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

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Abstract

Optimal pay-for-performance has to account for private benefits distorting managerial incentives. We focus on one aspect of private benefits – CEO health and mortality. Our identification exploits the staggered introduction of anti-takeover laws since the mid-1980s as well as exposure to industry shocks. Using a hand-collected data set on the dates of birth and death for more than 1,600 CEOs employed by large, publicly listed U.S. firms, we estimate that CEOs insulated from market discipline via anti-takeover laws enjoy an extra year or more of life as a result. Non-linear specifications suggest mortality rates initially fall by as much as 9 percent with each year of exposure, though the incremental effects diminish rather quickly as exposure increases. Our conclusions are unchanged by models that account for differences in tenure associated with exposure to the laws. We also find that CEOs who exclusively served under strict governance are more likely to pass away while in office or within the first five years of stepping down as CEO. We estimate similar effects on longevity arising from exposure to industry-wide distress 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 apparent age increases by roughly 1 year among CEOs exposed to distress shocks during the Great Recession.

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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) on the separation of ownership and control, 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 aims to maximize the value of the firm? A key ingredient in this classical agency problem are so-called "private benefits" that the CEO (but not shareholders) may obtain.

In a typical incentive-design problem, private benefits b are often an unspecified proxy for perks a manager might be extracting from the firm. The discussion of private benefits in the previous literature often revolves around these perks – from the $6,000 gold-and-burgundy shower curtains of Tyco's former CEO Dennis Kozlowski to multi-million dollar self-dealings via low-interest loans, transfer pricing, or greenmail payments with mixed motives. Less attention has been paid to the potential personal cost, especially health costs, that arise from assuming a top leadership position and being exposed to monitoring and criticism during times of crisis. While the news media may occasionally cover unexpected deaths "in office" and discuss the stress associated with C-suite positions, there is little research done to assess and quantify these costs. CEOs work the long hours and have demanding schedules (Bandiera et al. 2017; Porter and Nohria 2018). What are the implications of heightened supervision and pressure for the CEO's health, ability to stay on the job, and ultimately willingness to select into the CEO position?

In this paper, we make concrete the trade-off between better incentives to maximize shareholder value and private benefits, focusing on CEOs' health and longevity. Specifically, we document substantial effects on mortality rates arising from variation in the stringency of corporate governance and the experience of industry-wide distress shocks. Our work builds on research in labor and health economics that has established a link between job stress and health outcomes for lower-income employees (e.g., Cheng et al. 2005; Hummels et al. 2016).

To quantify the adverse effects of stringent monitoring and industry distress on CEOs' health, we hand-collect birth and death data of more than 1,600 former CEOs of large U.S. firms. The sample of CEOs we use as a starting point for these searches comes from Kevin Murphy (cf. Gibbons and Murphy 1992) and contains CEOs whose firms were included in the *Forbes* Executive Compensation Surveys between 1970 to 1991. Combining multiple information sources including historical newspapers, company websites, and Ancestry.com, we are able to find the exact birth and death information, or confirm their survival to the present day, for 87% of CEOs of our initial sample.

We first exploit variation in the intensity of CEO monitoring due to the staggered passage of anti-takeover laws since the mid-1980s. These laws, passed by the majority of U.S. states (66%), shielded CEOs from market discipline by making hostile takeovers by corporate raiders more difficult. Previous research documents significant personal benefits for CEOs associated with anti-takeover laws; after their passage, CEOs became less tough in wage negotiations and shied away from both plant creation and closures (Bertrand and Mullainathan 2003). In our main specifications, we focus on the introduction of Business Combination (BC) laws, following the prior literature. BC laws are generally recognized as the most effective anti-takeover law introduced since 1982. Our results are robust to using the first-time enactment of any of the five types of anti-takeover laws introduced since the mid-1980s, acounting for other firm or state anti-takeover provisions and excluding lobbying and opt-out firms (Karpoff and Wittry 2018), and different data cuts based on firms' industry affiliation or state of incorporation. Furthermore, to alleviate selection concerns, we restrict all analyses to CEOs appointed before the enactment of the laws we study.

To describe the relationship between CEOs' exposure to lenient corporate government laws and their lifespan, we fit hazard regression models. Our specifications control for CEOs' age, time trends, as well as firms' industry affiliation and location.

We find that anti-takeover laws led to significant improvements in the life expectancy of incumbent CEOs. Non-parametric plots show a visible divergence in longevity comparing CEOs with and without exposure to anti-takeover laws. These figures also suggest that the effect of reduced monitoring on lifespan may be nonlinear, with a diminishing effect as years of exposure increase. Our hazard estimates imply that 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 confirm 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, at age 60, the one-year mortality rate of a male American born in 1925 is 1.733%. The one-year exposure effect of approximately 4.5 percent pushes this rate down to 1.655%, in between those for 60-year-olds born in 1927 and 1928.[1] Our estimates imply that a typical range of prolonged exposure to lenient governance leads to life expectancy gains of up to one year.[2] While these effects are sizable, they are substantially lower than the health costs associated with health threats such as smoking, which is estimated to reduce life expectancy by ten years and more (HHS 2014; Jha et al. 2013).

In two alternative specifications, we account for the possibility that CEOs' length of tenure (and therefore exposure *intensity* to BC laws) might be affected by the introduction of the laws. First, 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 –

[1] At age 60, the one-year mortality rate of a male American born in 1927 is 1.675%, and that of a male American born in 1929 is 1.640%. We use 1925 as the baseline birth year as this is the median birth year of CEOs in our sample. Mortality probabilities are taken from the *Life Tables for the United States Social Security Area 1900-2100* and are available at ssa.gov/oact/NOTES/as116/as116_Tbl_9.html#wp1098815.

[2] Life expectancies are also taken from the *Life Tables for the United States Social Security Area 1900-2100* and are available at ssa.gov/oact/NOTES/as116/as116_Tbl_11.html#wp1098786. We discuss the calculation of life expectancy gains in more detail in Section 4.2. We note and, in fact, document ourselves in Section 2.4 that CEOs live significantly longer than the average population, on which the *Life Tables* are based. Therefore, we emphasize the interpretation in terms of *differences* in life expectancies rather than levels.

to survival rates. Crucially, 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 themseleves. The results are essentially unchanged when we use this prediction to form the treatment variable. Second, we estimate our main specification with explicit controls for tenure. Under certain assumptions that we discuss in detail in Section 3, including tenure controls isolates the effects of corporate governance laws on working conditions from the longer tenures of CEOs who serve under BC laws. We estimate slightly smaller effects when controlling for tenure, though we cannot reject that differences arise due to noise. In sum, our conclusions are unchanged by models which remove or control for effects arising from differences in tenure associated with the BC laws.

We also zoom in on the first five years after a CEO resigns to shed light on the short-to-medium-term impact of variation in exposure to market discipline and governance on longevity. Consistent with the long-term effects, the fraction of CEOs who pass away within the first five years after stepping down is higher for CEOs who exclusively served under strict governance. 5.6 percent of CEOs who began their tenure in the 1980s and eventually became protected by a business combination law pass away within five years of stepping down, compared to 7.8 percent of CEOs from the same cohorts who exclusively experienced the strict governance regime.

Next, we 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, 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. We utilize machine-learning algorithms designed to estimate a person's "apparent" age, to detect visible signs of aging in the faces of CEOs. 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.[3] We collect a sample of 3,113 pictures of CEOs at firms included in the 2006 *Fortune 500* list from different points during their tenure, to estimate how fast they age and, in particular, how the experience of industry shocks affects the speed of aging. We base our sample collection on the 2006 *Fortune 500* list to ensure sufficient variation in the exposure to industry shocks across CEOs as induced by the financial crisis. Using a difference-in-differences regression 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. We account for endogenous selection out of the CEO job through an instrumental variables approach based on 2006 CEO status (intention-to-treat analysis). We also document that accelerated aging as a result of experiencing industry shocks during the crisis is not a temporary phenomenon. Instead, we see a slight *increase* in aging differences between distressed and non-distressed CEOs over time, reaching about 1.3 years for pictures taken five years and more after the onset of the crisis.

Our results imply significant personal costs in form of detrimental health effects arising from increased monitoring and industry distress. Heightened stress from experiencing stricter governance and economic downturns appears to constitute 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 what the implications for selection are.

Related Literature. 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 diaries of 1,114 CEOs of manufacturing firms, with detailed information on CEOs' activities. The average CEO's weekly schedule was comprised of 202 15-minute time blocks, adding up an average of 50 work hours. 30% of CEOs worked a six-day or seven-day week. The

[3] 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.

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,[4] and record CEOs working an average of 62.5 hours per week, 79% of weekend days, and 70% of vacation days. It is easy to complement the literature with anecdotal evidence. In 2015, Oscar Munoz, CEO of United Airlines, Inc., suffered from a heart attack at age 56, just a few weeks after assuming the role of CEO, reportedly having been "extremely stressed in the days and weeks leading up to [the attack]."[5] Turning to examples from CEOs included in our sample, Leo D. Goulet, former CEO of the Gerber Products Company, died from a heart attack at age 61, two months after taking over the CEO position.[6] Frank P. Nykiel, former CEO of Chromalloy, opted for an early retirement, citing poor health as the reason.[7] Our paper is the first comprehensive assessment of the health costs associated with the CEO position.

Second, we contribute to the literature at the intersection of finance and health economics that studies the impact of the stock market on people's physical and psychological health. Engelberg and Parsons (2016) link shocks to firm performance (stock market crashes) to investors' health conditions. They report a strong and nearly instantaneous relationship between stock market crashes and hospital admissions, especially strong for anxiety-related reasons and panic disorders. Our paper builds on these findings by showing that periods of economic and financial distress have adverse health consequences for managers as well, and in addition, establishes a novel link between work-related stress and a CEO's health outcomes stemming from "good," i.e. stringent, corporate governance.

Third, our paper links to the health and labor literature on worker job stress and insecurity and resulting health effects. Cheng et al. (2005) report a strong positive association between job insecurity and poor health (mental health, vitality, and general health). Browning et al. (2006),

[4]The average annual revenue of their firms is $13.1 billion. For comparison, the average firm in Bandiera, Hansen, Prat, and Sadun (2017) has annual sales of $222 million.

[5] The Huffington Post, 10/26/2015, "United CEO's Heart Attack Underscores Health Hazards Of Stress."

[6] The New York Times, 07/07/1987, "Leo D. Goulet, Leader Of Gerber Foods, Dies."

[7] The New York Times, 06/30/1982, "Sun Chemical Chief Heads Chromalloy." Nykiel passed away on 11/18/1994 at the age of 76. During his time as CEO at Chromalloy, he fought an attempted takeover by The Sun Chemical Corporation.

instead, do not find any links between job loss and immediate health effects in terms of hospitalization. Caroli and Godard (2016) find evidence that job insecurity affects specific types of health outcomes, such as headaches or eyestrain. Kuka (2018) reports that higher generosity of unemployment insurance leads to better self-reported health. Hummels et al. (2016) show that when workers' job demands increase due to plausibly exogenous reasons (foreign demand shocks), they suffer from more stress, injury, and illness. The authors estimate increases in depression rates of 2.5% and in hospitalizations due to heart attacks of 15%. Our paper extends this literature by documenting adverse health effects of high job demands for CEOs, a group for which job loss will not imply financial constraints, and by focusing on corporate governance which is generally viewed as desirable from a shareholder and welfare perspective. 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 certain job environments, and instead of a rigorous survival analysis merely provides a simple comparison of means of CEOs' age at death across industries.

Fourth, our paper is related to the literature that studies how socioeconomic status affects people's health and their life expectancy. Cutler et al. (2006) write that "[o]utside of economics, the currently dominant theory of health differentials is that the poor health of low status individuals is caused by 'psychosocial stress'—the wear and tear that comes from subordinate status and from having little control over one's own life." The economics literature has oftentimes also studied the health benefits associated with high socioeconomic status. Rablen and Oswald (2008) compare the longevity of Nobel Prize winners and those who were merely nominated; their estimates suggest that winners outlive "losers" by one to two years. Similarly, Fleisher et al. (2017) analyze academicians in China and find that being elected to the Chinese Academy of Sciences or Engineering prolongs their lifespan by 1.2 years on average. Borgschulte and Vogler (2017) find that the winners of close elections outlive losing candidates. By focusing on CEOs, our study extends this stream of literature and takes it to the corporate setting. In particular, we also quantify the flipside of high socioeconomic status in the case of the CEO's position, arising from heightened stress and demanding work schedules.

Finally, in terms of identification and application, we add to the literature in corporate governance on the impact of business combination laws, and anti-takeover laws more generally. This literature has generally found that anti-takeover laws have weakened corporate governance and as a result, increased managerial slack. Bertrand and Mullainathan (2003) find that after their introduction, wages rose, and both creation and destruction of new plants fell, suggesting that managers "enjoy a quiet life" when possible. 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, 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. Cain et al. (2017) study the relation between anti-takeover laws and hostile takeovers. They find that fair price laws predict less hostile activity, but also that "business combination laws [...] have no discernible impact on hostile activity." Karpoff and Wittry (2018) formulate similar concerns regarding the reliance on BC laws without taking into account the institutional and legal context of the laws. We address these points by showing robustness of our results to all robustness tests suggested in their paper. In addition, we find similar adverse effects of harsh work environments on a CEO's lifespan when departing from concentrating on anti-takeover laws and focusing on exposure to industry distress instead. 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. In a broader sense, we show that corporate governance mechanisms do not only impact firm-level outcomes, but also influence the personal domain of relevant actors, with CEOs and C-suite managers being a leading candidate. We conclude that higher incentives can enhance productivity and shareholders value, but might come with a dark side and 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 Data and Methodology

2.1 Anti-Takeover Laws

Anti-takeover statutes were passed by states to increase the hurdles for hostile takeovers. The second-generation[8] 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.

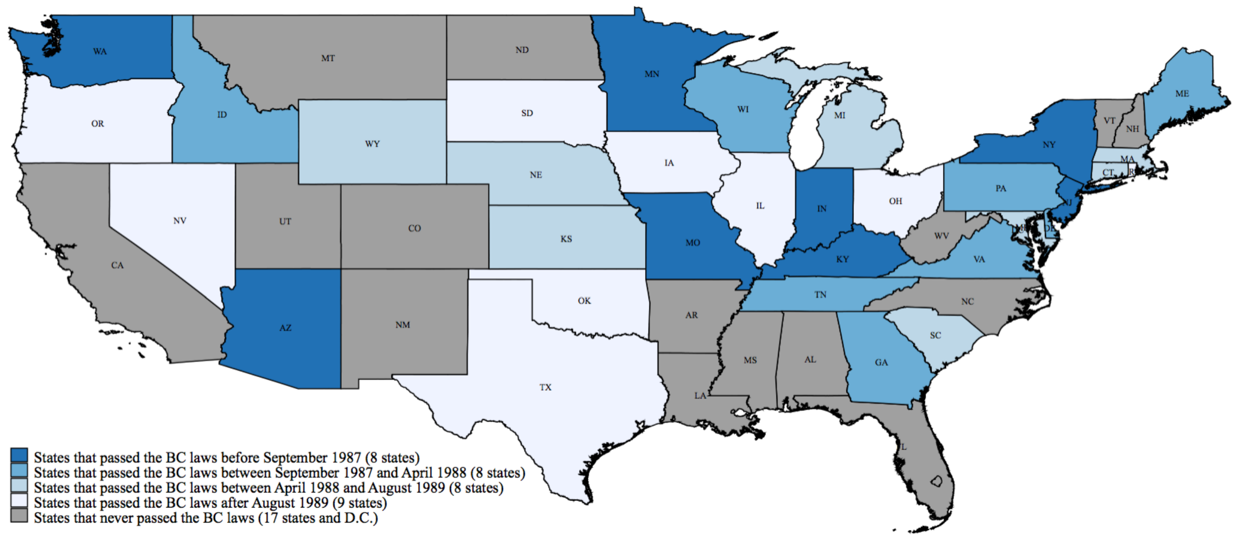
BC laws contributed to reducing the threat of hostile takeovers by imposing a moratorium that prohibits a large shareholder of a firm from conducting certain transactions with the firm, usually for a period of three to five years. Figure 1 visualizes the staggered introduction of BC laws across states. 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.

[8] 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 original sample of CEOs comes from Kevin Murphy's data on CEO compensation.[9] The data covers the years 1969 to 1991, and is an extended version based on Gibbons and Murphy (1992). They follow all CEOs listed in Executive Compensation Surveys published in *Forbes* during the sample years. These surveys are derived from corporate proxy statements and include the executives serving in the largest U.S. firms. The data set comprises more than 3,400 CEOs from over 1,600 firms. 102 CEOs serve as the CEO for multiple firms in the data set. Correcting for this and restricting to CEOs who are included in the data set in 1975 or later and whose firm can be assigned a PERMNO from CRSP reduces our sample to 2,720 unique CEOs from 1,501 firms.

For these CEOs, we manually search for (i) their exact dates of birth to verify the birth year

[9] The data was collected by Kevin Murphy and was provided to us by Emmanuel Saez. For a detailed overview on data construction rules and procedures, we refer to Gibbons and Murphy (1992).

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 have died since then. To obtain the information on birth and death, we use Google searches as well as Ancestry.com. Ancestry.com has comprehensive information on people's historical birth and death records, combining information from U.S. Census, Social Security Death Index, birth certificates, and other historical sources. Ancestry.com 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.com. 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.com, 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.[10] We are able to obtain the birth and death information for 2,361 out of the 2,720 CEOs from above, implying a finding rate of 87%, employed by 1,352 different firms.[11]

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

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

[11] To be conservative and reduce noise, in cases in which we are not sure about whether the CEO is still alive or has passed away, we drop the CEO from our sample.

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 their state of incorporation.[12] 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.[13] 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 about 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.[14] Second, we restrict our sample to

[12] 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.

[13] 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.

[14] 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

CEOs whose firm was included in CRSP during the time of their tenure.[15] After these restrictions, we end up with a sample of 1,900 CEOs. To alleviate selection concerns, we present all results 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.[16]

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 a dummy variable 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 firstly enacted 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

relatively uniformly distributed throughout the year (see Eisfeldt and Kuhnen (2013)).

[15]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.

[16]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.

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, $Tenure^2_{i,t}$, 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).[17] 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

[17] 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

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 a dummy variable 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.

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

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 done carefully is that of age at death. A simple comparison between groups would overlook the fact that CEOs with BC 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.

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 can define a CEO's BC law exposure in two ways. First, we can define it as as a binary indicator for ever being exposed to the BC law treatment. This approach corresponds to a traditional quasi-experimental design; it ignores, however, the intensity (i.e. length) of exposure, which is presumably an important factor to consider in our setting. Therefore, a second definition of treatment, which forms the basis for all our analyses, uses length of exposure rather than an indicator for exposure.

Using this approach, our main estimating equation relates the probability of death of CEO i in year t to the cumulative exposure to a BC law until year t as well as controls (recall that our data is a CEO-year-level panel, see Section 2.3):

$$\Pr(Death_{i,t}) = \beta BC_{i,t} + \boldsymbol{\delta}' \boldsymbol{X}_{i,t} + \varepsilon_{i,t} \tag{1}$$

$Death_{i,t}$ takes the value 1 in the year in which the CEO dies and exits the hazard model, and takes the value 0 otherwise. $BC_{i,t}$ counts a CEO's BC exposure in years until year t. $\boldsymbol{X}_{i,t}$ is a vector of control variables controlling for time trends (through a linear year control or fixed effects), a CEO's age, as well as firm location and industry.

Estimation Method and Censoring. To estimate Equation 1, we use the Cox (1972) proportional hazards model. Designed for survival analyses, the hazard model is an intuitive choice in our setting. 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. (CEOs' exposure to the BC law ends, however, with their departure as CEO). An attractive feature of the Cox (1972) hazard model is that it allows for censored observations, i.e., CEOs that are still alive today. Since our data collection begins on October 1st, 2017, we pick this date as the censoring date in the hazard analysis.

Alternative Specifications. We implement three alternative specifications that show robustness of our results and interpretation. We sketch these approaches here and defer a detailed discussion to Sections 4.3.1, 4.3.2, and 4.3.3, respectively.

Predicted Length of Exposure. A CEO's remaining tenure at time of the BC law passage (and hence, the CEO's exposure to BC laws) might be affected by the introduction of the laws. Our results are robust to using a 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.

Nonlinear Effects. Non-parametric Kaplan-Meier survival plots of the data (see Section 4.1 for details) suggest that the effect of insulation from takeover threat on a CEO's long-term health and mortality might 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 account for the possibility of nonlinear effects, we also 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.

Explicit Tenure Controls. As a further robustness test, we augment our main estimating equation, based on true BC exposure, with explicit controls for a CEO's tenure. Under the assumptions that longer-lived CEOs are likely to remain on the job for longer, and that the BC laws affect tenure monotonically (i.e. BC laws may increase the tenure of CEOs but preserve their rank), including tenure controls isolates the effects of the corporate governance laws on working conditions.[18] In Appendix C, we provide a detailed discussion of these assumptions. We conclude from this discussion that adding tenure controls likely attenuates our results, which is in line with our empirical estimates when comparing coefficients across specifications.

[18]For a discussion and derivation of the bias from the inclusion of controls that may themselves be outcomes, see Section 3.2.3 in Angrist and Pischke 2008).

4 Results: Corporate Governance and Life Expectancy

4.1 Graphical Evidence

We first provide graphical evidence supporting our conjecture that CEOs who served under stricter corporate governance regimes, i.e. harsher conditions, have worse life expectancies than their peers who were insulated from hostile takeovers by a BC law. Figure 2 plots the Kaplan-Meier survival graphs, split by whether CEOs served in stringent or lenient monitoring environments.[19] In both panels of Figure 2, 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, 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 least 30 CEOs in either cohort group are uncensored, explaining the slightly differential ends of the survival lines (after 36 and 45 years, respectively).

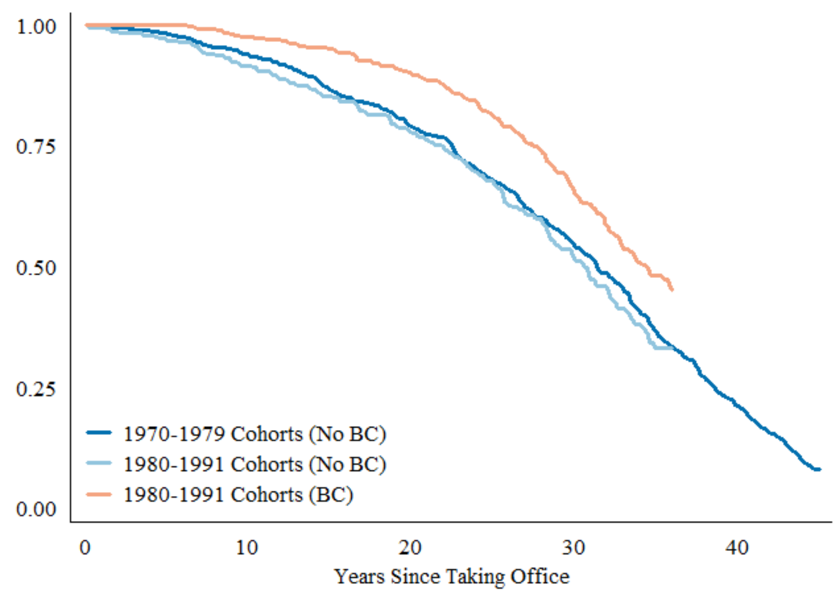
The survival functions provide first evidence that serving under more stringent corporate governance is associated with adverse consequences in terms of life expectancy. Two results emerge. First, the survival patterns of the "1970s cohorts" and the "1980s cohorts without BC exposure" are remarkably 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.

[19] 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.

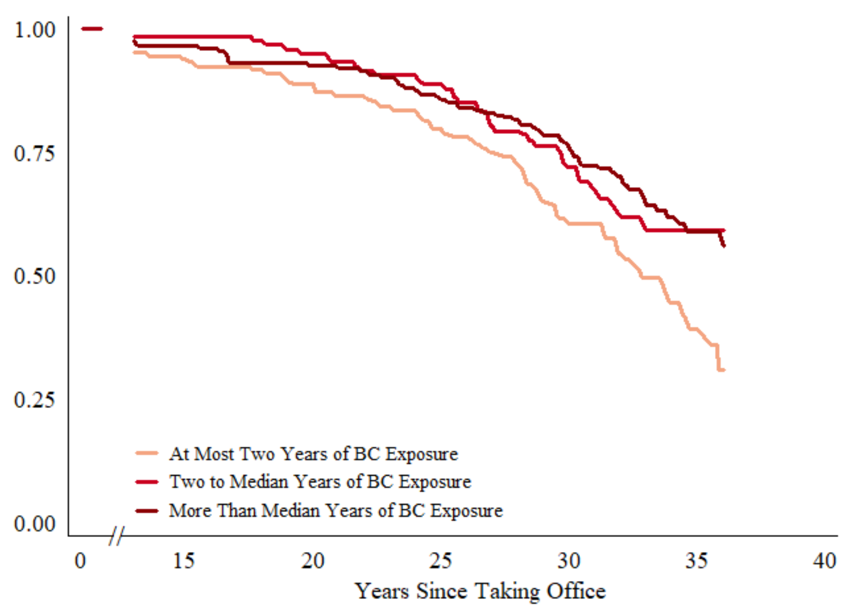
Figure 2: Kaplan-Meier Survival Estimates

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).

(a) By Cohort



(b) By Years Under BC Law



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.[20]

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 might taper off eventually.

4.2 Main Result

Table 2 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 dependent variable is one if the CEO dies in a given year and zero otherwise. The main independent variable of inter-

[20]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.

est is *BC*, a CEO's cumulative exposure to a business combination law. All regressions control for a CEO's age and firm location fixed effects.[21] 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.

Table 2: Hazard Model

This table shows the effect of variation in corporate governance stringency 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 *BC*, a CEO's cumulative exposure to a business combination 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. 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.

DEPENDENT VARIABLE: $Death_{i,t}$			
	(1)	(2)	(3)
BC	0.955***	0.958***	0.959***
	[0.005]	[0.005]	[0.005]
Age	1.111***	1.121***	1.122***
	[0.007]	[0.005]	[0.005]
Year	1.005	1.001	
	[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

[21] 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.

Columns (1) adds a linear control for time trends to the specification, in addition to CEOs' age. The estimated hazard ratio on BC law exposure is 0.955, significant at 1%. Thus, a one year increase in exposure to more lenient governance is estimated to reduce a CEO's mortality risk by 4.5%. The inclusion of industry fixed effects in Column (2) (using the Fama and French (1997) classification of firms into 49 industries) has virtually no effect on the hazard ratio on BC law exposure. The estimate is now 0.958 and remains significant at 1%. Similarly, adding year fixed effects in Column (3), instead of the linear time control, has virtually no effect on the BC exposure coefficient.

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.

The hazard ratios on BC exposure imply sizable effects. Applying the 4.5% reduction in mortality rates estimated in Column (1) to the one-year mortality rate of a 60-year-old male American born in 1925, we estimate a shift from 1.733% to 1.655% resulting from one additional year of lenient monitoring; the latter rate is in between the death probabilities of 60-year-olds born in 1927 and 1928. In turn, at age 60, American males born in 1925 have a remaining life expectancy of 18.89 years, compared to 19.10 (19.21) years for those born in 1927 (1928). Hence, our estimates translate into a birth year effect of several years, as well as meaningful shifts of remaining life expectancy.[22]

For the median BC exposure effect of 4.4 years, our estimates yield that prolonged exposure to lenient governance can lead to life expectancy gains of up to one year.[23] While these estimated effects are sizable, they are many times smaller than the health costs associated with known health

[22] As noted above, our CEOs have a longer average life expectancy compared to the *Life Tables* data, which are based on the general population. Hence, we emphasize the interpretation in terms of *differences* rather than levels.

[23] A 4.4-year BC effect yields a reduction in the one-year mortality rate of 18%; $\exp(4 \times \ln(0.955)) = 0.82$. This translates into birth cohort differences of ten years, and the *Life Tables* indicate that American males born in 1935 have a remaining life expectancy of 19.92 years at age 60, roughly one year more than those born in 1925.

threats; smoking, for example, is associated with reduced lifespans by ten years and more (HHS (2014), Jha et al. (2013)).

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 long-term 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. 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.

Step 1 – Construction of Predicted Treatment. The first step (the "zero" stage) 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 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 $D(BCLawPassed_{s(i),t} = 1)$ 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:

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

$\widehat{BC}_i^*$ 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.

Step 2 – Hazard Estimation. We can then use the predicted cumulative BC law exposure in our hazard models. Specifically, we estimate the following reduced form[24] hazard regression:

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

Table 3 presents the reduced form hazard regression results. We include controls and fixed effects in the same order as in Table 2. 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.

[24] Alternatively, we could use an instrumental variables (IV) model estimated via the residual inclusion (i.e. control function) method, given by:

$$BC_{i,t} = \alpha \widehat{BC}_{i,t} + X'_{i,t}B + \eta_{i,t} \quad \text{(First Stage)}$$

$$h(\widehat{BC}_{i,s(i)}, X_i) = h_0(t)\ \exp\{\beta BC_{i,t} + \boldsymbol{\delta' X}_{i,t} + f(\eta_{i,t})\} \quad \text{(Second Stage)}$$

For reference, the estimated coefficient $\widehat{BC}$ in the First Stage regression is $\hat{\alpha} = 0.87$.

Table 3: Hazard Model (Predicted Exposure)

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.

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

The results using predicted BC exposure corroborate our baseline findings: also 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 2 (recall that we show the estimates for the reduced form regression rather than the IV-control function approach in footnote 24; the estimated coefficient on $\widehat{BC}$ in the First Stage regression shown in footnote 24 is $\hat{\alpha} = 0.87$). While the bootstrapped standard errors rougly double in size compared to Table 2, the coefficient of interest remains significant in all columns, either at 1% or 5%.

In sum, the association between variation in 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

In light of the survival plots shown in Figure 2, pointing to potential diminishing incremental effects of exposure to a more lenient governance regime on survival rates, we also 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.[25]

Table 4 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 concentrated in the first few years of exposure to reduced monitoring.

4.3.3 Explicit Tenure Controls

As an additional robustness test, we also re-estimate the previous table showing nonlinear effects when including specific controls for CEO tenure (see Section 3 and Appendix C for a discussion of the assumptions under which including tenure controls isolates the effects of anti-takeover laws on working conditions from possible effects on tenure).

[25] 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 4: Hazard Model (Nonlinear Effects)

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.

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

Table 5 presents the results with explicit controls for a CEO's cumulative tenure. The hazard ratio on the linear tenure term is clearly insignificant. The hazard ratio on the squared tenure term is also close to one but significant, pointing to a small but significant nonlinear effect of tenure on life expectancy.

The conclusions regarding the effects of corporate monitoring stringency on CEOs' mortality risk remain, however, unchanged. We continue to find a significant effect of below-median BC exposure on survival rates, which remains significant at 1% in Columns (1) and (2) and 5% in Col-

Table 5: Hazard Model (Tenure Controls)

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, when including explicit controls for tenure (see Appendix C for a discussion of this approach). 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 $BC_{i,t}^{(\text{min-}p50)}$, 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.

DEPENDENT VARIABLE: $Death_{i,t}$			
	(1)	(2)	(3)
$BC^{(\text{min-}p50)}$	0.935***	0.939***	0.950**
	[0.020]	[0.023]	[0.023]
$BC^{(p51\text{-max})}$	1.023	1.025	1.028
	[0.023]	[0.026]	[0.026]
Tenure	1.008	1.014	1.008
	[0.010]	[0.011]	[0.011]
$Tenure^2$	0.999***	0.999***	0.999***
	[0.000]	[0.000]	[0.000]
Age	1.107***	1.117***	1.116***
	[0.005]	[0.005]	[0.004]
Year	0.994	0.991	
	[0.008]	[0.008]	
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

umn (3). We note, though, that the hazard ratio is slightly closer to one in all columns compared to Table 4. We interpret this as being consistent with our discussion in Appendix C that adding tenure controls likely attenuates our results. The hazard ratio on above-median BC exposure remains close to one and insignificant, consistent with our prior results. Adding the tenure controls has little effects on other control variables.

4.4 Robustness Tests

We conduct three series of robustness tests. For brevity, we report them in the appendix (Appendix B), and only re-estimate the model with nonlinear effects and tenure controls (Table 5) – the specification in which we estimate smaller effect sizes. In the first robustness test (Section 4.4.1), we include additional controls for CEO and firm characteristics. In Section 4.4.2, we use the first passage of any of the five most common second-generation anti-takeover laws as the year that governs the switch from high-monitoring to low-monitoring.[26] In Section 4.4.3, we implement the tests suggested in Karpoff and Wittry (2018), considering the legal and institutional context of the laws, and also show robustness to various data cuts.

4.4.1 Additional CEO and Firm Controls

Appendix-Table B1 contains the results when controlling 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.[27]

Two findings emerge. First, the coefficients on the BC law exposure variables are almost identical to those in Table 5 and remain significant at 5% or 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. 2016).

[26]This approach follows Cheng, Nagar, and Rajan (2004) and the robustness check in Atanassov (2013).

[27]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.

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 (Bertrand and Mullainathan 2003, Gormley and Matsa 2016).[28] 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.[29] 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 B2 re-estimates Table 5 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.[30] The estimated effect sizes are very similar compared to our main specification using BC laws; the hazard ratios range between 0.941 and 0.952, compared to between 0.935 and 0.950 in Table 5.[31]

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.

[28] 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).

[29] 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.

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

[31] The *First Law* results are also robust to including the additional CEO and firm level controls from Section 4.4.1 (see Appendix-Table B3).

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 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 B4 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).[32] Across panels, our findings are robust to these restrictions suggested by Karpoff and Wittry (2018). In all columns, the hazard ratio on below-median BC exposure remains significant at 5% or 1%; if anything, the significance levels slightly increase. In addition, the hazard ratio estimates are nearly unchanged, ranging between 0.927 (Panel C, Column (1)) to 0.951 (Panel B, Column (3)).

Appendix-Table B5 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

[32]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, 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 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.

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 5% in Panels A and B. In the most restrictive sample in Panel C, we lose statistical power (standard errors triple), though the point estimate remains unaffected (in fact, it is further away from one).

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.[33] 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 Short-Term Effects of Stress on Health

We also analyze the 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. To do so, 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 indicates a clear gap in the cumulative fraction of deaths between the latter two groups, while the difference between the first two groups is smaller and undetermined towards the beginning. The "1980s cohorts with BC exposure" have consistently lower rates of deaths within the first five years after stepping down compared to earlier cohorts, and most importantly, compared

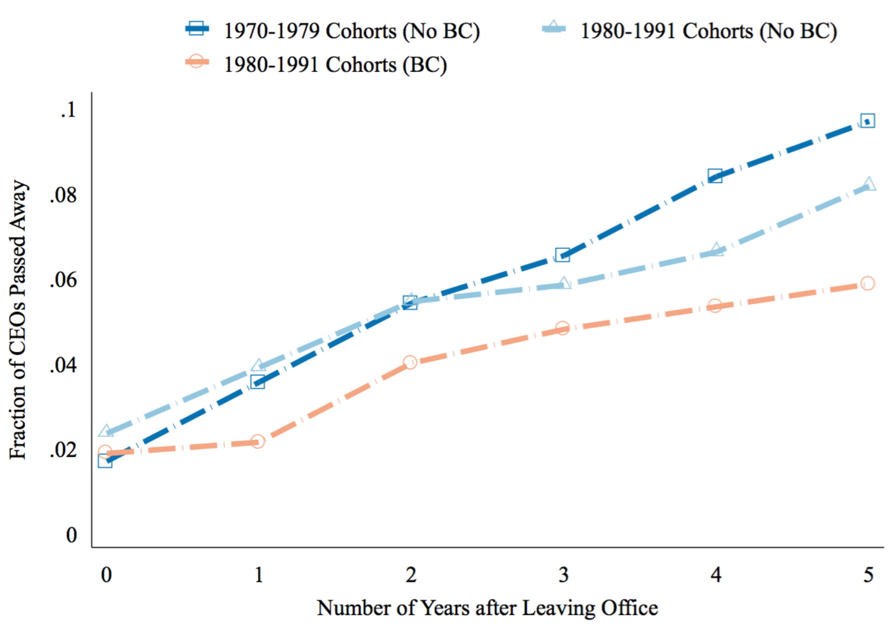
[33] See Giroud and Mueller (2010) and Gormley and Matsa (2016) for similar robustness checks.

to CEOs from the same cohorts but who never had any BC exposure. For example, 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%.

This analysis reveals that not only is more stringent corporate governance associated with higher mortality rates of CEOs in the long run, it also appears to be linked to higher probability of death soon after a CEO steps down.

Figure 3: Fraction of CEOs that Passed Away after Leaving Office

The figure shows the cumulative fraction of CEOs that passed 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.



4.6 Corporate Governance Stringency and CEO Pay

If more stringent corporate governance translates into poorer health outcomes in terms of reduced life expectancy, will CEOs who are exposed to harsher conditions bargain for higher compensation? From a theoretical perspective, the hypothesis of a positive relationship between job demands and pay follows from contract theory and compensating wage differentials. However, models of agency conflicts and managerial entrenchment predict higher pay once CEOs are insulated from the risk of a hostile takeover, allowing them to skim more resources. Bertrand and Mullainathan (2001) discuss the motivating theories based on contracting and skimming models and find empirical support for the latter in the context of anti-takeover laws. They estimate a statistically significant *increase* in total pay of approximately 5% for CEOs protected by BC laws.

Our data allows us to revisit the findings in Bertrand and Mullainathan (2001). As mentioned above, the intital data set from Kevin Murphy (Gibbons and Murphy 1992) contains yearly information on a CEO's pay. The pay data is available for 63% of CEO-years until 1991.[34]

In Table 6, we regress CEOs' total compensation (in logs) on exposure to BC laws and a set of control variables following Bertrand and Mullainathan (2001). Columns (1) and (2) include cumulative BC exposure as the main regressor in line with our previous hazard models. Columns (3) and (4) include dummy variables for BC exposure instead, in line with Bertrand and Mullainathan (2001). Across columns, we observe a positive coefficient on the BC coefficient. The magnitudes are close to those estimated in Bertrand and Mullainathan (2001). While our results lack the same statistical significance compared to Bertrand and Mullainathan (2001) – none of the BC coefficients in Table 6 are significantly different from zero – we can reject the *one-sided* null hypothesis that the coefficient is smaller or equal to zero at conventional levels in all four regressions (the *p*-values vary between 0.054 and 0.100). The loadings on control variables are sensible

[34]The data set in Gibbons and Murphy (1992) ends in 1991; therefore, we do not have data on CEO pay after 1991. Since no states passed BC laws before 1985, we include total BC exposure in the regressions in Table 6, as a split into below and above the median exposure (4.4 years in our sample taking into account years after 1991) is not meaningful given the sample used for Table 6 ends in 1991.

Table 6: Business Combination Laws and CEO Pay

This table presents regressions in which we regress CEO's total compensation on BC exposure and a set of control variables following Bertrand and Mullainathan (2001). All variables are defined in Appendix A. 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. *p*-values for a *one-sided* test H_0: $\beta_k \leq 0$, H_A: $\beta_k > 0$, where k refers to the coefficient on *BC* or *BC Dummy*, are shown in parentheses. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

DEPENDENT VARIABLE: $Pay_{i,t}$ (ln)				
	(1)	(2)	(3)	(4)
BC	0.033	0.026		
	[0.020]	[0.020]		
	(0.054)	*(0.100)*		
BC Dummy			0.072	0.059
			[0.047]	[0.043]
			(0.066)	*(0.087)*
Tenure		0.042***		0.042***
		[0.003]		[0.003]
$Tenure^2$		-0.001***		-0.001***
		[0.000]		[0.000]
Assets (ln)		0.179***		0.180***
		[0.011]		[0.011]
Employees (ln)		0.088***		0.088***
		[0.013]		[0.012]
Age		-0.003**		-0.003**
		[0.001]		[0.001]
Location FE (HQ)	Y	Y	Y	Y
FF49 FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Number of CEOs	1,587	1,550	1,587	1,550
Observations	11,367	11,208	11,367	11,208

and consistent with those in Bertrand and Mullainathan (2001); for example, we estimate a strong, positive associated between CEO compensation and firm size as measured by assets and number of employees.

These results, consistent with those in Bertrand and Mullainathan (2001), suggest that CEOs became more entrenched and skimmed more resources from the firm when they were less disciplined by corporate governance. The findings provide evidence *against* the idea that CEOs who worked during periods of higher scrutiny and increased takeover threats bargained for a compensating wage differential.

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.[35]

In the spirit of Opler and Titman (1994) 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%.[36] 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.[37]

[35] 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 (2016) explores the impact of distress of worker exit rates and entrepreneurship.

[36] We follow Babina (2016) 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.

[37] 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

Table 7 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 2 for BC law exposure. In addition to the controls and fixed effects from Table 2, 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 1.043, significant at conventional levels.

These estimates point to similar effect magnitudes as before. 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 *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, below-median BC exposure is estimated to result in meaningful reductions in mortality rates in all specifications; the hazard ratios are virtually unchanged compared to Table 4, and remain significant at 1%.

Again, we present an array of robustness checks in the appendix. First, we again estimate a version in which explicitly control for CEO tenure (Appendix-Table B7). In addition, we also estimate a version with tenure controls as well as additional CEO and firm controls (Appendix-Table B8), and a version with tenure controls based on an extended sample, including the 295

incremental effects of high industry shock exposure (which would be estimated off of very few CEOs).

Table 7: Hazard Model (Industry Distress)

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.

DEPENDENT VARIABLE: $Death_{i,t}$			
	(1)	(2)	(3)
Industry Distress	1.042**	1.043*	1.042*
	[0.019]	[0.024]	[0.025]
$BC^{(\text{min-}p50)}$	0.903***	0.910***	0.912***
	[0.020]	[0.023]	[0.023]
$BC^{(p51\text{-max})}$	0.988	0.988	0.987
	[0.015]	[0.017]	[0.017]
Age	1.111***	1.120***	1.121***
	[0.007]	[0.005]	[0.005]
Year	1.010*	1.007	
	[0.005]	[0.005]	
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

CEOs we dropped from the analysis thus far as they were appointed after the introduction of BC laws (Appendix-Table B9). 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-death 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 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 D 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 5,613 facial pictures for which at least ten people estimated 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 final output of the software 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.

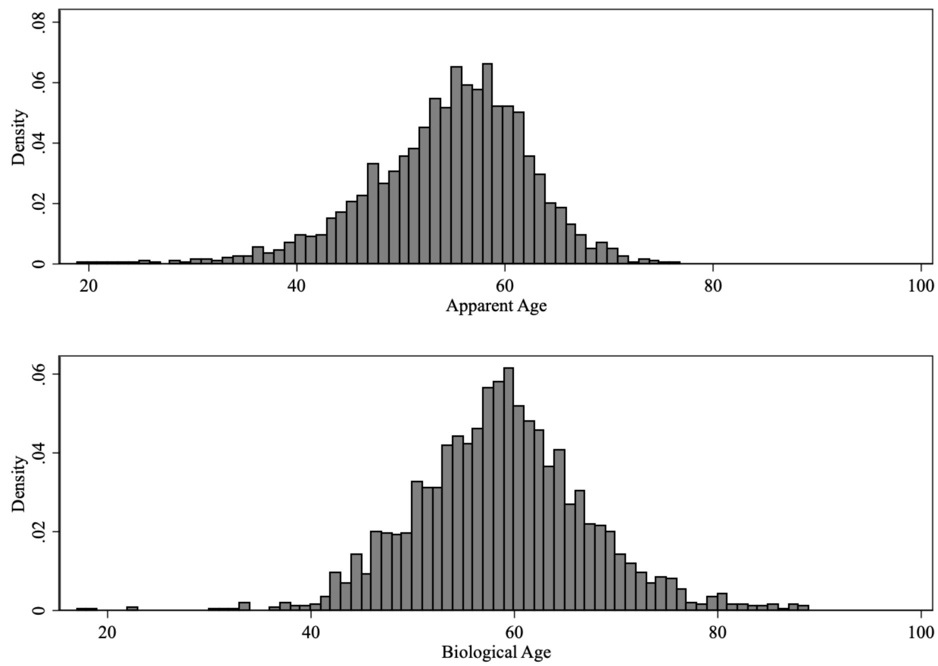
To carry out our analysis, we manually collect a large set of pictures of CEOs of *Fortune 500* firms in 2006. Note that the *Fortune 500* actually publishes 1,000 firms in their list each year, which generates an initial sample of of 1,000 CEOs. Collecting pictures based on the 2006 *Fortune* list not 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 collect pictures from gettyimages.com as well as Google Images. For 466 CEOs, we are able to find at least two pictures from different points in time during or after their tenure. In total, we collect 3,113 pictures for these 466 CEOs; this set of pictures constitutes the main sample for the analyses below.

We proceed in two steps. First, we document the *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 shows the age distribution of CEOs based on 3,113 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. 2016). 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.

Figure 4: CEO Apparent and Biological Age Distribution

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 3,113 pictures collected from *Fortune 500* CEOs who were CEO in 2006. The second plot shows the associated biological age distribution.



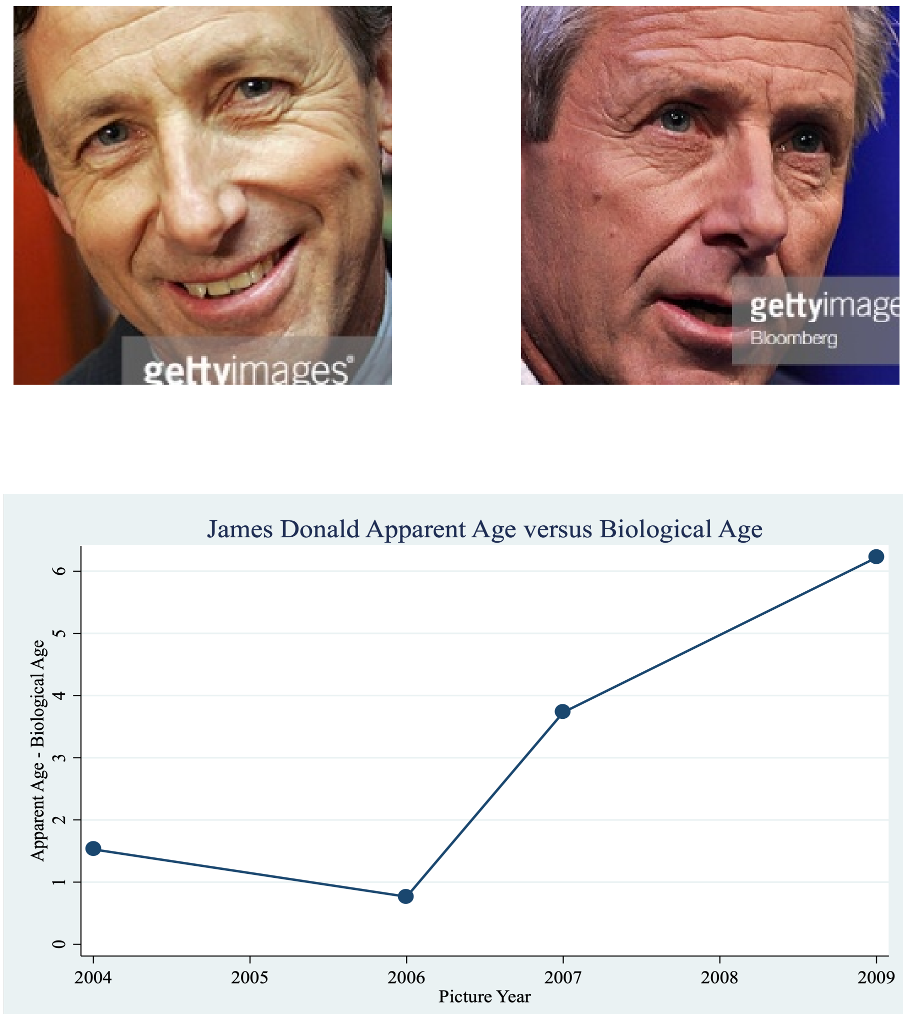
To illustrate the mechanism between industry shocks and aging we have in mind, we first zoom in on one specific CEO: James Donald, the CEO of Starbucks from April 2005 until January 2008, when he was fired.[38] Figure 5 shows two pictures of Donald; the one on the left was taken on December 8, 2004, 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

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

older than his true age; asnd, 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.

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

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.



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 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 38).

We formalize our analysis of job-induced aging by using the 3,113 collected pictures in a difference-in-differences design. Table 8 shows summary statistics for the 466 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 8: Summary Statistics

The table presents summary statistics for the CEOs for whom we find at least two pictures from different points in time during their tenure.

	PANEL A: CEO CHARACTERISTICS					
	N	mean	sd	p10	p50	p90
Biological Age in 2006	466	56.35	8.46	46	57	66
Pre-2006 Tenure	466	8.00	7.71	2	6	17
Pre-2006 Ind. Shocks	466	0.70	1.52	0	0	3
2007-2008 Ind. Shocks	466	0.81	0.78	0	1	2
2007-2008 Ind. Shocks (ITT)	466	0.96	0.81	0	1	2
No. of Pictures per CEO	466	6.68	4.41	2	6	12

	PANEL B: INDUSTRY DISTRIBUTION		
Industry (Number of CEOs)	Manufacturing (182)	Construction (12)	Mining (21)
	Wholesale (17)	Retail (53)	Services (44)
	Trans.; Commns.; Elec., Gas, and Sanitary Service (71)		
	Finance, Insurance, Real Estate (65)		Others (1)

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:

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

where i represents a CEO and t represents a year. The *IndShock* variable is a dummy variable indicating 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.[39] 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 *TimeDummy* 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

[39] 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.

not they personally experienced industry shocks during 2007-2008.

Table 9: Industry Distress and CEO Aging

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 dummy 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 dummy as an instrument for the actual industry shock experience interacted with the post-2006 dummy. Column (3) is similar to Column (1) but splits the post period into two sub-periods (2007-2011 and post 2011). "$Pre2006\, Ind.\, Shock$" and "$Pre2006Tenure$" are categorical variables that we control for by including fixed effects. 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.

DEPENDENT VARIABLE: *Apparent Age*	(1)	(2)	(3)
$Ind.\, Shock_{ITT} * \mathbb{1}_{\{PictureYear>2006\}}$	1.067**		
	[0.495]		
$Ind.\, Shock_{IV} * \mathbb{1}_{\{PictureYear>2006\}}$		1.166**	
		[0.533]	
$Ind.\, Shock_{ITT} * \mathbb{1}_{\{2006<PictureYear<2012\}}$			0.897*
			[0.525]
$Ind.\, Shock_{ITT} * \mathbb{1}_{\{PictureYear\geq 2012\}}$			1.316**
			[0.588]
Biological Age	0.790***	0.789***	0.785***
	[0.124]	[0.122]	[0.124]
$Pre2006\, Ind.\, Shock$ Control	Y	Y	Y
$Pre2006\, Tenure$ Control	Y	Y	Y
CEO FE	Y	Y	Y
Year FE	Y	Y	Y
Number of CEOs	466	466	466
Observations	3,113	3,113	3,113

Table 9 presents results of the difference-in-differences analysis. In column (1), we use the "intention-to-treat" version of the industry experience, interacted with a time dummy indicating 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 1.067, 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 the post-2006 dummy as an instrument for actual industry shock experience interacted with the post-2006 dummy. The coefficient on the interaction term is 1.166 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 0.915 ($= \frac{1.067}{1.166}$), 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 0.9 years for the first five-year period, increasing to about 1.3 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 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 2000-2015, 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 1.3 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.[40] Likewise, heightened workplace stress can also adversely likely affect other aspects of life, including CEOs' marriage and divorce rates as well as parenting.

[40]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	A dummy variable indicating 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).
Age Taking Office	A CEO's age when appointed as CEO.
Year Taking Office	The year in which a CEO is appointed.
$Age_{i,t}$	A CEO's age in year t.
$Tenure_{i,t}$	A CEO's cumulative tenure (in years).
$Tenure^2_{i,t}$	A CEO's squared cumulative tenure (in years).
$BC_{i,t} = BCExposure_{i,t}$	A CEO's cumulative exposure to a BC law during his tenure over time (in years).
$BC^{(\text{min-}p50)}_{i,t}$	A CEO's below-median (4.4 years), cumulative exposure to a BC law during his tenure over time (in years).
$BC^{(p51\text{-max})}_{i,t}$	A CEO's above-median (4.4 years), cumulative exposure to a BC law during his tenure over time (in years).
$BC\ Dummy_{i,t}$	1 if CEO i is insulated from a BC law in year t.
$FL_{i,t} = FLExposure_{i,t}$	A CEO's cumulative exposure to the first-time enactment of a 2nd generation anti-takeover law (FL) during his tenure over time (in years).
$FL^{(\text{min-}p50)}_{i,t}$	A CEO's below-median (4.4 years), cumulative FL law exposure during his tenure over time (in years).
$FL^{(p51\text{-max})}_{i,t}$	A CEO's above-median (4.4 years), cumulative FL law exposure during his tenure over time (in years).
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

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.

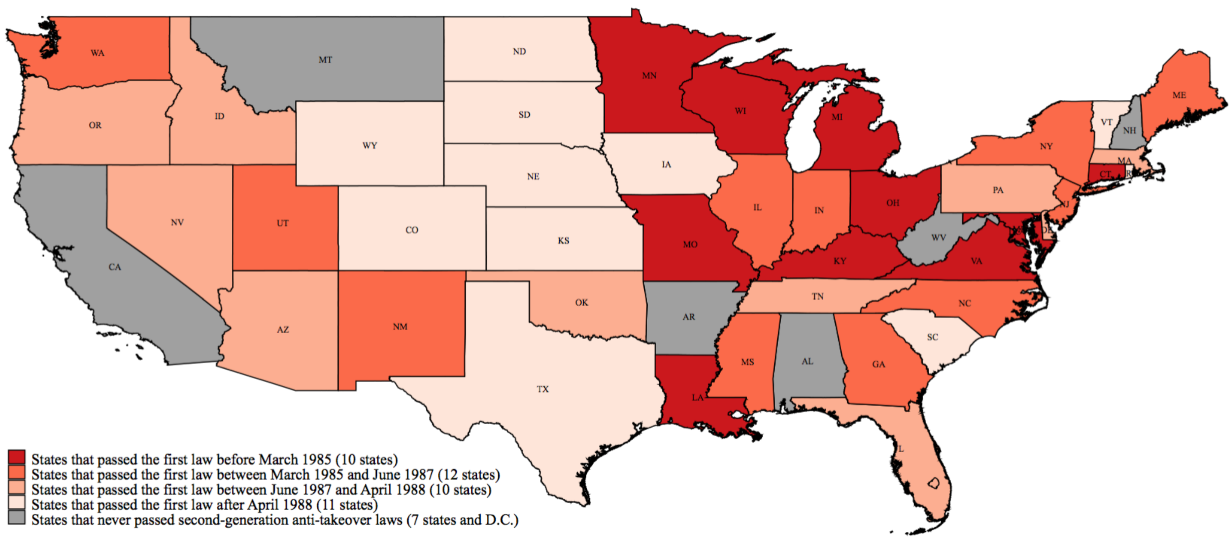


Figure B2: Average Number of Pictures per CEO Across Years

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.

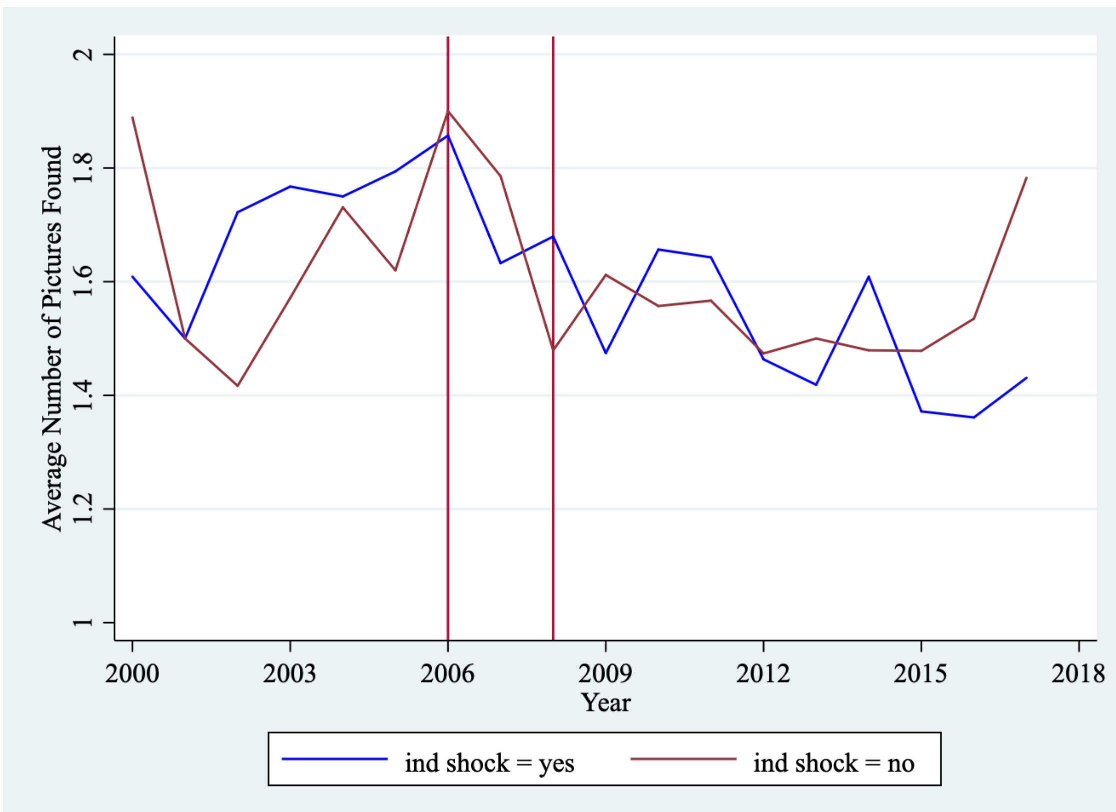


Table B1: Hazard Model (Additional Controls)

This table re-estimates Table 5 except that we include additional controls for CEO characteristics (pay) and firm characteristics (assets, employees). See Table 5 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.

DEPENDENT VARIABLE: $Death_{i,t}$			
	(1)	(2)	(3)
$BC^{(\text{min-}p50)}$	0.935***	0.939***	0.950**
	[0.021]	[0.023]	[0.023]
$BC^{(p51\text{-max})}$	1.024	1.026	1.030
	[0.023]	[0.026]	[0.027]
Tenure	1.004	1.011	1.005
	[0.012]	[0.013]	[0.013]
$Tenure^2$	0.999***	0.999***	0.999***
	[0.000]	[0.000]	[0.000]
Pay (ln)	1.029	1.033	1.037
	[0.037]	[0.045]	[0.045]
Assets (ln)	1.025	1.017	1.014
	[0.026]	[0.042]	[0.039]
Employees (ln)	0.969	0.959	0.957
	[0.022]	[0.039]	[0.038]
Age	1.107***	1.118***	1.117***
	[0.007]	[0.005]	[0.005]
Year	0.989	0.987	
	[0.010]	[0.011]	
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

Table B2: Hazard Model (First Law)

This table re-estimates Table 5 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 5 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.

DEPENDENT VARIABLE: $Death_{i,t}$			
	(1)	(2)	(3)
$FL^{(\text{min-}p50)}$	0.946***	0.941***	0.952**
	[0.020]	[0.020]	[0.021]
$FL^{(p51\text{-max})}$	1.013	1.016	1.019
	[0.021]	[0.023]	[0.024]
Tenure	1.006	1.013	1.006
	[0.013]	[0.013]	[0.014]
$Tenure^2$	0.999***	0.999***	0.999***
	[0.000]	[0.000]	[0.000]
Age	1.105***	1.115***	1.115***
	[0.005]	[0.005]	[0.005]
Year	0.992	0.991	
	[0.010]	[0.010]	
Location FE (HQ)	Y	Y	Y
FF49 FE		Y	Y
Year FE			Y
Number of CEOs	1,510	1,510	1,510
Observations	47,994	47,994	47,994

Table B3: Hazard Model (First Law, Additional Controls)

This table re-estimates Appendix-Table B2 except that we include additional controls for CEO characteristics (pay) and firm characteristics (assets, employees). See Appendix-Table B2 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.

DEPENDENT VARIABLE: $Death_{i,t}$			
	(1)	(2)	(3)
$FL^{(\min\text{-}p50)}$	0.950**	0.948**	0.959*
	[0.021]	[0.021]	[0.023]
$FL^{(p51\text{-}\max)}$	1.015	1.018	1.023
	[0.022]	[0.023]	[0.025]
Tenure	0.999	1.007	0.999
	[0.013]	[0.015]	[0.016]
$Tenure^2$	0.999***	0.999***	0.999***
	[0.000]	[0.000]	[0.000]
Pay (ln)	1.037	1.046	1.050
	[0.033]	[0.041]	[0.040]
Assets (ln)	1.028	1.006	1.004
	[0.027]	[0.040]	[0.038]
Employees (ln)	0.967*	0.967	0.964
	[0.019]	[0.038]	[0.037]
Age	1.106***	1.117***	1.117***
	[0.007]	[0.005]	[0.005]
Year	0.985	0.984	
	[0.013]	[0.013]	
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

Table B4: Excluding Lobbying Firms, Opt-Out Firms, and Firms with Firm-Level Defenses

This table re-estimates Table 5 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 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 5 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.

DEPENDENT VARIABLE: $Death_{i,t}$			
	(1)	(2)	(3)
		Panel A: Excluding Lobbying Firms	
$BC^{(\min\text{-}p50)}$	0.931***	0.929**	0.939**
	[0.024]	[0.027]	[0.027]
$BC^{(p51\text{-}\max)}$	1.027	1.028	1.031
	[0.026]	[0.028]	[0.029]
Number of CEOs	1,530	1,530	1,530
Observations	48,105	48,105	48,105
		Panel B: Excluding Opt-out Firms	
$BC^{(\min\text{-}p50)}$	0.938***	0.940**	0.951**
	[0.021]	[0.023]	[0.023]
$BC^{(p51\text{-}\max)}$	1.020	1.023	1.026
	[0.025]	[0.028]	[0.028]
Number of CEOs	1,575	1,575	1,575
Observations	49,556	49,556	49,556
		Panel C: Excluding Firm-level Defenses	
$BC^{(\min\text{-}p50)}$	0.927***	0.933***	0.943**
	[0.021]	[0.024]	[0.025]
$BC^{(p51\text{-}\max)}$	1.025	1.028	1.028
	[0.026]	[0.028]	[0.028]
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

Table B5: Restricting to Years After the First-Generation Laws (*Edgar v. MITE* Case)

This table re-estimates Table 5 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 5 for further details. All variables are defined in Appendix A. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

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	
$BC^{(\text{min-}p50)}$	0.950**	0.946**	0.923***	0.940**	0.913	0.912
	[0.019]	[0.021]	[0.027]	[0.028]	[0.062]	[0.064]
$BC^{(p51\text{-max})}$	1.032	1.030	1.007	1.025	1.081	1.079
	[0.025]	[0.025]	[0.028]	[0.025]	[0.053]	[0.052]
Tenure	1.007	1.008	1.027	1.010	0.953	0.952
	[0.010]	[0.010]	[0.018]	[0.018]	[0.054]	[0.056]
$Tenure^2$	0.999***	0.999***	0.999***	0.999***	1.000	1.000
	[0.000]	[0.000]	[0.000]	[0.000]	[0.002]	[0.002]
Age	1.115***	1.116***	1.123***	1.122***	1.119***	1.121**
	[0.005]	[0.005]	[0.006]	[0.006]	[0.015]	[0.015]
Year	0.984*		1.008		0.942	
	[0.008]		[0.015]		[0.044]	
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

Table B6: Excluding DE or NY Incorporated, Banking, or Utility Firms

This table re-estimates Table 5 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 5 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.

DEPENDENT VARIABLE: $Death_{i,t}$			
	(1)	(2)	(3)
	Panel A: Excluding DE/NY Firms		
$BC^{(\text{min-}p50)}$	0.934	0.940	0.950
	[0.045]	[0.046]	[0.048]
$BC^{(p51\text{-max})}$	1.045	1.047	1.054
	[0.041]	[0.042]	[0.041]
Number of CEOs	738	738	738
Observations	22,103	22,103	22,103
	Panel B: Excluding Firms in Banking		
$BC^{(\text{min-}p50)}$	0.924***	0.928***	0.943**
	[0.021]	[0.021]	[0.022]
$BC^{(p51\text{-max})}$	1.002	1.007	1.011
	[0.030]	[0.034]	[0.035]
Number of CEOs	1,328	1,328	1,328
Observations	42,327	42,327	42,327
	Panel C: Excluding Utility Firms		
$BC^{(\text{min-}p50)}$	0.910***	0.912***	0.922***
	[0.020]	[0.022]	[0.021]
$BC^{(p51\text{-max})}$	1.018	1.020	1.024
	[0.022]	[0.024]	[0.025]
Number of CEOs	1,422	1,422	1,422
Observations	45,017	45,017	45,017
Year (Linear Control)	Y	Y	
Age & Tenure Controls	Y	Y	Y
Location FE (HQ)	Y	Y	Y
FF49 FE		Y	Y
Year FE			Y

Table B7: Hazard Model (Industry Distress, Tenure Controls)

This table re-estimates Table 7 except that we include controls for CEO tenure. See Table 7 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.

DEPENDENT VARIABLE: $Death_{i,t}$			
	(1)	(2)	(3)
Industry Distress	1.054***	1.060***	1.059***
	[0.017]	[0.020]	[0.021]
$BC^{(\min\text{-}p50)}$	0.934***	0.938***	0.948**
	[0.020]	[0.023]	[0.023]
$BC^{(p51\text{-}\max)}$	1.019	1.020	1.023
	[0.023]	[0.026]	[0.026]
Tenure	1.007	1.012	1.007
	[0.011]	[0.011]	[0.011]
$Tenure^2$	0.999***	0.999***	0.999***
	[0.000]	[0.000]	[0.000]
Age	1.108***	1.117***	1.116***
	[0.005]	[0.005]	[0.005]
Year	0.996	0.994	
	[0.009]	[0.009]	
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

Table B8: Hazard Model (Industry Distress, Additional Controls)

This table re-estimates Appendix-Table B7 except that we include additional controls for CEO characteristics (pay) and firm characteristics (assets, employees). See Appendix-Table B7 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.

DEPENDENT VARIABLE: $Death_{i,t}$			
	(1)	(2)	(3)
Industry Distress	1.057***	1.065***	1.064***
	[0.017]	[0.021]	[0.021]
$BC^{(\min\text{-}p50)}$	0.934***	0.938***	0.948**
	[0.021]	[0.022]	[0.023]
$BC^{(p51\text{-}\max)}$	1.020	1.021	1.025
	[0.022]	[0.026]	[0.027]
Tenure	1.002	1.008	1.002
	[0.013]	[0.013]	[0.013]
$Tenure^2$	0.999***	0.999***	0.999***
	[0.000]	[0.000]	[0.000]
Pay (ln)	1.033	1.043	1.047
	[0.039]	[0.045]	[0.045]
Assets (ln)	1.027	1.014	1.012
	[0.026]	[0.040]	[0.038]
Employees (ln)	0.964*	0.960	0.959
	[0.021]	[0.038]	[0.037]
Age	1.108***	1.118***	1.118***
	[0.007]	[0.005]	[0.005]
Year	0.991	0.989	
	[0.010]	[0.011]	
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

Table B9: Hazard Model (Industry Distress, Additional CEOs)

This table re-estimates Appendix-Table B7 except that we use an extended sample including CEOs who were appointed after the passage of BC laws. SeeAppendix-Table B7 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.

DEPENDENT VARIABLE: $Death_{i,t}$			
	(1)	(2)	(3)
Industry Distress	1.047***	1.064***	1.063***
	[0.017]	[0.022]	[0.023]
$BC^{(\min\text{-}p50)}$	0.956**	0.963*	0.975
	[0.021]	[0.021]	[0.022]
$BC^{(p51\text{-}\max)}$	0.996	0.996	0.999
	[0.027]	[0.031]	[0.032]
Tenure	0.996	1.000	0.994
	[0.010]	[0.010]	[0.010]
$Tenure^2$	0.999**	0.999**	0.999**
	[0.000]	[0.000]	[0.000]
Age	1.109***	1.117***	1.116***
	[0.006]	[0.004]	[0.004]
Year	0.998	0.995	
	[0.009]	[0.009]	
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

Table B10: Industry Shock and CEO Aging: Restricted Sample 2000-2015

The table shows the results of the picture analysis in Section 5.1 when we restrict the sample to 2000-2015 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 dummy 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 dummy as an instrument for the actual industry shock experience interacted with the post-2006 dummy. Column (3) is similar to Column (1) but splits the post period into two sub-periods (2007-2011 and post 2011). "$Pre2006\ Ind.\ Shock$" and "$Pre2006Tenure$" are categorical variables that we control for by including fixed effects. 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.

DEPENDENT VARIABLE: *Apparent Age*			
	(1)	(2)	(3)
$Ind.\ Shock_{ITT} * \mathbb{1}_{\{PictureYear>2006\}}$	0.861*		
	[0.504]		
$Ind.\ Shock_{IV} * \mathbb{1}_{\{PictureYear>2006\}}$		0.935*	
		[0.543]	
$Ind.\ Shock_{ITT} * \mathbb{1}_{\{2006<PictureYear<2012\}}$			0.612
			[0.538]
$Ind.\ Shock_{ITT} * \mathbb{1}_{\{PictureYear\geq 2012\}}$			1.471**
			[0.611]
Biological Age	0.756***	0.632***	0.764***
	[0.122]	[0.069]	[0.122]
$Pre2006\ Ind.\ Shock$ Control	Y	Y	Y
$Pre2006\ Tenure$ Control	Y	Y	Y
CEO FE	Y	Y	Y
Year FE	Y	Y	Y
Number of CEOs	432	432	432
Observations	2,528	2,528	2,528

Appendix C Tenure Controls and Selection Bias

We run the hazard models in the specifications using true BC law exposure both with and without tenure controls. The inclusion of tenure controls is motivated by two issues. First, we are concerned that the length of BC exposure may itself be endogenous, if long-lived CEOs are more likely to stay on the job. This would generate a spurious correlation between the intensity (i.e. intensive margin) of the treatment and the outcome, even if treatment status (i.e. the binary or extensive margin of treatment) is randomly assigned. Thus, controlling for tenure is intended to capture differences between CEOs that may correlate with both treatment intensity and the outcome. Second, we would like to condition out the effect of tenure itself. Appendix-Table C1 shows that anti-takeover laws increased the tenure of incumbent CEOs (which is consistent with reduced stress on the job). We would like, however, to isolate the effects of the corporate governance laws on working conditions from any possible effects of the longer tenure. Adding controls for tenure serves both of the above purposes, at the cost of the introduction of a selection bias term in the estimand. Here, we discuss the intuition for the bias and its likely sign.

To simplify the discussion, assume that there are two tenure lengths, Long and Short. When we condition on a given tenure we estimate:

$$E[Lifespan_{BC}|Tenure_{BC} = Long] - E[Lifespan_{NoBC}|Tenure_{NoBC} = Long]$$

The selection problem arises because of the difference in the conditioning statements, which is to say, the composition of CEOs with Long tenure changes with the business combination laws. (The choice of Long in the conditional does not matter for the discussion.) We assume monotonicity, which is to say that no CEO reduces their tenure as a result of the BC laws. Since the BC laws push people from Short tenure into Long, the bias will be a function of the life expectancy of the Long-only-under-BC group relative to the always-Long group.

This can be seen by adding and subtracting our desired counterfactual:

$$\begin{aligned} &\overbrace{E[Lifespan_{BC} - Lifespan_{NoBC}|Tenure_{BC} = Long]}^{\textbf{Causal Effect of BC Laws}} - \\ &\underbrace{[E[Lifespan_{NoBC}|Tenure_{BC} = Long] - E[Lifespan_{NoBC}|Tenure_{NoBC} = Long]]}_{\textbf{Selection Bias from Endogenous Control}} \end{aligned} \tag{B1}$$

Although we cannot observe $E[Lifespan_{NoBC}|Tenure_{BC} = Long]$, this statement can inform us about the likely direction of the bias. Specfically, the direction of the bias depends on whether we are adding relatively long- or short-lived CEOs the Long tenured group. Although we cannot isolate the switchers as a function of treatment status, the motivation for this specification is that we suspect that longer-serving CEOs have a life expectancy advantage. If this is the case, then our estimates will be downward biased relative to the true impact of the BC laws conditional on tenure. This is because the BC laws have pushed ex ante shorter-lived CEOs up the tenure distribution.

We can additionally examine the average relationship between tenure and longevity for some empirical guidance as to the direction of bias. Appendix Figure C1 plots the relationship between tenure and lifespan for the CEOs in our sample, where we control for a CEO's birth year and age at appointment as well as firm location and industry affiliation. Panel (C1a) depicts the entire sample and panel (C1b) shows the CEOs who are not exposed to BC laws. We see that there is a positive correlation between tenure and life expectancy in the full sample which grows weaker but is still present in the group of CEOs who do not serve under the business combination laws. This is exactly the source of bias in the analysis which uses BC exposure — CEO tenure and longevity are positively correlated — and also the basis for our assumption that the Long-only-under-BC CEOs have shorter life expectancy than the always-Long group. If the CEOs who have their tenure lengthened by the business combination laws have shorter life expectancy than those who would have had Long tenure in either regime, then the bias term will be negative, and our results will be attenuated. This direction of bias would be consistent with the overall tenure-life expectancy relationship. In particular, it would be consistent with a story in which some CEOs who did not have the protection of the business combination laws were induced to retire by diminished health capital. The analysis can be generalized by allowing multiple tenure levels and integrating over them.

Table C1: Business Combination Laws and Tenure

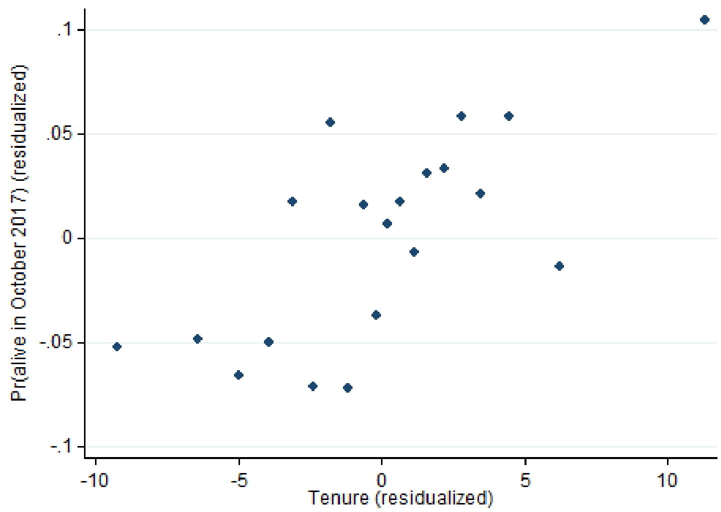
This table presents hazard models predicting CEO departure using CEOs' exposure to BC laws. Fixed effects are included as indicated at the bottom of the table. Standard errors, clustered at the state of incorporation level, are shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively. All variables are defined in Appendix A.

DEPENDENT VARIABLE: $CEO\ Departure_{i,t}$			
	(1)	(2)	(3)
$BC^{(\text{min-}p50)}$	0.891***	0.895***	0.936*
	[0.028]	[0.029]	[0.037]
$BC^{(p51\text{-max})}$	0.919***	0.912***	0.971
	[0.018]	[0.018]	[0.020]
Age	1.100***	1.107***	1.104***
	[0.011]	[0.012]	[0.012]
Year	1.098***	1.101***	
	[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

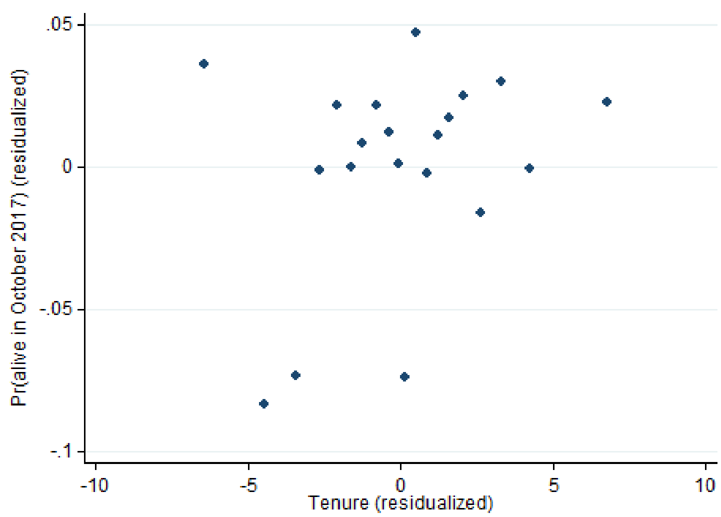
Figure C1: Tenure and Lifespan

This figure shows binned scatterplots of the relationship between tenure and lifespan, obtained from a regression of an indicator variable that is 1 if a CEO is still alive as of the censoring date on CEO tenure. The regression uses either the full sample (Panel A) or restricting to CEOs without BC exposure (Panel B). The estimation includes birth year fixed effects, fixed effects for CEO age at appointment, location fixed effects, and industry fixed effects; i.e., both the x-axis and y-axis display residualized variables.

(a) Main Sample ($N = 1,605$)



(b) Main Sample Restricted to CEOs Without BC Exposure ($N = 980$)



Appendix D Age Estimation Software Details and Convolutional Neural Networks

Apparent Age Estimation:

- Different from biological age estimation
- min $f(EstAge - ApparentAge)$ vs. min $f(EstAge - TrueAge)$

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

1. Pre-processing step: detect faces in images and resize to 224x224
2. Pre-training step: train for biological age using 250,367 IMDB-Wiki pictures (training time: 1.5 days)
3. Training and validation step: fine-tune for apparent age using 5,613 "age-annotated" pictures with ≥ 10 human age estimations per picture (training time: 5 hours)
   - 11-fold cross-validation (11 sub-models)
4. Run on test data (CEOs, politicians, ...)

Convolutional Neural Network:

- **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