934	1927	6.90	1921	1926	1938	1933	6.51	1926	1933	1942
DeadByOct2017	0.82	0.38	0	1	1	0.68	0.47	0	1	1	0.38	0.49	0	0	1
Death Year	2002	10.24	1987	2003	2015	2008	8.05	1994.08	2010	2016	2009	7.05	1997	2012	2017
Age of Death	82.30	10.10	68.00	83.83	94.00	81.89	9.52	68.00	84.17	92.42	79.64	9.13	66.83	81.17	90.42
AgeTak.Office	52.88	6.69	44	53	61	51.47	6.94	42	52	60	47.79	6.34	40	48	56
YearTak.Office	1975	7.08	1966	1974	1984	1979	6.60	1971	1980	1986	1981	5.89	1972	1982	1987
Tenure	8.70	5.72	2	7.50	16	10.83	6.48	4	9.04	20.08	16.54	7.21	8.42	15.08	27.33
BC	0.00	0.00	0	0	0.00	1.93	1.24	0.5	1.86	3.82	9.61	4.52	5.41	8.33	14.74
FL	0.58	2.12	0	0	1.21	2.80	2.05	0.5	2.41	5.76	10.49	4.63	5.82	9.50	17.03

Notes. This table presents summary statistics for our main sample covering 1,605 CEOs. We collapse variables that are defined in a cumulative way (i.e., in CEO-years), such as tenure and BC exposure, to the CEO-level. That is, we calculate a CEO's total tenure, total BC exposure etc. and base our summary statistics on these values. *DeadByOct2017* is an indicator that equals 1 if the CEO has passed away before the censoring date and zero otherwise. *AgeTak.Office* and *YearTak.Office* refer to the CEO's age and the year at appointment, respectively. All variables are defined in Appendix A.

exposure are, on average, born later. As a result, conditional on having passed away until 2017, we would expect their age of death be lower compared to their No-BC peers. Our main takeaway from this cross-group comparison is that it underscores the importance of controlling for covariates such as age and cohort.

Panel C on the upper right presents information on the most frequent Fama and French (1997) 49 industries in our sample, as well as most common states of incorporation. Across CEO sub-groups split by BC exposure, our CEOs are frequently employed by firms in the banking, utilities, and retail industry. Further common industries are petroleum and natural gas, as well as transportation for CEOs with no BC exposure and insurance for CEOs with BC exposure. While differences in industry frequencies are small across groups, we include industry fixed effects in all analyses. In addition, we show that the results are robust to excluding specific industries, in particular "Banking" and "Utilities." Consistent with the prior literature, the most common state of incorporation is Delaware in all sub-groups. Other common states include New York, Ohio, New Jersey, and Pennsylvania.

Table 2 presents the proportions of death as of Oct. 1st, 2017 for CEOs of different cohorts, and their average age at death conditional on having died. We show the statistics for the CEOs that ever served under the BC law and those who never served under the BC law. We can see, for almost every cohort, the survival probability is higher for the BC-treated group. Furthermore, the average age at death is higher for the BC-treated group than the non-treated group for every cohort. If we calculate the average difference of age at death between the two groups weighted by the total number of deaths in each cohort, we obtain a difference of 3.76 years. These comparisons offer direct evidence that the BC law may have implications on CEO death probability and life expectancy. Our empirical analysis below serves to formalize the patterns in this table.

Table 2: Death Proportion and Death Age for CEOs w/ or w/o Business Combination Laws

	BC-Treated			Non-BC		
Birth Year	No.	% Death	Death Age	No.	% Death	Death Age
Before 1915	12	100%	91.83	209	98.1%	84.87
1916 - 1920	25	92.0%	88.45	248	99.2%	84.58
1921 - 1925	115	82.6%	86.76	235	88.9%	82.98
1926 - 1930	202	62.4%	83.96	137	70.1%	81.86
1931 - 1935	134	35.6%	82.08	77	40.3%	81.97
1936 - 1940	82	23.2%	77.70	39	35.9%	74.68
After 1941	55	23.6%	72.12	35	14.3%	71.67

Notes. This table shows the proportions of death as of Oct. 1, 2017 for CEOs of different cohorts, and the mean age of death conditional on being dead. We divide the sample into BC-treated group (if the CEO ever served under the BC law) and the non-BC group.

3 Empirical Strategy

Our main hypothesis is that exposure to plausibly exogenous changes in CEOs' job demands — such as the staggered introduction of BC laws — affects CEOs' mortality rates. We define a CEO's BC law exposure in two ways. First, we use a binary indicator for having been exposed to the BC law treatment. In the proportional hazard framework, this assumes that mortality risk shifts once and for all at the time of passage of a BC laws for a CEOs serving in an exposed firm. To capture intensity (i.e. length) of exposure, our second definition of treatment uses length of exposure rather than an indicator for exposure. Two complicating factors arise with the second specification: one, the length of exposure (i.e. remaining tenure at the time the BC law is passed) is endogenous; and two, effects may be non-linear in the length of exposure. We return to these issues below.

Our primary analysis uses the Cox (1972) proportional hazards model. Designed for single-spell survival analyses, the hazard model is an intuitive choice in our setting. Specifically, we estimate:

$$\lambda(t|X_{i,t}) = \lambda_0(t,\alpha)\ exp(\beta I(BC_{i,t}) + \delta' X_{i,t}) \tag{1}$$

To implement the first definition of treatment, $I(BC_{i,t})$ is an indicator for whether CEO i had been exposed to the BC law by year t. In the second approach, we replace this with $BC_{i,t}$, which counts a CEO's BC exposure in years until year t. $\boldsymbol{X}_{i,t}$ is a vector of control variables including time trends (either a linear year control or fixed effects), a CEO's age, as well as firm location and industry. A CEO enters the analysis (i.e. "becomes at risk") in the year he is appointed CEO and exits when he dies. Thus, importantly, the period in which a CEO is included in our analysis is *not* restricted to when he served as CEO; we follow CEOs over time after they step down. The count of CEOs' exposure to the BC law, $BC_{i,t}$, will remain constant their departure as CEO. An attractive feature of the proportional hazards model is that it allows for censored observations, i.e., CEOs that are still alive today. Since our data collection began on October 1st, 2017, we pick this date as the censoring date in the hazard analysis.

We implement two alternative specifications to address issues with the length of exposure. First, a CEO's remaining tenure at time of the BC law passage will likely reflect unobserved characteristics of the CEO, and might further be affected by the introduction of the laws. For example, we may worry that longer-lived CEOs are likely to remain on the job for longer, hence creating a mechanical correlation between length of exposure and longevity. Directly controlling for realized tenure will introduce endogeneity, as tenure is likely affected by the BC laws. To address this issue, we replace $BC_{i,t}$ with the CEO's *predicted* length of exposure at the time of the BC law passage rather than true exposure. To implement this approach, we develop a prediction model for remaining tenure at the time of the BC law introduction. Importantly, this prediction model only uses information from prior to the BC law passage. Since this approach entails a generated regressor (predicted remaining tenure), we bootstrap standard errors, using the block bootstrap method (a block corresponds to a state of incorporation cluster), and using 200 bootstrap replications. More details regarding this approach are illustrated in Section 4.3.1.

Second, we may be concerned that the linear dose-response function represented by $BC_{i,t}$ does not describe the true relationship between exposure and mortality risk. Introspection suggests a diminishing effect as CEOs adapt to the new business environment and exhaust their opportunities to adjust their activities. Non-parametric Kaplan-Meier survival plots of the data (see Section 4.1 for details) confirm that the effect of insulation from takeover threat on a CEO's long-term health and mortality may be nonlinear: initially, there is a strong positive response of long-term health to lowered monitoring and takeover threats, but the *incremental* benefits from prolonged exposure to reduced monitoring and stress appear to taper off eventually. To allow for nonlinear effects, we estimate a modified version of our main Equation 1, in which we separate the effects of initial and later years of exposure to lenient governance on survival rates.

4 Results: Corporate Governance and Life Expectancy

4.1 Graphical Evidence

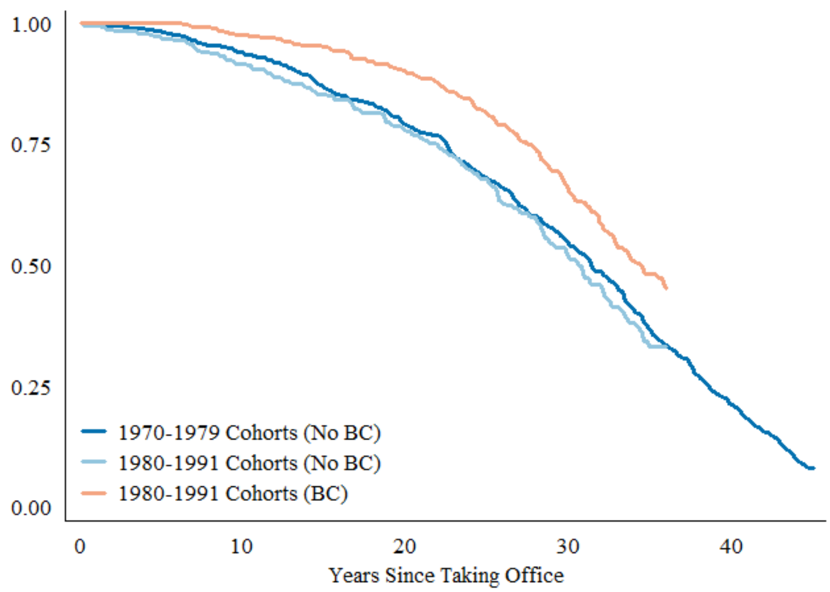
We first provide graphical evidence on the mortality effects of serving under stricter corporate governance regimes. Figure 2 plots the Kaplan-Meier survival graphs, split by whether CEOs serve in stringent or lenient monitoring environments.[15] In both panels of Figure 2, the vertical axis shows the fraction of CEOs who are still alive (the survival rate). The horizontal axis reflects time elapsed (in years) since becoming CEO.

Panel 2a compares the survival of CEOs who became CEO in the 1970s and left office before 1980, those who became CEO in the 1980s and never served under a BC law, and those who became CEO in the 1980s and were eventually insulated by a BC law during their tenure (independent of exposure length). For the "1970s cohorts," maximal elapsed time since our sample start is $t = 47.75$ (time elapsed between 1/1/1970 and the censoring date, 10/1/2017). Similarly, for the "1980s cohorts," maximal elapsed time is $t = 37.75$. We restrict the graph to periods when at

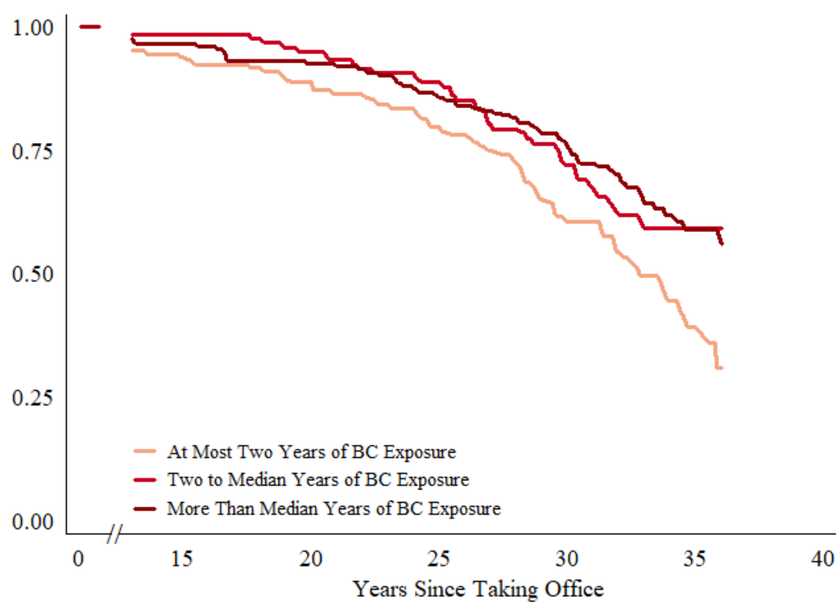
[15] The Kaplan-Meier estimator is a non-parametric estimator of the discrete hazard. Discretizing time into intervals $t_1, ..., t_J$, it is defined as $\widehat{\lambda_j^{KM}} = \frac{f_j}{r_j}$, where f_j is the number of spells ending at time t_j and r_j is the number of spells that are at risk at the beginning of time t_j.

Figure 2: Kaplan-Meier Survival Estimates

(a) By Cohort



(b) By Years Under BC Law



Notes. This figure shows Kaplan-Meier survival plots. The vertical axis shows the fraction of CEOs who are still alive (percent survival). The horizontal axis reflects time elapsed (in years) since a person became CEO. Panel 2a compares the survival of CEOs who became CEO in the 1970s and left office before 1980 (light blue), those who became CEO in the 1980s and never served under a BC law (dark blue), and those who became CEO in the 1980s and were eventually insulated by a BC law during their tenure (orange). Panel 2b zooms in on this last group with BC exposure, and plots survival separately for CEOs with positive but at most two years of BC exposure (orange), with two to median exposure (red), and with above-median exposure (brown). Survival estimates in panel 2b are adjusted to a tenure of 12 years (median tenure of CEOs with BC exposure).

least 30 CEOs in either cohort group are uncensored, explaining the slightly differential ends of the survival lines (after 36 and 45 years, respectively).

The survival functions provide first evidence that serving under more stringent corporate governance is associated with adverse consequences in terms of life expectancy. Two results emerge. First, the survival patterns of the "1970s cohorts" and the "1980s cohorts without BC exposure" are remarkably similar, allaying concerns that our results might pick up *general* changes in survival patterns between the 1970s and 1980s. Second, consistent with our hypothesis, the survival line for the "1980s cohorts with BC exposure" is visibly right-shifted compared to the No-BC-cohorts. Most importantly, survival rates are substantially more favorable even when holding the cohort fixed, i.e. by comparing the "1980s cohorts" with and without BC exposure. For example, 20 years after a CEO's appointment, about 25 percent of CEOs from the "1980s cohorts without BC exposure" have died, whereas it takes closer to 30 years until a quarter of the CEOs in the "1980s cohorts with BC exposure" have passed away.

Panel 2b zooms in on the CEO group with BC exposure and explores potential nonlinearities in the insulating effect of more lenient governance on lifespan. Specifically, we plot survival rates separately for three sub-groups, formed as (i) positive but at most two years of BC exposure, (ii) more than two years of but at most the median BC exposure (4.4 years), and (iii) more than the median BC exposure.[16]

Comparing CEOs with low (up to 2 years of) BC exposure to those with more exposure, we observe increased benefits in survival rates for the latter groups, visible by the rightward shifted survival lines for CEOs with more exposure. However, there is no *further* rightward shift comparing CEOs with medium and high BC exposure This suggests that there are increasing health benefits from leniency in monitoring initially, but that the *incremental* effect of more BC exposure

[16]The estimated survival functions in panel 2b are adjusted to a tenure of 12 years, which is the median tenure of CEOs with BC exposure. Since we are holding fixed the cohort ("1980s cohort with BC exposure") across sub-groups, which in turn are formed based on differential exposure to BC laws, we would otherwise run the risk of comparing CEOs with substantially different lengths of tenure, potentially conflating the independent effect of tenure with the direct effects of corporate governance on working conditions.

might taper off eventually.

4.2 Main Results on Business Combination Laws

Table 3 shows the hazard model results on the relationship between exposure to plausibly exogenous changes in CEOs' job demands – the staggered introduction of BC laws – and CEOs' mortality rates, based on our main estimating equation (Equation 1). The main independent variable of interest is $I(BC_{i,t})$, which indicates whether the CEO's firm became protected by BC law for the left three columns[17], and $BC_{i,t}$, a CEO's cumulative exposure to a business combination law (by year t) for the right three columns. All regressions control for a CEO's age and firm location fixed effects.[18] All coefficients are shown as hazard ratios: a coefficient smaller than 1 means that the risk of failure (death) decreases with positive values of that variable. We cluster standard errors at the state of incorporation level, given that the BC laws were introduced based on firms' state of incorporation. As pointed out in Section 2.2, we restrict the sample to CEOs who were appointed before the enactment of a BC law to alleviate selection concerns.

In columns (1) through (3), we summarize the total effect of the BC laws with the indicator for having been exposed to a BC law by time t. These estimates quantify to the group-level divergence in survival reported in Figure 2. In columns (4) through (6) we then estimate a linear (in hazards) effect in years of exposure to more lenient corporate governance. Columns (1) and (4) include linear controls for time trends and CEO age. The estimated hazard ratio on the BC indicator is 0.763, and the ratio on the BC law exposure is 0.955; both are significant at 1%. The difference between the estimates primarily reflects that the indicator captures the total effect of BC exposure, while the cumulative exposure measure should be interpreted as the effect of an additional year of

[17]Note that this indicator equals 1 for all the years after the BC law passage, including the years after the CEO left the firm. We drop subscripts in the tables.

[18]We follow Gormley and Matsa (2016) in assigning location based on headquarters instead of state of incorporation, since most firms' main operations are in the state where its headquarters is located. In robustness checks, we verify that our main results remain unaffected when we instead include state of incorporation fixed effects.

Table 3: Business Combination Laws and Mortality

DEPENDENT VARIABLE: $Death_{i,t}$						
	(1)	(2)	(3)	(4)	(5)	(6)
$I(BC)$	0.763***	0.769***	0.776***			
	[0.062]	[0.068]	[0.067]			
BC				0.955***	0.958***	0.959***
				[0.005]	[0.005]	[0.005]
Age	1.113***	1.123***	1.124***	1.111***	1.121***	1.122***
	[0.006]	[0.005]	[0.004]	[0.007]	[0.005]	[0.005]
Year	1.005	1.002		1.005	1.001	
	[0.004]	[0.005]		[0.004]	[0.004]	
Location FE (HQ)	Y	Y	Y	Y	Y	Y
FF49 FE		Y	Y		Y	Y
Year FE			Y			Y
Number of CEOs	1,605	1,605	1,605	1,605	1,605	1,605
Observations	50,530	50,530	50,530	50,530	50,530	50,530

Notes. This table shows the effect of business combination laws on CEO mortality rates (see Equation 1). The dependent variable is an indicator variable that equals one if the CEO dies in a given year and zero otherwise. The main independent variable of interest is $I(BC)$ in the left three columns, and BC in the right three columns. All variables are defined in Appendix A. The estimated model is the Cox (1972) proportional hazards model. Fixed effects are included as indicated at the bottom of the table. Standard errors, clustered at the state of incorporation level, are shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

exposure. Based on the latter estimate, a one-year increase in exposure to more lenient governance is estimated to reduce a CEO's mortality risk by 4.5%[19].

The results do not change when we make comparisons within industry or include a more flexible control for time. The inclusion of industry fixed effects in Column (2) and (5) (using the Fama and French (1997) classification of firms into 49 industries) has virtually no effect on the hazard ratio on BC law exposure. These fixed effects address the possibility that certain industries are dif-

[19] To address any concerns regarding the use of fixed effects in non-linear models, we also estimate the model with only linear age and linear year as controls. Estimates are 0.776 for $I(BC)$ and 0.955 BC respectively, which are very similar to the estimates shown in the Table 3.

ferentially incorporated in BC-law states. The estimates are now 0.769 and 0.958 and both remain significant at 1%. Similarly, adding year fixed effects in Column (3) and (6), instead of the linear time control, has virtually no effect on the estimated coefficients. Turning to the control variables, the coefficient on *Age* is significantly positive (i.e. the hazard ratio is above one), as expected. This merely reflects that older people have a higher estimated risk of dying. The linear time control, by contrast, is close to one and insignificant, suggesting no general time trends in the survival of CEOs over the sample period.

We next translate our estimated BC effects on hazard into effects on life expectancy to get an idea of their magnitude. The thought is to calculate how much older a CEO needs to be to offset the life-extending effects of the BC law treatment. Note that our estimated effect of age on death hazard is 1.124, i.e., 12.4% increase per year older. We can then calculate the number of years we need to compensate the BC effect to be 2.17 years by solving $(\frac{1}{1.124})^x = 0.776$. Alternatively, we can also compare our estimated hazard with general U.S. population. For example, at age 57 (the median age of CEOs in our sample), the one-year mortality rate of a male American born in 1925 (the median birth year of CEOs in our sample) is 1.366%[20]. The typical range of prolonged exposure to lenient governance, i.e., 4.4. years, pushes this rate down to 1.119%, which is roughly the mortality rate of a male born in 1925 and aged 54. These differences imply gains in remaining life expectancy of around 3 years for a typical CEO in our sample. This effect is sizable and comparable to known health threats. For example, smoking until age 30 is associated with a reduction in longevity by roughly one year (Jha et al. (2013)).

If the differences in life expectancy result from stress experienced on the job, it is natural to ask to know whether CEOs are at greater risk of death while in office and shortly after leaving. Deaths in office can be costly to firm valuation (Johnson et al. (1985)). Our evidence on the timing of mortality can only be suggestive due to the sample size. With this caveat, we examine the

[20]The mortality rate numbers come from Human Mortality Database (Human Mortality Database (2019)). We use the mortality rates for Unites States.

short-term effects of stress on CEO mortality by comparing the fraction of CEOs who passed away within the first five years after leaving office across different sub-groups. We again group CEOs into cohorts based on their true overall exposure to a BC law. Following the sorting in Figure 2a, we look at CEOs who enter and exit our data set in the 1970s, those who enter in the 1980s and never serve under a BC law, and those who enter in the 1980s and do eventually serve under lenient governance.

Figure 3 shows a gap in the cumulative fraction of deaths between the latter two groups that emerges after CEOs leave office. There is, however, no difference in the probability of death while in office; of course, it is likely that CEOs who are in poor health step down before dying. Five years after leaving office, the fraction of CEOs in the "1980s cohorts without BC exposure" who have passed away is 7.8 percent, whereas that of CEOs in the "1980s cohorts with BC exposure" is only 5.6 percent—a decrease of 2.2 percentage points, or 28%. As in the long-run depicted in Figure 2, the difference between the first two groups is smaller and undetermined in the immediate years after stepping down. While lacking the statistical power to conduct statistical tests over this short follow-up, the point estimates are consistent with effects which appear immediately after (but not before) stepping down as CEO.

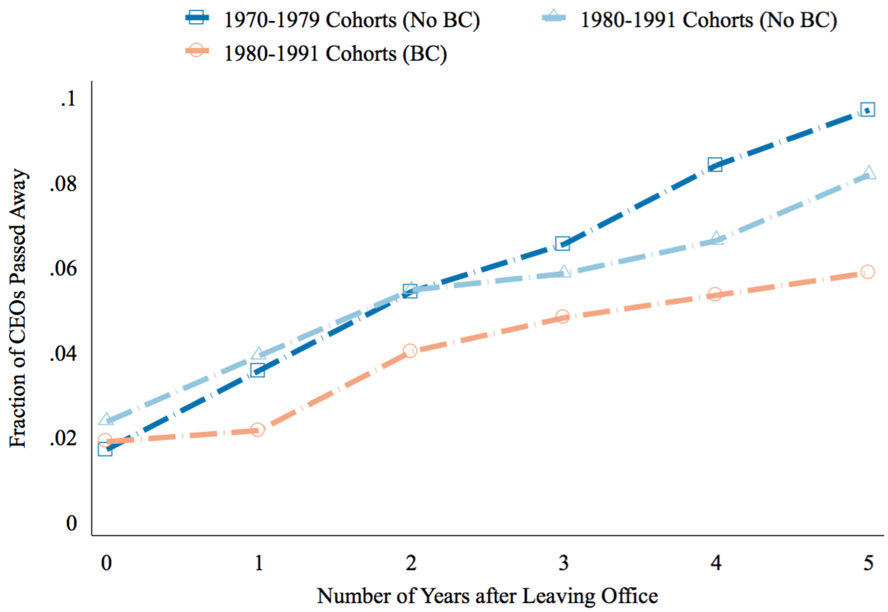
In sum, these results lend strong support to the hypothesis that changes in job demands experienced as a CEO, such as arising from a more lenient corporate governance regime, have significant effects on a CEO's health.

4.3 Alternative Specifications

4.3.1 Predicted Length of Exposure

Our first alternative specification uses a CEO's predicted exposure to BC laws rather than true exposure. These estimates are intended to purge the above per-year estimates of any endogeneity related to the length of exposure. It is important to note that this concern does not apply to the

Figure 3: Fraction of CEOs Passing Away after Leaving Office



Notes. The figure shows the cumulative fraction of CEOs that pass away within the first five years after leaving office for different cohorts. The length of time between stepping down and passing away is calculated using month-level data. If the time length is t years and m months with $m < 6$, it will be classified as t years. If $m \geq 6$ instead, it will be classified as $t+1$ years. For example, 0 years means that the CEO died in office or within the first six months after stepping down. The figure compares the share of deaths of CEOs who enter and exit our data set in the 1970s and thus never experienced a BC law, those who entered in the 1980s and never served under a BC law, and those who entered in the 1980s and did eventually serve under a BC law.

indicator strategy, and thus, the endogeneity concern here does not threaten our main findings in Table 3, merely the magnitude of the per-year estimates.[21] This approach requires us to first estimate a prediction model for CEO tenure. From this model, we can construct predicted BC exposure, which we can then use as the main variable of interest in the hazard regressions.

The first step is to construct the variable capturing predicted exposure to BC laws, given by a CEO's predicted remaining tenure at the time the BC law is passed. We first predict, for every

[21] We examine effects on tenure directly in Section 4.5.

CEO-year (including years after the passage of a BC law), the CEO's remaining tenure:

$$RemainTenure_{i,t} = X_{i,t}'A + e_{i,t} \tag{2}$$

The control variables are an age cubic, tenure cubic, the CEO's cumulative exposure to a BC year until year t, $BC_{i,t}$, interacted with an indicator for above or below median BC exposure, and fixed effects for industry (Fama French 49 industries), year, state of headquarters, and tenure start year. We then define t^* as the year when $I(BCLawPassed_{s(i),t} = 1)$ for CEO serving in state $s(i)$. In other words, t^* is the year the BC law is passed, i.e. treatment turns on. We use the predicted remaining tenure in the year of BC law (i.e. t^*) from Equation (2) to construct:

$$\widehat{BC}_i^* = I(BCLawPassed_{s(i),t} = 1) \times \widehat{RemainTenure}_{i,t^*} \tag{3}$$

$\widehat{BC}_i^*$ is a CEO's predicted exposure to BC laws, as determined by predicted remaining tenure in the year of the BC law passage. Note that $\widehat{RemainTenure}_i^*$ is backward-looking, i.e. its construction only uses information from years up to t^* and not from years after the BC law passage. Using this variable, we construct a CEO's cumulative predicted BC exposure, $\widehat{BC}_{i,t}$ (i.e. a variable that counts the length of predicted exposure until year t) as: (i) in the control group, $\widehat{BC}_{i,t} = 0\ \forall t$; (ii) if not yet treated $\widehat{BC}_{i,t} = 0\ \forall t < t^*$; (iii) for each year k following t^*, i.e. $t = t^* + k$, define $\widehat{BC}_{i,t} = \min\{k+1, \widehat{BC}_i^*\}$. Note that k is allowed to be fractional if the BC law goes into effect in the middle of the year.

We can then use the predicted cumulative BC law exposure in our hazard models. Specifically, we estimate the following hazard regression:

$$h(\widehat{BC}_{i,t}, X_{i,t}) = h_0(t)\ \exp\{\beta \widehat{BC}_{i,t} + \boldsymbol{\delta}'\boldsymbol{X}_{i,t}\} \tag{4}$$

Table 4 presents the hazard regression results for predicted BC exposure. We include controls and fixed effects in the same order as in Table 3. Since this approach involves a generated regressor, we bootstrap standard errors, using the block bootstrap method (a block is a state of incorporation cluster), with 200 iterations.

Table 4: Predicted Exposure to Business Combination Laws and Mortality

DEPENDENT VARIABLE: $Death_{i,t}$			
	(1)	(2)	(3)
$\widehat{BC}$	0.960***	0.966**	0.966**
	[0.013]	[0.017]	[0.017]
Age	1.112***	1.122***	1.122***
	[0.010]	[0.010]	[0.010]
Year	1.004	1.000	
	[0.006]	[0.007]	
Location FE (HQ)	Y	Y	Y
FF49 FE		Y	Y
Year FE			Y
Number of CEOs	1,605	1,605	1,605
Observations	50,530	50,530	50,530

Notes. This table shows the effect of predicted exposure to BC laws on on CEO mortality rates (see Equation 4). The dependent variable is an indicator variable that equals one if the CEO dies in a given year and zero otherwise. The main independent variable of interest is $\widehat{BC}$, a CEO's predicted cumulative exposure to a BC law (see Equations 2 and 3 for details on the prediction model). All variables are defined in Appendix A. The estimated model is the Cox (1972) proportional hazards model. Fixed effects are included as indicated at the bottom of the table. Bootstrapped standard errors, using the block bootstrap method with 200 iterations, are shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

The results using predicted BC exposure corroborate our baseline findings: predicted BC exposure is estimated to significantly affect CEOs' mortality rates. The estimated hazard ratios, ranging between 0.960 and 0.966, are very similar to those in Table 3. A regression of true BC exposure on predicted exposure yields a coefficient of 0.87, which indicates that the prediction closely approximates the true exposure. As the realized BC exposure can be well-predicted from a few variables that were determined before treatment, there is little evidence of endogeneity in the BC exposure regressions reported in Table 3. While the bootstrapped standard errors roughly double in size compared to Table 3, the coefficient of interest remains significant in all columns, either at 1% or 5%.

In sum, the association between years of exposure to lenient corporate governance stringency and CEO lifespan holds irrespective of whether we analyze true exposure to BC laws or predicted exposure as of the year in which a CEO was first affected by the laws.

4.3.2 Nonlinear Effects

Introspection suggests the effects of the BC laws may have the largest effect in the initial years after passage, as CEOs adjust their work schedules, pace and focus of new projects, and expectations regarding the threat of a hostile takeover. The survival plots shown in Figure 2 support this notion, as there appears to be a diminishing incremental effects of exposure to a more lenient governance regime on survival rates.

To examine this empirically, we estimate a modified version of Equation 1 that allows for nonlinear effects. Specifically, we split the cumulative BC exposure variable into below-median ($BC_{i,t}^{(\text{min-}p50)}$) and above-median ($BC_{i,t}^{(p51\text{-max})}$), and jointly include these variables in our hazard model. We define these variables such that above-median exposure picks up incremental exposure, in addition to initial exposure.[22]

Table 5 presents the results, with controls and fixed effects included as before. Across columns, the hazard ratio on below-median BC exposure is strongly significant (at 1%), and ranges between 0.907 and 0.915. These estimates imply that initial insulation from market discipline yields substantial reductions in mortality risk; the initial exposure effect corresponds to a 9% more beneficial survival rate. By contrast, the coefficient on above-median BC exposure is close to one and insignificant. Thus, in line with the survival plots of the data, we see that survival gains are indeed

[22]For example, for a CEO with a current BC exposure of four years, $BC_{i,t}^{(\text{min-}p50)}$ would take the value 4, and $BC_{i,t}^{(p51\text{-max})}$ the value 0. In the following year $(t+1)$, assuming sufficient predicted BC exposure, $BC_{i,t+1}^{(\text{min-}p50)}$ would be set to 4.4, and $BC_{i,t+1}^{(p51\text{-max})}$ to 0.6. In year $(t+2)$, $BC_{i,t+2}^{(\text{min-}p50)}$ remains at 4.4, and $BC_{i,t+2}^{(p51\text{-max})}$ increases to 1.6.

Table 5: Business Combination Laws and Mortality, Nonlinear Effects

DEPENDENT VARIABLE: $Death_{i,t}$			
	(1)	(2)	(3)
$BC^{(\min\text{-}p50)}$	0.907***	0.913***	0.915***
	[0.021]	[0.023]	[0.023]
$BC^{(p51\text{-}\max)}$	0.992	0.993	0.992
	[0.015]	[0.017]	[0.016]
Age	1.110***	1.120***	1.120***
	[0.007]	[0.005]	[0.005]
Year	1.007*	1.004	
	[0.004]	[0.004]	
Location FE (HQ)	Y	Y	Y
FF49 FE		Y	Y
Year FE			Y
Number of CEOs	1,605	1,605	1,605
Observations	50,530	50,530	50,530

Notes. This table shows the effect of variation in corporate governance stringency on CEO mortality rates, split by below-median and above-median exposure to BC laws. The dependent variable is an indicator variable that equals one if the CEO dies in a given year and zero otherwise. The main independent variables of interest are $BC_{i,t}^{(\min\text{-}p50)}$, capturing below-median exposure, and $BC_{i,t}^{(p51\text{-}\max)}$, capturing any incremental BC exposure. All variables are defined in Appendix A. The estimated model is the Cox (1972) proportional hazards model. Fixed effects are included as indicated at the bottom of the table. Standard errors, clustered at the state of incorporation level, are shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

concentrated in the first few years of exposure to reduced monitoring.[23] Although we document increases in tenure below (in Section 4.5), these results suggest that gains in life expectancy accrue in the first few years after exposure, consistent with the change in working conditions as the primary mechanism of action.

[23]Point estimates from a non-linear predicted-exposure model are similar in magnitude to those reported here, though less precisely estimated.

4.4 Robustness Tests

We conduct three series of robustness tests. For brevity, we report their results in the appendix (Appendix B).

4.4.1 Additional CEO and Firm Controls

As the first robustness test, Appendix-Table B1 contains the results when we control for CEO pay and firm size. Information on executive compensation is included in the data provided by Kevin Murphy (Gibbons and Murphy 1992). Data on firm assets and employees is from CRSP and Compustat.[24]

Two findings emerge. First, the coefficient on the BC law exposure variable is almost identical to those in Table 3 and remain significant at 1%. Second, in none of the three specifications, any of the coefficients on the additional control variables is significant. This might reflect endogenous selection on observables. In terms of pay, the results are also in line with the notion that in the very upper tail of the income distribution, income is no longer correlated with health outcomes (Chetty et al. 2016b).

We also re-estimate the models in Table 3, Table 4, and Table 5 with CEO birth year fixed-effects. We obtain similar coefficients and levels of significance for all specifications (see Table B2).

4.4.2 First-Time Enactment of Second-Generation Anti-Takeover Laws

Our focus on BC laws thus far is motivated by the notion that they have been shown to create substantial conflicts of interest between managers and shareholders and unveil managerial preferences

[24]For all three additional regressors, we linearly interpolate missing data points. Nonetheless, the number of observations decreases, as there are observations where data on one of the three additional controls is missing in all years.

(Bertrand and Mullainathan 2003, Gormley and Matsa 2016).[25] In addition to BC laws, there are four other types of laws that were passed by individual states since the 1980s: Control Share Acquisition laws, Fair Price laws, Directors' Duties laws, and Poison Pill laws.[26] We refer to the first of these five laws passed by a given state as the *First Law*. Appendix-Figure B1 visualizes the *First Law* enactment by states over time.

Appendix-Table B3 re-estimates Table 3 using the first-time enactment of any of the five anti-takeover laws listed above as identifying variation. Consistent with our main findings, we estimate a significant increase in longevity for CEOs who served under a less stringent governance regime, again concentrated within the initial years of exposure.[27] The estimated effect sizes are very similar compared to our main specification using BC laws; the hazard ratios range between 0.955 and 0.957, compared to between 0.955 and 0.959 in Table 3.[28]

This additional test highlights that our results should not be interpreted narrowly as applying to BC laws specifically; rather, they are replicable using other types of laws that induced plausibly exogenous variation in corporate monitoring intensity.

4.4.3 Institutional and Legal Context of the Anti-Takeover Laws

Karpoff and Wittry (2018) emphasize that the institutional and legal context of anti-takeover laws should be taken into consideration when using these laws for identification in corporate finance

[25]We do not to take a stance on whether BC laws constituted the most important legal variation in the 1980s impacting the effectiveness of corporate governance (see Cain, McKeon, and Solomon 2017 and Karpoff and Wittry 2018).

[26]Control Share Acquisition laws raised the bar for a hostile acquirer to gain control of a firm, since they prohibit anyone acquiring a large equity stake to use their voting rights. Fair Price laws also increased the hurdles and cost of a hostile takeover, since they dictate that acquirers pay a fair price for shares acquired in a takeover attempt, where fair could, e.g., mean the highest price paid by the acquirer for shares of the target within the last 24 months (cf. Cheng, Nagar, and Rajan (2004)). Directors' Duties laws extended the board members' duties to incorporate the interests of non-investor stakeholders, providing legal justifications to make decisions that do not necessarily maximize shareholder value. Poison Pill laws guaranteed that the firms covered by the law had the right to use poison pill takeover defense, and thus further protected these firms from takeovers.

[27]We now split into below-median and above-median *First Law* exposure; the cutoff is 4.45 (before 4.41 years).

[28]The *First Law* results are also robust to including the additional CEO and firm level controls from Section 4.4.1 (see Appendix-Table B4).

settings. They propose two sets of robustness tests, revolving around endogenous firm responses to anti-takeover laws and possible confounding effects of first-generation anti-takeover laws.

Appendix-Table B5 shows the estimates after excluding firms that lobbied for the passage of the second-generation laws (Panel A), excluding firms that opted out of coverage by the laws (Panel B), and excluding firm-years in which firms had adopted firm-level anti-takeover defenses (Panel C).[29] Across panels, our findings are robust to these restrictions suggested by Karpoff and Wittry (2018). In all columns, the hazard ratio on BC exposure remains significant at 1%. In addition, the hazard ratio estimates are nearly unchanged, ranging between 0.953 (Panel C, Column (1)) to 0.959 (Panel A, Column (3); Panel B, Column (2) and (3)).

Appendix-Table B6 accounts for potential confounding effects of first-generation anti-takeover laws. Karpoff and Wittry (2018) raise the concern that firms in the control (i.e. no BC exposure) group before 1982 might also experience lenient governance because of the coverage by first-generation laws (since the first-generation anti-takeover laws truly lost their effect only starting from June 1982 after the *Edgar v. MITE* ruling). We address this concern through three cuts of the data: restricting the sample to years from 1982 onwards only (Panel A), restricting the sample to CEOs who stepped down in or after 1982 and thus served during the "post-first-law period" (Panel B), and restricting to the CEOs who began their tenure in or after 1982. In all sub-samples, we continue to estimate hazard ratios substantially below one for initial BC exposure, similar in size to those in the main table. The coefficient remains significant at 1% in sub-samples A and B. In the most restrictive sub-sample C, we lose statistical power (standard errors more than triple), though

[29] For all sample restrictions, we follow the suggested procedure in Karpoff and Wittry (2018). In Panel A, we remove the 46 firms identified by these authors as having lobbied for the passage of laws. In Panel B, we use data from the Institutional Shareholder Services (ISS) Governance (formerly, the RiskMetrics) database, covering 1990 to 2017, to identify opt-out firms. In Panel C, we combine data from two sources to identify firms with firm-level defenses: the ISS database, as before, as well as the data provided to us by Cremers and Ferrell (2014), extending the G-index measure of corporate governance (Gompers, Ishii, and Metrick 2003) for the time period from 1977 to 1989. We back out whether firms used firm-level defenses during 1977-1989 by "subtracting" the state-wide laws in place from the G-index, which combines firm-level and state-level defenses. The firm-level defenses include Golden Parachutes, Cumulative Voting, etc. For details on which firm-level defenses are included, please refer to Gompers, Ishii, and Metrick 2003.

the point estimate remains unaffected.

In a final robustness check, we move beyond the tests suggested in Karpoff and Wittry (2018) and create sub-samples based on firms' state of incorporation and industry affiliation.[30] We exclude firms that are incorporated in Delaware or in New York, the two most common states of incorporation in our sample (Panel A), firms in the Banking industry (Panel B), or firms in the Utilities industry (Panel C). We note that as before, our regressions sometimes lack statistical power due to the large reductions in sample size (especially in Panel A, which excludes more than 50% of our sample). Nonetheless, in all three panels, the hazard ratio estimates on below-median BC exposure are barely affected by these data cuts.

4.5 Intermediate Outcomes: Tenure, Retirement, and Pay

In the above analysis, we show private benefits to CEO health arising from anti-takeover protection. We observe several intermediate outcomes, pay and CEO tenure, which may inform why CEOs live longer when facing a less stressful work environment.

We begin with an analysis of CEO tenure. We have, in theory, no strong prediction as to how tenure should respond to the anti-takeover laws. On one hand, CEOs may become entrenched as a result of protection from the laws, a form of rent-seeking. CEOs may even stay on the job longer precisely because they experience health gains. On the other hand, CEOs who are appointed before the passage of anti-takeover laws may specialize in holding off hostile takeovers, or be pushed out of their positions earlier if they reduce effort on the job. To estimate the effects we once again use the hazard model from the survival analysis.

The results for tenure reveal that the anti-takeover laws increase the tenure of incumbent CEOs. In column 1 of Table 6, we find that the separation hazard falls by 9 percent in our baseline model. Adding industry effects in column 2 has no effect on the coefficients. In column 3, we find attenu-

[30] See Giroud and Mueller (2010) and Gormley and Matsa (2016) for similar robustness checks.

Table 6: Business Combination Laws and Retirement

DEPENDENT VARIABLE: $TenureExit_{i,t}$			
	(1)	(2)	(3)
$I(BC)$	0.910***	0.907***	0.958**
	[0.020]	[0.020]	[0.021]
Age	1.100***	1.107***	1.104***
	[0.011]	[0.012]	[0.012]
Year	1.096***	1.100***	
	[0.016]	[0.016]	
Location FE (HQ)	Y	Y	Y
FF49 FE		Y	Y
Year FE			Y
Number of CEOs	1,605	1,605	1,605
Observations	17,895	17,895	17,895

Notes. This table shows the effect of exposure to BC laws on the tenure of CEOs. The dependent variable is an indicator variable that equals one if the CEO ends his or her tenure in a given year and zero otherwise. The main independent variable of interest is $I(BC)$, a CEO's cumulative exposure to a BC law. All variables are defined in Appendix A. The estimated model is the Cox (1972) proportional hazards model. Fixed effects are included as indicated at the bottom of the table. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

ated but still significantly lower separation rates for CEOs after the inclusion of year fixed effects. These shift in the hazard of separation are notably smaller than the effects on life expectancy.

In further analysis reported in Figure B3, we examine the age-specific shift the retirement hazard rate.[31] The results show that increases in tenure are driven by fewer retirements when CEOs are in their 50s and early 60s, and an increase in retirements above age 65, including a long tail of tenures into the 80s and 90s. These results suggest that the business combination laws allowed CEOs to extend their tenures into ages in which most workers are reducing labor supply and beginning the transition to partial or full retirement.[32] Thus, we suspect that additional years

[31]CEOs may continue to work after they separate, however, we find very few cases in which the CEOs step down at one firm and then become CEO at another firm in our sample.

[32]The hazard results also allow us to rule out an important role for the end of mandatory retirement that occurs

of service as CEO replaced years of retirement.

Given these effects on tenure, it is in principle possible that CEOs experience a longer life as a result of their increase in tenure, rather than or in addition to the changes in stress. This would be surprising, since the position of CEO is generally thought to be quite demanding even in the presence of anti-takeover laws. It is also not supported by our analysis of non-linearities in the effect, which suggest the largest gains in life expectancy occur with the first 5 or so years of exposure. Previous studies of the effect of retirement on longevity in the general population have found small or even beneficial effects of retirement on health (Hernaes, Markussen, Piggott, and Vestad (2013), Insler (2014), Fitzpatrick and Moore (2018)). Nevertheless, effects may differ for high-prestige positions such as CEOs.

We next turn to CEO pay. As noted by Bertrand, Mullainathan, et al. (1999), it is theoretically unclear how CEO pay should respond to anti-takeover laws. On one hand, in a model of compensating differentials CEOs may experience a decrease in pay as the working conditions improve. On the other hand, CEOs may use the increase in autonomy to extract additional private benefits in the form of higher compensation. In column 1 of Table 7, we first estimate linear regressions of CEO pay on the same controls and fixed-effects as in the hazard analyses above. In column 2, we further add the control variables that appear in Bertrand, Mullainathan, et al. (1999). Finally, in the column 3 of Table 7, we add firm fixed-effects (in place of industry fixed effects)[33] as in Bertrand, Mullainathan, et al. (1999):

$$ln(Pay_{it}) = \alpha_t + \beta_i + \gamma X_{i,t} + \delta I(BC_{i,t}) + e_{i,t}.$$

We find suggestive evidence in favor of an increase in pay, however, the results are somewhat sensitive to specification. In column 1 of Table 7, we find a 8.6 percent increase in pay when we

during our sample period. Although there is a large spike in retirements at ages 64 and 65, there is no association between retirement at these ages and exposure to the business combination laws.

[33]Note that the variable $I(BC_{i,t})$ takes 1 only for the years when a CEO became protected under Business Combination Laws. Therefore, it is the same as the interaction term $Treatment_i * After_{it}$ in Bertrand, Mullainathan, et al. (1999).

Table 7: Business Combination Laws and CEO Pay

DEPENDENT VARIABLE: $ln(Pay_{it})$	(1)	(2)	(3)
$I(BC)$	0.086	0.086*	0.040
	[0.058]	[0.046]	[0.050]
Age	-0.005***	0.001	-0.026*
	[0.001]	[0.011]	[0.014]
Age^2		-0.000	0.000
		[0.000]	[0.000]
$Tenure$		0.035***	0.024***
		[0.003]	[0.006]
$Tenure^2$		-0.001***	-0.001***
		[0.000]	[0.000]
$ln(Assets)$		0.145***	0.066***
		[0.016]	[0.020]
$ln(Employees)$		0.017	-0.029
		[0.013]	[0.030]
Location FE (HQ)	Y	Y	
FF49 FE	Y	Y	
Year FE	Y	Y	Y
Firm FE			Y
Number of CEOs	1,553	1,553	1,553
Observations	17,720	17,720	17,720

Notes. This table estimates the compensation differentials between CEOs who were under the BC law and those who were not. The third column follows the specification in Bertrand, Mullainathan, et al. (1999) exactly, but our CEO sample is larger. Standard errors, clustered at the state of incorporation level, are shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

use our basic specification from the hazard model. This is our preferred specification, as it excludes any post-treatment outcomes from the right-hand side and parallels our last column in the survival analysis. In column 2, we include controls for tenure and firm's assets and employees. These controls may themselves be affected by the reform, and may therefore absorb the effect of the anti-takeover laws. Nevertheless, we still obtain a 8.6 percent increase in pay, and the precision of the estimate increases. In column 3, we use the baseline specification from Bertrand, Mullainathan, et al. (1999), which also includes firm fixed-effects. We find a positive coefficient that is quite close to the result in the earlier paper (a 5.4 percent increase in pay). In comparing the results to the earlier work, it is important to note that our analysis is conducted at the CEO-level sample, and restricts the sample to incumbent, pre-BC CEOs. Firm-level analyses which allows for CEO turnover would answer a different question than our interest here.

Taken together, the evidence strongly speaks against a reduction in annual pay, i.e. compensating differentials, and instead suggests that CEOs may have earned additional rents in the form of higher pay. More definitively, the increase in tenure we document in combination with suggestive evidence on an increase in pay implies that lifetime compensation rises as a result of exposure to the laws. It is unlikely, however, that higher pay increases life expectancy directly for CEOs, as these are already-wealthy individuals with access to health care and other health inputs that can be purchased. More broadly, the literature has found little causal relationship of income on life expectancy for wealthy individuals (Cesarini, Lindqvist, Östling, and Wallace (2016)).

5 Results: Financial Distress and Life Expectancy

We now turn to a different shock to a CEO's stress level: industry-wide distress shocks. Distress shocks constitute a useful alternative approach to analyzing the health consequences of a CEO's job demands. They induce a shift in stress levels in opposite direction compared to that of insulating anti-takeover laws, and are of less permanent nature; yet, they plausibly lead to a substantial temporary increase in stress factors. Like anti-takeover laws, industry distress has been frequently used to analyze firm-worker-related questions, but not much is known about the potential health consequences for the parties affected.[34]

[34]For example, Opler and Titman (1994) study the interaction of industry distress and high leverage. Acharya, Bharath, and Srinivasan (2007) analyze the effect of industry distress on defaulted firms and creditor recoveries. Babina (2019) explores the impact of distress of worker exit rates and entrepreneurship.

In the spirit of Opler and Titman (1994), Babina (2019), and Acharya, Bharath, and Srinivasan (2007), we define an industry as distressed in year t if the median firm's two-year stock return (forward-looking) is less than -30%.[35] Using this definition, we find that 648 out of the 1,605 CEO, or 40%, experience at least one industry shock during their tenure. However, industry shocks are rare. Conditional on witnessing distress, the median CEO experiences one year of distress, the 75th percentile CEO two years, and the maximum distress experienced is 9.7 years.[36]

Table 8 reports the effect of cumulative distress experience on CEO life expectancy, again using the Cox (1972) proportional hazards model and following the main specification in Table 3 for BC law exposure. In addition to the controls and fixed effects from Table 3, all models control for exposure to BC laws, given our evidence that these laws significantly contribute to a CEO's lifespan. Across specifications, the hazard ratios on *Industry Distress* reveal substantial adverse effects of industry shocks on CEOs' long-term health. The hazard ratios are nearly unchanged across specifications, ranging between 1.042 and 1.045, significant at conventional levels.

These estimates point to similar effect magnitudes as those estimated for lenient governance in Section 4. For example, a four-percent increase in mortality rates of a 60-year-old born in 1925 increases his one-year mortality likelihood from 1.733% to 1.802%. The latter probability is close to the mortality rate of 60-year-olds born in 1921, again implying multi-year effect when expressed in terms of birth year differences. Similar to above, these differences correspond to plausible impacts on life expectancy; at age 60, the difference in remaining life expectancy between American men born in 1925 and 1921 is 0.40 years.

The estimated coefficients on the control variables are similar to above. The coefficients on

[35]We follow Babina (2019) for the details of this definition. In particular, we (i) use SIC3 industry classes for this analysis, (ii) restrict to single-segment firm from CRSP/Compustat, i.e. disregard firms with multiple reported segments in the Compustat Business Segment Database, (iii) drop single-segments firms if the reported segment sales differ from those in Compustat by more than 5%, (iv) restrict to firms with sales of at least $20 million, and (v) exclude industry-years with fewer than four firms.

[36]Again, we allow for non-integer values of distress experienced, if a person is appointed as CEO or steps down during the year, and we classify that industry-year as distressed. Also, given that industry shocks are infrequent, with the large majority of CEOs experiencing no more than two years of distress, we do not separately analyze the incremental effects of high industry shock exposure (which would be estimated off of very few CEOs).

Table 8: Industry Distress and Mortality

DEPENDENT VARIABLE: $Death_{i,t}$			
	(1)	(2)	(3)
Industry Distress	1.042**	1.045*	1.044*
	[0.019]	[0.024]	[0.024]
Age	1.112***	1.122***	1.122***
	[0.007]	[0.005]	[0.005]
Year	1.007	1.004	
	[0.005]	[0.005]	
Linear BC Control	Y	Y	Y
Location FE (HQ)	Y	Y	Y
FF49 FE		Y	Y
Year FE			Y
Number of CEOs	1,605	1,605	1,605
Observations	50,530	50,530	50,530

Notes. This table shows the effect of industry distress exposure on CEO mortality rates. The dependent variable is an indicator variable that equals one if the CEO dies in a given year and zero otherwise. The main independent variable of interest is *Industry Distress*, a CEO's cumulative exposure to industry distress shocks. All variables are defined in Appendix A. The estimated model is the Cox (1972) proportional hazards model. Fixed effects are included as indicated at the bottom of the table. Standard errors, clustered at the state of incorporation level, are shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

Age continue to be positive (hazard ratio above one); the hazard ratios on *Year*, in the linear time controls specifications, are again very close to one. We also point out that the coefficients on BC exposure are largely unaffected by the addition of industry distress to the model. As before, BC exposure is estimated to result in meaningful reductions in mortality rates in all specifications; the hazard ratios are virtually unchanged (if anything, the effects are estimated to be larger) compared to Table 3, and remain significant at 1%.

Again, we present an array of robustness checks in the appendix. Following the BC analysis, we estimate a version with additional CEO and firm controls (Appendix-Table B8), and a version based on an extended sample, including the 295 CEOs we dropped from the analysis thus far as they were appointed after the introduction of BC laws (Appendix-Table B9).[37] Across these robustness checks, the coefficient on industry distress exposure remains similar in magnitude, ranging between 1.047 and 1.065. In addition, in all robustness tests, the significance level in fact increases to the 1% level.

All together, the industry shock analysis lends support to the notion that significant and unexpected changes in the work environment of CEOs have meaningful effects on their health in terms of life expectancy.

6 Results: Financial Distress and Aging

In this final section of the paper, we move beyond the focus on longevity and ask instead whether there are more immediate, non-fatal manifestations on CEOs' health associated with heightened stress and demanding job environments. We ask whether stress and, in particular, experience of industry distress translates into accelerated aging of CEOs. For this analysis, we make use of the very recent advances in machine learning related to *apparent* age estimation. Thus far, most age estimation softwares have focused on estimating a person's *biological*, i.e. "true" age. Only recently, there has been a boost in research aimed at estimating a person's apparent age, i.e. how old a person looks. The progress in this area has been made possible by the development of deep learning methods (convolutional neural networks or CNNs) and the increased availability of large data sets of facial images with associated true and apparent ages (the latter estimated by people).

For our analysis, we use a machine-learning based software provided to us by Antipov, Baccouche, Berrani, and Dugelay (2016), which has been specifically developed for the problem of

[37] We also test whether experiencing a specific shock, such as the stock market downturn in 1987 or economic recession during 1981 to 1982, has impact on CEO mortality. We obtain estimates that indicate the CEOs who experienced those shocks tend to have higher mortality hazard. However, we are under-powered (only fewer than 5% of the CEOs in our sample experienced such specific shocks) and the estimates are not statistically significant.

apparent age estimation. Their software is the winner of the 2016 Looking At People Apparent Age Estimation competition. We provide a detailed discussion of CNNs and the training steps associated with our software in Appendix C and provide a brief summary here. The software is based on the Oxford's Visual Geometry Group deep convolutional neural network architecture. In a first step, it has been pre-trained on more than 250,000 pictures with information on people's true age using the Internet Movie Database and pictures from Wikipedia. In a second step, it has been fine-tuned for apparent age estimation using a newly available data set of 7,591 facial pictures, each of which was rated by at least ten people in terms of the person's age. Both the distribution of true ages used for pre-training and human age estimations used for software validation covers people from all age groups, including children and elderly people. The output of the neural network is a 100×1 vector of probabilities associated with all apparent ages between 0 and 99 years. The apparent age point estimate is derived by multiplying each apparent age with its associated probability. The software also carries out eleven-fold cross-validation by drawing 5,613 images for training and validation each time. The ultimate output of the software is the average apparent age estimation of the eleven models.

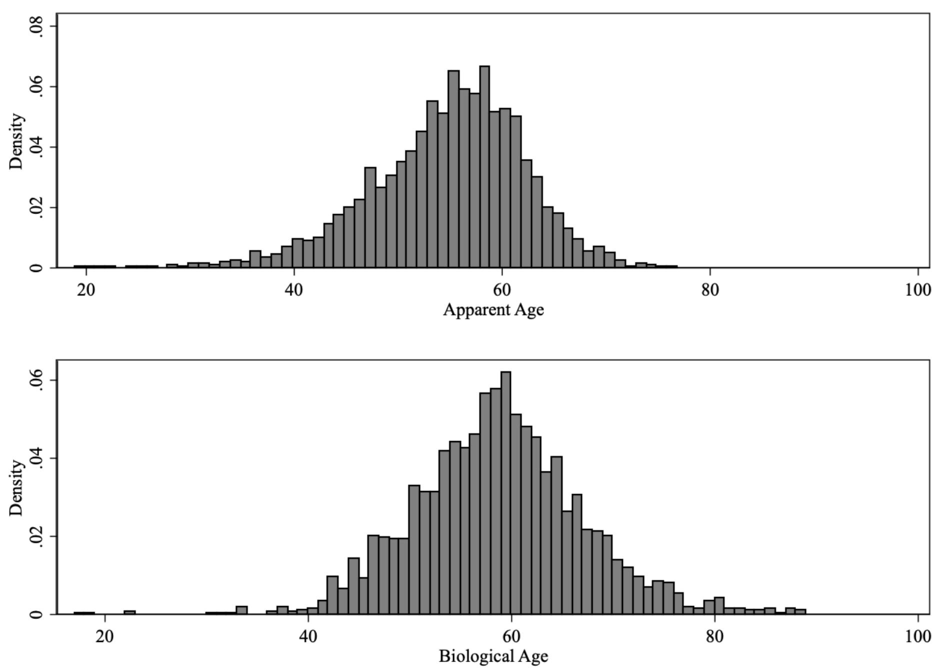
To carry out our analysis, we manually collect a large set of pictures of CEOs of *Fortune 500* firms in 2006. Note that the *Fortune 500* actually publishes 1,000 firms in their list each year, which generates an initial sample of of 1,000 CEOs. Among those CEOs, 983 are male and 967 are White[38]. Collecting pictures based on the 2006 *Fortune* list not only allows us to exploit the substantial variation in CEOs' exposure to industry shocks induced by the 2007-2008 recession, but also prevents survivorship bias that we might introduce if we included CEOs from earlier years who left the CEO position a considerable amount of time before the recession. In the collection process, we aim for five pictures from the beginning of a CEO's tenure and two additional pictures every four years after that, all the way to present-day. We also avoid the LinkedIn type of pictures,

[38]Among the 33 non-White CEOs, 8 are Hispanic or Latino, 7 are African American, and 18 are Asian (including Indian).

but instead collect pictures that are taken in daily life, social events or conferences, etc. We collect pictures from gettyimages.com as well as Google Images. For 463 CEOs, we are able to find at least two pictures from different points in time during or after their tenure[39]. In total, we collect 3,086 pictures for these 463 CEOs; this set of pictures constitutes the main sample for the analyses below.

We proceed in two steps. First, we document the estimated *apparent* age distribution based on the main picture sample and provide summary statistics. Second, we run a difference-in-differences regression to estimate the effect that stress – measured by industry shocks during the 2007-2008 recession – has on CEOs' apparent age.

Figure 4: CEO Apparent and Biological Age Distribution



Notes. This figure shows CEOs' apparent and biological age distributions. The first plot shows the distribution of CEOs' apparent age estimated using the machine-learning based age estimation software from Antipov, Baccouche, Berrani, and Dugelay (2016) for a sample of 3,086 pictures collected from *Fortune 500* CEOs who were CEO in 2006. The second plot shows the associated biological age distribution.

[39]Among those 463 CEOs in our final sample, 452 are male and 447 are White.

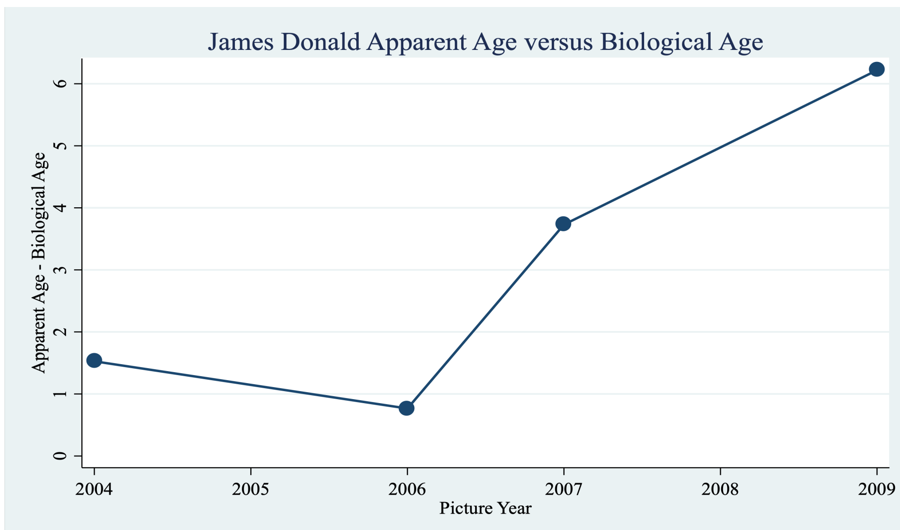
Figure 4 shows the age distribution of CEOs based on 3,086 collected pictures. The upper part shows the apparent age distribution, the lower part the associated biological age distribution. Both distributions are, reassuringly, highly correlated, though it appears that many CEOs look younger relative to their true age (i.e., the apparent age distribution is shifted to the left). The fact that, on average, the software estimates CEOs to look younger compared to their biological age might be unsurprising given that the age estimation software is targeted for the average population. CEOs, instead, are high SES people, have better access to health care, can afford healthier food, and live longer than the average population (see Table 1 and Chetty et al. 2016b). We emphasize that all of our results below on the effect on industry shocks on CEO aging entail a *within-CEO* comparison and do not rely on comparisons between CEOs and the general population.

To illustrate the mechanism between industry shocks and aging we have in mind, we first zoom in on one specific CEO: James Donald, the CEO of Starbucks from April 2005 until January 2008, when he was fired.[40] Figure 5 shows two pictures of Donald; the one on the left was taken on December 8, 2004, i.e. before his CEO appointment at Starbucks; the one on the right on Monday, May 11, 2009, i.e. after he was dismissed. Hence, the two pictures were taken 4.42 years apart. Donald was 50.76 years old in the first picture, and 55.18 years old in the second picture. The machine-learning based aging software predicts his age in the earlier picture at 53.47 years, and his age in the later picture as 60.45 years. Thus, for both pictures, the software thinks that he looks older than his true age; and, importantly, the software estimates that he aged by 6.98 years, more than 2.5 years more compared to the actual time passed between the two pictures.

We extend this analysis and search for additional pictures of James Donald from between three years before and after the onset of the crisis in 2007, i.e., between 2004 and 2010. We are able to find a total of 20 pictures from these years. Consistent with the initial two pictures, we find that the mean difference between his apparent and biological age is 0.96 years prior to 2007, and

[40] He was dismissed in January 2008 after Starbucks' stock had plunged by more than 40% during his last year of tenure.

Figure 5: Sample Pictures (James Donald, CEO of Starbucks from 2005 to 2008)



Notes. The first two pictures show James Donald, CEO of Starbucks from 2005 to 2008. Based on data from Ancestry.com, Donald was born on March 5, 1954. The picture on the left was taken on December 8, 2004, that on the right on Monday, May 11, 2009. Biological ages: 50.76 and 55.18 years, respectively. Apparent ages based on aging software: 53.47 and 60.45 years, respectively. The figure at the bottom shows how James Donald's apparent age compares to his true age over time based on 20 pictures collected between 2004 and 2009.

increases to 4.97 years in or after 2007. The bottom half of Figure 5 summarizes these estimates in an event-study-type graph; the graph visualizes the jump in Donald's apparent versus biological age in 2007, and the continued aging effects after the crisis shock. This example nicely typifies our approach, especially in light of Donald's and Starbucks' struggles during his last year of tenure (see footnote 40).

We formalize our analysis of job-induced aging by using the 3,086 collected pictures in a difference-in-differences design. Table 9 shows summary statistics for the 463 CEOs for which we find at least two pictures. The average CEO is 56.35 years old in 2006, and the mean pre-2006 tenure is 8 years. On average, we are able to find about 7 pictures of a CEO (conditional on finding at least two pictures). The majority of CEOs head firms in the manufacturing, transportation, communications, electricity and gas, and finance industries.

Table 9: Summary Statistics

	PANEL A: CEO CHARACTERISTICS					
	N	mean	sd	p10	p50	p90
Biological Age in 2006	463	55.54	6.55	47	56	63
Pre-2006 Tenure	463	8.00	7.73	2	6	17
Pre-2006 Ind. Shocks	463	0.54	1.13	0	0	2
2007-2008 Ind. Shocks	463	0.81	0.79	0	1	2
2007-2008 Ind. Shocks (ITT)	463	0.96	0.81	0	1	2
No. of Pictures per CEO	463	7.35	4.51	3	6	13

	PANEL B: INDUSTRY DISTRIBUTION		
Industry (Number of CEOs)	Manufacturing (180)	Construction (12)	Mining (21)
	Wholesale (16)	Retail (53)	Services (44)
	Trans.; Commns.; Elec., Gas, and Sanitary Service (71)		
	Finance, Insurance, Real Estate (65)		Others (1)

Notes. The table presents summary statistics for the CEOs for whom we find at least two pictures from different points in time during their tenure. "Pre-2006 Ind. Shocks" counts the number of industry shocks experienced from taking office up until year 2006. "2007-2008 Ind. Shocks" and "2007-2008 Ind. Shocks (ITT)" count the number of industry shocks experienced during 2007 to 2008 according to their definitions described in the text.

To test whether experiencing industry shocks during the recent financial crisis has detectable effects on how old CEOs look, we follow our approach in Section 5 and use 3-digit SIC codes as well as the 30% decline in equity value criterion to classify firms into industries and assign industry shocks. Our approach classifies 79 out of a total of 149 industries as experiencing an industry shock during the years 2007 to 2008.

We implement the difference-in-difference analysis using the following regression model:

$$ApparentAge_{i,t} = \beta_0 + \beta_1 BiologicalAge_{i,t} + \beta_2 IndShock \times Post + \beta_3 \times X_{i,t} + \delta_t + \theta_i + \varepsilon_{i,t}$$

where i represents a CEO and t represents a year. The *IndShock* variable is an indicator variable for whether or not the CEO experienced industry shocks during 2007 to 2008 (either in 2007 or 2008, or in both years). We construct the *IndShock* variable in two ways. First, we check whether the CEO's firm operates in an industry that experienced shocks during 2007 to 2008 regardless of whether or not the CEO stepped down between 2006 and 2008. We call this version of constructing the industry shock experience the "intention-to-treat" (ITT) version. This version addresses concerns that the industry shock experience might be correlated with CEOs' decision to step down from their position, potentially introducing bias. Second, we check whether the CEO actually experienced industry shocks during 2007 to 2008, not counting industry shock years after a CEO already stepped down.[41] We call the second version the "instrumental variables" (IV) version, as we will use the ITT assignment as an instrument for actually experienced industry shocks to account for potential selection bias. The vector of control variables, $X_{i,t}$, includes the number of industry shocks a CEO experienced before 2006 as well as CEO tenure up to year 2006. We include CEO fixed effects (θ_i) and year fixed effects (δ_t). Note that these fixed effects absorb the main effects of *IndShock* and *Post* in the regression. The key coefficient of interest is β_2, indicating the difference in how old CEOs *look* in post-crisis years depending on whether or not they personally experienced industry shocks during 2007-2008.

To take care of the concerns on picture heterogeneity, we manually rate the pictures over the following dimensions: "logo", "side face", "professional", "magazine", "natural", "natural lighting", and "glasses". All of those are dummy variables. For "logo", the variable takes value 1 if there is some logo (for instance, part of the "gettyimage" logo) on the face in the picture. For "side

[41] For example, if a CEO stepped down in year 2007 and the industry she was in experienced a shock only in 2008, the ITT version will generate her industry shock experience as 1 and the second version will generate it as 0.

face", the variables takes on 1 if the CEO in the picture shows a side face instead of front. For "professional", the variable takes on 1 if the CEO is in the work mode, say wearing business clothes (for men: suit, tie, etc.; for women: dress pants, skirt with matching jacket, wtc.) and otherwise in casual mode, say wearing short-sleeved shirt, T-shirt etc. For "magazine", the variable takes on 1 if the picture is from a magazine cover. For "natural", the variable reflects whether the CEO expects picture or not, i.e., whether it is natural posing or photo call. For "natural lighting", the variable reflects whether it is natural lighting or black and white. The variable "glasses' takes on 1 if the CEO in the picture wears glasses. We put those ratings as control variables in our regressions to make sure that the picture heterogeneity does not affect our results.

Table 10 presents results of the difference-in-differences analysis. In column (1), we use the "intention-to-treat" version of the industry experience, interacted with whether the picture is is taken in 2007 or later, i.e. after the onset of the crisis. The coefficient on the interaction term is 0.978, significant at 5%, indicating that CEOs look around 1 year older during and post-crisis if they experienced industry shocks between 2007 and 2008 relative to if they did not. Column (2) shows the second-stage results when we use the ITT version of the industry shock experience interacted with post-2006 as an instrument for actual post-2006 industry shock experience. The coefficient on the interaction term is 1.067 and remains significant at 5%. The size of the coefficient is very close to that in column (1); this reflects the fact that assigned and actual industry shock experience are highly correlated; the first-stage coefficient is 0.917 $(= \frac{0.978}{1.067})$, significant at 1%. In column (3), we split the post-period into two parts, one indicating whether the picture is from between 2007 to 2011 and the other indicating whether it is from after 2011. We find that the acceleration in aging as a result of experiencing industry shocks during the crisis becomes stronger over time. Our estimates imply an aging effect of about 0.84 years for the first five-year period, increasing to about 1.18 years in the 2012 and after period.

We further verify that our results are not affected by differential finding rates of pictures depending on whether CEOs experienced distress during the crisis. For example, it could be that

Table 10: Industry Distress and CEO Aging

DEPENDENT VARIABLE: *Apparent Age*			
	(1)	(2)	(3)
$Ind.\ Shock_{ITT} * \mathbb{1}_{\{YearPicture>2006\}}$	0.978**		
	[0.491]		
$Ind.\ Shock_{IV} * \mathbb{1}_{\{YearPicture>2006\}}$		1.067**	
		[0.528]	
$Ind.\ Shock_{ITT} * \mathbb{1}_{\{2006<YearPicture<2012\}}$			0.841
			[0.531]
$Ind.\ Shock_{ITT} * \mathbb{1}_{\{YearPicture\geq 2012\}}$			1.178**
			[0.564]
Biological Age	0.944***	0.942***	0.940***
	[0.095]	[0.093]	[0.095]
*Pre*2006 *Ind. Shock*	Y	Y	Y
*Pre*2006 *Tenure*	Y	Y	Y
Picture Controls	Y	Y	Y
CEO FE	Y	Y	Y
Year FE	Y	Y	Y
Number of CEOs	463	463	463
Observations	3,086	3,086	3,086

Notes. This table shows the effect of industry distress exposure on CEO apparent age. Column (1) shows the results when we use the "intention-to-treat" (ITT) version of the industry shock experience interacted with the post-2006 indicator as the main independent variable. Column (2) shows the second-stage results when we use the ITT version of the industry shock experience interacted with the post-2006 indicator as an instrument for the actual industry shock experience interacted with the post-2006 indicator. Column (3) is similar to Column (1) but splits the post period into two sub-periods (2007-2011 and post 2011). "*Pre*2006 *Ind. Shock*" and "*Pre*2006*Tenure*" are categorical variables that we control for by including fixed effects. Picture Controls include all the ratings we describe in the text, e.g., "logo", "professional", etc. We weight observations by the inverse of the number of pictures collected for each CEO. Fixed effects are included as indicated at the bottom of the table. Standard errors are clustered at the industry level and shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

experiencing industry shocks makes CEOs more likely to step down earlier, which might make it more difficult to find more recent pictures. This could introduce selection bias. Appendix-Figure B2 depicts the average number of pictures per CEO we find in each year for the group of CEOs who experienced industry shocks between 2007 and 2008 and for the group that did not. In general, the finding rates closely follow each other over time, though we note a small divergence in finding rates between the two groups post 2015. Therefore, we repeat our analysis restricting our sample to pre-2015 years, as shown in Appendix-Table B10. The size and significance of the coefficients on the interaction terms remain similar across all columns.

All together, we find evidence that increased job demands in the form of industry distress leave detectable signs of aging in CEOs' faces.

7 Conclusion

In this paper, we assess the health consequences associated with serving as CEO in harsher business environments. Exploiting the staggered introduction of Business Combination laws in different states over time, we document that CEOs who serve under stricter corporate governance regimes face poorer long-term health outcomes, reflected in an earlier age of death. We estimate a four to five percent difference in mortality rates as result of exposure to less stringent corporate governance. The effect is driven by the initial years of reduced monitoring; incremental health benefits taper off at higher levels of exposure to more lenient governance.

To complement these findings, we explore the health consequences of industry-wide distress shocks as a second shifter in a CEOs' job demands. In line with our initial results, we observe significant adverse health effects in terms of reduced life expectancy for CEOs who experienced periods of industry-wide distress during their tenure.

We then analyze whether industry distress is also reflected in more immediate, non-mortal adverse health consequences, namely faster aging as gauged by machine-learning based age estimation software. Based on a difference-in-differences design that exploits variation in industry shock exposure during the financial crisis, we estimate that CEOs who experienced industry shocks during 2007 to 2008 look roughly one year older relative to had they experienced no industry shocks.

The effect becomes slightly larger over time – up to 1.18 years, if we analyze pictures from 2012 and afterwards.

Our results contribute to the research on managerial performance and incentives and, in particular, on the trade-offs between managerial incentives and private benefits arising from the separation of ownership and control. We document and quantify a previously unnoticed yet important cost, in terms of shorter longevity of the CEOs, associated with serving under strict corporate governance – a governance structure that is generally viewed as desirable. Our results suggest that one ought to take these hidden, potentially large costs into consideration when designing and evaluating contracts. The findings regarding the detrimental health effects of industry shocks underscore that independent of incentives and monitoring, periods of heightened job demands can leave a visible and lasting imprint on CEOs in terms of adverse health outcomes.

We emphasize that other job situations apart from more stringent monitoring and industry distress might trigger adverse health outcomes as well, such as corporate restructurings and layoffs.[42] Likewise, heightened workplace stress can also adversely likely affect other aspects of life, including CEOs' marriage and divorce rates, as well as parenting.

[42]For example, in a 2016 documentary titled "Lonely at the Top: Top-Level Managers at Their Limit" about the job experiences of German executives, Brigitte Ederer, former member of the executive team at Siemens AG, recalled: "Laying people off is something that took its toll on me ... something I don't ever want to do again." The documentary is available (in German) at youtube.com/watch?v=FcRH3r0nEDE.

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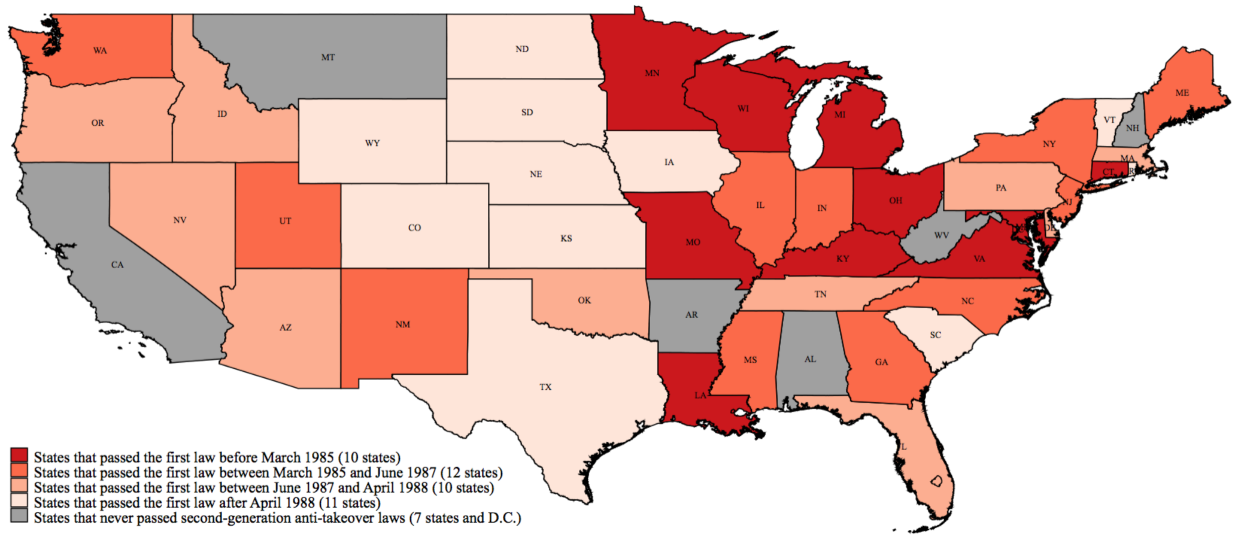
Appendix A Variable Definitions

Table A1: Variable Definitions

Variable Name	Definition
Birth Year	The birth year of a CEO.
DeadByOct2017	Indicator for whether a CEO has passed away by October 1st, 2017.
Death Year	The year of death of a CEO, calculated up to the monthly level (e.g., 2010.5 for a person who dies on 6/30/2010).
AgeTak.Office	CEO's age when appointed as CEO.
YearTak.Office	The year in which a CEO is appointed.
$Age_{i,t}$	CEO's age in year t.
$Tenure_{i,t}$	CEO's cumulative tenure (in years).
$BC_{i,t}$	CEO's cumulative exposure to a BC law during his tenure over time (in years).
$BC_{i,t}^{(\text{min-}p50)}$	CEO's below-median (4.4 years), cumulative exposure to a BC law during his tenure over time (in years).
$BC_{i,t}^{(p51\text{-max})}$	CEO's above-median (4.4 years), cumulative exposure to a BC law during his tenure over time (in years).
$I(BC_{i,t})$	1 if CEO i is insulated from a BC law in year t.
$FL_{i,t} = FLExposure_{i,t}$	CEO's cumulative exposure to the first-time enactment of a 2nd generation anti-takeover law (FL) during his tenure over time (in years).
$FL_{i,t}^{(\text{min-}p50)}$	CEO's below-median (4.4 years), cumulative FL law exposure during his tenure over time (in years).
$FL_{i,t}^{(p51\text{-max})}$	CEO's above-median (4.4 years), cumulative FL law exposure during his tenure over time (in years).
$I(FL_{i,t})$	1 if CEO i is insulated from the first enactment of a 2nd generation anti-takeover law (FL) in year t.
IndDistress	1 if median two-year stock return (forward-looking) of firms in the 3-digit SIC-code industry is less than -30%.
$Year_{i,t}$	Year of a given subspell, used in hazard specifications when linearly controlling for time.
$Pay_{i,t}$	CEO i's total pay in year t (from Gibbons and Murphy (1992)).
$Assets_{j,t}$	Firm j's total assets in year t (from Compustat); data for years with missing information is interpolated.
$Employees_{j,t}$	Firm j's total number of employees in year t (from Compustat); data for years with missing information is interpolated.

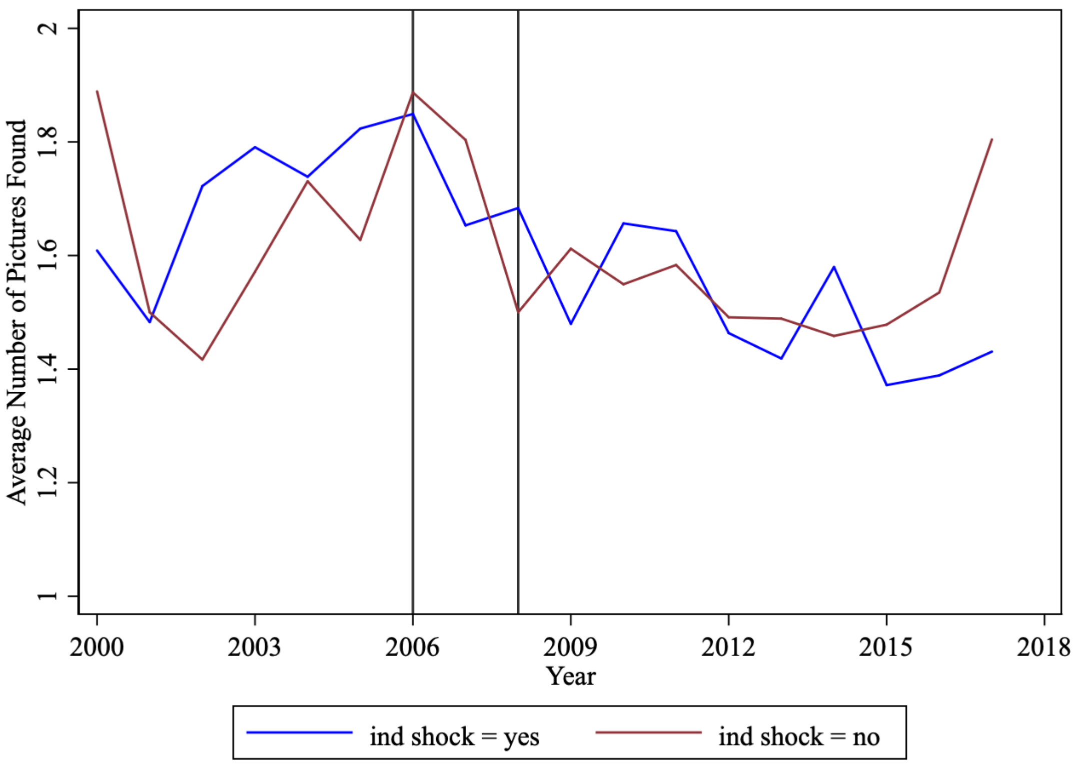
Appendix B Appendix Figures and Tables

Figure B1: First-Time Introduction of Second-Generation Anti-Takeover Laws Over Time



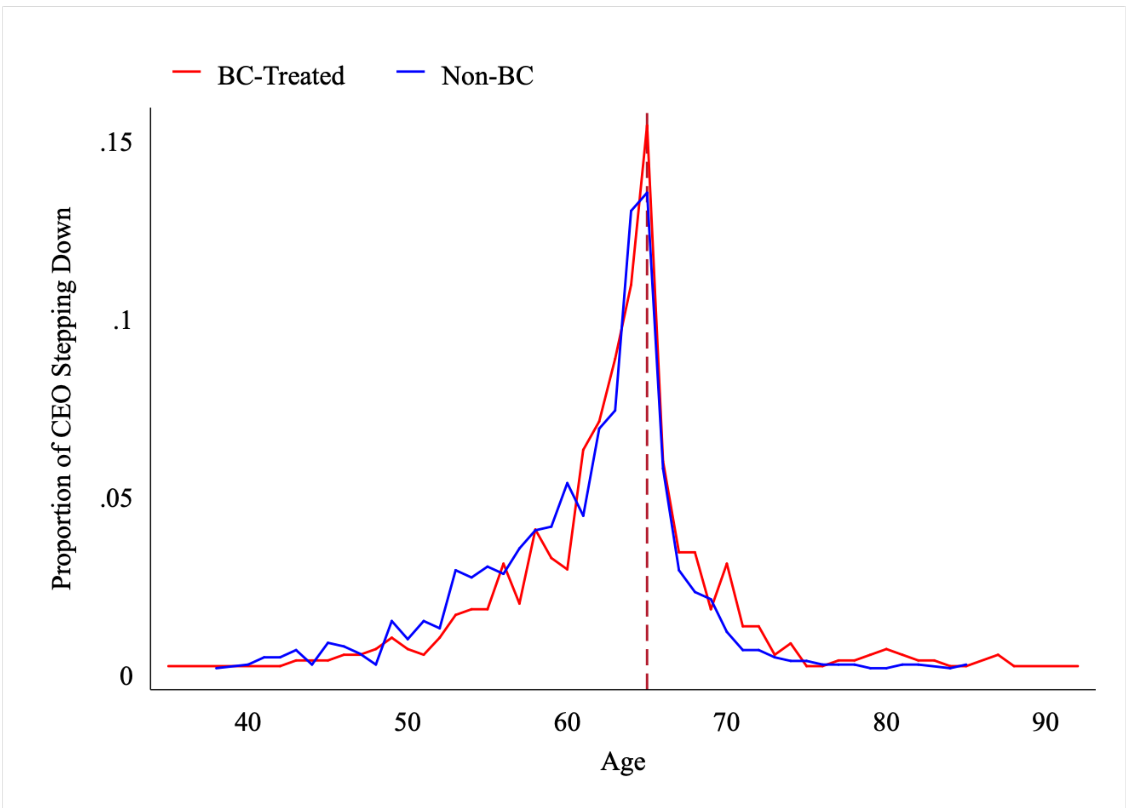
Notes. This figure visualizes the distribution of first-time enactments of any of the five most common second-generation anti-takeover laws over time, i.e., Business Combination (BC), Fair Price (FP), Control Share Acquisition (CSA), Poison Pills (PP), and Directors' Duties (DD) laws. The graph omits the state of Hawaii, which adopoted a CSA law on 4/23/1985 and DD and PP laws on 6/7/1988.

Figure B2: Average Number of Pictures per CEO Across Years



Notes. This figure depicts the average number of pictures per CEO we find each year for the group of CEOs who experienced industry shocks during 2007-2008 and for the group that did not. The two red vertical lines represent the years 2006 and 2008.

Figure B3: Proportion of CEOs Stepping Down over Age



Notes. This figure depicts the proportion of CEOs stepping down at each age, for both Business-Combination-Laws-treated group and non-treated group. The vertical dash line indicates age 65.

Table B1: Business Combination Laws and Mortality, Additional Controls

DEPENDENT VARIABLE: $Death_{i,t}$			
	(1)	(2)	(3)
BC	0.956***	0.961***	0.962***
	[0.007]	[0.007]	[0.007]
$ln(Pay)$	0.976	0.986	0.984
	[0.043]	[0.048]	[0.048]
$ln(Assets)$	1.024	0.988	0.982
	[0.024]	[0.036]	[0.034]
$ln(Employees)$	0.988	1.007	1.011
	[0.021]	[0.036]	[0.037]
Age	1.111***	1.121***	1.121***
	[0.008]	[0.006]	[0.006]
Year	1.003	1.001	
	[0.005]	[0.005]	
Location FE (HQ)	Y	Y	Y
FF49 FE		Y	Y
Year FE			Y
Number of CEOs	1,553	1,553	1,553
Observations	49,052	49,052	49,052

Notes. This table re-estimates the right three columns in Table 3 except that we include additional controls for CEO characteristics (pay) and firm characteristics (assets, employees). See Table 3 for further details. All variables are defined in Appendix A. Standard errors, clustered at the state of incorporation level, are shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

Table B2: Business Combination Laws and Mortality, Birth Year FE

DEPENDENT VARIABLE: $Death_{i,t}$				
	(1)	(2)	(3)	(4)
$I(BC)$	0.772**			
	[0.089]			
BC		0.958***		
		[0.007]		
$\widehat{BC}$			0.966**	
			[0.017]	
$BC^{(\text{min-}p50)}$				0.918**
				[0.033]
$BC^{(p51\text{-max})}$				0.989
				[0.018]
Age	1.130***	1.128***	1.123***	1.129***
	[0.006]	[0.005]	[0.010]	[0.006]
Location FE (HQ)	Y	Y	Y	Y
FF49 FE	Y	Y	Y	Y
Birth Year FE	Y	Y	Y	Y
Number of CEOs	1,605	1,605	1,605	1,605
Observations	50,530	50,530	50,530	50,530

Notes. This table re-estimates the model in Table 3, Table 4, and Table 5 but including birth year fixed-effects. All variables are defined in Appendix A. Standard errors, clustered at the state of incorporation level, are shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

Table B3: First Second-generation Anti-takeover Laws and Mortality

DEPENDENT VARIABLE: $Death_{i,t}$						
	(1)	(2)	(3)	(4)	(5)	(6)
$I(FL)$	0.801***	0.801***	0.806***			
	[0.053]	[0.061]	[0.061]			
FL				0.955***	0.957***	0.957***
				[0.006]	[0.006]	[0.006]
Age	1.113***	1.123***	1.124***	1.109***	1.119***	1.119***
	[0.006]	[0.005]	[0.005]	[0.007]	[0.005]	[0.005]
Year	1.003	1.000		1.005	1.002	
	[0.004]	[0.004]		[0.004]	[0.004]	
Location FE (HQ)	Y	Y	Y	Y	Y	Y
FF49 FE		Y	Y		Y	Y
Year FE			Y			Y
Number of CEOs	1,510	1,510	1,510	1,510	1,510	1,510
Observations	47,994	47,994	47,994	47,994	47,994	47,994

Notes. This table re-estimates Table 3 except that we use first-time introduction of any of the five most common second-generation anti-takeover laws as a measure of lenient governance. Note that the number of CEOs is lower compared to the regressions using BC law exposure. This results from the fact that we restrict to CEOs appointed in years prior to the introduction of the anti-takeover law(s) used in the analysis to alleviate selection concerns. Mechanically, a smaller set of CEOs passes this requirement when using the *First Law* introduction instead of the passage of a BC law. See Table 3 for further details. All variables are defined in Appendix A. Standard errors, clustered at the state of incorporation level, are shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

Table B4: First Second-generation Anti-takeover Laws and Mortality, Additional Controls

DEPENDENT VARIABLE: $Death_{i,t}$			
	(1)	(2)	(3)
FL	0.962***	0.967***	0.968***
	[0.010]	[0.008]	[0.009]
$ln(Pay)$	0.977	0.991	0.988
	[0.043]	[0.046]	[0.047]
$ln(Assets)$	1.027	0.985	0.978
	[0.024]	[0.035]	[0.033]
$ln(Employees)$	0.987	1.013	1.017
	[0.021]	[0.037]	[0.038]
Age	1.111***	1.121***	1.121***
	[0.008]	[0.006]	[0.006]
$Year$	1.004	1.001	
	[0.005]	[0.005]	
Location FE (HQ)	Y	Y	Y
FF49 FE		Y	Y
Year FE			Y
Number of CEOs	1,464	1,464	1,464
Observations	46,660	46,660	46,660

Notes. This table re-estimates the right three columns in Appendix-Table B3 except that we include additional controls for CEO characteristics (pay) and firm characteristics (assets, employees). See Appendix-Table B3 for further details. All variables are defined in Appendix A. Standard errors, clustered at the state of incorporation level, are shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

Table B5: Excluding Lobbying Firms, Opt-Out Firms, and Firms with Firm-Level Defenses

DEPENDENT VARIABLE: $Death_{i,t}$			
	(1)	(2)	(3)
		Panel A: Excluding Lobbying Firms	
BC	0.955***	0.958***	0.959***
	[0.005]	[0.005]	[0.005]
Number of CEOs	1,530	1,530	1,530
Observations	48,105	48,105	48,105
		Panel B: Excluding Opt-out Firms	
BC	0.956***	0.959***	0.959***
	[0.006]	[0.006]	[0.006]
Number of CEOs	1,575	1,575	1,575
Observations	49,556	49,556	49,556
		Panel C: Excluding Firm-level Defenses	
BC	0.953***	0.957***	0.957***
	[0.006]	[0.006]	[0.006]
Number of CEOs	1,595	1,595	1,595
Observations	42,624	42,624	42,624
Year (Linear Control)	Y	Y	
Age & Tenure Controls	Y	Y	Y
Location FE (HQ)	Y	Y	Y
FF49 FE		Y	Y
Year FE			Y

Notes. This table re-estimates the right three columns in Table 3 except that we exclude lobbying firms, firms that opted out of the laws, or firm-years in which firms used firm-level defenses. In Panel A, we exclude 46 firms that Karpoff and Wittry (2018) identify as firms that lobbied for the enactment of the second-generation anti-takeover laws. In Panel B, we exclude 30 firms that opted out of the second-generation anti-takeover laws, based on data from the Institutional Shareholder Services (ISS) Governance database. In Panel C, we exclude firm-years in which firms used firm-level defenses as identified from the the ISS data and data from Cremers and Ferrell (2014). Controls and fixed effects are included as indicated at the bottom of the table. See Table 3 for further details. All variables are defined in Appendix A. Standard errors, clustered at the state of incorporation level, are shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

Table B6: Restricting to Years After the First-Generation Laws (*Edgar v. MITE* Case)

DEPENDENT VARIABLE: $Death_{i,t}$						
	(1)	(2)	(3)	(4)	(5)	(6)
	Subsample A: Post-1982 Years		Subsample B: CEOs Stepping down after 1982		Subsample C: CEOs Starting after 1982	
BC	0.958***	0.957***	0.959***	0.960***	0.964	0.965
	[0.005]	[0.005]	[0.006]	[0.006]	[0.026]	[0.027]
Age	1.121***	1.122***	1.122***	1.124***	1.124***	1.125***
	[0.006]	[0.006]	[0.006]	[0.006]	[0.020]	[0.020]
Year	1.000		1.011*		0.956	
	[0.005]		[0.006]		[0.038]	
Location FE (HQ)	Y	Y	Y	Y	Y	Y
FF49 FE	Y	Y	Y	Y	Y	Y
Year FE		Y		Y		Y
Number of CEOs	1,573	1,573	1,231	1,231	477	477
Observations	40,834	40,834	39,623	39,623	13,562	13,562

Notes. This table re-estimates the right two columns in Table 3 except that we restrict attention to the period from 1982 onwards when the first-generation anti-takeover laws lost their effect (in June 1982 after the *Edgar v. MITE* ruling). In Subsample A, we restrict our sample to the years 1982 and after. In Subsample B, we restrict to CEOs who stepped down in or after 1982. In Subsample C, we restrict to CEOs who were appointed in or after 1982. Standard errors, clustered at the state of incorporation level, are shown in brackets. See Table 3 for further details. All variables are defined in Appendix A. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

Table B7: Excluding DE or NY Incorporated, Banking, or Utility Firms

DEPENDENT VARIABLE: $Death_{i,t}$			
	(1)	(2)	(3)
		Panel A: Excluding DE/NY Firms	
BC	0.958***	0.958**	0.962**
	[0.016]	[0.019]	[0.019]
Number of CEOs	738	738	738
Observations	22,103	22,103	22,103
		Panel B: Excluding Firms in Banking	
BC	0.942***	0.944***	0.945***
	[0.007]	[0.007]	[0.007]
Number of CEOs	1,328	1,328	1,328
Observations	42,327	42,327	42,327
		Panel C: Excluding Utility Firms	
BC	0.957***	0.961***	0.962***
	[0.005]	[0.004]	[0.005]
Number of CEOs	1,422	1,422	1,422
Observations	45,017	45,017	45,017
Year (Linear Control)	Y	Y	
Age (Linear Control)	Y	Y	Y
Location FE (HQ)	Y	Y	Y
FF49 FE		Y	Y
Year FE			Y

Notes. This table re-estimates the right three columns in Table 3 except that we exclude firms in certain states of incorporation of industries. In Panel A, we exclude firms that are incorporated in Delaware or New York (the two most common states of incorporation in our sample, see Table 1). In Panel B, we exclude firms that are classified as "Banking" firms based on the Fama-French 49 industry classification. In Panel C, we exclude firms that are classified as "Utilities" based on the Fama-French 49 industry classification. Controls and fixed effects are included as indicated at the bottom of the table. See Table 3 for more details. All variables are defined in Appendix A. Standard errors, clustered at the state of incorporation level, are shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

Table B8: Industry Distress and Mortality, Additional Controls

DEPENDENT VARIABLE: $Death_{i,t}$			
	(1)	(2)	(3)
Industry Distress	1.043**	1.048**	1.046*
	[0.018]	[0.024]	[0.025]
$ln(Pay)$	0.976	0.991	0.988
	[0.044]	[0.047]	[0.048]
$ln(Assets)$	1.025	0.984	0.978
	[0.024]	[0.035]	[0.033]
$ln(Employees)$	0.985	1.012	1.015
	[0.020]	[0.037]	[0.038]
Age	1.112***	1.122***	1.122***
	[0.008]	[0.006]	[0.006]
Year	1.006	1.004	
	[0.006]	[0.006]	
Linear BC Control	Y	Y	Y
Location FE (HQ)	Y	Y	Y
FF49 FE		Y	Y
Year FE			Y
Number of CEOs	1,553	1,553	1,553
Observations	49,052	49,052	49,052

Notes. This table re-estimates Table 8 except that we include additional controls for CEO characteristics (pay) and firm characteristics (assets, employees). See Table 8 for further details. All variables are defined in Appendix A. Standard errors, clustered at the state of incorporation level, are shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

Table B9: Industry Distress and Mortality, Additional CEOs

DEPENDENT VARIABLE: $Death_{i,t}$			
	(1)	(2)	(3)
Industry Distress	1.034*	1.047*	1.045*
	[0.019]	[0.026]	[0.026]
Age	1.114***	1.122***	1.122***
	[0.007]	[0.005]	[0.005]
Year	1.009*	1.007	
	[0.005]	[0.005]	
Linear BC Control	Y	Y	Y
Location FE (HQ)	Y	Y	Y
FF49 FE		Y	Y
Year FE			Y
Number of CEOs	1,900	1,900	1,900
Observations	58,034	58,034	58,034

Notes. This table re-estimates Table 8 except that we use an extended sample including CEOs who were appointed after the passage of BC laws. See Table 8 for further details. All variables are defined in Appendix A. Standard errors, clustered at the state of incorporation level, are shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

Table B10: Industry Shock and CEO Aging: Restricted Sample Pre-2015

DEPENDENT VARIABLE: *Apparent Age*			
	(1)	(2)	(3)
$Ind.\ Shock_{ITT} * \mathbb{1}_{\{YearPicture>2006\}}$	0.907*		
	[0.495]		
$Ind.\ Shock_{IV} * \mathbb{1}_{\{YearPicture>2006\}}$		0.984*	
		[0.529]	
$Ind.\ Shock_{ITT} * \mathbb{1}_{\{2006<YearPicture<2012\}}$			0.694
			[0.535]
$Ind.\ Shock_{ITT} * \mathbb{1}_{\{YearPicture\geq 2012\}}$			1.418**
			[0.579]
Biological Age	0.976***	0.977***	0.967***
	[0.094]	[0.091]	[0.093]
*Pre*2006 *Ind. Shock*	Y	Y	Y
*Pre*2006 *Tenure*	Y	Y	Y
Picture Controls	Y	Y	Y
CEO FE	Y	Y	Y
Year FE	Y	Y	Y
Number of CEOs	434	434	434
Observations	2,656	2,656	2,656

Notes. The table shows the results of the picture analysis in Section 5.1 when we restrict the sample to pre-2015 pictures. Column (1) shows the results when we use the "intention-to-treat" (ITT) version of the industry shock experience interacted with the post-2006 indicator as the main independent variable. Column (2) shows the second-stage results when we use the ITT version of the industry shock experience interacted with the post-2006 indicator as an instrument for actual post-2006 industry shock experience. Column (3) is similar to Column (1) but splits the post period into two sub-periods (2007-2011 and post 2011). "*Pre*2006 *Ind. Shock*" and "*Pre*2006*Tenure*" are categorical variables that we control for by including fixed effects. Picture Controls include all the ratings we describe in the text, e.g., "logo", "professional", etc. We weight observations by the inverse of the number of pictures collected for each CEO. Fixed effects are included as indicated at the bottom of the table. Standard errors are clustered at the industry level and shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

Appendix C Age Estimation Process Details and Convolutional Neural Networks

Apparent Age Estimation:

- Difference from biological age estimation: Apparent age refers to the age that a person looks like and biological age is simply how old the person is. Therefore, by definition, these two may not be the same as each other. Here in this paper, we estimate the apparent age because we aim to study whether stress affects how old a CEO looks.

- To carry out apparent age estimation, the Orange Lab software we use train their model in a two-step manner. In the first step, they train their Convolution Neural Networks (details illustrated in the figure below) by minimizing the mean absolute error (MAE) between predicted age and real biological age based on a large image database from IMDB and Wikipedia, for which the real biological age is known. In the second step, they fine-tune their Convolution Neural Networks for apparent age estimation based on 7,591 images (4,113 images for training, 1,500 for validation, 1,978 for testing), for which the human-based apparent age is known. Specifically, each of those 7,591 images is at least rated by 10 people for apparent age. Based on those human-based apparent age guesses, they fit a normal distribution for each image and calculate the mean μ and standard deviation σ of the fitted distribution. To fine-tune their neural networks, they minimize the following metric: $\varepsilon = 1 - e^{-\frac{(\hat{x}-\mu)}{2\sigma^2}}$, where $\hat{x}$ is the predicted apparent age.

Implementation of Estimation:
Oxford's Visual Geometry Group deep convolutional neural network architecture

1. Pre-processing step: detect faces in images and resize to 224x224. This step is not directly included in the Orange Lab software, and the Orange Lab software takes this image format as given. To process our downloaded pictures, which are of different sizes initially, we use the "face_recognition" package developed for Python. The full documentation for this package is available here: github.com/ageitgey/face_recognition/blob/master/README.md. The "face_recognition" package help us detect the face in the image we download, and then we resize the face the package detect into 224x224.

2. Pre-training step: This is the first step of the two-step procedure described above. Train Convolution Neural Networks based on 250,367 IMDB-Wiki pictures with known biological age. This step is done by the Orange Lab.

3. Training and validation step: fine-tune the neural networks for apparent age using the 7,591 "age-annotated" pictures with $\geq$ 10 human age estimations per picture. This step is done by the Orange Lab.

    - The Orange Lab uses 11-fold cross-validation. They draw 5,613 images for training and validation each time, and do this 11 times. Their fine-tune the neural networks 11 times based on those 11 training and validation sets. In the end, for each image we feed in, we will also receive 11 estimates based on those differently trained models.

4. Run the apparent age estimation on our CEO images using the fine-tuned neural networks. For the 3,113 pictures we have, the estimation process takes about 3 - 4 hours using the GPU from University of California at Berkeley. The returned output contains 12 variables: one apparent age estimate for each of the 11 models and their average. We use the average of the 11 models as our final estimate of apparent age.

Convolutional Neural Networks:

- **Input:** 224x224x3 array of pixel values (x3 is color, RGB)
- **Convolutional layer:** Flashlight (aka filter, neuron, kernel) sliding (aka convolving) around input image
  - Say, dimension of 5x5x3: each neuron in conv layer has 75 connections to input volume
  - Can use more than one filter: increases output volume
- **Pool layer:** Downsampling along spatial dimensions
- **Fully-connected layer:** Transformation of previous layer into N-dimensional output vector ($N = 100$)
- **Output:** $p_i, i \in \{0, 99\}$ are probabilities of apparent age = i years

Sources: Antipov, Baccouche, Berrani, and Dugelay (2016), Siddharth Das, Adit Deshpande, Andrej Karpathy