# CEO Stress, Aging, and Death*

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March 11, 2022

**ABSTRACT**

We assess the long-term effects of managerial stress on aging and mortality. First, we show that exposure to distress shocks during the Great Recession produces visible signs of aging in CEOs. Applying neural-network based machine-learning techniques to pre- and post-distress pictures, we estimate an increase in their so-called apparent age by one year. Second, using data on CEOs since the 1980s, we estimate a 1.2-year decrease in life expectancy after an industry distress shock, but a two-year increase when anti-takeover laws insulate CEOs from market discipline. The estimated health costs are significant, also relative to other known health risks.

**Keywords:** Managerial stress, life expectancy, apparent-age estimation, job demands, industry distress, visual machine-learning, corporate governance.

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## I. Introduction

Much of the academic and policy discussion about high-profile jobs in business and other arenas revolves around pay, performance, and incentives. In the classical agency problem, a manager aims to extract private benefits such as higher pay, loans, or perks at the expense the owner (shareholders). Less attention has been paid to another type of private benefit (or private cost)—managers' personal health and well-being. The more effort managers exert and the more stressors they are exposed to on their job, the less they reap this benefit.

In this paper, we document the mortality and accelerated-aging implications of variation in managerial stress for chief executive officers (CEOs) of large U.S. companies. CEOs work long hours, make high-stakes decisions such as layoffs and plant closures, and face uncertainty in times of crisis (Bandiera et al. 2020, Porter and Nohria 2018). They are closely monitored and criticized when their firm is underperforming, and media frequently reports on "overworked [and] overstressed" CEOs.[1]

Job demands and work-related stress are increasingly recognized as key determinants of population health and well-being across hierarchy levels in organizations.[2] In fact, the pressures and looming consequences of underperformance are in many ways harsher for lower-ranked workers as they include (long-term) unemployment, financial hardship, and loss of health insurance. Yet, there is little quasi-experimental evidence that links health outcomes to variation in job demands and stress across worker contexts. One reason for the lack of causal evidence is that income loss and financial hardship tend to confound the estimation of the causal effects of job stressors. The CEO context helps overcome these identification hurdles. It allows to isolate the effects of high job demands from financial and other confounds. CEOs of large, publicly traded U.S. companies are wealthy and unlikely to be affected by financial hardships even if they lose their job.

That said, the CEO context is of particular interest on its own right for at least two reasons. First, CEOs bear the ultimate responsibility for the firm and its employees. Given their overarching importance for firm performance and job stability of workers, it matters how incentives and performance affect CEOs personally. Second, the health implications of CEOs' job demands affect their ability to stay on the job and, if anticipated, their willingness to select into the CEO job.

[1] See CNN's Route to the Top segment (cnn.com/2010/business/03/12/ceo.health.warning/index). Cf. also Harvard Business Review on "How Top CEOs Cope with Constant Stress" (hbr.org/2011/04/how-top-ceos-cope-with-constan) and expert psychologists offering "Strategies for CEOs to reduce stress" (vistage.com/research-center/personal-development/20200402-ceo-stress).

[2] See, e. g., Marmot (2005), Ganster and Rosen (2013), and Kaplan and Schulhofer-Wohl (2018).

We capture variation in work-related stress exploiting two sources of variation, periods of industry-wide distress and variation in the intensity of CEO monitoring due to changing corporate-governance regulation. As for the first, prior work has shown that industry shocks and financial distress affect CEO pay and turnover (Bertrand and Mullainathan 2001; Garvey and Milbourn 2006; Jenter and Lewellen 2015). We identify industry distress shocks based on a 30% median firm stock-price decline over a two-year horizon, similar to prior work (Opler and Titman 1994; Acharya et al. 2007; Babina 2020). As for the second, we identify variation in CEO monitoring from the staggered passage of anti-takeover laws across U.S. states in the mid-1980s. The laws shielded CEOs from market discipline by making hostile takeovers more difficult, reducing CEOs' job demands and allowing them to "enjoy the quiet life" (Bertrand and Mullainathan 2003).[3] A large prior literature has used the passage of these laws as proxies for less intense monitoring and shown the effects on CEO behavior.[4] While some studies question whether the passage of anti-takeover laws in fact reduced hostile takeover activity (e. g., Cain et al. 2017), it arguably constituted a significant shift in managers' *perception* of their job environment.[5] Thus, the two sources of variation constitute separate and oppositely-signed changes in job demands. Both build on the notion of distress and protection from stress in the economic literature (and on the popular notion of stress), rather than a biomedical measurement of adrenaline or cortisol levels.[6]

To investigate the link between work-related stress and health, we assemble two measures of health outcomes: visible patterns of aging and life expectancy. Visible signs of aging are widely used by clinicians to assess patient health, and validated as biological markers predicting mortality and health outcomes at least since the Baltimore Longitudinal Study of Aging (BLSA) beginning in 1958 (Borkan and Norris 1980). Building on prior medical

[3] The prevailing view in law and economics at the time of the passage of the laws was that the "continuous threat of takeover" is an important means to counteract lagging managerial performance (Easterbrook and Fischel 1981). Scharfstein (1988) develops a formal model in which the threat of a takeover disciplines management, and then-U.S. Supreme Court Justice Byron White's opinion in *Edgar vs. MITE* emphasizes "[t]he incentive the tender offer mechanism provides incumbent management to perform well."

[4] For example, when protected by anti-takeover laws, CEOs become less tough in wage negotiations, and their rate of plant closures and plant openings decreases (Bertrand and Mullainathan 2003); they undertake value-destroying actions to reduce their firms' risk of distress (Gormley and Matsa 2016) and reduce their stock ownership (Cheng et al. 2004); their patent count and quality decreases (Atanassov 2013).

[5] Consistent with CEOs' perceptions of anti-takeover laws changing job demands, we find suggestive evidence that takeover-protected CEOs remain on the job for longer. We also show, though, that prolonged tenure cannot explain the estimated longevity effects.

[6] Stress arises from experiencing demands without sufficient resources to cope (Lazarus and Folkman 1984). Biomedically, changes in hormones and other bodily processes due to stress can cause long-term damage and accelerate aging (Brondolo et al. 2017; Franceschi et al. 2018; Kennedy et al. 2014).

studies that measure so-called "perceived" or "apparent" age from facial photographs (e. g., Christensen et al. (2004) and Christensen et al. (2009)), we construct a new dataset of photographs of CEOs' faces. We utilize recent visual machine learning (ML) techniques to determine apparent age, and estimate the influence of work-related stress. The ML techniques are a promising avenue for the assessment of work-induced strains in broader samples and, to the best of our knowledge, we are the first to introduce them into the finance and economics literature. We also estimate the direct influence on mortality, in a new data set on the lifespan of CEOs, and find consistent results of very similar magnitudes.

Our analysis has three main parts. In the first part, we document the more immediate health implications of industry crises captured by visible signs of aging in the faces of CEOs. We employ the ML algorithms from Antipov et al. (2016) that are specifically designed to estimate a person's apparent age, i. e., how old a person looks, rather than a person's biological age. The software, trained on more than 250,000 pictures, is the winner of the *2016 ChaLearn Looking At People* competition in the *apparent-age estimation* track.[7]

We collect a sample of 3,086 pictures of the *Fortune 1000* CEOs from 2006, taken at different points during their tenure (the "CEO Apparent Aging Data"). Using a difference-in-differences design, we estimate that CEOs look one year older post crisis if their industry experienced a severe decline in 2007-2008, relative to CEOs in other industries. The estimated difference between distressed and non-distressed CEOs increases over time and amounts to 1.178 years for pictures taken five years and more after the onset of the crisis. We include a detailed description of the procedure and examine issues that have been shown to impact the use of visual machine learning in other settings (Wang and Kosinski 2018; Dotsch et al. 2016; Agüera y Arcas et al. 2018). To the best of our knowledge, this represents the first application of visual machine learning to a quasi-experimental research design. Our application illustrates its potential for the study of health and aging to complement standard measures based on mortality, hospital admissions, or survey responses.

In the second and third part of the analysis, we study direct mortality effects associated with industry distress and corporate governance legislation. For these analyses, we extend Gibbons and Murphy (1992) data of CEOs in the *Forbes* Executive Compensation Surveys from 1975 to 1991. We merge it with hand-collected data on the exact dates of birth and death of more than 1,600 CEOs of large U.S. firms (the "CEO Mortality Data"), and also add information about pay, tenure, and firm characteristics. We restrict all analyses to CEOs appointed before the enactment of the anti-takeover laws we study in the third part of the

[7] The certification effect is roughly comparable to a first-tier publication in other academic fields.

analysis to address selection concerns.

In the analysis exploiting industry distress shocks, we estimate a hazard regression model. Controlling for a CEO's biological age, time trends, industry affiliation, and firm location, we show that industry distress increases CEOs' mortality hazard by 15%. These results are robust to an array of alternative specifications, including models with CEO birth-cohort fixed effects and birth-cohort-specific age controls.

The estimated mortality effect sizes are large. The effect of experiencing industry distress is equivalent to reducing a CEO's biological age by 1.2 years. We can also compare the estimated mortality effects to known health threats. For example, smoking until age 30 is associated with a reduction in longevity by roughly one year, and lifelong smoking with a reduction by ten years and more (General 2014; Jha et al. 2013).

In the analysis exploiting the staggered passage of anti-takeover laws, we estimate significantly positive effects on the life expectancy of incumbent CEOs. Using analogous hazard models, we find that one additional year under lenient governance lowers mortality rates by four to five percent, corresponding to an overall life expectancy gain of two years for the average "protected" CEO in the sample. The slightly larger mortality effect sizes, compared to the distress effects, might reflect the more permanent nature of the changes in monitoring intensity, relative to the temporary nature of distress-induced changes in job demands.

The results are robust to a wide range of robustness tests, including alternative subsampling and classifications of anti-takeover laws that account for other firm- or state-level anti-takeover provisions, exclude lobbying and opt-out firms, or cut data differently based on firms' industry affiliation or state of incorporation (cf. Cain et al. 2017; Karpoff and Wittry 2018).

We also test for a compensating differential in the form of lower pay for CEOs as a result of being permanently protected from hostile takeovers. Our analysis of pay builds on Bertrand and Mullainathan (1998) and on the predictions of the CEO market model in Edmans and Gabaix (2011). We find no evidence of any response in pay. This may indicate that not all parties fully account for the health implications of job demands, though we also note that prior literature has generally struggled to find evidence of compensating differentials outside of select settings and carefully designed experiments (e. g., Mas and Pallais 2017; Lavetti 2020).

Overall, our analysis establishes significant health consequences for CEOs arising from variation in job demands. The findings motivate further research on the interplay of job demands and CEO selection, compensation, and feedback effects on firm performance. For example, we might ask whether aspiring CEOs are (over-)confident about their health. Or, whether women are vastly underrepresented in the C-suite not only because of discrimination but also because they anticipate the health costs of assuming such positions. Another question would be which job characteristics and hierarchy levels likely come with the highest personal cost. While all comparisons in our analysis are within the CEO group, an important next step will be to explore other hierarchy levels as well as other professions or population groups.

Our paper contributes to the literatures examining CEOs and firms. A recent literature sheds light on CEOs' demanding job and time requirements (Bandiera et al. (2020); Bandiera et al. (2018); Porter and Nohria (2018)). Working these long hours requires physical stamina, and consistent with our hypothesis, previous work has documented that CEO health is an input into production. Bennedsen et al. (2020) study the negative effect of CEO hospitalizations on firm performance. Keloharju et al. (2020) find that corporate boards in Scandinavia factor CEO health into CEO appointment and retention decisions. None of these papers, however, examines the effect of CEO job demands on CEOs' health trajectories. To the best of our knowledge, we are the first to explore quasi-random variations to establish significant health costs, in terms of the mortality and visible aging. The only prior work on executives' health outcomes is Yen and Benham (1986), who compare the age-adjusted mortality rates of 125 executives in the banking industry to those in other industries. Our significantly larger sample and quasi-experimental design allows to control for industry-specific selection into job environments, and to implement a rigorous survival analysis.

Second, our paper contributes to the literature on the health effects of stress, socioeconomic status, and financial insecurity. A vast literature in psychology, medicine, and biology associates chronic stress with changes in hormone levels, brain function, cardiovascular health, DNA, and deleterious health outcomes (McEwen 1998, Epel et al. 2004, Sapolsky 2005). This has led researchers outside of economics to embrace stress, and the damage it causes, as the mechanism underlying many health disparities (Cutler et al. 2006, Pickett and Wilkinson 2015, Puterman et al. 2016, Snyder-Mackler et al. 2020). As Kaplan and Schulhofer-Wohl (2018) document, the amount of stress experienced at work has steadily grown since at least the 1950s, even as shifts in the composition of occupations have reduced job-related physical pain and tiredness for the average worker. In health and labor economics, stress has been proposed as an explanation for pro-cyclical mortality (Ruhm 2000); the efficacy of child-tax credits (Milligan and Stabile 2011); the association between job loss and higher mortality (Sullivan and Von Wachter 2009); the health benefits of the EITC (Evans and Garthwaite 2014), unemployment insurance (Kuka 2020), access to health care (Koijen and Van Nieuwerburgh 2020); and early-life health disparities (Camacho 2008; Black et al. 2016). Relatedly, guaranteed, job-protected leave has been proposed to reduce adverse effects of mothers' job stress during pregnancy on infant health (Currie and Rossin-Slater 2015). Stress is also implicated in the intergenerational persistence of poverty (Aizer et al. 2016; Persson and Rossin-Slater 2018; East et al. 2017).

Few papers examine causal effects of job demands on health. Hummels et al. (2016) document the negative impact of quasi-random trade shocks on workers' stress, injury, and illness. Evolving worker-firm interactions may have eroded protections from competition in the product market, likely exposing workers to higher levels of stress (Bertrand 2004). Outside of economics, stress arising from social hierarchies (sometimes called psychosocial stress), especially in the workplace, has been proposed as an explanation for the strong relationship between socioeconomic status and life expectancy (Marmot et al. 1991, Marmot 2005). Quasi-experimental evidence on a causal effect of workplace promotions is, however, limited and reaches mixed conclusions (Boyce and Oswald 2012, Anderson and Marmot 2012, Johnston and Lee 2013). Turning from the general or poorer populations to wealthier populations, income appears to play a small role in health disparities among the already-wealthy, though Engelberg and Parsons (2016) document a link between stock-market crashes and hospital admissions, especially for anxiety and panic disorders. Social factors, such as the prestige of winning a Nobel prize or an election, may be protective (Rablen and Oswald 2008; Cesarini et al. 2016; Borgschulte and Vogler 2019). Our paper offers complementary evidence that work-related stressors impose long-term health costs, even for successful and wealthy individuals.

In the remainder of the paper, Section II describes the data and discusses the identifying variation. Section III presents the results on apparent aging and industry-wide distress shocks. The results pertaining to life expectancy and distress shocks are in Section IV, and those pertaining to life expectancy and corporate-governance regulation in Section V. Section VI concludes.

## II. CEO Datasets and Variation in CEO Job Demands

### *A. CEO Apparent Aging Data*

To study visible signs of aging in CEOs' faces, we collect pictures of CEOs of the firms in the 2006 *Fortune 1000* list. Using a relatively recent CEO sample is necessary as both picture availability and quality have substantially improved over time. We focus on the 2006 cohort to exploit differential exposure to industry shocks during the Great Recession.

We search for five pictures from the beginning of a CEO's tenure and two additional pictures every four years after that. The main challenge is finding *dated* pictures. (Pictures from LinkedIn, for instance, do not satisfy this criterion as we generally have no information on when LinkedIn profile pictures were taken.) In addition, we aim for pictures that are taken in daily life, such as at social events or conferences, rather than posed pictures. The most useful source given these criteria is gettyimages.com, followed by Google Images. We are able to find at least two pictures from different points in time during or after their tenure for 463 CEOs, of whom 452 are male and 447 are White. Among the sixteen non-White CEOs, seven are African-American, two are Hispanic or Latinx, and seven are Asian (including Indian). The final data includes 3,086 pictures; we call this the CEO Apparent Aging Data.

Table I provides the summary statistics for this data set, both at the image level (Panel A) and the CEO level (Panel B). The median picture is from 2009. On average, we are able to find about 7 pictures of a CEO (conditional on finding at least two pictures), and the average CEO is 55.54 years old in 2006 and has been in office for eight years. 65% of the CEOs in the data experienced an industry distress shock during the financial crisis (see Section II.C for details on the definitions of our job-demand measures). In our analyses, we will control for any prior industry shocks, which are zero at the median and 0.54 on average. The majority of CEOs head firms in the manufacturing, transportation, communications, electricity and gas, and finance industries (Panel C). We will discuss the picture-level characteristics from Panel A in Section III.A.

### *B. CEO Mortality Data*

To study CEO mortality, we conduct a mortality follow-up for the universe of CEOs included in the *Forbes* Executive Compensation Surveys from 1975 to 1991, as originally collected by Gibbons and Murphy (1992).[8] These surveys are derived from corporate proxy statements and include the executives serving in the largest U.S. firms. We choose 1975 as the start

[8] We are very grateful to Kevin J. Murphy for providing the data.

year given the source of identifying variation in the third part of our analysis, i. e., the timing of anti-takeover laws (see Section II.C), and also in line with prior studies in this literature.[9] We include all firms with a PERMNO identifier in CRSP. The initial sample comprises 2,720 CEOs employed by 1,501 firms.

We manually search for (i) the exact dates of CEOs' birth, (ii) whether a CEO has died, and, if so, (iii) the date of death. All CEOs who did not pass away by the cutoff date of October 1st, 2017 are treated as censored. Our main source of birth and death information is Ancestry.com, which links historical birth and death records from the U.S. Census, the Social Security Death Index, birth certificates, and other historical sources. We validate Ancestry's information with online and newspapers searches, e. g., on birth place, elementary school, or city of residence. Identifying a person as alive turns out to be more difficult as there is little coverage of retired CEOs. We classify a CEO as alive whenever recent sources confirm the alive status, such as newspaper articles or websites that listing the CEO as a board member, sponsor, donor, or chair of an organization or event.[10]

We obtain the birth and death information for 2,361 CEOs from 1,352 firms in the post-1975 sample, implying a finding rate of 87%. We call this the CEO Mortality Data. We test and confirm that the availability of birth and death information is not correlated with our measures of variation in work-related stressors, namely, industry distress experience and incorporation in a state that passed a BC law.

We augment the CEO Mortality Data with several key variables. We identify the historical states of incorporation during CEOs' tenure, needed to measure their exposure to anti-takeover laws. Since CRSP/Compustat backfills the current state of incorporation, we access historical Comphist and Compustat Snapshot data as well as incorporation data recorded at issuances and merger events in the SDC database. In case of discrepancies, we use 10-Ks and other SEC filings, legal documents, and news articles to identify the correct information. We correct the state of incorporation in 169 cases, or 6.7% of the initial sample with state-of-incorporation information (2,514 CEOs). Out of the sample of 2,361 CEOs with birth and death information, we are able to identify the historical state of incorporation for 2,209 CEOs.

[9] Bertrand and Mullainathan (2003), Giroud and Mueller (2010), Gormley and Matsa (2016) all start their sample in the mid-1970s. Our results are robust to varying the start year (see Section V.E).

[10] We use sources dated 01/2010 or later to infer alive status since recent coverage of a retired CEO makes it very likely that news outlets would also have reported their passing had it occurred by the cutoff date (October 1st, 2017). Our results are robust to ending our sample in 2010 (Sections IV.E and V.E), and to restricting the sample period to end in 01/2010 only for CEOs classified as alive as of 10/2017 (but not for deceased CEOs).

We collect tenure information for all individuals in the CEO Mortality Data to fill gaps and correct misrecorded data in the *Forbes* Executive Compensation Surveys. We use Execucomp, online searches, and especially the *New York Times* Business People section, which frequently reports executive changes in our sample firms. When the exact month of a CEO transition is missing, we use the "mid-year convention" (Eisfeldt and Kuhnen 2013) motivated by the relatively uniform distribution of CEO starting months in Execucomp. We further restrict the sample to firms included in CRSP during the time of the CEOs' tenure (1,900 CEOs).[11]

Finally, we address selection concerns with regard to variation in job demands. For example, it would confound our analyses if less resilient managers, i. e., those more prone to health ailments, became more likely to seek the CEO position when governance regulation softens CEO monitoring. To alleviate such concerns, we focus on CEOs appointed prior to the enactment of the business combination laws as our main sample (1,605 CEOs). That said, our results are robust to being estimated on the enlarged sample of 1,900 CEOs. Similarly, in the analysis exploiting industry distress, we consider CEOs appointed before the distress shock, whether or not they left their position during the crisis.

Table II presents the summary statistics. The median CEO in this sample was born in 1925, became CEO at age 52, and has served as CEO for nine years so far. The heterogeneity in tenure is relatively large, with an interdecile range of 17 years. Non-integer values result from CEOs starting or ending their tenure not at the end of the year. Seventy-one percent of our CEOs have passed away by the censoring date (October 1st, 2017). The median CEO died at age 83, and passed away in 2006. Forty percent of CEOs witness industry distress during their tenure, but multiple distress shocks are rare. More than 80% of CEOs experienced at most one distress shock. Throughout, we will focus on a binary rather than a cumulative distress measure.[12] Similarly, about forty percent of CEOs are protected by a BC law at some point during their tenure.

### *C. Variation in Job Demands*

We exploit two sources of variation in job demands: industry-wide distress shocks and the implementation of state-level anti-takeover protection.

*Industry-Wide Distress Shocks.* Industry distress shocks induce a temporary increase

[11] Relative to the previously mentioned restriction to firms with a PERMNO in CRSP, we drop CEOs who served, for instance, before their firm went public.

[12] The indicators also helps avoid endogeneity from picking up additional industry shocks long into a CEO's tenure. Long-serving CEOs are associated with longer lifespans in the data.

in job demands. Similarly to Opler and Titman (1994), Babina (2020), and Acharya et al. (2007), we define an industry as distressed in year $t$ if the median firm's two-year stock return (forward-looking) is less than $-30\%$. As in Babina (2020), we generate the annual industries-in-distress panel (i) restricting to single-segment CRSP/Compustat firms, i. e., dropping firms with multiple reported segments in the Compustat Business Segment Database; (ii) dropping firms if the reported single segment sales differ from those in Compustat by more than 5%; (iii) restricting to firms with sales of at least \$20 million; and (iv) excluding industry-years with fewer than four firms.[13] This annual distress panel serves as the foundation for our mortality analysis (Section IV). For the apparent-aging analysis of the 2006 *Fortune 1000* CEO cohort, we specifically examine industry distress during the Great Recession (see Section III for additional detail). Following prior work, we use 3-digit SIC classes to measure industry affiliation and rely on historical SIC codes for the firms in our sample.

*Anti-Takeover Laws.* Anti-takeover statutes increase the hurdles for hostile takeovers and help protect a CEO's job. They induce a shift in the opposite direction of industry distress, and are of a more permanent nature.

Following prior literature, we focus on the second-generation anti-takeover laws, which states started passing in the mid-1980s after the first-generation laws were struck down by courts in the 1970s and early 1980s (cf. Cheng et al. 2004; Cain et al. 2017). The statutes included Business Combination (BC) laws, Control Share Acquisition, Fair Price, and Directors' Duties laws, and Poison Pills. We follow prior literature and focus on BC laws as the most potent type of statutes, but return to the other types of laws in Section V.E. BC laws significantly reduced the threat of hostile takeovers by imposing a moratorium on large shareholder conducting certain transactions with the firm, usually for a period of three to five years.

An advantage of using anti-takeover laws as identifying variation is that these laws apply based on the state of incorporation, not the state of firms' headquarters or operation. The frequent discrepancies between firms' location and state of incorporation enables us to assess the impact of the laws while controlling for shocks to the local economy.

Figure 1 visualizes the staggered introduction of BC laws across states.[14] The map illustrates the variation across both time and states as a source of identification: 33 states

[13] Sections III and IV also discuss more restrictive distress definitions, using industry returns in conjunction with sales growth, or specific recession periods.

[14] Appendix-Figure D.1 contains a similar map based on the earliest enactment of any of the five types of second-generation anti-takeover laws listed above.

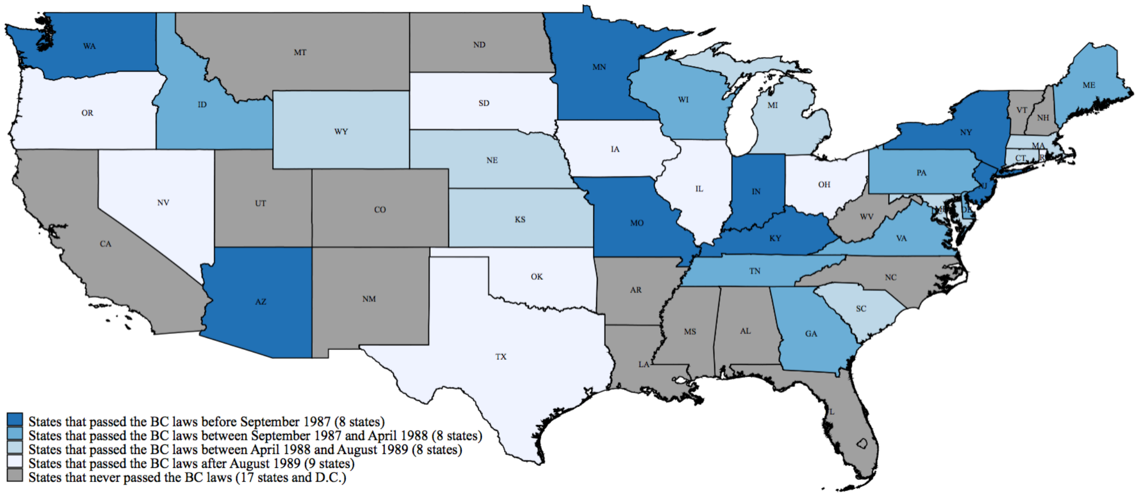


**Figure 1.**—Introduction of Business Combination laws over time. *Notes:* The map omits the states of Alaska and Hawaii, which never passed a BC law.

passed a BC law between 1985 and 1997, with most laws being passed in 1987-1989. Consistent with prior literature, the most common state of incorporation in our dataset is Delaware. Other common states include New York and Ohio.

## III. Industry-Wide Distress Shocks and Apparent Aging

In the first step of our analysis, our focus in on visible manifestations of adverse health effects in CEOs' faces. Research in medicine and biology has established numerous links between stress and signs of visible aging, such as hair loss (Choi et al. 2021), hair whitening (Zhang et al. 2020), and inflammation, which in turn accelerates skin aging (Heidt et al. 2014; Kim et al. 2013). Moreover, visible signs of aging predict mortality in longitudinal studies. For example, the apparent age of Danish twins, rated from facial photographs, predicts short-term mortality over the next two years (Christensen et al. 2004) and long-term survival of twins aged 70 and older (Christensen et al. 2009). Christensen et al. (2009) also establish strong correlations between apparent age and physical functioning (e. g., strength and endurance tests), cognitive functioning (e. g., verbal fluency and recall), and leucocyte telomere length (which is associated with ageing-related diseases and mortality).

We test whether experiencing industry distress predicts accelerated apparent-aging in the CEO Apparent Aging Data. We exploit CEOs' differential exposure to industry shocks during the Great Recession in a difference-in-differences framework.

*A. Apparent-Age Estimation*

To analyze visible CEO aging, we make use of recent advances in machine learning on estimating people's age. While most of the earlier generations of age-estimation software focused on a person's *biological*, i. e., "true" age (Antipov, Baccouche, Berrani, and Dugelay 2016), recent research aims at estimating a person's *apparent* age, i. e., how old a person looks. Progress in this area has been made possible by the development of deep learning in convolutional neural networks (CNNs) and the increased availability of large datasets of facial images with associated true and apparent ages, the latter estimated by people.

We provide a detailed discussion of CNNs and the software training steps in Appendix B.1, and give a brief summary here. A CNN is a neural network which employs the method of convolution, i. e., of transforming the data by sliding (or, convolving) over it using a slider matrix, to abstractly determine intermediate features about the data such as edges or corners.

We use a machine-learning based software (Antipov, Baccouche, Berrani, and Dugelay 2016) that has been specifically developed for apparent-age estimation. This software is the winner of the *2016 Looking At People apparent-age estimation* competition. The software is based on Oxford's Visual Geometry Group deep CNN architecture.

In a first step, the software was 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 was fine-tuned for apparent-age estimation using a newly available dataset of 5,613 facial pictures, each of which was rated by at least ten people in terms of the person's age. The addition of fine-tuning on these human estimates of age is particularly important. It led to the software's largest accuracy improvement (of more than 20%) in the apparent-age estimation of the competition data (see Table 2 in Antipov et al. 2016 and Appendix 1). Both the distribution of true ages used for training and human age estimations used for software fine-tuning covers people from all age groups, including elderly people.

The output of the neural network is a $100 \times 1$ vector of probabilities associated with all apparent ages from 0 to 99 years. The apparent-age point estimate is the expected value. The software carries out an eleven-fold cross-validation, drawing 5,113 images for each training and 500 (non-overlapping) images for each validation sample. The ultimate output is the average apparent-age estimate of the ensemble of eleven models. The approach is akin to bootstrap-aggregating ("bagging") procedures typically aimed at improving prediction accuracy (Breiman 1996).

Figure 2 graphs the distributions and correlations of biological and apparent ages for the 3,086 pictures in the CEO Apparent Aging Data. Panel (a) shows that the distributions

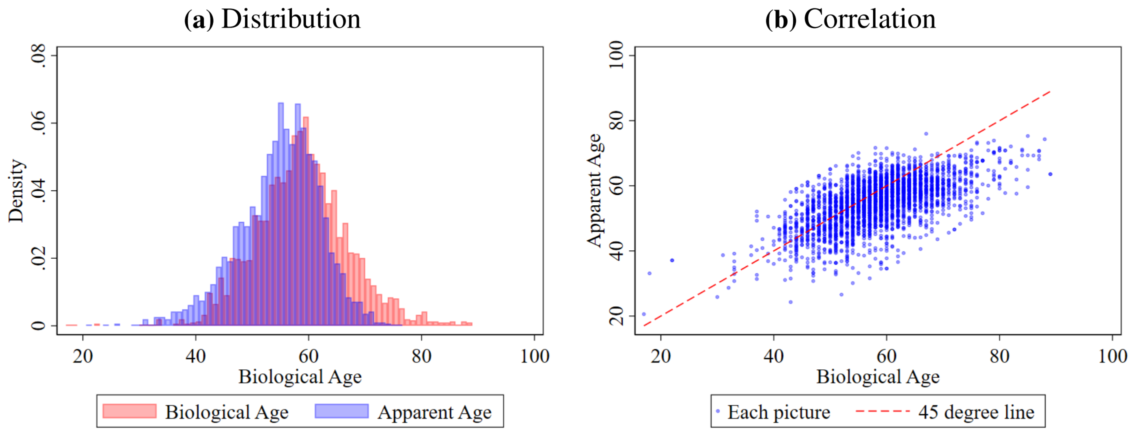


**Figure 2.**—CEO apparent and biological age. *Notes*: The figure plots apparent and biological ages for our sample of 3,086 CEO images. Panel (a) shows the CEO apparent-age distribution in blue, and the biological-age distribution in red, with the overlapping areas appearing as purple. Panel (b) shows a scatter plot of CEOs' apparent age against biological age. The dotted line represents the 45°-line. We winsorize the apparent age by first winsorizing the top and bottom 0.5% of the difference between apparent and biological age, and then adding this winsorized difference to the biological age.

of apparent and biological ages largely overlap, though the apparent-age distribution is shifted to the left. That is, consistent with a large prior literature on the ”looks”, stature, and health-related measures of CEOs and other high-earning individuals,[15] the software estimates CEOs to look younger than their biological age. (See also Table II discussed in Section II.B). We note that our analyses do not rely on comparisons between CEOs and the general population but entail solely *within-CEO* comparisons and control for biological age.

The scatter plot of CEOs' apparent age against biological age in panel (b) confirms both high correlation and the shift in apparent age relative to biological age, with a greater mass below the 45°-line. In this figure and in the regression analysis below, we winsorize the estimated apparent-age variable to ensure that the outliers in age estimation do not affect the results. To do that, we first winsorize the top and bottom 0.5% of the difference between apparent and biological age, and then add this winsorized difference to the biological age.

### *B. Illustrative Example*

To illustrate the link between industry shocks and aging, we first discuss a specific example. James Donald was the CEO of Starbucks from April 2005 until January 2008, when he was

[15] Cf. Hamermesh and Biddle (1994), Persico et al. (2004), Loh (1993), Steckel (1995), Averett and Korenman (1996). Also see Chetty et al. (2016) on better access to health care, healthier nutrition, and higher life expectancy of of individuals with high socio-economic status.

fired after Starbucks' stock had plunged by more than 40% over the preceding year. The top of Figure 3 shows two pictures of Donald: the left one from December 8, 2004, before his appointment at Starbucks, and the right one 4.42 years later, on May 11, 2009, after his dismissal. Donald was 50.76 years old in the first picture, and 55.18 years in the second. The machine-learning based aging software estimates his apparent age in the earlier picture at 53.47 years, and in the later picture as 60.45 years. Thus, for both pictures, the software determines that he looks older than his true age. Most importantly, the software estimates that he aged by 6.98 years, i. e., 2.5 years more than actual time passed.

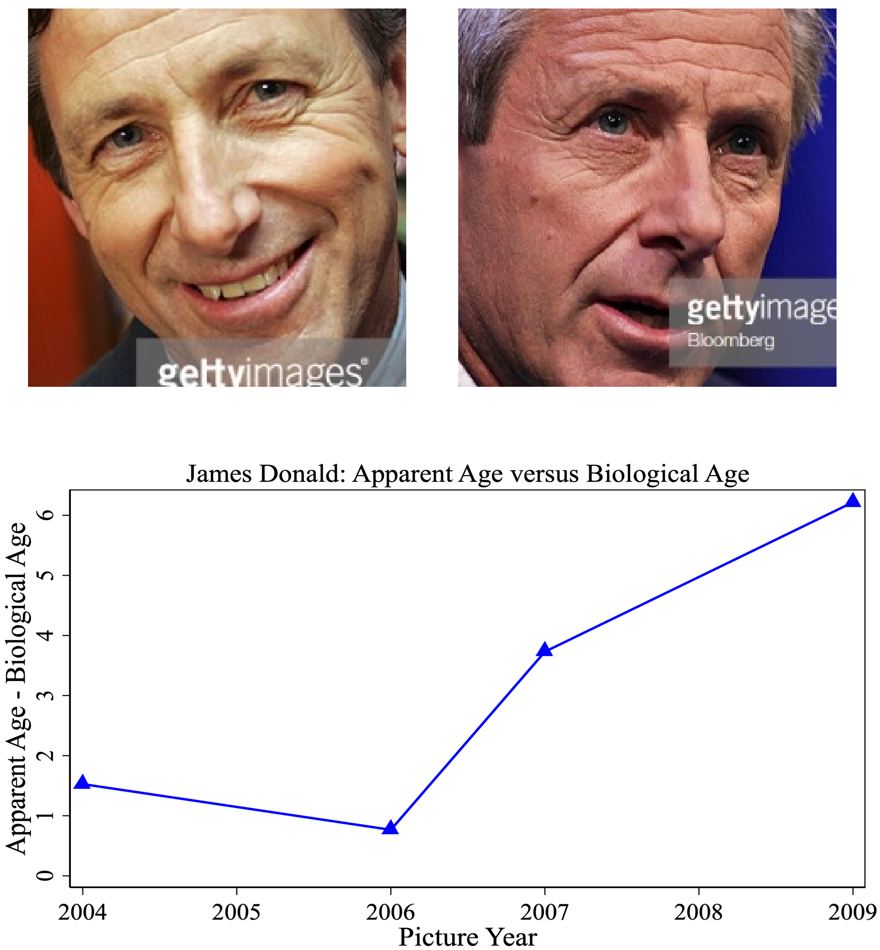


**Figure 3.**—Sample pictures (James Donald, CEO of Starbucks from 2005 to 2008). *Notes*: The left picture was taken on December 8, 2004, the right one on Monday, May 11, 2009. Biological ages based on data from Ancestry.com (birthdate March 5, 1954): 50.76 and 55.18 years, respectively. Apparent ages based on aging software: 53.47 and 60.45 years. The figure at the bottom shows how James Donald's apparent age compares to his true age over time based on 20 pictures collected for the period from 2004 to 2010.

Turning to the full set of 20 pictures of Donald in our samples, from three years before to three years after the onset of the crisis in 2007, i. e., 2004-2010, we find that the mean difference between his apparent and his biological age is 0.96 years prior to 2007 but 4.97 years from 2007 on. The bottom half of Figure 3 summarizes these estimates and visualizes the jump in Donald's apparent versus biological age in 2007 as well as the continued aging effects after the crisis. The example typifies our approach, especially in light of Donald's struggles during his final year as Starbucks' CEO.

The example also allows us to discuss concerns one may have about picture heterogeneity in the CEO Apparent Aging Data. For example, the lighting in the two pictures seems to be different, and the left picture, with Donald smiling into the camera, might be from a more staged setting than the right one. Researchers have pointed to the importance of accounting for picture context and facial positioning in other settings, such as in inferring people's character, attractiveness, or sexual orientation from facial images (Wang and Kosinski 2018; Dotsch et al. 2016; Agüera y Arcas et al. 2018). While the *image pre-processing* and *fine-tuning* steps described in Appendix B.1 already account for such image heterogeneity, we go one step further and manually assess all pictures along seven dimensions: *logo*, *side face*, *professional*, *magazine*, *natural*, *natural lighting*, and *glasses*. The indicator variable *logo* takes value 1 if there is a logo (for instance, the "gettyimages" logo) on the face in the picture. The indicator *side face* is 1 if the CEO shows a side face instead of front. For *professional*, the indicator is 1 if the CEO is in work mode, say wearing business clothes, and 0 if in casual mode, say wearing a short-sleeved shirt, T-shirt, etc. *Magazine* is 1 if the picture is from a magazine cover. *Natural* reflects whether the CEO expects the picture or not, i. e., whether it is natural posing or a photo call. *Natural lighting* is 1 if the lighting feels natural (with light from all directions), and 0 if it is unusual, e. g., black and white, stage lighting, etc. The variable *glasses* is 1 if the CEO wears glasses.

By controlling for all of these variables in our estimations, we further alleviate concerns about spurious correlations between picture characteristics and changes in apparent age.

### *C. Difference-in-Differences Results*

We formalize our analysis of job-induced apparent aging in a difference-in-differences design, analyzing differences in visible signs of aging between CEOs whose industry was in distress during the financial crisis in 2007 and 2008 versus those whose industry was not in distress. As we have detailed in Section II.C, we use three-digit SIC codes and a 30% decline in equity value criterion to identify firms that experienced an industry shock during the crisis years. This approach classifies 79 out of a total of 149 industries represented in the CEO Apparent Aging Data as distressed during at least one of the crisis years 2007 and 2008. Industries classified as distressed during these years include real estate and banking. Non-distressed industries include agriculture, food products, and utilities.

To account for potential selection bias due to CEOs departing from their job during the Great Recession, potentially introducing selection bias, we identify treated CEOs based on intended exposure. That is, we define the treatment variable, $Industry\ Distress_j$, as equal to 1 if CEO $j$'s firm operates in an industry that was distressed in 2007, 2008, or both years, regardless of whether the CEO stepped down between 2006 and 2008. For example, $Industry\ Distress_j$ is encoded as 1 for a CEO departing in 2007 and whose firm's industry was distressed in 2008.[16]

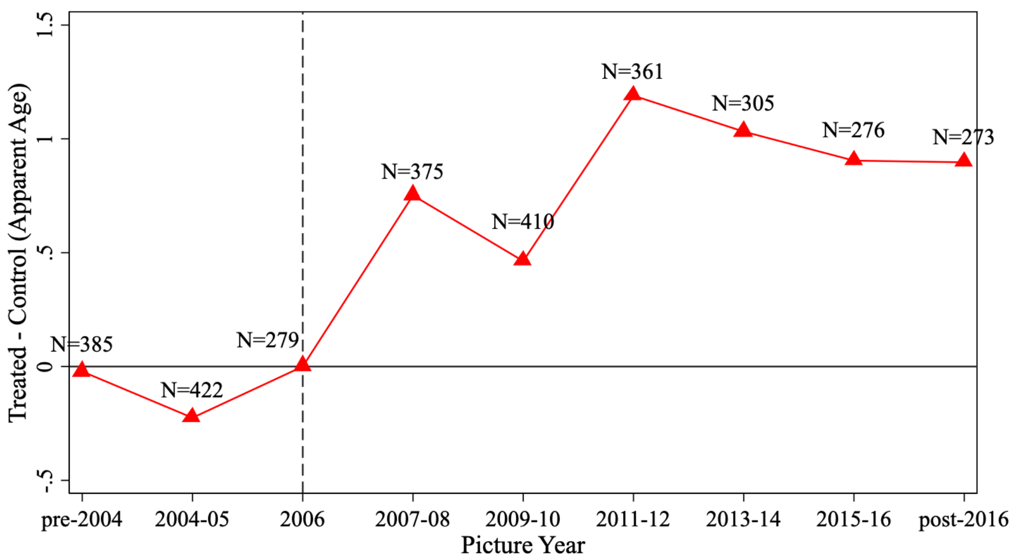


**Figure 4.**—Differences in apparent aging between CEOs with and without industry distress exposure during the Great Recession. *Notes*: This figure depicts the estimated coefficients $\beta_2$ of the interaction terms between the time-period indicators and the *Industry Distress* indicator from model (1), where *Industry Distress* is equal to 1 if the CEO's firm was exposed to industry-wide distress during 2007 or 2008. *N* denotes the number of pictures for each time period. We winsorize the estimated apparent-age variable by first winsorizing the top and bottom 0.5% of the difference between apparent and biological age, and then adding this winsorized difference to the biological age. Observations are weighted by the inverse of the number of pictures per CEO.

We start from plotting the difference in aging trends between distressed and non-distressed CEOs in Figure 4. For this graphical illustration, we bin our data into nine

[16] Regressing actual 2007-2008 industry shock exposure on intended exposure yields a coefficient of 0.92 ($F$-statistic of 331.66).

roughly equal-sized groups of pictures from the beginning of the sample period to the end, $t \in T = \{\text{pre-2004}, \text{2004-05}, ..., \text{post-2016}\}$, and estimate the following difference-in-difference model:

$$\begin{aligned} \mathit{Apparent\ Age}_{i,j,t} = \; & \beta_0 + \beta_1 \mathit{Biological\ Age}_{i,j,t} + \sum_{t \in T} \beta_{2,t} \mathit{Industry\ Distress}_j \times \mathbb{1}_t \\ & + \boldsymbol{\beta_3'} \boldsymbol{X}_{i,j,t} + \delta_t + \theta_j + \varepsilon_{i,j,t} \end{aligned} \tag{1}$$

where $i$ represents a picture, $j$ represents a CEO, and $t$ represents a time bin. $\mathbb{1}_t$ are time indicators, where the $t^{\text{th}}$ indicator is equal to 1 for pictures taken at time $t$. They are interacted with $\mathit{Industry\ Distress}_j$, so that the interaction is 1 if the firm of CEO $j$ shown in picture $i$ was distressed in 2007 or 2008. The vector of control variables $\boldsymbol{X}_{i,j,t}$ includes the number of industry shocks a CEO experienced before 2006 and CEO tenure until 2006. We also include CEO fixed effects $\theta_j$ and time fixed effects $\delta_t$. The CEO fixed effects absorb any time-invariant CEO facial characteristics such as facial shape. The time fixed effects absorb time trends, such as improving picture quality. While the aging software has been trained on a large number of faces and pictures of differing quality, these fixed effects tighten the identification (and absorb the main effects of the time-industry shock interaction in the regression). As discussed above, we additionally include extensive controls for picture setting and characteristics. We note that for any of these variables to affect the results in the first place, they would have to systematically affect the software's age estimate (rather than introducing noise) and be correlated with industry distress experience.

Figure 4 plots the estimated components of vector $\boldsymbol{\beta_2} = (\beta_{2,\text{pre-2004}}, ..., \beta_{2,t}, ..., \beta_{t,\text{post-2016}})$, capturing the apparent-age differences between the treated group and the control group at the different points in time, after controlling for the biological age and other covariates. We see that the difference in apparent age between future distressed and non-distressed CEOs is small and stable over time before the crisis, consistent with the notion that aging in both groups follows parallel pre-trends. After the onset of the Great Recession, however, the apparent-age difference increases markedly, first to about half a year, and then to a full year. It stays and stabilizes at a high level of about one year of apparent-age difference after around five years post-crisis. In other words, exposure to industry distress significantly accelerates aging over the next few years, with the apparent-age difference stabilizing at one year.

The large estimated difference in aging post-crisis is robust to estimating the standard difference-in-differences regression model:

$$
\begin{aligned}
\textit{Apparent Age}_{i,j,t} = &\beta_0 + \beta_1 \textit{Biological Age}_{i,j,t} + \beta_2 \textit{Industry Distress}_j \times \mathbb{1}_{\{t>2006\}} \\
&+ \boldsymbol{\beta}_3' \boldsymbol{X}_{i,j,t} + \delta_t + \theta_j + \varepsilon_{i,j,t}
\end{aligned} \tag{2}
$$

where $i$ represents a picture, $j$ represents a CEO, and $t$ represents a calendar year. We continue to code *Industry Distress* as an indicator of intended industry-distress exposure during the Great Recession to account for possible selection bias. The vector of control variables, $\boldsymbol{X}_{i,j,t}$, is the same as in estimating equation 1, and $\delta_t$ and $\theta_j$ capture the year and CEO fixed effects, respectively. Observations are weighted by the inverse of the number of pictures per CEO. Standard errors are clustered at the individual level. The key coefficient of interest is $\beta_2$, indicating the difference in how old CEOs look in post-crisis years depending on whether they personally experienced industry shocks during 2007 to 2008.

Table III presents the regression results. In column (1), the coefficient on the interaction term between *Industry Distress* and the post-2006 indicator, $\mathbb{1}_{\{t>2006\}}$, is 0.948, indicating that CEOs look around one year older if they have experienced (or are experiencing) an industry distress shock between 2007 and 2008. In column (2), we add the extensive set of picture controls described above ("logo," "side face," "professional," "magazine," "natural," "natural lighting," and "glasses"). This barely changes the coefficient on the post-treatment interaction term (now 0.978, significant at 5%).

In columns (3) and (4), we split the post-period into two sub-periods, 2007–2011 and from 2012 on. Our estimates indicate that longer-horizon effects are driving our results: we estimate a distress-induced apparent-aging effect of around 0.8 years over the earlier five-year horizon that is insignificant but increases to about 1.2 years over the later horizon and becomes significant. Again, the estimated effects are very similar whether or not we include the additional picture controls.

The fact that CEO aging effects appear to be permanent also ameliorates potential concerns about the media or firms engaging in "picture management" that is correlated with distress exposure and could thus affect the apparent-age estimates. For example, the media might select pictures that show the CEO in distress (and hence possibly looking older) during the crisis, while firms might engage in "CEO appearance management" and try to put forward particularly positive pictures. The significant (and larger) coefficient over the longer horizon helps dispel such concerns. By 2012, it is unlikely that variation in distress exposure during the 2007/08 financial crisis still affects picture selection, especially since more than 50% of CEOs have departed from their position.

In sum, the apparent-aging analysis provides robust evidence that increased job demands in the form of industry distress accelerate visual aging. Our results on CEO mortality in Sections IV and V suggest that the appearance of aging may presage a shorter lifespan for CEOs whose industries experienced downturns in the Great Recession.

### *D. Robustness Tests*

We perform a series of additional tests, with all tables relegated to Appendix B.2.

First, we verify that all results are similar when we estimate the difference-in-differences model on the non-winsorized sample (Appendix-Table B.1).

Second, we consider using a more restrictive distress definition that requires negative industry sales growth, as in the robustness tests in Acharya et al. (2007), and in Opler and Titman (1994) and Babina (2020). Around 29% of the CEOs are still classified as experiencing distress under this more stringent definition, and the estimation continues to yield economically and statistically significant aging effects (Appendix-Table B.2). If anything, the estimated effect of industry distress on apparent aging becomes slightly larger, with the differential-aging coefficient increasing from 0.948 to 1.173 in column (1) and from 0.978 to 1.064 in column (2). Moreoever, when splitting the post-crisis period into a five-year and a longer horizon in columns (3) and (4), we now estimate significant effects both for both subperiods.

Lastly, we verify that our results are not affected by differential finding rates of pictures depending on whether CEOs experienced distress during the crisis. For example, if experiencing industry distress shocks makes CEOs more likely to step down earlier, it may be more difficult to find recent, post-tenure pictures. Appendix-Figure B.3 depicts the average number of pictures per CEO we find in each year, split by whether a CEO experienced industry distress shocks in 2007-2008. In general, the finding rates closely follow each other over time, though there is a small divergence after 2015. Therefore, we repeat our analysis restricting our sample to the years up to 2015, as shown in Appendix-Table B.3. The size and significance of the coefficients on the interaction terms remain similar across all columns.

## IV. Industry-Wide Distress Shocks and Life Expectancy

In the remaining two parts of the analysis, we move from apparent aging to mortality as the outcome variable. This section examines the link between mortality and industry distress, and Section V the link between mortality and governance regulation. For both sets of analyses, we use the longer and earlier CEO Mortality Data described in Section II.B, which allows us to analyze mortality outcomes and accommodates the timing of variation in anti-takeover laws.

### A. Empirical Strategy

We employ the Cox (1972) proportional hazards model to estimate the effect of variation in job demands on longevity. CEOs enter the analysis ("become at risk") in the year they are appointed, and they exit at death or the censoring date. We capture the effect of variation in CEOs' exposure to industry distress by estimating

$$\ln\,\lambda(t|Industry\ Distress_{i,t}, X_{i,t}) = \ln\,\lambda_0(t) + \beta Industry\ Distress_{i,t} + \boldsymbol{\delta'} \boldsymbol{X}_{i,t}, \qquad (3)$$

where $\lambda$ is the hazard rate and $\lambda_0$ is the baseline hazard rate. $Industry\ Distress_{i,t}$ is an indicator variable equal to 1 if CEO $i$ has experienced distress, i. e., an industry-wide 30%-decline in equity value over a two-year horizon, by year $t$. Note that, when a CEO steps down, the value of the distress indicator remains constant at its value at departure. $\boldsymbol{X}_{i,t}$ is a vector of control variables. In our main specifications, we consecutively add as controls biological age, time trends (linear or fixed effects), location fixed effects and industry fixed effects. The location fixed effects are based on firms' state of headquarters and absorb state-level characteristics such as general business conditions, pollution, and eating habits to the extent that these are time-invariant. The industry fixed effects represent the Fama and French (1997) 49 industries. We later present robustness specifications with birth-cohort fixed effects and birth-cohort-specific age controls. We cluster standard errors at the three-digit SIC code level, at which industry shocks are defined (Abadie, Athey, Imbens, and Wooldridge 2017).

### B. Graphical Evidence

Before presenting the main results, we provide graphical evidence on the mortality effects of variation in CEOs' industry distress exposure. Figure 5 plots Kaplan-Meier survival graphs, split by whether CEOs experienced industry distress during their tenure. The non-parametric Kaplan-Meier estimator discretizes time into intervals $t_1, ..., t_J$, and 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 $t_j$. We plot unadjusted survival in Figure 5, where the vertical axis shows the survival rate, and the horizontal axis the time elapsed (in years) since becoming CEO. In lieu of age controls (which are included in the regression analysis), we limit the sample to CEOs appointed up to ten years prior to the retirement age of 65 and stepping down up to ten years after the retirement age.[17]

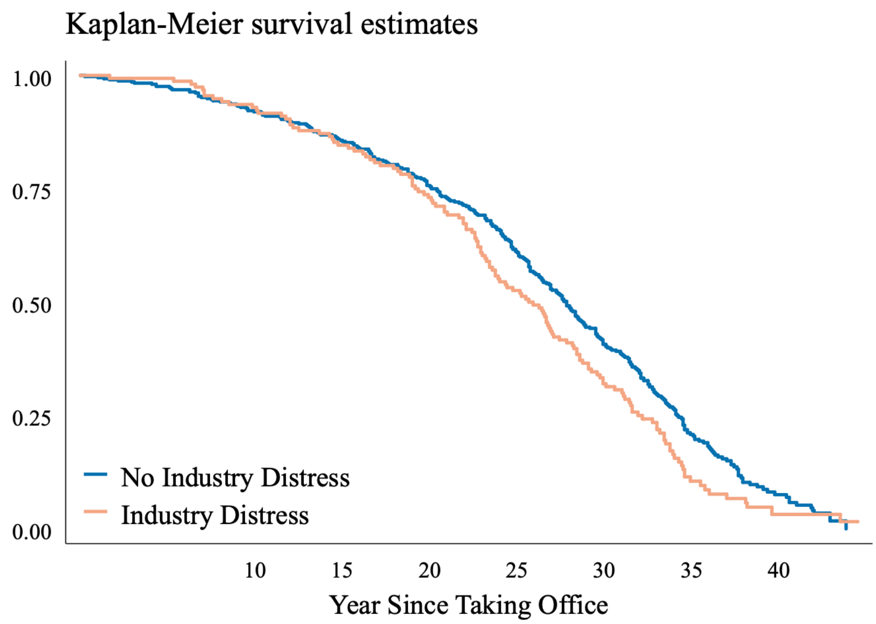


**Figure 5.**—Kaplan-Meier survival estimates. *Notes*: This figure shows Kaplan-Meier survival plots of the relation between industry distress experience and longevity. The vertical axis shows the fraction of CEOs who are still alive. The horizontal axis reflects time elapsed (in years) since a person became CEO. The figure compares the survival of CEOs who never experienced industry-wide distress during their tenure (dark blue) to those who experienced distress (light orange). The figure is limited to CEOs appointed up to ten years prior to the retirement age of 65 and stepping down up to ten years after the retirement age.

Figure 5 shows that CEOs who were exposed to industry distress during their tenure have significantly worse long-run survival patterns than never-distressed CEOs. The survival line for distressed CEOs is visibly left-shifted at longer horizons. For example, 25 years after a CEO's appointment, about 50 percent of distressed CEOs have passed away, whereas this takes closer to 30 years for the non-distressed CEO group.

The survival plot offers first, suggestive evidence that experiencing industry distress as CEO is associated with adverse consequences in terms of life expectancy. Our hazard analysis below formalizes the observed patterns.

[17] Appendix-Figure C.1 plots the retirement hazard for CEOs in our main sample, confirming a large spike in retirements at age 65, consistent with the evidence in Jenter and Lewellen (2015). While CEOs may continue to work after stepping down, few (34 in total) become CEO at another firm in our sample.

### *C. Main Results*

Table IV shows the hazard model results on the relationship between industry distress and CEOs' mortality rates, based on estimating equation (3). All columns show the estimated hazard coefficients $(\beta, \delta)$, such that a coefficient greater than zero indicates that the risk of failure (death) is positively associated with a given variable.

The specification in column (1) estimates the model with solely the industry-distress indicator and a linear control for age, without including any fixed effects. In column (2), we add a linear control for time trends as well as location fixed effects based on firms' state of headquarters, which absorb state-level characteristics such as general business conditions, pollution, and eating habits to the extent that these are time-invariant. In both specifications, the estimated hazard coefficient on the distress indicator is statistically and economically significant, amounting to 0.123 and 0.145, respectively. The results do not change when we make comparisons within Fama and French (1997) 49 industries in column (3) and replace the linear time control with year fixed effects in column (4). The coefficient estimates on distress exposure are almost unchanged, 0.146 and 0.137, both significant at 5%. Averaging the estimates across columns, distress experience increases CEOs' log mortality hazard by 0.138, i.e., the mortality hazard by approximately 13.8%.[18]

Turning to the control variables, the effect of age is significantly positive across specifications, reflecting that older people have a higher risk of dying. Note that the linear age term is motivated by the Gompertz (1825) "law of mortality," i. e., the empirical regularity that the risk of dying follows a geometric increase after middle age (Olshansky and Carnes 1997). In untabulated results, we obtain virtually identical estimates when including higher-order age terms. The linear time control in columns (2) and (3) is close to zero and insignificant, suggesting no general time trends in the survival of CEOs over the sample period. Nevertheless, we further address concerns around improvements in life expectancy over time in Section IV.D, where we test and confirm the robustness of our results to including birth-year fixed effects and age-birth-cohort interactions.

*Economic Significance.* One way to evaluate the magnitude of the estimated distress effect on longevity is relative to other predictors of CEO life expectancy in our hazard model, in particular relative to (biological) age: How much "older" does a CEO become due to distress exposure in terms of mortality hazard? The estimated effect of age on death hazard in our sample (averaging across all columns) is 0.118, which implies an 11.8% increase in

[18] Without approximating, the average coefficient of 0.138 implies a hazard ratio of $\exp(0.138) = 1.148$, i. e., a 14.8% increase in the mortality hazard.

the mortality hazard per year of age. This means that the effect of industry distress exposure corresponds to the effect of a 1.2-year shift in age ($0.138/0.118 \approx 1.2$).

This "in-sample" comparison has the advantage that it is directly based on data from the sample CEOs, who have a higher baseline life expectancy. Alternatively, we can compare the estimated mortality hazard with mortality statistics of the general U.S. population. For example, at age 57 (the median CEO age in our sample), the one-year mortality rate of a male American born in 1925 (the median birth year in our sample) is 1.366% (Human Mortality Database 2019). Industry distress experience as CEO pushes this rate up to 1.5709%, which is roughly the mortality rate of a male born in 1925 at age 58.5, i. e., when one and a half years older. Also with respect to the general population, Sullivan and Von Wachter (2009) estimate that job displacement at age 40 increases the mortality hazard by 10–15% and reduces life expectancy by one to one and a half years.

Another benchmark for comparison are other known health threats. For example, smoking until age 30 is associated with a reduction in longevity by roughly one year (Jha et al. 2013). The reduction in life expectancy from industry distress exposure is thus slightly larger than the reduction from smoking in the first three decades of one's life.

In sum, unexpected changes in the work environment and job demands of CEOs arising from industry-wide distress have substantial health consequences not only in terms of short- to medium-term visible aging but also in terms of long-term mortality.

### *D. CEO Birth Cohort Robustness*

One concern with the results from Table IV may be that the year and age controls are insufficient to control for trends in longevity (even though we find the linear time control and higher-order age controls to be insignificant). To further examine potential confounds around heterogeneity in life expectancy over time, and this heterogeneity being correlated with industry distress exposure, Table V estimates alternative specifications accounting for CEO birth cohorts. Specifically, Columns (1) and (2) directly include CEO birth-year fixed effects in lieu of year controls or fixed effects. The industry distress coefficients remain similar to those in Table IV and, if anything, become statistically and economically slightly more significant. For example, the estimation in column (2) implies a distress-induced mortality effect equivalent to a 1.5-year increase in age. Columns (3) and (4) revert to year fixed effects but allow the effect of age on mortality to be cohort-specific: we sort CEOs into quintiles based on birth year, and allow for separate age estimates. While there are small differences in age effects across CEO cohorts, the industry distress coefficients are again barely affected and remain statistically and economically significant.

### *E. Additional Robustness Tests*

We perform several additional robustness tests, with all tables relegated to Appendix C. First, we re-estimate equation (3) including as additional control variables CEO pay (from Gibbons and Murphy 1992) and firm size (assets and employees from CRSP and Compustat). The estimated hazard coefficients on distress exposure become slightly larger and remain significant (Appendix-Table C.1). In terms of economic magnitude, the distress effect is now roughly equivalent to being 1.3 years older.

Second, we re-estimate the model on an extended sample that includes the 295 CEOs we had dropped from the analysis in the final step of Section II.B (Appendix-Table C.2, with Panel A including the baseline controls and Panel B the additional controls from above). The estimated coefficients remain similar throughout and the estimated distress effect, averaging across columns, is now equivalent to being 0.9 years older.

We also estimate the effect of industry distress when varying the censoring year for CEOs' alive status. This check can help alleviate concerns that we may have failed to identify some deaths, hence granting "extra years" of life to these CEOs (cf. footnote 10). The coefficients remain stable as we gradually move the censoring date from Oct. 1, 2017 to Dec. 31, 2010 (Appendix-Figure C.2).

Finally, we have also explored specific recession periods similar to the analysis in Section III, such as the 1987 stock-market downturn or the 1981-82 recession. However, fewer than five percent of the CEOs in our sample experienced either of these shocks so that we lack statistical power when applying the same methodology. While the corresponding estimates indicate that CEOs who experienced these specific shocks tend to have a higher mortality hazard, they are not statistically significant. We have also again considered the more restrictive return- and sales-based distress definition from Section III.D. In our CEO mortality sample preceding the Great Recession, this definition classifies fewer than five percent of CEOs as distressed and substantially increases standard errors. Nonetheless, we estimate similar effect sizes as above, with the average point estimate across specifications implying a distress effect that corresponds to a seven-month increase in age.

## V. Corporate Monitoring and Life Expectancy

In the final analysis, we exploit the staggered passage of anti-takeover laws across U.S. states in the mid-1980s as the source of identifying variation to study CEO mortality effects.

### A. *Empirical Strategy*

Our main analysis continues to use the Cox (1972) proportional hazards model. First, we estimate a modified version of equation (3):

$$\ln\,\lambda(t|BC_{i,t},\boldsymbol{X}_{i,t}) = \ln\,\lambda_0(t)\; + \beta I(BC_{i,t}) + \boldsymbol{\delta}'\boldsymbol{X}_{i,t}, \tag{4}$$

where $I(BC_{i,t})$ is an indicator variable equal to 1 if CEO $i$ has been exposed to a BC law by year $t$. As in Section IV.A, $\boldsymbol{X}_{i,t}$ includes biological age, time trends (or fixed effects), and state-of-headquarters and industry fixed effects. We also verify again that all of our results are robust to specifications that account for CEO birth cohorts.

Second, we test for differential effects depending on differential BC law exposure intensity. Given that the laws led to a permanent corporate governance regime change, rather than a temporary exposure (as in the case of industry distress), we replace the indicator $I(BC_{i,t})$ with a measure $BC_{i,t}$ that counts the exposure length in years until year $t$:[19]

$$\ln\,\lambda(t|BC_{i,t},\boldsymbol{X}_{i,t}) = \ln\,\lambda_0(t)\; + \beta BC_{i,t} + \boldsymbol{\delta}'\boldsymbol{X}_{i,t}. \tag{5}$$

As with the distress indicator in the previous section, the values of the BC law indicator and BC law exposure length variables remain constant once a CEO has stepped down. We now cluster standard errors at the state-of-incorporation level, given that the BC laws applied based on firms' state of incorporation (Abadie, Athey, Imbens, and Wooldridge 2017).

### B. *Graphical Evidence*

We again start by plotting Kaplan-Meier survival graphs, separating between BC-insulated and non-insulated CEOs. As in Figure 5, we focus on CEOs who started and ended their tenure within a ten-year period around the retirement age of 65.

Panel (a) of Figure 6 plots the survival lines of this set of CEOs comparing those who became CEO in the 1970s and were never shielded by a BC law, those who became CEO in the 1980s and were never shielded by a BC law, and those who became CEO in the 1980s and were eventually insulated by BC law protection during their tenure.[20]

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[19] We calculate this measure up to daily precision levels. For example, Delaware's BC law was adopted on 2/2/1988, and a CEO's exposure in Delaware in 1988 is calculated as $BC_{i,1988} = \frac{365-\text{doy}(2/2/1988)}{365} = 0.92$. Relative to the binary measure, the cumulative exposure measure is more prone to endogeneity concerns for long-serving CEOs. We address this concern in Section V.E, where we examine initial vs. incremental exposure as well as *predicted* exposure length.

[20] For the 1970s cohorts, maximum 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).

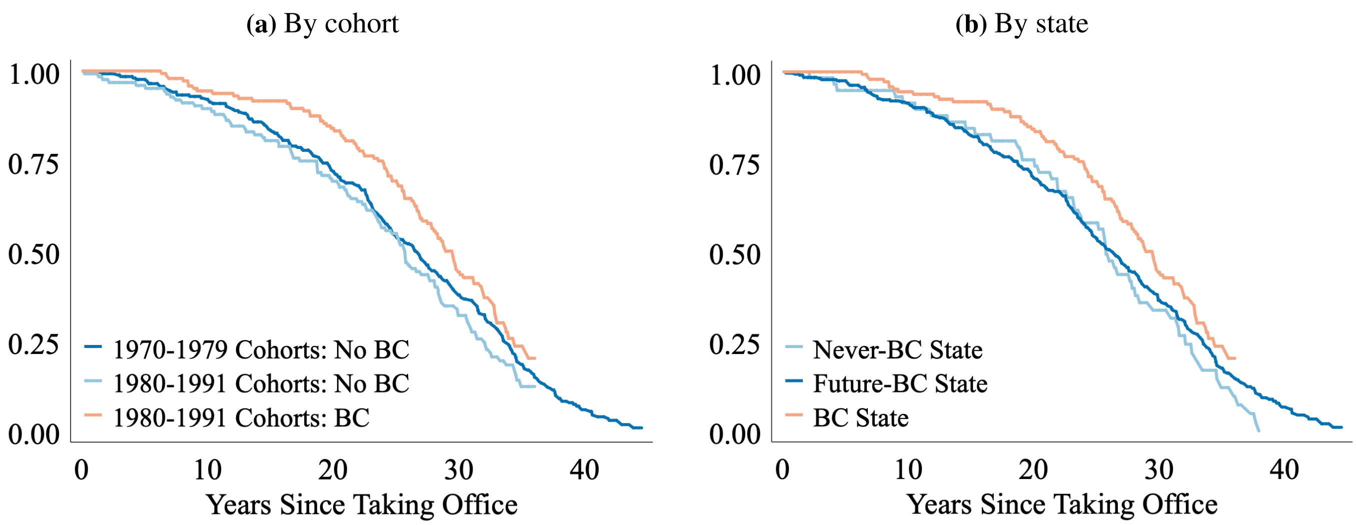


**Figure 6.**—Kaplan-Meier survival estimates. *Notes*: This figure shows Kaplan-Meier survival plots for the relation between anti-takeover law protection and longevity. The vertical axis shows the fraction of CEOs who are still alive. The horizontal axis reflects time elapsed (in years) since a person became CEO. Panel (a) compares the survival of CEOs starting in the 1970s who never served under a BC law (light blue) to 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 exposed to a BC law (light orange). Panel (b) splits the CEOs from Panel (a) based on whether their state never passed a BC law (light blue), passed a BC law after the CEO stepped down (dark blue), or passed a BC while in office. As in Figure 5, both panels focus on CEOs appointed up to ten years prior to the retirement age of 65 and stepping down up to ten years after the retirement age.

Two results emerge. First, the survival patterns of the 1970s and 1980s cohorts without BC exposure are remarkably similar, allaying concerns that our BC-law-mortality results pick up *general* changes in survival patterns between the 1970s and 1980s. Second, the survival line for the 1980s cohorts with BC exposure is visibly right-shifted compared to the No-BC-cohorts. For example, 20 years after a CEO's appointment, about 25 percent of CEOs in the 1980s cohorts without BC exposure have died, whereas it takes closer to 25–30 years for CEOs in the 1980s cohorts with BC exposure.

One possibility is that the patterns in Panel (a) might pick up systematic differences between BC and non-BC states—despite the fact that these laws apply based on state of incorporation as opposed to firms' location. To examine this graphically, Panel (b) reshuffles CEOs in Panel (a)'s No-BC-cohorts, grouping them instead by whether their state eventually enacted a BC law after the CEO stepped down (dark blue) or not (light blue). The survival lines for these groups are virtually identical, and only CEOs in BC states *with* BC exposure (orange) show a more beneficial survival curve. Thus, there is no evidence of BC states being inherently different prior to BC enactment. We also note that all our estimations will include location fixed effects and are robust to using state of incorporation fixed effects.

The survival plots suggest significant adverse consequences in terms of life expectancy associated with serving under more stringent corporate governance regimes. We next turn to the hazard model based results that control for other determinants of mortality.

### *C. Main Results*

Table VI shows the hazard model results relating BC law exposure to CEO mortality rates based on the estimating equations (4) and (5). In Columns (1) through (4), we summarize the total effect of the BC laws with the indicator $I(BC_{i,t})$ for CEO $i$ having been exposed to a BC law by time $t$. In Columns (5) through (8), we estimate the linear (in hazards) effect in years of exposure to more lenient corporate governance, $BC_{i,t}$. The control variables and fixed effects in the left four and right four columns are the same as in the corresponding columns in the distress-mortality analysis of Table IV.

Across all columns, we estimate a statistically and economically strong effect of BC law protection on mortality. For the BC indicator, the hazard coefficients range from $-0.225$ to $-0.269$ as we move from just a linear control for age in column (1) to specifications with linear or fixed-effect time controls, state-of-headquarters fixed effects, and industry fixed effects in columns (2) to (4). For the cumulative BC exposure measure in columns (5) to (8), the coefficients range from $-0.041$ to $-0.046$, indicating that a one-year increase in exposure to more lenient governance is estimated to reduce a CEO's mortality risk by 4.3% averaging across columns. For a CEO with a typical BC law exposure, both BC exposure measures imply very similar effects on longevity.[21]

The estimated effects of control variables on mortality rates mirror those in Table IV, with the linear time control being insignificant and age strongly predicting mortality rates. As before, adding higher-order age terms yields nearly identical results (untabulated).

*Economic Significance.* The estimates imply meaningful effect sizes. First, we use again the "in-sample" comparison to other CEOs. Based on the hazard coefficient estimates from the most conservative specification in column (4), $-0.252$ for BC exposure and 0.117 for age, the effect of BC law protection on mortality is equivalent to being about two years younger. This effect size is of similar order of magnitude, but slightly larger than the estimated effect size of exposure to industry distress, which corresponded to about 1.2 years

[21] The cumulative measure estimates a 19% shift in mortality hazard associated with the median BC exposure of 4.4 years ($4.3\% \times 4.4 = 18.92\%$), close to the 22-27% shift estimated with the BC indicator.

in CEO age. The somewhat larger mortality effect, relative to industry-shock experiences, might reflect the more permanent nature of the BC law experiences.

Second, we use the general U.S. population life tables approach. Here, the median exposure to lenient governance of 4.4 years pushes the 1.366% mortality rate of males born in 1925 rate down to 1.119%, which is roughly the mortality rate of males born in 1925 at age 54, i. e., when three years younger.

Overall, we find variation in job demands based on anti-takeover laws to be associated with substantial mortality effect sizes.

### *D. CEO Birth Cohort Robustness*

Mirroring Section IV.D, we estimate alternative specifications that account for different CEO birth cohorts. This is of particular importance in the context of the BC law analysis, in light of the fact that BC laws disproportionately protected more recent CEOs who were born later and are thus younger on average.

We re-estimate estimating equations (4) and (5) including CEO birth-year fixed effects (columns (1) and (2)) and age-by-cohort controls (columns (3) and (4)). As shown in Table VII, the hazard coefficients on the indicator and cumulative BC measures are barely affected when adding these alternative fixed effects and controls, imply very similar effect sizes, and continue to be significant at 5% or 1%. The robustness of the results alleviate potential concerns around differences between BC-protected and non-protected CEOs resulting from systematic age or cohort differences.

### *E. Additional Robustness Tests*

Our results are robust to a series of additional tests, some analogous to the robustness checks of the distress-longevity relation in Section IV.E, and some specific to the use of anti-takeover laws as identifying variation. For brevity, we only provide a brief overview of the tests here and present a detailed discussion of the tests, where applicable, in Appendix D.

*Other Specifications and Sample Choices.* Mirroring the robustness tests in Section IV.E, we test and confirm that our results are robust to including CEO pay, firm assets, and number of employees as additional control variables (Panel A of Appendix-Table D.1), and robust to different censoring date choices (Appendix-Figure D.2). While it is standard in the BC law literature to assign location fixed effects based on firms' headquarters (cf. Gormley and Matsa (2016)), the results are also robust to using state-of-incorporation fixed effects

(Panel B of Appendix-Table D.1). The stability of the estimates suggests that firm locations do not affect the relationship between governance and longevity. Alternatively, the results are robust to keeping state of headquarters fixed effects but dropping CEOs who stepped down significantly before the passage the BC laws (Appendix-Figure D.3), which further ameliorates potential concerns specific to the BC law results around differences between BC-protected and non-protected CEOs.

*Other Anti-Takeover Laws.* The results are also robust to using the first-time enactment of any of the five second-generation anti-takeover laws as identifying variation (Appendix-Table D.2). This test highlights that our results should be interpreted more broadly, applying to different corporate governance mechanisms rather than narrowly to BC laws.

*Karpoff-Wittry and Related Tests.* All results are robust to extensive robustness checks proposed in Karpoff and Wittry (2018) to account for firms lobbying for the passage of BC laws or opting-out, as well as confounding effects of firm-level defenses and first-generation anti-takeover laws (Appendix-Tables D.3 and D.4). Additionally, the results are robust to data cuts based on state of incorporation and industry affiliation (Appendix-Table D.5).

*Nonlinear Exposure Effects and Predicted Length of Exposure.* The final two robustness checks address concerns that the cumulative BC specification in equation (5) picks up CEOs' endogenous selection into a long tenure. We first address this by examining separately the mortality effects of initial years and incremental years of BC law exposure. Columns (1) to (4) in Appendix-Table D.6 re-estimate the last four columns of Table VI, splitting $BC_{i,t}$ into below- and above-median exposure, $BC_{i,t}^{(\text{min-}p50)}$ and $BC_{i,t}^{(p51\text{-max})}$, with the above-median exposure variable picking up incremental exposure in addition to initial exposure.[22] Across columns, the hazard coefficient on below-median BC exposure is strongly significant. By contrast, the coefficient on above-median BC exposure is close to one and insignificant. These results imply that the estimated survival gains are driven by the initial years of reduced monitoring, rather than by the tails of long-tenured CEOs.

To further address any remaining endogeneity concerns related to BC law exposure intensity, we estimate a hazard model using a CEO's predicted, rather than true, length of BC exposure. The prediction model only uses information from prior to the BC law passage and, in a nutshell, predicts CEOs' remaining tenure over time (from which predicted BC

[22] For example, for a CEO with a current BC exposure of four years, $BC_{i,t}^{(\text{min-}p50)}$ would take the value 4, and $BC_{i,t}^{(p51\text{-max})}$ the value 0. In the following year ($t+1$), $BC_{i,t+1}^{(\text{min-}p50)}$ 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.

exposure is derived; see the appendix for full details). The results in columns (5) to (8) of Appendix-Table D.6 corroborate our baseline findings. Predicted BC exposure is estimated to significantly affect CEOs' mortality rates. The estimated hazard coefficients range from $-0.049$ to $-0.059$ and are very similar to those in Table VI. While the standard errors, now bootstrapped because we are using a generated regressor, are slightly larger compared to Table VI, the coefficient of interest remains significant in all columns, either at 1% or 5%. A regression of true BC exposure on predicted exposure yields a coefficient of 1.21, which indicates that the prediction well approximates the true exposure. The estimated effects remain sizable if we divide them by 1.21. For instance, using the coefficient in column (8) of Appendix-Table D.6 yields $-0.049/1.21 = 0.040$.[23]

### *F. Business Combination Laws and CEO Pay*

The permanent corporate governance regime changes induced by BC laws also lend themselves to studying the extent to which managers account for the health implications of their jobs, for example in negotiating pay.

As laid out in detail in Appendix D, we conduct a simple calibration exercise that builds on the literature on the value of a statistical life (Viscusi and Aldy 2003). We calculate that, if CEO pay reflects working conditions, then a reduction in mortality risk of 4.1% per year of BC exposure (as estimated in column (5) of Table VI) would imply a CEO pay change between $-2\%$ and $-9\%$.[24]

By contrast, when we turn from the theoretical calibration to the empirical relationship between BC law protection and pay, we estimate a positive, albeit mostly insignificant effect of BC law passage on pay (Appendix-Table D.7). The estimates indicate a pay increase of around 4.1–8.7%.[25] The apparent lack of a compensating differential casts doubt on whether all parties fully account for the health implications of different governance regimes.

## VI. Conclusion

In this paper, we assess the health consequences of being exposed to increased job demands and a more stressful work environment while in a high-profile CEO position. We analyze the

[23] In Appendix D, we also analyze how tenure responds to anti-takeover laws. We find suggestive evidence of moderate increases in tenure under BC laws, consistent with the laws affecting managers' perceptions of job demands.

[24] We thank Xavier Gabaix for suggesting this calibration exercise.

[25] In comparing the results to the earlier work (Bertrand and Mullainathan 1998), who estimate a (more significant) 5.4 percent pay increase, it is important to note that our analysis is conducted on our CEO Mortality Data, a CEO-level sample, and restricts the sample to incumbent, pre-BC CEOs.

consequences for CEOs' aging and mortality using two sources of variation in job demands, industry-wide distress shocks and the staggered introduction of anti-takeover laws.

We first show that industry distress is reflected in short- to medium-term signs of adverse health consequences, namely faster visible aging. To the best of our knowledge, we are the first to collect and utilize panel data of facial images and apply machine-learning based apparent-age estimation software in social-science research. Implementing a difference-in-differences design that exploits variation in industry distress during the Great Recession, we estimate that CEOs who experienced industry distress during the 2007-2008 financial crisis look roughly one year older than those whose industry did not suffer the same level of distress. The effect of distress on aging becomes slightly larger over time, increasing to 1.178 years if we analyze pictures from 2012 and afterwards.

We then document, using an earlier CEO sample, more long-term adverse health outcomes associated with strenuous job demands. CEOs who experienced periods of industry-wide distress during their tenure die significantly earlier. We estimate a mortality effect corresponding to that of a 1.2-year increase in biological age. In line with these results, we observe significant improvements in life expectancy for CEOs who became shielded by an anti-takeover law during their tenure.

In sum, our results indicate that financial distress and stricter corporate governance regimes—the latter of which are generally viewed as desirable and welfare-improving—impose significant personal health costs to CEOs. While we lack direct physical or medical measures of heightened stress, the evidence implies that economic downturns and stricter governance constitute a substantial personal cost for CEOs in terms of their health and life expectancy. As such, our findings also contribute to the literature 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—personal health cost—associated with serving under strict corporate governance.

Our findings suggest further avenues of investigation. One open question is whether managers fully account for these personal health costs as they progress in their careers, and how these costs affect selection into service as a CEO. Are some high-ability candidates for a Forbes-level CEO career more aware of these consequences than others and select out? Alternatively, candidates might differ in their preferences, with some embracing the "wild ride" of a CEO career and others aiming for a healthier and more balanced life.

Another important question is which jobs and hierarchy levels come with the largest adverse health consequences. For the reasons discussed in the introduction, the highest tier of management is an important application, which offers identification opportunities. But managers just one tier below might be more affected by work-related stressors, and workers at the bottom might suffer the most, also in light of looming financial hardships. Going beyond the realm of corporations, we might hypothesize that minimum wage and temporary workers with rigid schedules, such as delivery drivers, suffer even more. Or, it could be people in "life-or-death" jobs, such as emergency room doctors and airline pilots. In all cases, it would be important to understand the health consequences and explore whether or not there are dimensions of compensation that respond to these job demands.

Finally, another promising avenue is the more fine-grained identification of stressors. What aspects of individual job situations and which decisions tend to have the largest adverse health consequences, for either management or regular employees: pending layoffs and downsizing; restructurings; hostile merger attempts? Likewise, heightened workplace stress can also adversely affect other aspects of life, including marriage, divorce rates, parenting, and alcohol consumption. We leave these topics for future research.

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**Table I**
SUMMARY STATISTICS OF CEO APPARENT AGING DATA

| | Panel A: Image-Level Statistics | | | | | |
|---|---|---|---|---|---|---|
| | N | Mean | SD | P10 | P50 | P90 |
| Picture Year | 3,086 | 2008.69 | 6.16 | 2003 | 2009 | 2016 |
| Biological Age | 3,086 | 58.33 | 8.29 | 48 | 58 | 69 |
| Industry Distress (2007-2008) | 3,086 | 0.66 | 0.47 | 0 | 1 | 1 |
| Apparent Age | 3,086 | 54.90 | 7.23 | 45 | 56 | 63 |
| Logo in the Picture | 3,086 | 0.18 | 0.38 | 0 | 0 | 1 |
| Side Face | 3,086 | 0.19 | 0.39 | 0 | 0 | 1 |
| Professional Clothes | 3,086 | 0.97 | 0.17 | 1 | 1 | 1 |
| Magazine Shot | 3,086 | 0.01 | 0.08 | 0 | 0 | 0 |
| Natural Pose | 3,086 | 0.70 | 0.46 | 0 | 1 | 1 |
| Natural Lighting | 3,086 | 0.16 | 0.37 | 0 | 0 | 1 |
| Glasses | 3,086 | 0.33 | 0.47 | 0 | 0 | 1 |
| | Panel B: CEO-Level Statistics | | | | | |
| | N | Mean | SD | P10 | P50 | P90 |
| No. of Pictures per CEO | 463 | 7.35 | 4.51 | 3 | 6 | 13 |
| Biological Age in 2006 | 463 | 55.54 | 6.55 | 47 | 56 | 63 |
| Tenure (Pre-2006) | 463 | 8.00 | 7.73 | 2 | 6 | 17 |
| Industry Distress (2007-2008) | 463 | 0.65 | 0.48 | 0 | 1 | 1 |
| Industry Distress (Pre-2006) | 463 | 0.54 | 1.13 | 0 | 0 | 2 |
| | Panel C: Industry Distribution | | | | | |
| Industry (Number of CEOs) | Manufacturing (180) | | Finance, Insur, Real Estate (65) | | | |
| | Retail (53) | | Services (44) | | Others (50) | |
| | Trans.; Commns.; Elec., Gas, and Sanitary Services (71) | | | | | |

NOTE. — This table shows summary statistics for the CEO apparent-aging analyses. *Industry Distress* during 2007-2008 is an indicator for distress experience during these years. *Industry Distress* pre-2006 counts the number of industry distress experiences prior to 2006.

**Table II**
SUMMARY STATISTICS OF CEO MORTALITY DATA

| | N | Mean | SD | P10 | P50 | P90 |
|---|---|---|---|---|---|---|
| Birth Year | 1,605 | 1925 | 8.96 | 1914 | 1925 | 1937 |
| Dead (by October 2017) | 1,605 | 0.71 | 0.45 | 0 | 1 | 1 |
| Year of Death | 1,140 | 2004 | 9.98 | 1989 | 2006 | 2016 |
| Age at Death | 1,140 | 81.95 | 9.92 | 67.58 | 83.42 | 93.50 |
| Age Taking Office | 1,605 | 51.63 | 6.95 | 43 | 52 | 60 |
| Year Taking Office | 1,605 | 1977 | 7.21 | 1968 | 1977 | 1986 |
| Tenure | 1,605 | 10.62 | 6.86 | 3 | 9.08 | 20 |
| Industry Distress | 1,605 | 0.40 | 0.49 | 0 | 0 | 1 |
| BC | 1,605 | 2.21 | 4.19 | 0 | 0 | 8.24 |
| BC \| BC>0 | 625 | 5.68 | 5.05 | 0.54 | 4.41 | 12.37 |

NOTE. — This table shows summary statistics for the CEO longevity analyses. All variables are defined at the CEO level. *BC* denotes years of exposure to business combination laws. *Industry Distress* is an indicator variable that equals one if a CEO experienced industry-wide distress during his tenure.

**Table III**

INDUSTRY DISTRESS AND CEO AGING

| Dependent Variable: *Apparent Age*$_{i,j,t}$ | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Industry Distress $\times$ $\mathbb{1}_{\{t>2006\}}$ | 0.948* | 0.978** | | |
| | [0.484] | [0.478] | | |
| Industry Distress $\times$ $\mathbb{1}_{\{2006<t<2012\}}$ | | | 0.790 | 0.799 |
| | | | [0.533] | [0.525] |
| Industry Distress $\times$ $\mathbb{1}_{\{t\geq 2012\}}$ | | | 1.178** | 1.193** |
| | | | [0.547] | [0.538] |
| Biological Age | 0.915*** | 0.910*** | 0.943*** | 0.938*** |
| | [0.092] | [0.092] | [0.094] | [0.093] |
| CEO FE | Y | Y | Y | Y |
| Year FE | Y | Y | Y | Y |
| Picture Controls | | Y | | Y |
| Number of CEOs | 463 | 463 | 463 | 463 |
| Observations | 3,086 | 3,086 | 3,086 | 3,086 |

NOTE. — This table shows OLS estimates of the effect of industry distress exposure during the Great Recession on CEO apparent age. We winsorize the estimated apparent age by first winsorizing the top and bottom 0.5% of the difference between apparent and biological age and then adding this winsorized difference to the biological age. We weight observations by the inverse of the number of pictures per CEO. Standard errors, clustered at the industry level, are shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

**Table IV**

INDUSTRY DISTRESS AND MORTALITY

| Dependent Variable: $Death_{i,t}$ | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Industry Distress | 0.123* | 0.145** | 0.146** | 0.137** |
| | [0.065] | [0.062] | [0.066] | [0.066] |
| Age | 0.112*** | 0.115*** | 0.123*** | 0.123*** |
| | [0.006] | [0.006] | [0.007] | [0.007] |
| Year | | 0.000 | -0.003 | |
| | | [0.006] | [0.006] | |
| Location FE (HQ) | | Y | Y | Y |
| FF49 FE | | | Y | Y |
| Year FE | | | | Y |
| Number of CEOs | 1,605 | 1,605 | 1,605 | 1,605 |
| Observations | 50,530 | 50,530 | 50,530 | 50,530 |

NOTE. — This table shows hazard coefficients estimated from a Cox (1972) proportional hazards model. The dependent variable is an indicator that equals one if the CEO dies in a given year. The main independent variable *Industry Distress* is an indicator of a CEO's exposure to industry distress shocks. All variables are defined in Appendix A. Standard errors, clustered at the industry level, are shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

**Table V**

INDUSTRY DISTRESS AND MORTALITY: BIRTH COHORT ROBUSTNESS

| Dependent Variable: $Death_{i,t}$ | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Industry Distress | 0.160*** | 0.188*** | 0.139** | 0.143** |
| | [0.061] | [0.067] | [0.063] | [0.067] |
| Age | 0.120*** | 0.126*** | | |
| | [0.007] | [0.008] | | |
| Age × Birth Cohort 1 (oldest) | | | 0.087*** | 0.092*** |
| | | | [0.011] | [0.011] |
| Age × Birth Cohort 2 | | | 0.086*** | 0.090*** |
| | | | [0.012] | [0.012] |
| Age × Birth Cohort 3 | | | 0.084*** | 0.088*** |
| | | | [0.012] | [0.012] |
| Age × Birth Cohort 4 | | | 0.080*** | 0.084*** |
| | | | [0.013] | [0.013] |
| Age × Birth Cohort 5 (youngest) | | | 0.075*** | 0.079*** |
| | | | [0.015] | [0.014] |
| Location FE (HQ) | Y | Y | Y | Y |
| FF49 FE | | Y | | Y |
| Year FE | | | Y | Y |
| Birth Year FE | Y | Y | | |
| Number of CEOs | 1,605 | 1,605 | 1,605 | 1,605 |
| Observations | 50,530 | 50,530 | 50,530 | 50,530 |

NOTE. — This table shows hazard coefficients estimated as in Table IV but with birth-year fixed effects in lieu of year controls or fixed effects or allowing for birth cohort-specific age effects. Birth cohorts are defined by sorting CEOs into quintiles by birth year. The dependent variable is an indicator that equals one if the CEO dies in a given year. The main independent variable *Industry Distress* is an indicator of a CEO's exposure to industry distress shocks. All variables are defined in Appendix A. Standard errors, clustered at the industry level, are shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

**Table VI**
BUSINESS COMBINATION LAWS AND MORTALITY

| Dependent Variable: $Death_{i,t}$ | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| I(BC) | -0.225*** | -0.269*** | -0.262*** | -0.252*** | | | | |
| | [0.068] | [0.081] | [0.088] | [0.087] | | | | |
| BC | | | | | -0.041*** | -0.046*** | -0.042*** | -0.042*** |
| | | | | | [0.006] | [0.005] | [0.006] | [0.006] |
| Age | 0.107*** | 0.108*** | 0.116*** | 0.117*** | 0.105*** | 0.105*** | 0.114*** | 0.115*** |
| | [0.005] | [0.006] | [0.004] | [0.004] | [0.006] | [0.006] | [0.005] | [0.005] |
| Year | | 0.005 | 0.002 | | | 0.005 | 0.001 | |
| | | [0.004] | [0.005] | | | [0.004] | [0.004] | |
| Location FE (HQ) | | Y | Y | Y | | Y | Y | Y |
| FF49 FE | | | Y | Y | | | Y | Y |
| Year FE | | | | Y | | | | Y |
| Number of CEOs | 1,605 | 1,605 | 1,605 | 1,605 | 1,605 | 1,605 | 1,605 | 1,605 |
| Observations | 50,530 | 50,530 | 50,530 | 50,530 | 50,530 | 50,530 | 50,530 | 50,530 |

NOTE.— This table shows hazard coefficients estimated from a Cox (1972) proportional hazards model. The dependent variable is an indicator that equals one if the CEO dies in a given year. The main independent variables are a binary indicator of BC law exposure, $I(BC)$, in the left four columns and a count variable of years of exposure, *BC*, in the right four columns. All variables are defined in Appendix A. Standard errors, clustered at the state-of-incorporation level, are shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

**Table VII**

BUSINESS COMBINATION LAWS AND MORTALITY: BIRTH COHORT ROBUSTNESS

| DEPENDENT VARIABLE: $Death_{i,t}$ | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| I(BC) | -0.256** | | -0.231** | |
| | [0.100] | | [0.108] | |
| BC | | -0.045*** | | -0.035*** |
| | | [0.006] | | [0.006] |
| Age | 0.120*** | 0.117*** | | |
| | [0.004] | [0.005] | | |
| Age × Birth Cohort 1 (oldest) | | | 0.091*** | 0.088*** |
| | | | [0.010] | [0.010] |
| Age × Birth Cohort 2 | | | 0.089*** | 0.086*** |
| | | | [0.010] | [0.010] |
| Age × Birth Cohort 3 | | | 0.088*** | 0.084*** |
| | | | [0.011] | [0.010] |
| Age × Birth Cohort 4 | | | 0.084*** | 0.081*** |
| | | | [0.013] | [0.012] |
| Age × Birth Cohort 5 (youngest) | | | 0.079*** | 0.076*** |
| | | | [0.013] | [0.012] |
| Location FE (HQ) | Y | Y | Y | Y |
| FF49 FE | Y | Y | Y | Y |
| Year FE | | | Y | Y |
| Birth Year FE | Y | Y | | |
| Number of CEOs | 1,605 | 1,605 | 1,605 | 1,605 |
| Observations | 50,530 | 50,530 | 50,530 | 50,530 |

NOTE. — This table shows hazard coefficients estimated as in Table VI but with birth-year fixed effects in lieu of year controls or fixed effects or allowing for birth cohort-specific age effects. Birth cohorts are defined by sorting CEOs into quintiles by birth year. The dependent variable is an indicator that equals one if the CEO dies in a given year. The main independent variables are a binary indicator of BC law exposure, $I(BC)$, in columns (1) and (3), and a count variable of years of exposure, $BC$, in columns (2) and (4). 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.

# Internet Appendix

## Appendix A Variable Definitions

| Variable Name | Definition |
|---|---|
| *Apparent Age*$_{i,t}$ | How old CEO $i$ looks in year $t$. The apparent age is estimated using a machine-learning based software by Antipov et al. (2016) that has been specifically developed for apparent-age estimation. See Appendix B.1 for additional detail. |
| *(Biological) Age*$_{i,t}$ | CEO $i$'s age in year $t$. |
| *Tenure*$_{i,t}$ | CEO $i$'s cumulative tenure (in years) at time $t$. |
| *Birth Year* | CEO's year of birth. |
| *Dead (by Oct. 2017)* | Indicator for whether a CEO has passed away by October 1st, 2017. |
| *Year of Death* | CEO's year of death, calculated up to monthly level (e.g. 2010.5 for a person who dies on 6/30/2010). |
| *Age Taking Office* | CEO's age when appointed as CEO. |
| *Year Taking Office* | Year in which a CEO is appointed. |
| *Industry Distress*$_{i,t}$ | Indicator equal to 1 if CEO $i$ is exposed to an industry shock by year $t$. Industry shock is defined as median two-year stock return (forward-looking) of firms in the same industry below $-30\%$. As in Babina (2020), we (i) use SIC3 industry classes, (ii) restrict to single-segment CRSP/Compustat firms, i. e., drop firms with multiple segments in the Compustat Business Segment Database (CBSD), (iii) drop firms if the reported single-segment sales differ from those in Compustat by more than 5%, (iv) restrict to firms with sales of at least $20m, and (v) exclude industry-years with fewer than four firms. We use firms' modal SIC across CRSP, Compustat, and CBSD, and the latter in case of a tie. |
| $I(BC_{i,t})$ | Indicator equal to 1 if CEO $i$ is insulated by a BC law in year $t$; remains at 1 in all subsequent years $\tau > t$, including after CEO departure. |
| $BC_{i,t}$ | CEO $i$'s cumulative exposure to a BC law during tenure up to time $t$ (in years); remains constant after CEO departure. |
| $BC_{i,t}^{(\text{min-}p50)}$ | CEO $i$'s below-median (4.4 years) cumulative BC law exposure during tenure up to time $t$ (in years); remains constant after CEO departure. |
| $BC_{i,t}^{(p51\text{-max})}$ | CEO $i$'s above-median (4.4 years) cumulative BC law exposure during tenure up to time $t$ (in years); remains constant after CEO departure. |
| $I(FL_{i,t})$ | Indicator equal to 1 if CEO $i$ is insulated by the first-time enactment of a 2nd generation anti-takeover law ($FL$) in year $t$; constant after CEO departure. |
| $FL_{i,t}$ | CEO $i$'s cumulative exposure to the first-time enactment of a 2nd generation anti-takeover law ($FL$) during tenure up to time $t$ (in years); constant after CEO departure. |
| *Year*$_{i,t}$ | Year of a subspell; used in hazard models 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); missing data is interpolated. |
| *Employees*$_{j,t}$ | Firm $j$'s total number of employees in year $t$ (from Compustat); missing data is interpolated. |

## Appendix B Industry-Wide Distress Shocks and Apparent Aging: Apparent-Age Estimation Details and Robustness Tests

### *1. Apparent-Age Estimation*

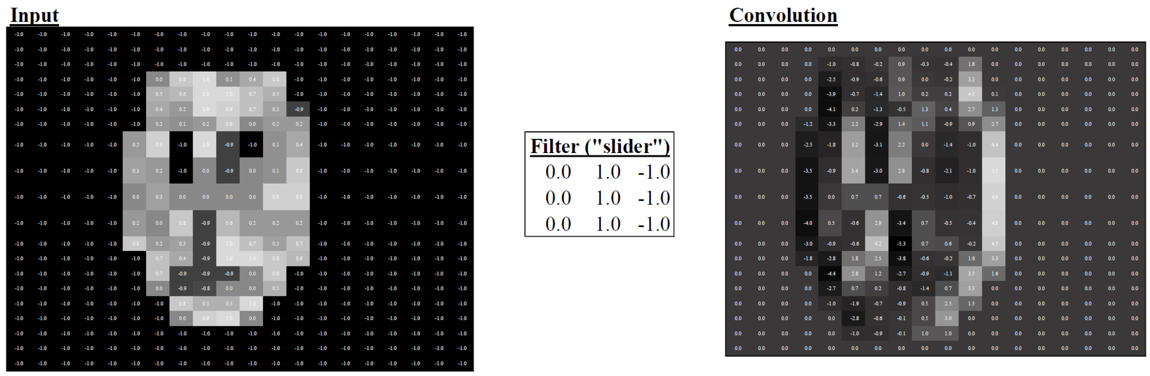
We use machine learning based software by Antipov et al. (2016), henceforth referred to as the ABBD software. This software was developed for the purpose of apparent-age estimation, and was the winning solution of the second edition of the *ChaLearn Looking At People* competition in the *apparent-age estimation* track. A person's *apparent age* traces visible signs of aging in their faces top capture how old they *look*. By contrast, biological age is time elapsed since birth, and generally differs from the apparent age.

At the core of ABBD's apparent-age estimation tool is the training of a *convolutional neural network* (CNN). A CNN is a special class of *neural networks* that is particularly useful for image recognition and computer vision problems. A neural network is a system that learns to perform a task by studying training data.[26] It is architectured with three classes of layers: input, output, and hidden layers. The input layer receives the external data being evaluated, and the output data contains the network's response to the input. The hidden layers in between abstractly determine intermediate features of the data. A CNN is a neural network in which some of the hidden layers employ the method of convolution, i. e., of transforming the input by sliding (or, convolving) over it, to detect patterns (such as edges or corners), which are then passed on to the next layer.

Appendix-Figure B.1 provides a simplified example of how convolution works in CNNs. Here, the fictional input is a shape that is roughly recognizable as a face (numbers between $-1$ and 1 determine pixel color). The filter matrix slides over the input and produces the output as the sum of element-wise matrix multiplication of $3 \times 3$ pixel regions with the filter matrix. As can be seen in the convoluted output, this specific filter matrix identifies right vertical edges. Convolutional layers further along in a system may be able to detect more advanced patterns such as, in our application, eyes or wrinkles.

CNNs have become widely used over the past ten to twenty years, with numerous applications, in particular to image recognition and classification. In an influential article on *deep learning*[27] published in *Nature*, LeCun et al. (2015) summarize that CNNs have "brought about a revolution in computer vision" and "breakthroughs in processing images, video, speech and audio," and they are "now the dominant approach for almost all recognition and detection tasks."

[26] The task is referred to as *supervised learning* if the data is labeled (annotated), as is our training data.
[27] A neural network is considered *deep* if it has multiple hidden layers.




**Appendix-Figure B.1. Simplified example of convolution.** The fictional input image (left) with $20 \times 20$ pixels is roughly recognizable as a face. In this fictional image, each pixel ("cell") is encoded with a number between $-1$ and $+1$ determining its color, with $-1.0$ defined as black and $+1.0$ defined as white. The output image (right) is obtained through convolution. The $3 \times 3$ filter matrix (center) slides over each possible $3 \times 3$ region in the input image and outputs the sum of element-wise matrix multiplication of these $3 \times 3$ image regions and the filter matrix. Example inspired by material by Jeremy Howard (youtube.com/watch?v=V2h3IOBDvrA) and deeplizard (deeplizard.com/learn/video/YRhxdVk_sIs).

ABBD's apparent-age estimation software starts from a pre-trained version of a state-of-the-art CNN for face recognition called VGG-16,[28] and involves two key steps: *training* and *fine-tuning* of the CNN. In a first step, this CNN is trained on a large dataset of more than 250,000 facial images from the IMDb (Internet Movie Database) and Wikipedia, which also contains information on the biological age of the person. The training step is implemented by minimizing the mean absolute error between predicted age and biological age. In a second step, the software is fine-tuned for apparent-age estimation on a unique dataset of 5,613 facial images that also contains information on people's *apparent* age, consisting of at least 10 human age estimates (per picture), which were specifically collected for the *ChaLearn Looking At People* competition. The fine-tuning step is implemented by minimizing a metric that penalizes deviations from the average (human) age estimate more when the disagreement about the person's apparent age is low.[29] Training and fine-tuning essentially mean that the software learns to estimate the age of the people in the two datasets using the information on biological and apparent age by adapting learning parameters in the

[28] VGG-16 is a deep CNN introduced by Simonyan and Zisserman (2014). ABBD's software uses a VGG-16 version by Parkhi et al. (2015), which was trained for the purposes of face recognition (identifying identities from facial images) on 2.6 million images. Both works have been widely used and cited.

[29] The metric is defined as $\varepsilon = 1 - \exp\left(-\frac{(\hat{x}-\mu)^2}{2\sigma^2}\right)$, where $\hat{x}$ is the predicted apparent age, and $\mu$ and $\sigma$ are the image-level mean and standard deviation of across the human-based age estimates.

hidden layers.

ABBD's software and apparent-age estimation tool have a variety of notable features:

*Age distribution in training datasets.* Both the IMDb-Wikipedia data and the dataset employed for human-based fine-tuning include people from all age groups, and in particular people aged 50 and above. This ensures that the software is trained and fine-tuned on data that includes people with similar facial characteristics as our CEOs, such as with regard to baldness patterns, hair color, and wrinkle development. For reference, the CEO at the $10^{\text{th}}$ ($50^{\text{th}}$, $90^{\text{th}}$) percentile in our dataset is 47 (56, 63) years old in 2006 (see Table I).

*Image pre-processing.* Before feeding the pictures into the CNN for training and fine-tuning, ABBD "standardize" them, a process they label picture pre-processing. Specifically, they use existing software solutions to detect, scale, and align the face in each image, and resize each image to $224 \times 224$ pixels. Intuitively, standardizing images reduces the noise present when training and fine-tuning the software and improves performance (cf. Table 2 in Antipov, Baccouche, Berrani, and Dugelay 2016). The software's performance on the *ChaLearn Looking At People* competition data improves by approximately 1% as a result of image-preprocessing (cf. Table 2 in Antipov, Baccouche, Berrani, and Dugelay 2016).

ABBD's trained software does not include image pre-processing code (and can, in fact, be applied to "raw images" so long as they are resized). We nonetheless replicate some of their pre-processing steps in order to increase the similarity between our CEO images and the images used for software training. Before pre-processing a picture, we make sure that the image contains only the face of the CEO. If a picture contains multiple faces, such as a CEO with their partner, other managers, or a journalist, we first manually crop the picture and keep only the portion that shows the CEO. We then use the Python-based "face_recognition" package[30] to detect the picture region showing the CEO's face, extract the face, center it in the image, and resize the image to $224 \times 224$ pixels. Note that any remaining differences to ABBD's image pre-processing might increase the noise in our apparent-age estimates, but not introduce bias as any potential systematic differences in pre-processing steps would need to be correlated with industry shock exposure during the Great Recession.

Appendix-Figure B.2 shows several examples of pre-processed facial images. Panel (a) shows pre-processed images used to train ABBD's software. One can see that they differ in terms of "tint" and background. For example, the leftmost picture has a bluish tint and dark background, whereas the rightmost picture has a yellowish tint and light background.

[30] The full package documentation is at github.com/ageitgey/face_recognition/blob/master/README.md.

This underscores the spectrum of image characteristics the software is "exposed" to while being trained for apparent-age estimation. Panel (b) shows pre-processed CEO images from our sample. Again, there are differences in terms of tint and background, so it is worth reiterating that these are image features that the software can learn to take into account in its estimation during the training stage. Furthermore, comparing images across the two panels illustrates that our implementation of the image pre-processing step indeed leads to similar results compared to ABBD's original implementation on the training datasets.

**(a)** Training sample

**(b)** CEO sample

**Appendix-Figure B.2. Examples of pre-processed images.** Panel (a) shows examples of pre-processed facial images used in the training of the apparent-age estimation software. Panel (b) shows examples of pre-processed CEO images from our sample.

*Accuracy gains from software fine-tuning.* In ABBD's software development, a fine-tuning on human age estimates led to the biggest accuracy improvement across all training and image pre-processing steps, amounting to more than 20% (cf. Table 2 in Antipov, Baccouche, Berrani, and Dugelay 2016). This underscores the importance of using a software trained for apparent-age estimation, rather than an "off-the-shelf" software solely trained on images annotated with people's biological age.

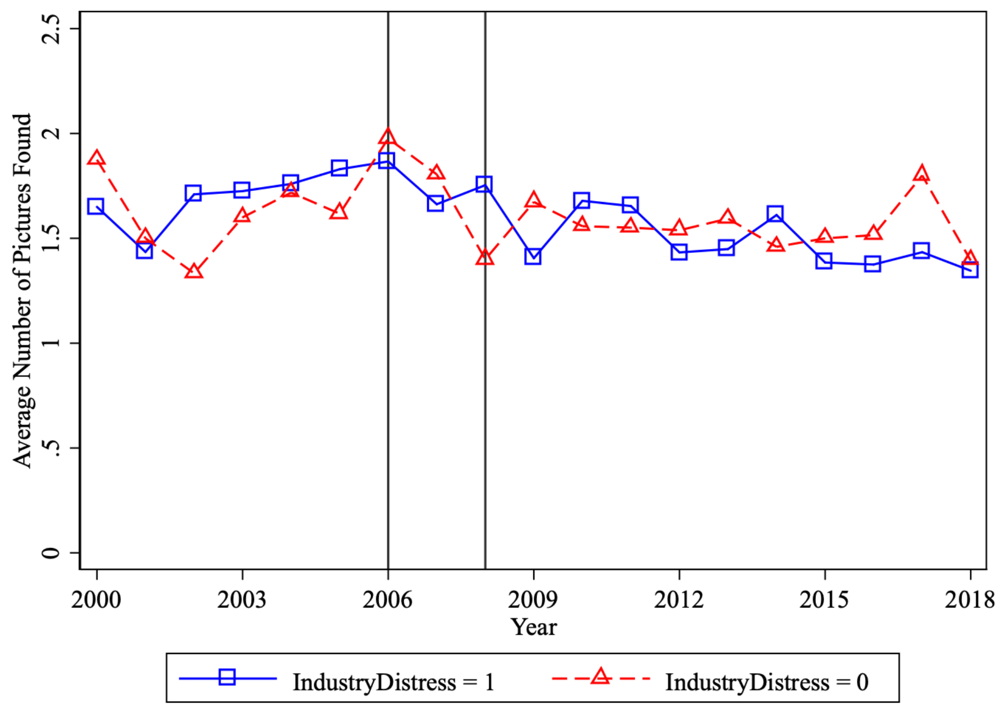
*Cross-validation.* Rather than training one CNN on the 5,613 training images, ABBD's apparent-age estimation merges eleven CNNs, which were trained using eleven-fold cross-validation. Cross-validation is a popular technique in prediction problems. As part of the training step, a portion of the data (the *validation sample*) is set aside for out-of-sample tests, i. e., tests on data the algorithm was not trained on. Moreover, instead of fixing the validation sample, it is common to train separate models using non-overlapping validation samples and to then average the results. In ABBD's implementation, each of the eleven "sub-CNNs" uses 5,113 images for training and 500 (non-overlapping) images for validation; this corresponds to a near-complete partition of the full training data into equal-sized validation samples ($5,613/11 \approx 500$). Each sub-model outputs a $100 \times 1$ vector of probabilities associated with all apparent ages between 0 and 99 years. ABBD's final solution, on which our analyses are based, uses the average of the probabilities across all sub-models. Averaging across the ensemble of eleven models is akin to bootstrap aggregating ("bagging") procedures typically aimed at improving prediction accuracy (Breiman 1996).

*Data augmentation.* In the fine-tuning step of the software development, ABBD use five-times data augmentation. This is a popular technique to enlarge the training (or fine-tuning) sample, i. e., to allow the software to learn on more data. Specifically, each apparent-age annotated image is fed into the algorithm jointly with five modified versions: the mirrored image, a rotated image ($\pm 5°$), a horizontally shifted image ($\pm 5\%$), and a scaled image ($\pm 5\%$). To see the potential benefit of data augmentation to reduce overfitting, suppose that among the fine-tuning sample of 5,613 images, people who look older happen to look slightly to the upper right, but that there were *no* intrinsic relation between apparent age and camera angle. Including mirrored and rotated images in the fine-tuning step reduces the likelihood that the software may learn to associate apparent age with camera angle. In our application, data augmentation also further alleviates concerns about effects of slight differences in image pre-processing.

To match the steps during training, ABBD's final solution uses the same image modifications also on new images that are fed into the tool, i. e., it estimates different apparent ages for each image in our CEO sample based on the original image and modified images as outlined above. The final apparent age is the average across the different estimates.

*2. Additional Figures and Tables*

This section contains all robustness figures and tables on industry-wide distress shocks and apparent aging.




**Appendix-Figure B.3.**—Average number of pictures per CEO across years. *Notes*: This figure depicts the average number of pictures per CEO we are able to collect each year for the group of CEOs that experienced industry shocks during 2007-2008 and the group that did not. The two black vertical lines indicate the years 2006 and 2008.

**APPENDIX-TABLE B.1**

INDUSTRY DISTRESS AND CEO AGING – NO WINSORIZATION

| Dependent Variable: $Apparent\ Age_{i,j,t}$ | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Industry Distress × $\mathbb{1}_{\{t>2006\}}$ | 0.940* | 0.978** | | |
| | [0.496] | [0.491] | | |
| Industry Distress × $\mathbb{1}_{\{2006<t<2012\}}$ | | | 0.807 | 0.841 |
| | | | [0.536] | [0.531] |
| Industry Distress × $\mathbb{1}_{\{t\geq 2012\}}$ | | | 1.135* | 1.178** |
| | | | [0.576] | [0.564] |
| Biological Age | 0.912*** | 0.908*** | 0.944*** | 0.940*** |
| | [0.093] | [0.093] | [0.095] | [0.095] |
| CEO FE | Y | Y | Y | Y |
| Year FE | Y | Y | Y | Y |
| Picture Controls | | Y | | Y |
| Number of CEOs | 463 | 463 | 463 | 463 |
| Observations | 3,086 | 3,086 | 3,086 | 3,086 |

**APPENDIX-TABLE B.2**

INDUSTRY DISTRESS AND CEO AGING – RESTRICTIVE INDUSTRY DISTRESS DEFINITION

| Dependent Variable: $Apparent\ Age_{i,j,t}$ | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Industry Distress × $\mathbb{1}_{\{t>2006\}}$ | 1.173** | 1.064** | | |
| | [0.469] | [0.463] | | |
| Industry Distress × $\mathbb{1}_{\{2006<t<2012\}}$ | | | 1.102** | 1.007* |
| | | | [0.504] | [0.510] |
| Industry Distress × $\mathbb{1}_{\{t\geq 2012\}}$ | | | 1.261** | 1.135** |
| | | | [0.563] | [0.551] |
| Biological Age | 1.272*** | 1.274*** | 1.268*** | 1.269*** |
| | [0.021] | [0.022] | [0.027] | [0.028] |
| CEO FE | Y | Y | Y | Y |
| Year FE | Y | Y | Y | Y |
| Picture Controls | | Y | | Y |
| Number of CEOs | 463 | 463 | 463 | 463 |
| Observations | 3,086 | 3,086 | 3,086 | 3,086 |

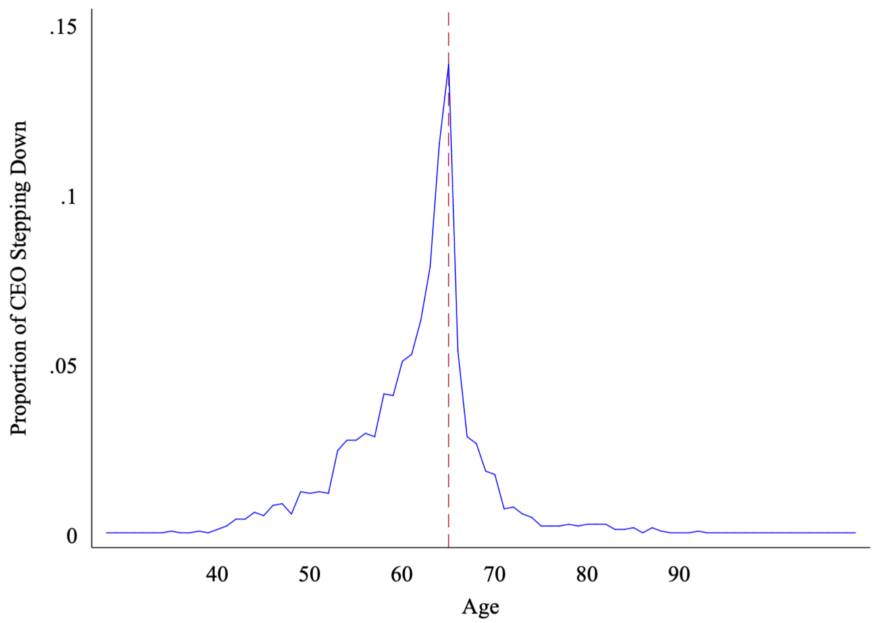
**APPENDIX-TABLE B.3**

INDUSTRY DISTRESS AND CEO AGING – PRE-2016 SAMPLE

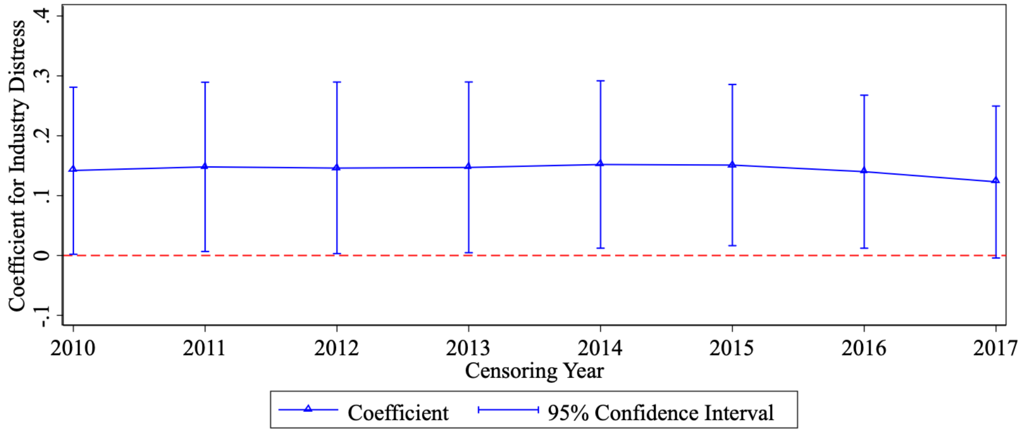
| Dependent Variable: *Apparent Age*$_{i,j,t}$ | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Industry Distress × $\mathbb{1}_{\{t>2006\}}$ | 0.816* | 0.839* | | |
| | [0.487] | [0.485] | | |
| Industry Distress × $\mathbb{1}_{\{2006<t<2012\}}$ | | | 0.604 | 0.622 |
| | | | [0.535] | [0.525] |
| Industry Distress × $\mathbb{1}_{\{t\geq 2012\}}$ | | | 1.323** | 1.360** |
| | | | [0.566] | [0.568] |
| Biological Age | 0.952*** | 0.943*** | 0.983*** | 0.974*** |
| | [0.095] | [0.096] | [0.094] | [0.093] |
| CEO FE | Y | Y | Y | Y |
| Year FE | Y | Y | Y | Y |
| Picture Controls | | Y | | Y |
| Number of CEOs | 463 | 463 | 463 | 463 |
| Observations | 3,086 | 3,086 | 3,086 | 3,086 |

## Appendix C Industry-Wide Distress Shocks and Life Expectancy: Robustness Tests

This appendix presents the robustness tests of the relation between industry distress and CEOs' life expectancy referenced in Section IV.




**Appendix-Figure C.1.** —Proportion of CEOs stepping down by age. *Notes*: This figure depicts the proportion of CEOs stepping down at each age. The vertical dashed line indicates age 65.




**Appendix-Figure C.2.** —Estimated effect of industry distress when varying the censoring year. *Notes*: This figure shows the estimated coefficients on the industry distress indicator variable using the specification from Table IV, column 1, but varying the censoring date. In the main analysis, the cutoff date is Oct. 1, 2017, i. e., CEOs who did not pass away before this date are treated as censored. The alternative censoring dates are Dec. 31, 2016; Dec. 31, 2015; ...; up to Dec. 31, 2010. The number of CEOs in the sample remains unchanged when varying the cutoff, i.e. $N = 1,605$.

**APPENDIX-TABLE C.1**

INDUSTRY DISTRESS AND MORTALITY – ADDITIONAL CONTROLS

| Dependent Variable: $Death_{i,t}$ | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Industry Distress | 0.117* | 0.150** | 0.178*** | 0.169** |
| | [0.066] | [0.062] | [0.069] | [0.070] |
| Age | 0.111*** | 0.113*** | 0.121*** | 0.121*** |
| | [0.005] | [0.005] | [0.006] | [0.006] |
| Year | | 0.001 | 0.002 | |
| | | [0.006] | [0.006] | |
| ln(Pay) | -0.066* | -0.059 | -0.019 | -0.022 |
| | [0.037] | [0.037] | [0.048] | [0.049] |
| ln(Assets) | 0.012 | 0.006 | -0.080* | -0.086* |
| | [0.031] | [0.034] | [0.049] | [0.049] |
| ln(Employees) | 0.003 | 0.007 | 0.070 | 0.072 |
| | [0.038] | [0.038] | [0.053] | [0.053] |
| Location FE (HQ) | | Y | Y | Y |
| FF49 FE | | | Y | Y |
| Year FE | | | | Y |
| Number of CEOs | 1,553 | 1,553 | 1,553 | 1,553 |
| Observations | 49,052 | 49,052 | 49,052 | 49,052 |

NOTE. — This table reports hazard coefficients estimated as in Table IV but with additional controls for CEO pay, assets, and employees. The dependent variable is an indicator that equals one if the CEO dies in a given year. The main independent variable *Industry Distress* is an indicator of a CEO's exposure to industry distress shocks. All variables are defined in Appendix A. Standard errors, clustered at the industry level, are shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

**APPENDIX-TABLE C.2**

INDUSTRY DISTRESS AND MORTALITY – ADDITIONAL CEOS

| Dependent Variable: $Death_{i,t}$ | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | Panel A: Baseline Controls | | | | Panel B: Additional Controls | | | |
| Industry Distress | 0.075 | 0.095* | 0.110** | 0.103* | 0.072 | 0.097* | 0.141** | 0.133** |
| | [0.062] | [0.056] | [0.054] | [0.055] | [0.063] | [0.057] | [0.060] | [0.060] |
| Age | 0.113*** | 0.116*** | 0.123*** | 0.123*** | 0.112*** | 0.115*** | 0.122*** | 0.122*** |
| | [0.006] | [0.006] | [0.007] | [0.007] | [0.005] | [0.005] | [0.006] | [0.006] |
| Year | | -0.001 | -0.003 | | | -0.001 | 0.001 | |
| | | [0.005] | [0.005] | | | [0.005] | [0.006] | |
| ln(Pay) | | | | | -0.050 | -0.043 | 0.002 | -0.001 |
| | | | | | [0.036] | [0.037] | [0.044] | [0.045] |
| ln(Assets) | | | | | 0.020 | 0.015 | -0.084* | -0.088* |
| | | | | | [0.030] | [0.032] | [0.049] | [0.049] |
| ln(Employees) | | | | | -0.020 | -0.022 | 0.048 | 0.049 |
| | | | | | [0.036] | [0.035] | [0.051] | [0.051] |
| Location FE (HQ) | | Y | Y | Y | | Y | Y | Y |
| FF49 FE | | | Y | Y | | | Y | Y |
| Year FE | | | | Y | | | | Y |
| Number of CEOs | 1,900 | 1,900 | 1,900 | 1,900 | 1,818 | 1,818 | 1,818 | 1,818 |
| Observations | 58,034 | 58,034 | 58,034 | 58,034 | 55,796 | 55,796 | 55,796 | 55,796 |

NOTE. — This table reports hazard coefficients estimated as in Table IV but using an extended CEO sample as described in Section II.B. The dependent variable is an indicator that equals one if the CEO dies in a given year. The main independent variable *Industry Distress* is an indicator of a CEO's exposure to industry distress shocks. Panel A includes the baseline controls from Table IV and Panel B adds additional additional controls for CEO pay, assets, and employees as in Appendix-Table C.1. All variables are defined in Appendix A. Standard errors, clustered at the industry level, are shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

## Appendix D Corporate Monitoring and Life Expectancy: Robustness Tests

This appendix presents the robustness tests of the relation between anti-takeover laws and CEOs' life expectancy referenced in Section V. We first present additional details on BC-specific robustness tests and then report additional figures and tables.

*Other Anti-Takeover Laws: First-Time Passage of Second-Generation Anti-Takeover Laws*

Our main analysis exploits the enactment of BC laws as they have been shown to create substantial conflicts of interest between managers and shareholders (Bertrand and Mullainathan 2003, Gormley and Matsa 2016). Some researchers have questioned whether BC laws were the most important legal development impacting corporate governance at the time (see the discussion in Cain et al. (2017) and Karpoff and Wittry (2018)). Here, we replicate our analyses for other anti-takeover legislation from the 1980s that induced plausibly exogenous variation in corporate monitoring intensity.

In addition to BC laws, four other types of anti-takeover laws were passed by individual states since the 1980s: (1) Control Share Acquisition laws prohibited acquirers of large equity stakes from using their voting rights, making it more difficult for hostile acquirers to gain control. (2) Fair Price laws required acquirers to pay a fair price for shares acquired in a takeover attempt. Fair could mean, for example, the highest price paid by the acquirer for shares of the target within the last 24 months (cf. Cheng, Nagar, and Rajan 2004). (3) Directors' Duties laws extended the board members' duties to incorporate the interests of non-investor stakeholders, even if not necessarily maximizing shareholder value. (4) Poison Pill laws guaranteed that the firms had the right to use poison pill takeover defenses. We refer to the first of these five laws (including BC laws) passed by a state as the *First Law* (*FL*). Anti-takeover law exposure is similar when jointly looking at all five second-generation laws. For example, conditional on any *FL* exposure, the median CEO experiences 4.45 years, close to the 4.41 years in the BC-based analysis.

Appendix-Figure D.1 visualizes the *FL* enactment by states over time.

Appendix-Table D.2 re-estimates Table VI using *FL* enactment as identifying variation. We limit the sample to the 1,510 CEOs who are appointed in years prior to the *FL* enactment of any of the five second-generation anti-takeover laws. Consistent with our main findings, we estimate a significant increase in longevity for CEOs under less stringent governance regimes. The estimated effect sizes are very similar to our main specification using BC laws. For example, for the specifications in Panel A based on cumulative law exposure, the hazard coefficients range from $-0.039$ to $-0.046$, compared to $-0.041$ to $-0.046$ in Table VI. As Panel B shows, the *FL* results are also robust to including the additional CEO and firm level controls from Panel A of Appendix-Table D.1.

*Karpoff-Wittry and Related Tests: Institutional and Legal Context of the Anti-Takeover Laws*

Karpoff and Wittry (2018) propose several robustness tests to address endogenous firm responses to anti-takeover laws, which we implement in Appendix-Table D.3. For all sample restrictions, we follow the procedure suggested in Karpoff and Wittry (2018): In Panel A, we remove the 46 firms identified by these authors as having lobbied for the passage of the second-generation laws. In Panel B, we use Institutional Shareholder Services (ISS) Governance (formerly, RiskMetrics) data from 1990 to 2017 to identify firms that opted out of coverage by the laws and exclude them from the analysis. In Panel C, we exclude firm-years in which firms had adopted firm-level anti-takeover defenses. We identify firms with firm-level defenses combining ISS data with data provided to us by Cremers and Ferrell (2014), which extends the Gompers et al. (2003) G-index backwards to 1977-1989. We back out whether firms used firm-level defenses in 1977-1989 by "subtracting" the state-wide laws from the G-index, which combines firm- and state-level defenses. Firm-level defenses include Golden Parachutes and Cumulative Voting (cf. Gompers et al. (2003) for details).

In all subsamples, the hazard coefficient on BC exposure remains significant at 1%, both when using the indicator and the count variable for BC experience. In addition, the hazard coefficient estimates are nearly unchanged, ranging from $-0.218$ (Panel B, column 1) to $-0.276$ (Panel A, column 3) for the indicator version, and from $-0.041$ (Panel A, column 5) to $-0.047$ (Panel C, column 2) for the count version.

Karpoff and Wittry (2018) also point to possible confounding effects of first-generation anti-takeover laws. They raise the concern that firms without BC exposure might experience lenient governance before 1982 because first-generation anti-takeover laws effectively lost their effect only starting from June 1982 after the *Edgar v. MITE* ruling. We address this concern in Appendix-Table D.4 through three cuts of the data. In subsample A, we drop all CEO-years prior to 1982, i. e., we restrict the sample to years from 1982 onward (albeit including the post-1982 years for CEOs who stepped down prior to 1982). In subsample B, we drop all CEOs who stepped down prior to 1982, i. e., we restrict the sample to CEOs who served during the "post-first-law period" (including CEO-years prior to 1982). Note that in terms of number of CEOs remaining, subsample B is more restrictive than subsample A. In subsample C, we restrict the sample to CEOs who began their tenure in or after 1982, i. e., subsample C is a subset of subsample B. In all subsamples, we continue to estimate negative hazard coefficients for both the indicator and cumulative BC exposure variables, similar in size to those in the main table. The coefficients remain significant at 1% in subsamples A and B as well as in the most restrictive subsample C when using the indicator BC variable. In the latter sample, we lose statistical power when using the cumulative BC exposure (standard errors quintuple), though the point estimate remains similar.

Finally, in a last set of robustness checks, we move beyond the tests suggested in Karpoff and Wittry (2018) and create sub-samples based on firms' state of incorporation and industry affiliation, inspired by similar robustness checks in Giroud and Mueller (2010) and Gormley and Matsa (2016). In Appendix-Table D.5, 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). In all three panels, the hazard coefficient estimates on binary and cumulative BC exposure are barely affected by these data cuts.

*Predicted Length of Exposure: Prediction Model Details*

To purge the per-year estimates in the right four columns of Table VI of possible endogeneity in the length of exposure, we construct a measure of predicted BC law exposure, and relate predicted, rather than true exposure, to CEO mortality rates.[31] To this end, we proceed in three steps. First, we estimate a prediction model for CEO tenure; we then construct predicted BC exposure; and finally we re-estimate the hazard regressions using predicted BC exposure as the independent variable.

We first predict for every CEO-year, including years after the passage of a BC law:

$$RemainTenure_{i,t} = \boldsymbol{X'_{i,t}A} + e_{i,t}. \tag{D.1}$$

The control variables are an age cubic, tenure cubic, the CEO's cumulative exposure to the BC law until year $t$, $BC_{i,t}$, and fixed effects for industry, year, headquarters state, birth year, and tenure start-year. Denoting as $t^*$ the year when the BC law is passed, we use the predicted remaining tenure at $t^*$ from equation (D.1) to construct CEOs' predicted exposure to BC laws,

$$\widehat{BC}_i^* = I(BCLawPassed_{s(i),t}) \times \widehat{RemainTenure}_{i,t^*}, \tag{D.2}$$

where $I(BCLawPassed_{s(i),t}) = 1$ for CEO $i$ in state $s(i)$ at $t \geq t^*$. $\widehat{RemainTenure}_{i,t^*}$ is

[31] Directly controlling for realized tenure would result in a "bad control" problem and introduce bias (Angrist and Pischke 2008). .

backward-looking, i. e., constructed using information from years up to $t^*$.

Using this variable, we construct a CEO's predicted cumulative BC exposure until year $t$, $\widehat{BC}_{i,t}$ as (i) $\widehat{BC}_{i,t} = 0\ \forall t$ in the control group; (ii) $\widehat{BC}_{i,t} = 0\ \forall t < t^*$ if not yet treated; and (iii) $\widehat{BC}_{i,t} = \min\{k+1, \widehat{BC}_i^*\}$ for each year $t$ following $t^*$, with $t = t^* + k$. Note that $k$ is allowed to be fractional if the BC law goes into effect in the middle of the year.

We then use the predicted cumulative exposure in the following hazard estimations:

$$\lambda(t|\widehat{BC}_{i,t}, X_{i,t}) = \lambda_0(t)\ \exp\{\beta\widehat{BC}_{i,t} + \boldsymbol{\delta'}\boldsymbol{X}_{i,t}\} \tag{D.3}$$

Since this approach involves a generated regressor, we report bootstrapped standard errors, using the block bootstrap method (a block is a state of incorporation cluster) with 500 iterations.

*Business Combination Laws and CEO Pay*

As discussed in the main text, our aging and mortality results also prompt the question whether parties account for the health consequences of (permanent) changes in job demands. We explore this in the context of the permanent corporate governance regime change induced by BC laws.

The theoretical prediction on the link between BC laws and CEO pay is in fact unclear, as also noted by Bertrand and Mullainathan (1998). On the one hand, a model of compensating differentials would predict a decrease in pay as CEOs' working conditions improve and imposed health costs are reduced. In line with such a channel, Edmans and Gabaix (2011) present a theoretical model of the CEO market in which lower effort—which is isomorphic to lower job demands—is compensated by lower pay. On the other hand, a model of skimming would predict that CEOs use the increase in autonomy to extract additional private benefits in the form of higher compensation. It is thus an empirical question as to which effect dominates in our specific context.

Before estimating the empirical relation, it is useful to first calibrate what effect size we would expect if compensation for health ramifications were the primary channel empirically. In their meta-analysis of the literature on the value of a statistical life (VSL), Viscusi and Aldy (2003) report an estimate around \$6.7 million (in 2000 dollars) for a person with income of around \$26,000, and an income elasticity for the VSL of around 0.5. Applied to our CEO sample, this translates into a VSL of around \$47.3 million.[32] Given a baseline

[32] Given an average CEO pay of \$1.3 million (in 2000 dollars) in our sample, we can calculate the implied VSL for the average CEO as $VSL_{CEO} = \exp(0.5 \times (\ln(\$1.3\text{m}) - \ln(\$26\text{k})) + \ln(\$6.7\text{m})) = \$47.3\text{m}$.

mortality rate of 1.366% for 60-year-olds born in 1925 (Human Mortality Database 2019), a reduction in mortality risk of 4.1% per year of BC exposure (column (5) in Table VI) implies a CEO pay change between $-2\%$ and $-9\%$, depending on whether the wage adjustment reflects the entire BC-induced mortality risk shift over the expected remaining lifespan or solely the shift over the remaining years while serving as CEO.[33]

With these calibrated effects in mind, Appendix-Table D.7 presents the results on the relation between CEO pay and BC law exposure. In column (1), we estimate linear regressions of CEO pay on the BC indicator and the same controls and fixed effects as in the hazard analyses. This specification excludes any post-treatment outcomes from the right-hand side and parallels the survival analysis. In column (2), we add the control variables used in Bertrand and Mullainathan (1998): tenure, firm assets, and employees. We note that these controls may themselves be affected by the reform and therefore absorb the effect of the anti-takeover laws. Finally, in column (3), we add firm fixed effects (in place of industry fixed effects), as in the baseline specification of Bertrand and Mullainathan (1998):

$$\ln(Pay_{i,j,t}) = \alpha_t + \beta_j + \gamma \boldsymbol{I}(BC_{i,t}) + \boldsymbol{\delta}' \boldsymbol{X}_{i,j,t} + e_{i,j,t}$$

where $i$ represents a CEO, $j$ represents a firm, and $t$ represents a calendar year.

We estimate a positive, albeit mostly insignificant treatment effect. The estimates indicate a pay increase between 4.1–8.7%. Only the estimate in column (2) is marginally significant. In comparing the results to the earlier work, which estimated a (more significant) 5.4 percent pay increase, it is important to note that our analysis is conducted on a CEO-level sample, and restricts the sample to incumbent, pre-BC CEOs.

The evidence speaks against a compensating reduction in pay, but is instead suggestive of additional rents (higher pay). However, any resulting wealth increases are unlikely to explain the longevity results, given that the literature has found little evidence of a causal relation of income and life expectancy for wealthy individuals (Cesarini et al. 2016). Where evidence has been found of an effect of wealth on health, it appears to work through reductions in stress (Schwandt 2018). The apparent lack of a compensating differential casts doubt on whether all parties fully account for the health implications of different governance regimes.

[33] The calculations are based on an average length of BC exposure of 5.68 years (Table II), an average time of 24.77 years between onset of BC exposure and death, and an average annual CEO pay of $1.3 million in 2000 dollars). For example, if we assume that the wage adjustment reflects the mortality risk shift over the expected remaining lifespan, we can calculate the pay change as $(-24.77/5.68) \times (4.1\% \times 1.366\% \times \$47.3\text{mn})/\$1.3\text{mn} = -9\%$.

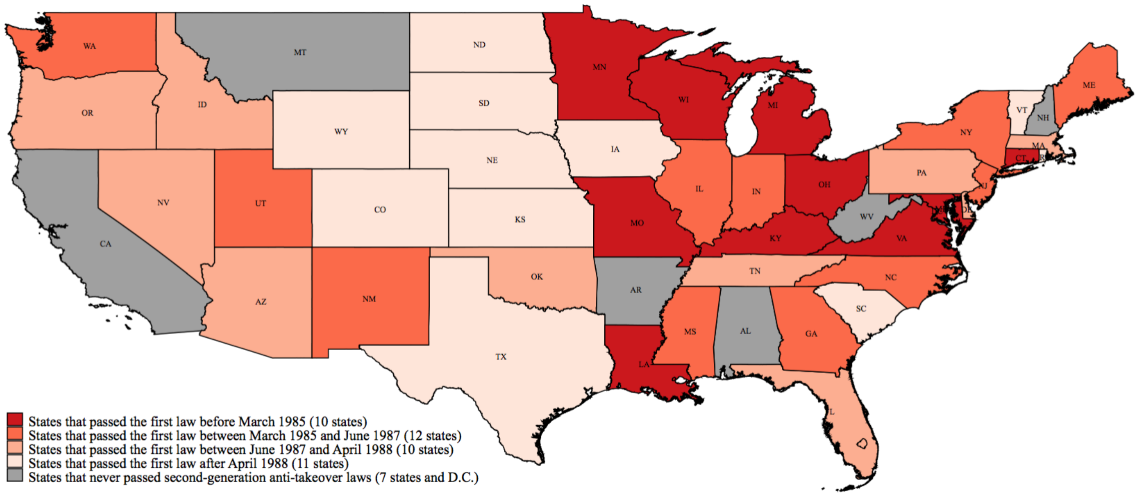
*Business Combination Laws and CEO Tenure*

In addition to CEO pay, we can also explore how CEO tenure responds to the introduction of anti-takeover laws, which can also provide insight into how managers perceive this permanent change in corporate monitoring to affect job demands.

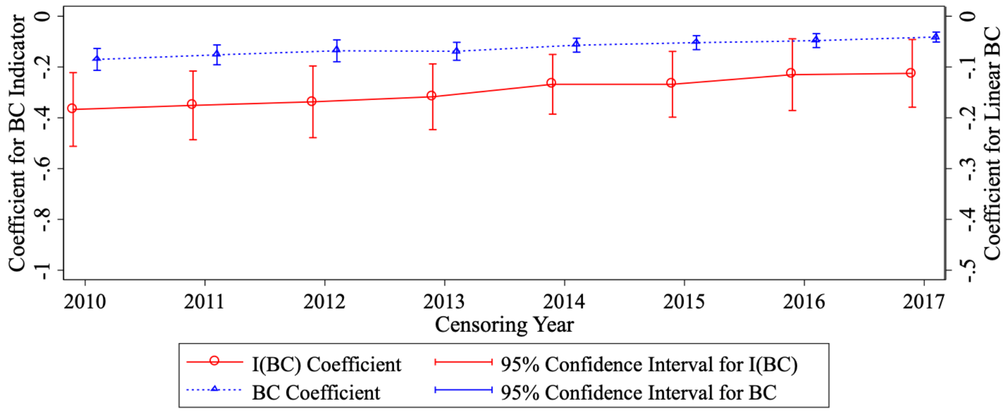
Similar to the case of CEO pay, theory does not provide a clear prediction as to how tenure should respond to the anti-takeover laws. On the one hand, CEOs may become entrenched and stay on the job longer. On the other hand, CEOs who reduce effort on the job might be fired more frequently. We estimate again hazard models, now analyzing CEO departure as the outcome variable. The results in columns (1) of Appendix-Table D.8 indicate that BC law treatment, $I(BC)$, decreases the separation hazard by around 22 percent, but the effect halves in magnitude and becomes statistically insignificant after controlling for year effects (column 2), with standard errors nearly doubling. In the specifications using the length of exposure variable $BC$ (in columns 3 to 4), the estimated separation hazard falls by 4 to 10 percent. These results are suggestive of moderate increases in tenure in response to BC law passage.

Further analyses of CEOs' age at the end of their tenure suggest that any increases in tenure would be driven by fewer CEOs stepping down in their 50s and early 60s. Appendix-Figure D.4 plots the retirement hazard separately for CEOs with and without BC law exposure. Exposure appears to lower the hazard before and increase it above age 65, including a long tail of tenures into the 80s and 90s. While the raw data is not as stark as for our longevity results, nor are the hazard estimates as robust, it is noteworthy for another reason: It helps rule out that the end of mandatory retirement through the amendment of the Age Discrimination in Employment Act (ADEA) in 1986 confounds our BC-law–longevity findings. Although there is a large spike in retirements at ages 64 and 65, there is no association between retirement at these ages and exposure to the BC laws.

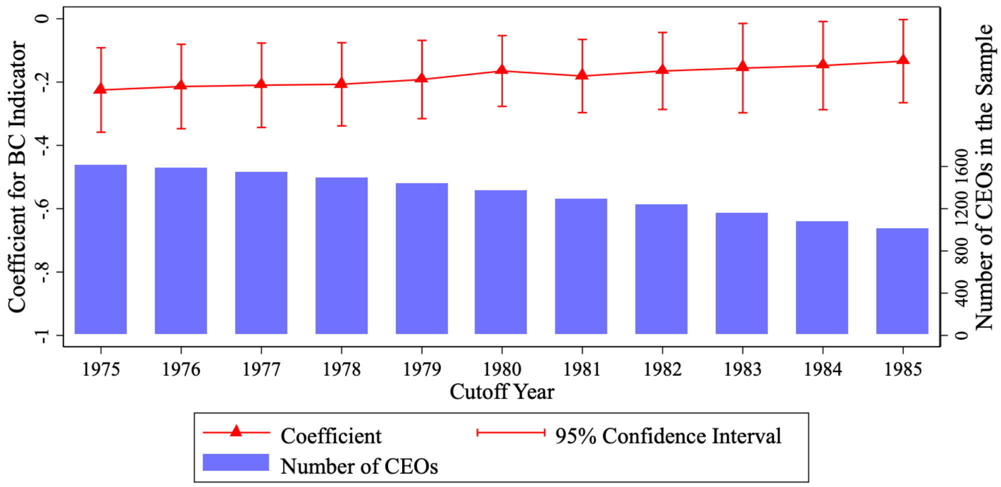
An increase in tenure (or delayed retirement) as a result of anti-takeover insulation is also unlikely to be the channel for the estimated increase in longevity. To begin with, prior research has found small or even beneficial effects of retirement on health in the general population (Hernaes et al. 2013, Insler 2014, Fitzpatrick and Moore 2018). In our population, a life expectancy advantage arising directly from tenure would run counter to the notion that the CEO job is demanding as the evidence in Bandiera et al. (2020) and Porter and Nohria (2018) on the intensity of CEO schedules and the constraints imposed by the CEO position imply. Moreover, the results in Section V.E on nonlinearities point to initial exposure effects, with prolonged exposure (from prolonged tenure) having no incremental impact on life expectancy.



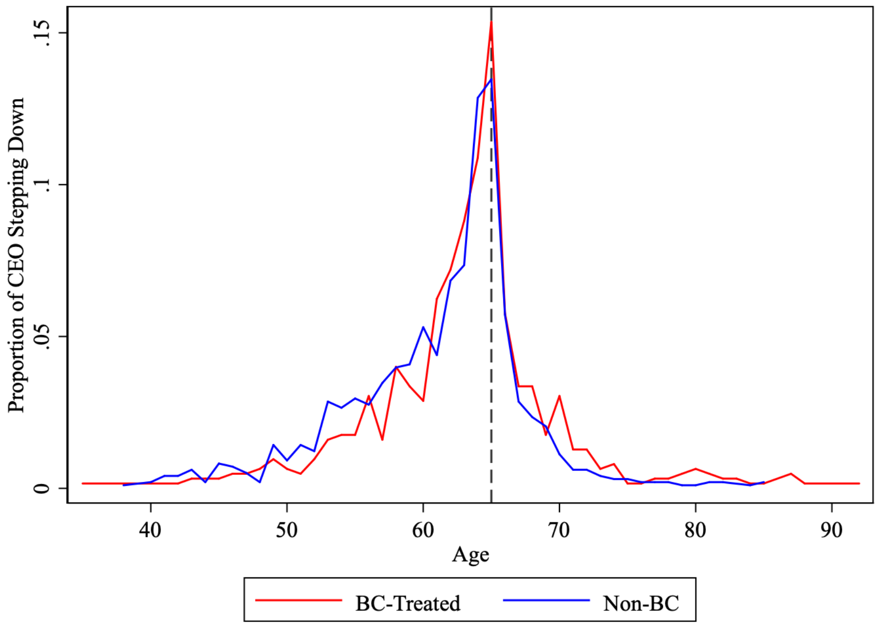

**Appendix-Figure D.1.**—First-time introduction of second-generation anti-takeover laws over time. *Notes*: This figure visualizes the distribution of first-time enactments of any of the five most common second-generation anti-takeover laws over time, i. e., business combination (BC), fair price (FP), control share acquisition (CSA), poison pills (PP), and directors' duties (DD) laws. The graph omits the states of Alaska and Hawaii. Alaska did not adopt any second-generation anti-takeover laws. Hawaii adopted a CSA law on 4/23/1985, and DD and PP laws on 6/7/1988.




**Appendix-Figure D.2.**—Estimated effect of the BC law exposure when varying the censoring year. *Notes*: This figure shows the estimated coefficients on the BC indicator variable $I(BC)$ and the cumulative BC variable $BC$ when using the specifications from Table VI, columns 1 and 5, but varying the censoring date. In the main analysis, the cutoff date is Oct. 1, 2017, i. e., CEOs who did not pass away before this date are treated as censored. The alternative censoring dates are Dec. 31, 2016; Dec. 31, 2015; ...; up to Dec. 31, 2010. The number of CEOs in the sample remains unchanged when varying the cutoff, i.e. $N = 1,605$.




**Appendix-Figure D.3.**—Estimated effect of the BC law exposure when varying the sample cutoff year. *Notes*: This figure shows the estimated coefficients on the BC indicator variable $I(BC)$ when using the specification from Table VI, column 1, but varying the sample. In the main sample, CEOs end their tenure in or later than 1975. We vary this cutoff year from 1975 to 1985, when the first BC law ever was passed. The blue (dark) bars are the number of CEOs in the sample. When the cutoff year is 1975 (our main sample), the number is 1,605 and the estimated coefficient is the same as shown in Table VI, column 4.




**Appendix-Figure D.4.** —Proportion of CEOs stepping down by age. *Notes*: This figure depicts the proportion of CEOs stepping down at each age, split by whether or not a CEO was exposed to a business combination (BC) law. The vertical dashed line indicates age 65.

**APPENDIX-TABLE D.1**

BUSINESS COMBINATION LAWS AND MORTALITY –
ADDITIONAL CONTROLS AND STATE-OF-INCORPORATION FIXED EFFECTS

| Dependent Variable: $Death_{i,t}$ | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | | | | Panel A: Additional Controls | | | | |
| I(BC) | -0.224*** | -0.261*** | -0.241*** | -0.225*** | | | | |
| | [0.064] | [0.073] | [0.086] | [0.082] | | | | |
| BC | | | | | -0.042*** | -0.045*** | -0.040*** | -0.039*** |
| | | | | | [0.007] | [0.007] | [0.007] | [0.007] |
| ln(Pay) | -0.019 | -0.014 | 0.008 | 0.003 | -0.023 | -0.023 | -0.012 | -0.015 |
| | [0.033] | [0.036] | [0.039] | [0.040] | [0.043] | [0.043] | [0.048] | [0.049] |
| ln(Assets) | 0.020 | 0.015 | -0.033 | -0.041 | 0.028 | 0.023 | -0.014 | -0.021 |
| | [0.019] | [0.025] | [0.043] | [0.041] | [0.018] | [0.023] | [0.036] | [0.035] |
| ln(Employees) | -0.016 | -0.010 | 0.017 | 0.022 | -0.018 | -0.012 | 0.008 | 0.012 |
| | [0.020] | [0.021] | [0.038] | [0.037] | [0.019] | [0.021] | [0.036] | [0.037] |
| Location FE (HQ) | | Y | Y | Y | | Y | Y | Y |
| Number of CEOs | 1,503 | 1,503 | 1,503 | 1,503 | 1,503 | 1,503 | 1,503 | 1,503 |
| Observations | 49,052 | 49,052 | 49,052 | 49,052 | 49,502 | 49,052 | 49,052 | 49,052 |
| | | | | Panel B: State-of-Incorporation Fixed Effects | | | | |
| I(BC) | -0.225*** | -0.265*** | -0.274*** | -0.264*** | | | | |
| | [0.068] | [0.083] | [0.088] | [0.086] | | | | |
| BC | | | | | -0.041*** | -0.048*** | -0.046*** | -0.045*** |
| | | | | | [0.006] | [0.007] | [0.007] | [0.007] |
| Location FE (Incorp.) | | Y | Y | Y | | Y | Y | Y |
| Number of CEOs | 1,605 | 1,605 | 1,605 | 1,605 | 1,605 | 1,605 | 1,605 | 1,605 |
| Observations | 50,530 | 50,530 | 50,530 | 50,530 | 50,530 | 50,530 | 50,530 | 50,530 |
| Age (Linear Control) | Y | Y | Y | Y | Y | Y | Y | Y |
| Year (Linear Control) | | Y | Y | | | Y | Y | |
| FF49 FE | | | Y | Y | | | Y | Y |
| Year FE | | | | Y | | | | Y |

NOTE. — Panel A reports hazard coefficients estimated as in Table VI, with additional controls for CEO pay, assets, and employees. Panel B reports hazard coefficients estimated as in Table VI, but including state-of-incorporation fixed effects instead of state-of-headquarters fixed effects. Controls and fixed effects (in addition to location fixed effects based on state-of-headquarters or state-of-incorporation) for both panels are indicated at the bottom of the table. 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.

**APPENDIX-TABLE D.2**

FIRST-TIME SECOND-GENERATION ANTI-TAKEOVER LAWS AND MORTALITY

| Dependent Variable: $Death_{i,t}$ | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | | | | Panel A: Baseline Results | | | | |
| I(FL) | -0.187*** | -0.220*** | -0.221*** | -0.215*** | | | | |
| | [0.064] | [0.066] | [0.076] | [0.075] | | | | |
| FL | | | | | -0.039*** | -0.046*** | -0.044*** | -0.044*** |
| | | | | | [0.006] | [0.006] | [0.006] | [0.006] |
| Number of CEOs | 1,510 | 1,510 | 1,510 | 1,510 | 1,510 | 1,510 | 1,510 | 1,510 |
| Observations | 47,994 | 47,994 | 47,994 | 47,994 | 47,994 | 47,994 | 47,994 | 47,994 |
| | | | | Panel B: Additional Controls | | | | |
| I(FL) | -0.173*** | -0.190*** | -0.170** | -0.157** | | | | |
| | [0.060] | [0.062] | [0.070] | [0.068] | | | | |
| FL | | | | | -0.040*** | -0.044*** | -0.040*** | -0.039*** |
| | | | | | [0.008] | [0.009] | [0.008] | [0.008] |
| ln(Pay) | -0.030 | -0.023 | 0.005 | -0.000 | -0.020 | -0.016 | 0.001 | -0.002 |
| | [0.032] | [0.037] | [0.037] | [0.037] | [0.041] | [0.043] | [0.045] | [0.045] |
| ln(Assets) | 0.017 | 0.014 | -0.058 | -0.066* | 0.027 | 1.026 | -0.024 | -0.031 |
| | [0.019] | [0.026] | [0.037] | [0.036] | [0.018] | [0.024] | [0.033] | [0.032] |
| ln(Employees) | -0.012 | -0.005 | 0.044 | 0.049 | -0.020 | -0.013 | 0.019 | 0.022 |
| | [0.019] | [0.020] | [0.034] | [0.035] | [0.019] | [0.020] | [0.035] | [0.037] |
| Number of CEOs | 1,464 | 1,464 | 1,464 | 1,464 | 1,464 | 1,464 | 1,464 | 1,464 |
| Observations | 46,660 | 46,660 | 46,660 | 46,660 | 46,660 | 46,660 | 46,660 | 46,660 |
| Age (Linear Control) | Y | Y | Y | Y | Y | Y | Y | Y |
| Year (Linear Control) | | Y | Y | | | Y | Y | |
| Location FE (HQ) | | Y | Y | Y | | Y | Y | Y |
| FF49 FE | | | Y | Y | | | Y | Y |
| Year FE | | | | Y | | | | Y |

NOTE.— This table reports hazard coefficients estimated as in Table VI, but using the first-time introduction of any of the five most common second-generation anti-takeover laws as measure of lenient governance. The sample is restricted to CEOs appointed prior to the introduction of the anti-takeover law(s). Panel B adds additional controls for CEO pay, assets, and employees. Controls and fixed effects for both panels are indicated at the bottom of the table. 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.

**APPENDIX-TABLE D.3**

EXCLUDING LOBBYING FIRMS, OPT-OUT FIRMS, AND FIRM-YEARS WITH FIRM-LEVEL DEFENSES

| Dependent Variable: $Death_{i,t}$ | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | | | | Panel A: Excluding Lobbying Firms | | | | |
| I(BC) | -0.232*** | -0.270*** | -0.276*** | -0.268*** | | | | |
| | [0.071] | [0.068] | [0.074] | [0.074] | | | | |
| BC | | | | | -0.041*** | -0.045*** | -0.043*** | -0.043*** |
| | | | | | [0.006] | [0.007] | [0.008] | [0.008] |
| Number of CEOs | 1,530 | 1,530 | 1,530 | 1,530 | 1,530 | 1,530 | 1,530 | 1,530 |
| Observations | 48,106 | 48,106 | 48,106 | 48,106 | 48,106 | 48,106 | 48,106 | 48,106 |
| | | | | Panel B: Excluding Opt-out Firms | | | | |
| I(BC) | -0.218*** | -0.244*** | -0.226*** | -0.216*** | | | | |
| | [0.065] | [0.081] | [0.081] | [0.080] | | | | |
| BC | | | | | -0.042*** | -0.045*** | -0.041*** | -0.040*** |
| | | | | | [0.006] | [0.006] | [0.006] | [0.006] |
| Number of CEOs | 1,532 | 1,532 | 1,532 | 1,532 | 1,532 | 1,532 | 1,532 | 1,532 |
| Observations | 48,180 | 48,180 | 48,180 | 48,180 | 48,180 | 48,180 | 48,180 | 48,180 |
| | | | | Panel C: Excluding Firm-level Defenses | | | | |
| I(BC) | -0.232*** | -0.271*** | -0.269*** | -0.256*** | | | | |
| | [0.068] | [0.078] | [0.087] | [0.087] | | | | |
| BC | | | | | -0.043*** | -0.047*** | -0.044*** | -0.044*** |
| | | | | | [0.006] | [0.005] | [0.005] | [0.005] |
| Number of CEOs | 1,599 | 1,599 | 1,599 | 1,599 | 1,599 | 1,599 | 1,599 | 1,599 |
| Observations | 43,417 | 43,417 | 43,417 | 43,417 | 43,417 | 43,417 | 43,417 | 43,417 |
| Age (Linear Control) | Y | Y | Y | Y | Y | Y | Y | Y |
| Location FE (HQ) | | Y | Y | Y | | Y | Y | Y |
| Year (Linear Control) | | Y | Y | | | Y | Y | |
| FF49 FE | | | Y | Y | | | Y | Y |
| Year FE | | | | Y | | | | Y |

NOTE. — This table reports hazard coefficients estimated as in Table VI, but with additional sample restrictions. 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 61 firms that opted out of the second-generation anti-takeover laws, based on data from the Institutional Shareholder Services (ISS) Governance database. In Panel C, we exclude firm-years in which firms used firm-level defenses as identified from the the ISS data and data from Cremers and Ferrell (2014). Controls and fixed effects for all three panels are indicated at the bottom of the table. 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.

**APPENDIX-TABLE D.4**

RESTRICTION TO YEARS AFTER THE END OF THE FIRST-GENERATION LAWS

| Dependent Variable: $Death_{i,t}$ | | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Subsample A: Drop CEO-years pre-1982 | | Subsample B: Drop CEOs stepping down pre-1982 | | Subsample C: CEOs starting in or after 1982 | |
| I(BC) | -0.267*** | | -0.227*** | | -0.417*** | |
| | [0.083] | | [0.062] | | [0.089] | |
| BC | | -0.044*** | | -0.042*** | | -0.036 |
| | | [0.006] | | [0.008] | | [0.028] |
| Age | 0.117*** | 0.115*** | 0.116*** | 0.112*** | 0.124*** | 0.118*** |
| | [0.005] | [0.005] | [0.005] | [0.006] | [0.013] | [0.018] |
| Location FE (HQ) | Y | Y | Y | Y | Y | Y |
| FF49 FE | Y | Y | Y | Y | Y | Y |
| Year FE | Y | Y | Y | 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 |

NOTE. – This table re-estimates columns (3) and (6) of Table VI with the sample restricted to the period when the first-generation anti-takeover laws lost their effect (in June 1982 after the *Edgar v. MITE* ruling). In subsample A, we drop all CEO-years prior to 1982, i. e., we restrict the sample to years from 1982 onward (albeit including the post-1982 years for CEOs who stepped down prior to 1982). In subsample B, we drop all CEOs who stepped down prior to 1982, i. e., we restrict the sample to CEOs who served during the "post-first-law period" (including CEO-years prior to 1982). Note that in terms of number of CEOs remaining, subsample B is more restrictive than subsample A. In subsample C, we restrict the sample to CEOs who began their tenure in or after 1982, i. e., subsample C is a subset of subsample B. 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.

**APPENDIX-TABLE D.5**

EXCLUDING DE OR NY INCORPORATED, BANKING, OR UTILITY FIRMS

| Dependent Variable: $Death_{i,t}$ | | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | | | Panel A: Excluding DE/NY Firms | | | |
| I(BC) | 0.707*** | 0.679*** | 0.688*** | | | |
| | [0.079] | [0.085] | [0.086] | | | |
| BC | | | | 0.958*** | 0.958** | 0.962** |
| | | | | [0.016] | [0.019] | [0.019] |
| Number of CEOs | 738 | 738 | 738 | 738 | 738 | 738 |
| Observations | 22,103 | 22,103 | 22,103 | 22,103 | 22,103 | 22,103 |
| | | | Panel B: Excluding Banking Firms | | | |
| I(BC) | 0.727*** | 0.717*** | 0.726*** | | | |
| | [0.056] | [0.060] | [0.060] | | | |
| BC | | | | 0.942*** | 0.944*** | 0.945*** |
| | | | | [0.007] | [0.007] | [0.007] |
| Number of CEOs | 1,328 | 1,328 | 1,328 | 1,328 | 1,328 | 1,328 |
| Observations | 42,322 | 42,322 | 42,322 | 42,322 | 42,322 | 42,322 |
| | | | Panel C: Excluding Utility Firms | | | |
| I(BC) | 0.777*** | 0.785*** | 0.794*** | | | |
| | [0.056] | [0.061] | [0.061] | | | |
| BC | | | | 0.957*** | 0.961*** | 0.962*** |
| | | | | [0.005] | [0.004] | [0.005] |
| Number of CEOs | 1,422 | 1,422 | 1,422 | 1,422 | 1,422 | 1,422 |
| Observations | 45,017 | 45,017 | 45,017 | 45,017 | 45,017 | 45,017 |
| Year (Linear Control) | Y | Y | | Y | Y | |
| Age (Linear Control) | Y | Y | Y | Y | Y | Y |
| Location FE (HQ) | Y | Y | Y | Y | Y | Y |
| FF49 FE | | Y | Y | | Y | Y |
| Year FE | | | Y | | | Y |

[a]This table reports hazard ratios estimated as in Table VI with the sample restricted by states of incorporation or 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 II). In Panel B, we exclude firms that are classified as "Banking" firms in the Fama-French 49 industry classification. In Panel C, we exclude firms that are classified as "Utilities" firms in the Fama-French 49 industry classification. Controls and fixed effects for all three panels are indicated at the bottom of the table. 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.

**APPENDIX-TABLE D.6**

NONLINEAR EFFECTS AND PREDICTED EXPOSURE

| Dependent Variable: $Death_{i,t}$ | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| $\text{BC}^{(\text{min-}p50)}$ | -0.081*** | -0.097*** | -0.091*** | -0.088*** | | | | |
| | [0.020] | [0.023] | [0.026] | [0.025] | | | | |
| $\text{BC}^{(p51\text{-max})}$ | -0.009 | -0.008 | -0.007 | -0.008 | | | | |
| | [0.014] | [0.015] | [0.017] | [0.017] | | | | |
| $\widehat{\text{BC}}$ | | | | | -0.052*** | -0.059*** | -0.050** | -0.049** |
| | | | | | [0.020] | [0.018] | [0.024] | [0.024] |
| Age | 0.105*** | 0.104*** | 0.113*** | 0.114*** | 0.108*** | 0.105*** | 0.115*** | 0.115*** |
| | [0.006] | [0.006] | [0.004] | [0.004] | [0.008] | [0.009] | [0.008] | [0.008] |
| Year | | 0.007* | 0.004 | | | 0.005 | 0.001 | |
| | | [0.004] | [0.004] | | | [0.006] | [0.004] | |
| Location FE (HQ) | | Y | Y | Y | | Y | Y | Y |
| FF49 FE | | | Y | Y | | | Y | Y |
| Year FE | | | | Y | | | | Y |
| Number of CEOs | 1,605 | 1,605 | 1,605 | 1,605 | 1,605 | 1,605 | 1,605 | 1,605 |
| Observations | 50,530 | 50,530 | 50,530 | 50,530 | 50,530 | 50,530 | 50,530 | 50,530 |

NOTE. — This table shows hazard coefficients estimated from a Cox (1972) proportional hazards model. The dependent variable is an indicator that equals one if the CEO dies in a given year. The main independent variable in the left four columns is $\widehat{BC}$, a count variable of years of predicted cumulative exposure to a BC law. The main independent variables in the right four columns are $BC_{i,t}^{(\text{min-}p50)}$ and $BC_{i,t}^{(p51\text{-max})}$, which capture BC law exposure up to the sample median and incremental exposure above the median, respectively. All variables are defined in Appendix A. For the left four columns, we present standard errors clustered at the state-of-incorporation level, in brackets. For the right four columns, we present bootstrapped standard errors, using the block bootstrap method with 500 iterations, in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

**APPENDIX-TABLE D.7**

BUSINESS COMBINATION LAWS AND CEO PAY

| Dependent Variable: $\ln(Pay_{i,t})$ | | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| I(BC) | 0.086 | 0.087* | 0.041 |
| | [0.058] | [0.047] | [0.051] |
| Age Controls | Y | Y | Y |
| Tenure Controls | | Y | Y |
| Firm Characteristics | | Y | Y |
| Location FE (HQ) | Y | Y | |
| FF49 FE | Y | Y | |
| Year FE | Y | Y | Y |
| Firm FE | | | Y |
| Number of CEOs | 1,553 | 1,553 | 1,553 |
| Observations | 17,719 | 17,719 | 17,719 |

NOTE. — The table shows OLS estimates where the dependent variable is the logarithm of a CEO's total pay in a given year. In column (1), "Age Controls" includes linear age, and in columns (2) and (3) it includes linear and quadratic age. "Tenure Controls" includes linear and quadratic tenure. "Firm Characteristics" includes logarithms of asset size and the number of employees. 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.

**APPENDIX-TABLE D.8**

BUSINESS COMBINATION LAWS AND TENURE

| Dependent Variable: $Retirement_{i,t}$ | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| I(BC) | -0.222*** | -0.094 | | |
| | [0.068] | [0.106] | | |
| BC | | | -0.098*** | -0.044** |
| | | | [0.021] | [0.021] |
| Age | 0.100*** | 0.099*** | 0.101*** | 0.099*** |
| | [0.010] | [0.010] | [0.010] | [0.011] |
| Year | 0.069*** | | 0.095*** | |
| | [0.007] | | [0.014] | |
| Location FE (HQ) | Y | Y | Y | Y |
| FF49 FE | Y | Y | Y | Y |
| Year FE | | Y | | Y |
| Number of CEOs | 1,605 | 1,605 | 1,605 | 1,605 |
| Observations | 17,864 | 17,864 | 17,864 | 17,864 |

NOTE. — This table shows hazard coefficients estimated from a Cox (1972) proportional hazards model. The dependent variable is an indicator that equals one if the CEO steps down in a given year. All variables are defined in Appendix A. Standard errors are clustered at the state-of-incorporation level, in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.