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CEO STRESS, AGING, AND DEATH

Mark Borgschulte
Marius Guenzel
Canyao Liu
Ulrike Malmendier

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ABSTRACT

We estimate the long-term effects of experiencing high levels of job demands on the mortality and aging of CEOs. The estimation exploits variation in takeover protection and industry crises. First, using hand-collected data on the dates of birth and death for 1,605 CEOs of large, publicly-listed U.S. firms, we estimate the resulting changes in mortality. The hazard estimates indicate that CEOs' lifespan increases by two years when insulated from market discipline via anti-takeover laws, and decreases by 1.5 years in response to an industry-wide downturn. Second, we apply neural-network based machine-learning techniques to assess visible signs of aging in pictures of CEOs. We estimate that exposure to a distress shock during the Great Recession increases CEOs' apparent age by one year over the next decade. Our findings imply significant health costs of managerial stress, also relative to known health risks.

Mark Borgschulte
UIUC Economics
214 David Kinley Hall
1407 W Gregory Dr
Urbana, IL 61801
markborg@illinois.edu

Marius Guenzel
The Wharton School
University of Pennsylvania
Steinberg Hall-Dietrich Hall
3620 Locust Walk
Philadelphia, PA 19104
mguenzel@wharton.upenn.edu

Canyao Liu
Yale School of Management
165 Whitney Avenue
New Haven, CT 06511
canyao.liu@yale.edu

Ulrike Malmendier
Department of Economics
549 Evans Hall # 3880 University
of California, Berkeley Berkeley,
CA 94720-3880
and NBER
ulrike@econ.berkeley.edu

1. INTRODUCTION

Job demands and work-related stress are increasingly recognized to be key determinants of population health and well-being.[1] 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. Health researchers argue that stress, and the damage it causes, is the mechanism underlying many health disparities (Cutler et al. 2006, Pickett and Wilkinson 2015, Puterman et al. 2016, Snyder-Mackler et al. 2020).

Yet, there is little quasi-experimental evidence that links job demands and stressors at work directly to health outcomes. While stress arising from social hierarchies, especially in the workplace, has been proposed as an explanation for the strong relationship between socioeconomic status and life expectancy, causal evidence on, for example, the effect of promotions is limited and reaches mixed conclusions (Boyce and Oswald 2012, Anderson and Marmot 2012, Johnston and Lee 2013).

A key reason for the lack of causal evidence is that it is challenging to disentangle the health effects of job stressors from those of income losses and financial hardship (Smith 1999). In this paper, we overcome these identification hurdles by focusing on CEOs of large publicly traded companies. CEOs in this sample are wealthy and unlikely to be affected by financial hardships even if they lost their job. Thus, the setting of top corporate jobs allows us to isolate direct effects on health from indirect effects due to financial constraints.

The CEO position is a suitable candidate to analyze work-related stress as CEOs work long hours, make high-stakes decisions such as layoffs or 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.[2] Needless to say, lower-ranked and non-corporate position might entail significantly higher levels of stress. (We can think of "life-or-death" jobs, such as emergency room doctors and airline pilots, but also of minimum-wage and temporary jobs with rigid schedules, such as delivery drivers.) Our analysis does not speak

[1] See, e. g., Marmot (2005) and Ganster and Rosen (2013). 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).

[2] 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).

to the question of which type of occupations come with the highest personal cost. Instead, it exploits plausibly exogenous variation in job demands within the CEO group to help establish and quantify the influence of job demands on health outcomes.

That said, the CEO context is of interest in its own right for at least two reasons. First, CEOs bear the ultimate responsibility for the success of the firm and satisfaction of employees. Given their overarching importance within their firms, 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. Our analysis might thus speak the prevalence of certain CEO characteristics and possible feedback effects: Are aspiring CEOs (over-)confident about their health? Are women vastly underrepresented in the C-suite not only because of discrimination but also because they (correctly) anticipate the health costs of assuming such positions?

We assemble new measures of health outcomes to investigate the link between CEO stress and health. By stress, we do not mean a biomedical analysis in the sense of measuring adrenaline or cortisol levels.[3] Instead, building on the popular notion of stress, we exploit periods of industry-wide distress and variation in the intensity of CEO monitoring to capture variation in work-related stress. We estimate the effect on CEOs' life expectancy and aging patterns. Our analysis uses new data on the lifespan of CEOs and a new data set of photographs of CEOs' faces, combined with recent visual machine learning (ML) techniques to estimate the effects on visible signs of aging. 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 economic literature. Our application illustrates their potential for the study of health and aging to complement standard measures based on mortality, hospital admissions, or survey responses.

Our analysis has three main parts. In the first part, we relate variation in the intensity of CEO monitoring due to corporate-governance legislation to CEO mortality. In the second part, we exploit variation in job demands due to industry-level distress shocks, and also study the effect on CEO mortality. In the third part, we continue to exploit industry-level distress shocks, here from the Great Recession, and relate them to visible signs of accelerated aging, identified by neural-network based ML estimations.

In the first part of the analysis, the source of identifying variation is the staggered passage

[3] 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).

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. Prior research has documented that they reduced CEOs' job demands and allowed them to "enjoy the quiet life" (Bertrand and Mullainathan 2003). For example, CEOs became less tough in wage negotiations, and their rate of plant closures as well as plant creations decreased. 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).[4] While some later 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.

For this analysis, we extend the CEO data from Gibbons and Murphy (1992) and 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. We restrict all analyses to CEOs appointed before the enactment of the anti-takeover laws to address the concern that their passage altered the selection of CEOs. Using a hazard regression model and controlling for CEO age, time trends, industry affiliation, and firm location, we find that anti-takeover laws significantly increase the life expectancy of incumbent CEOs. One additional year under lenient governance lowers mortality rates by four to five percent for an average CEO in the sample. Non-linear specifications indicate life expectancy gains as large as nine percent per year in the initial years of lenient governance, with incremental effects falling to zero within five years of initial exposure.

These results are robust to an array of alternative specifications, including models with CEO birth-cohort and appointment-year fixed effects, and alternative subsampling and classifications of anti-takeover laws that account for other firm or state 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).

The estimated effect sizes are large. For a typical CEO, the effect of the anti-takeover laws is equivalent to making the CEO two years younger. The effect size is even larger if we use life tables instead of the estimated CEO age effects for the comparison of takeover protection and increasing age in terms of mortality hazard. 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

[4] Other experts at the time made similar arguments. 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."

reduction by ten years and more (General 2014, Jha et al. 2013).

We find no evidence of a compensating differential in the form of lower pay for CEOs who are protected from hostile takeovers.[5] This may indicate that not all parties fully account for the health implications of job demands, though we 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).

Consistent with anti-takeover laws changing CEOs' perceptions of job demands, we find that protected CEOs remain on the job for longer. However, the increase in longevity estimated before is unlikely to arise from prolonged tenure because our nonlinear estimates imply that prolonged exposure (resulting from prolonged tenure) does not lead to incremental survival gains. We also note that our estimations that use an *indicator* for anti-takeover law exposure imply similar effect sizes as those which allow for an endogenous length of exposure. Nevertheless, we verify that our estimates are robust to using CEOs' *predicted* rather than actual anti-takever law exposure, where we predict exposure using only variables realized before the passage of the laws, such as CEO age and pre-law tenure, thereby purging the prediction of any endogeneity due to the laws themselves.

In the second part of the paper, we consider industry distress shocks. Typically defined based on a 30% median firm stock-price decline over a two-year horizon, industry shocks have been used to study effects on, e. g., market concentration, creditor recoveries, and employee exit (Opler and Titman 1994, Acharya et al. 2007, Babina 2020). In our analyses, they constitute a separate and oppositely-signed change in job demands compared to anti-takeover law passage. About 40% of CEOs in our sample experience at least one such industry-wide downturn during their tenure. We find that distress exposure significantly increases a CEO's mortality risk. The estimated mortality effect is equivalent to increasing age by 1.5 years, and comparable to serving three fewer years under lenient monitoring.

In the final part of the paper, we document more immediate health implications of industry crises in the form of visible signs of aging in the faces of CEOs. We utilize machine-learning algorithms designed to estimate a person's *apparent age*, i. e., how old a person looks rather than a person's biological age, from Antipov et al. (2016). 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, roughly comparable to the certification effect of a first-tier publication in other academic fields.

[5] The analysis of pay builds on Bertrand and Mullainathan (1998) and predictions in Edmans and Gabaix's (2011) CEO market model.

We collect a sample of 3,086 pictures of the 2006 *Fortune 500* CEOs from different points during their tenure to estimate differential apparent aging in response to industry-level exposure to the financial crisis. Using a difference-in-differences design, we estimate that CEOs look about one year older in post-crisis years if their industry experienced a severe decline in 2007-2008 relative to 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 paper adds to several strands of literature. A recent literature sheds light on CEOs' demanding job and time requirements. Bandiera et al. (2020) obtain weekly diaries of 1,114 CEOs of manufacturing firms and document long hours that often include six- and seven-day workweeks. Porter and Nohria (2018) record an even more intense schedule for 27 CEOs of multi-billion dollar firms. Bandiera et al. (2018) document that professional CEOs' job is especially taxing as they work longer hours and consume less leisure than family CEOs.

Few papers explicitly study health outcomes among CEOs. 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 costs for CEOs, both in terms of the mortality and in terms of visible aging. The only prior work on executives' health outcomes is Yen and Benham (1986), who calculate the age-adjusted mortality rates of 125 executives in the banking industry and compare them with 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 novel evidence to the literature on the health effects of stress, socioeconomic status, and financial insecurity. In health and labor economics, stress has been proposed as an explanation for 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), and access to health care (Koijen and Van Nieuwerburgh 2020); and early-life health disparities (Camacho 2008, Black et al.

2016). Stress is also implicated in the intergenerational persistence of poverty (Aizer et al. 2016, Persson and Rossin-Slater 2018, East et al. 2017). To the best of our knowledge, the only paper that relates quasi-random increases in job demands directly to health outcomes is Hummels et al. (2016), who document the negative impact of trade shocks on workers' stress, injury, and illness. Turning from the general or poorer populations to wealthier populations, income appears to play a small role in health disparities among the already-wealthy, while social factors, such as the prestige associated with a Nobel prize or a political election may be protective (Rablen and Oswald 2008; Cesarini et al. 2016; Borgschulte and Vogler 2019).

Third, we add to the corporate-governance literature on the impact of anti-takeover laws on firm productivity starting from Bertrand and Mullainathan (2003). Giroud and Mueller (2010) show that the effect of business combination (BC) laws on performance is concentrated in non-competitive industries. After the adoption of BC laws, managers undertake value-destroying actions that reduce their firms' risk of distress (Gormley and Matsa 2016), patent count and quality decrease (Atanassov 2013), and managers reduce their stock ownership (Cheng et al. 2004). The mechanisms suggested in these papers work through incentives; we are the first to quantify their long-term health consequences.

Finally, we contribute to the literature on industry shocks and financial distress. Prior work has documented their economic and financial consequences for firm performance (Opler and Titman 1994), creditors (Acharya et al. 2007), brain drain and entrepreneurship (Babina 2020), and CEO pay and turnover (Bertrand and Mullainathan 2001, Garvey and Milbourn 2006, Jenter and Lewellen 2015). Related to our setting, Engelberg and Parsons (2016) document a link between stock-market crashes and hospital admissions, especially for anxiety and panic disorders. Our paper offers complementary evidence that distress experiences impose long-term health costs, even for successful and wealthy individuals.

In the remainder of the paper, Section 2 describes the data and discusses the identifying variation. We present the results pertaining to life expectancy and exposure to anti-takeover laws in Section 3, and exposure to industry-wide distress shocks in Section 4. Section 5 presents the results on apparent aging and distress shocks. Section 6 concludes.

2. CEO DATASETS AND VARIATION IN CEO JOB DEMANDS

2.1. *CEO Data for Longevity Analyses*

The initial dataset consists of the universe of CEOs included in the *Forbes* Executive Compensation Surveys from 1975 to 1991, which extends the data in Gibbons and Murphy

(1992).[6] 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 year given the timing of anti-takeover laws (see Section 2.3), in line with prior studies,[7] but will consider a more recent sample for the visible aging analysis later. 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 (iii) the date of death if the CEO has passed away. 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. To ensure that we have identified the correct person, 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 their alive status, such as newspaper articles or websites that list the CEO as a board member, sponsor, donor, or chairman or chairwoman of an organization or event.[8] 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 test and confirm that the availability of birth and death information is not correlated with incorporation in a state that passed a BC law.[9]

To measure CEOs' exposure to anti-takeover laws, we identify the historical states of incorporation during CEOs' tenure. Since CRSP/Compustat backfills the current state of incorporation, we access its 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 firms' 10-Ks and other SEC filings, legal documents, and news articles to identify the correct historical state of incorporation. Overall, we correct the state of incorporation in 169 cases (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,

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

[7] 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, cf. Section 3.5 and Appendix B.

[8] 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 (by October 1st, 2017), had it occurred. Our results are robust to ending our sample in 2010 (Section 3.5 and Appendix B) and to restricting the sample period for CEOs classified as alive as of 10/2017 to end in 01/2010.

[9] We estimate $I(Found_i) = \beta_0 + \beta_1 \times I(BC\ State_i) + \eta_j + \delta_k + \varepsilon_i$, where η_j represents state-of-headquarters fixed effects and δ_k FF49-industry fixed effects on the sample with available state-of-incorporation information (2,514 out of the initial sample of 2,720 CEOs). We obtain $\widehat{\beta}_1 = 0.0142$ ($p = 0.627$).

we are able to identify the historical state of incorporation for 2,209 CEOs.

We collect tenure information for all sample CEOs to fill the 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 on executive changes in our sample firms. When the exact month of a CEO transition is missing, we use the "mid-year convention" motivated by the relatively uniform distribution of CEO starting months in Execucomp (Eisfeldt and Kuhnen 2013). We further restrict the sample to CEOs whose firm was included in CRSP during the time of their tenure (1,900 CEOs).[10] Finally, we address selection concerns revolving around CEO "types" responding to the more lenient BC law governance. For example, it would confound the analysis if less resilient managers, i. e., those more prone to health ailments, became more likely to seek the CEO position. 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.

2.2. *CEO Data for Apparent Aging Analysis*

To study visible signs of aging in CEOs' faces, we collect pictures of CEOs of the 1,000 firms included in the 2006 *Fortune 500* list. This analysis uses a more recent sample since picture availability and quality have substantially improved over time. We focus on the 2006 CEO 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 in order to compare CEOs' apparent age to their true age. 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,[11] for a total of 3,086 pictures.

2.3. *Variation in CEO Job Demands*

We exploit two sources of variation in CEO job demands, the passage of state-level anti-takeover laws and industry-wide distress shocks.

[10] 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.

[11] Among the sixteen non-White CEOs, seven are African-American, two are Hispanic or Latinx, and seven are Asian (including Indian). We collect this information through Google searches and Wikipedia.

Anti-takeover Laws. Anti-takeover statutes increase the hurdles for hostile takeovers. After the first-generation anti-takeover laws were struck down by courts in the 1970s and early 1980s, states started passing second-generation laws in the mid-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 first focus on BC laws as the most potent type of statutes, but will return to the other types of laws later (in Section 3.5). 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.

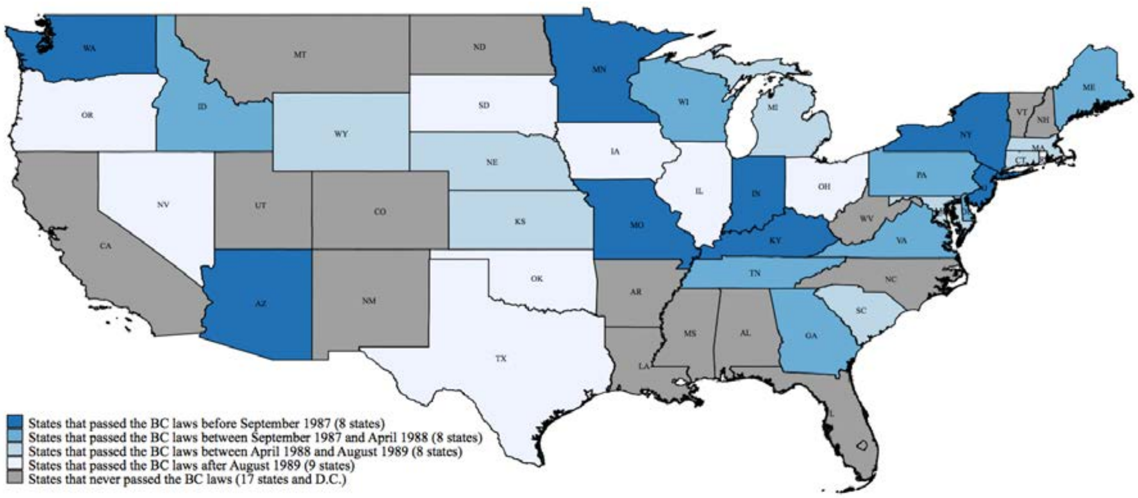

FIGURE 1.—Introduction of Business Combination laws over time. *Notes:* This figure visualizes the distribution of business combination law enactments over time. In total, thirty-three states passed a BC law between 1985 and 1997. The map omits the states of Alaska and Hawaii, which never passed a BC law.

Figure 1 visualizes the staggered introduction of BC laws across states.[12] The map illustrates the variation across both time and states as a source of identification: 33 states passed a BC law between 1985 and 1997, with most laws being passed in 1987-1989. An advantage of using anti-takeover laws as identifying variation is that these laws applied 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.

Industry-Wide Distress Shocks. Distress shocks induce a shift in job demands in the

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

opposite direction than anti-takeover laws, and are of a less permanent nature. Thus, they constitute a useful alternative approach to analyzing the health consequences of a CEO's job demands. In the spirit of 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] Following prior work, we use 3-digit SIC classes to measure industry affiliation and, as with state of incorporation, rely on historical SIC codes for the firms in our sample.

2.4. *Summary Statistics*

Table I presents the summary statistics of our main sample for the longevity analyses, consisting of 1,605 CEOs. (All variables are defined in Appendix A.) The median CEO in our sample was born in 1925, became CEO at age 52, and served as CEO for nine years. There is relatively large heterogeneity in tenure, with an interdecile range of 17 years. Non-integer values result from CEOs not starting or ending their tenure or stepping down at the end of the year. 71% 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. Conditional on being shielded by a BC law, the median CEO serves 4.4 years under the BC law regime. BC law experience is calculated at (up to) daily precision levels and, similar to tenure, can take non-integer values. For example, Delaware's BC law was adopted on 2/2/1988. A CEO's BC exposure in 1988 would then be calculated as $BC_{i,1988} = \frac{365-\text{doy}(2/2/1988)}{365} = 0.92$. 40% of CEOs experience industry-wide distress during their tenure.

We provide additional summary statistics in Appendix-Table B.1. Panel A splits the sample into CEOs with no BC exposure ($N = 980$), with positive but below-median exposure ($N = 320$), and with higher exposure ($N = 305$). Some of the observed differences across sub-groups are suggestive of the effects we have in mind. For example, 82% of CEOs without BC exposure have passed away, but only 68% (38%) of CEOs with below-median (higher) exposure. However, it is also the case that fewer CEOs from the beginning of

[13] Sections 4 and 5 also discuss more restrictive distress definitions, exploring specific recession periods or using industry returns in conjunction with sales growth.

our sample—who are more likely to have passed away, including at higher ages—became protected by the laws during their tenure, as BC laws were only introduced starting in 1985. In Section 3.2, we will discuss cohort-specific splits that directly account for such differences. Panel B provides information on the most common Fama and French (1997) 49 industries and most common states of incorporation. CEOs are frequently employed by firms in the banking, utilities, and retail industry. Across BC exposure sub-groups, there are only few differences in industry frequencies. We note that we include industry fixed effects in all analyses. Consistent with prior literature, the most common state of incorporation is Delaware in all CEO sub-groups. Other common states include New York and Ohio.

3. CORPORATE MONITORING AND LIFE EXPECTANCY

3.1. *Empirical Strategy*

Our main analysis uses 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 variation in CEOs' exposure to more lenient governance through the passage of BC laws in two ways. First, we use an indicator of exposure to the BC law treatment and estimate

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

where $\boldsymbol{X}_{i,t}$ is a vector of control variables. In our main specifications, it includes time trends (or fixed effects), CEO age, firm location and industry effects. We later present robustness specifications with birth-cohort or appointment-year fixed effects, to account for the fact the BC laws disproportionally affected more recent CEO cohorts. $I(BC_{i,t})$ is an indicator equal to 1 if CEO i has been exposed to a BC law by year t. The proportional hazard framework assumes that mortality risk shifts permanently at the passage of a BC law for an exposed CEO. Below, we investigate departures from the proportional hazard assumption by allowing for a non-linear effect. Note that, when a CEO steps down, the value of the BC law indicator remains constant from then on at its value at departure.

Second, to capture intensity of exposure, we replace the indicator $I(BC_{i,t})$ with a measure $BC_{i,t}$ that counts the exposure length in years until year t:

$$\lambda(t|BC_{i,t}, \boldsymbol{X}_{i,t}) = \lambda_0(t)\ \exp(\beta BC_{i,t} + \boldsymbol{\delta}' \boldsymbol{X}_{i,t}). \tag{2}$$

We also implement two refined measures of exposure length. First, we refine the linear dose-response function represented by $BC_{i,t}$ and separate the effects of initial and later years of exposure to lenient governance on survival rates. This refinement accounts, for example, for CEOs adapting to the new business environment and exhausting their opportunities to adjust their activities. The second refinement addresses the concerns that a CEO's remaining tenure after BC law passage might (a) reflect unobserved CEO characteristics and (b) be affected by the introduction of the laws. Directly controlling for realized tenure would introduce endogeneity, and the estimates would suffer from the "bad control" problem (Angrist and Pischke 2008).[14] Instead, we estimate a hazard model using a CEO's predicted, rather than true, length of exposure, where the prediction model only uses information from prior to the BC law passage. Additionally, we test the robustness of our results to estimating simple linear probability models instead of the hazard model.

3.2. *Within-Cohort Comparisons of Means and Graphical Evidence*

Before presenting the main results, we provide simple within-cohort comparisons of means as well as graphical evidence on the mortality effects of variation in governance regimes.

Table II presents the proportions of deaths as well as the average age at death, conditional on having passed away, in a two-way split by CEOs' birth cohort and BC law exposure.[15] The table reveals that, for all but the tail cohorts, CEOs with BC law exposure have lower mortality than those with no exposure. Furthermore, across all cohorts, the average age at death is higher for CEOs with BC law exposure. The average age difference, weighted by the total number of deaths in each cohort, is 3.76 years. Hence, the raw means reveal a sizeable and systematic difference between CEOs with and without anti-takeover protection.

Figure 2 plots the Kaplan-Meier survival graphs, also split by cohorts and by exposure. The non-parametric 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. In the plots, the vertical axes show the survival rate, and the horizontal axes the time elapsed (in years) since becoming CEO.

Panel (a) compares the survival of CEOs 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.[16] Two results emerge. First, the survival patterns of the 1970s

[14] While the estimates remain similar with the tenure control, including the effect being concentrated in the early years of treatment, it is unclear how to sign the resulting bias. We thus follow the general recommendation to exclude the "bad controls" from the estimation.

[15] We thank our discussant, Kevin J. Murphy, for suggesting this table.

[16] For the 1970s cohorts, maximal elapsed time since our sample start is $t = 47.75$ (time elapsed between 1/1/1970 and the censoring date, 10/1/2017). Similarly, for the 1980s cohorts, maximal elapsed time is $t = 37.75$. We restrict the graph to periods when at least 30 CEOs in either cohort group are uncensored,

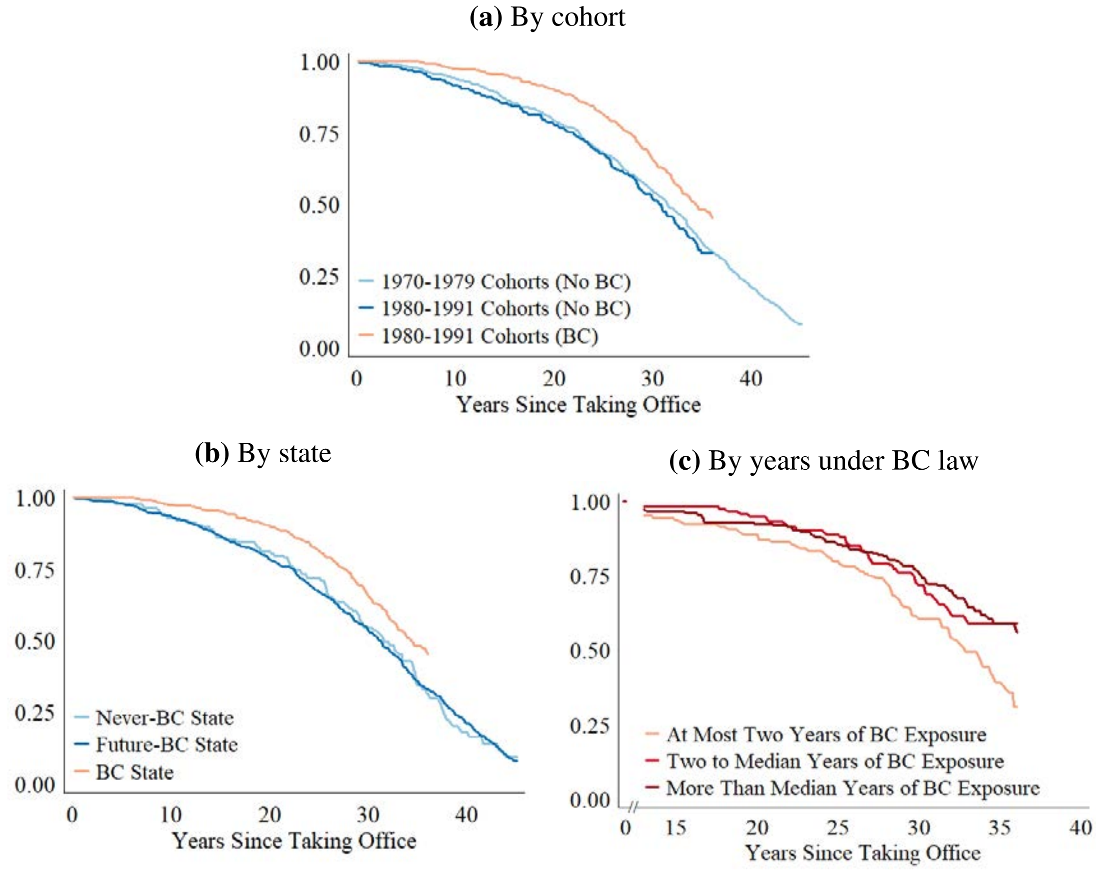

FIGURE 2.—Kaplan-Meier survival estimates. *Notes*: This figure shows Kaplan-Meier survival plots. The vertical axis shows the fraction of CEOs who are still alive. The horizontal axis reflects time elapsed (in years) since a person became CEO. Panel (a) compares the survival of CEOs starting in the 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 (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. Panel (c) zooms in on CEOs with BC exposure, and plots survival separately for CEOs with positive but at most two years of BC exposure (orange), with two to median exposure (red), and with above-median exposure (brown). Survival estimates in Panel (b) are adjusted to the 12 years median tenure of CEOs with BC exposure.

and 1980s cohorts without BC exposure are remarkably similar, allaying concerns that our results pick up *general* changes in survival patterns between the 1970s and 1980s. Second, consistent with our hypothesis, the survival line for the 1980s cohorts with BC exposure is visibly right-shifted compared to the No-BC-cohorts. 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 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 results will include location fixed effects and are robust to using state of incorporation fixed effects.

Panel (c) zooms in on the CEO group with BC exposure and explores potential non-linearities in the insulating effect of more lenient governance on lifespan. Specifically, we plot survival rates separately for three sub-groups, formed as (i) at most two years of BC exposure, (ii) more than two years of but at most the median BC exposure (4.4 years), and (iii) more than median BC exposure. We adjust the estimated survival functions to a tenure of 12 years, which is the median tenure of CEOs with BC exposure to ensure that we do not conflate the independent effect of tenure with the direct effects of corporate governance. Comparing CEOs with low BC exposure up to 2 years to those with more exposure, we observe higher (right-shifted) survival rates for the latter groups. However, there is no *further* rightward shift comparing CEOs with medium and high BC exposure. This suggests that the health benefits from insulation against takeover threats increase initially, but the *incremental* effects might taper off eventually.

The comparisons of means and survival plots offer first evidence that serving under more stringent corporate governance is associated with adverse consequences in terms of life expectancy. Our hazard model based analysis below formalizes the observed patterns.

3.3. *Main Results on Business Combination Laws*

Table III shows the hazard model results on the relationship between BC laws and CEOs' mortality rates, based on our main estimating equations (1) and (2). In Columns (1) through (3), 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. These estimates are akin to the group-level divergence in survival reported in Figure 2(a). In Columns (4) through (6), we estimate a linear (in hazards) effect in years of exposure to more lenient corporate governance. All regressions

explaining the slightly differential ends of the survival lines (after 36 and 45 years, respectively).

control for a CEO's age and include firm location fixed effects. Following Gormley and Matsa (2016), we assign location fixed effects based on headquarters as most firms' main operations are in the state of its headquarters. These fixed effects thus absorb state-level characteristics, such as general business conditions, pollution, and eating habits, to the extent that these are time-invariant. In robustness checks, we verify that our main results remain unaffected when we instead include state of incorporation fixed effects (cf. Section 3.5). In the specifications of columns (1) and (4), we include linear controls for time trends and CEO age; in columns (2) and (5), we add industry fixed effects, using the Fama and French (1997) classification of firms into 49 industries; and in columns (3) and (6), we include year fixed effects instead of the linear time controls. To address any concerns regarding the use of fixed effects in non-linear models, we also estimate the model with only linear age and linear year as controls, with very similar results.[17] All coefficients are shown as hazard ratios so that a coefficient smaller than one indicates that the risk of failure (death) decreases with positive values of that variable. We cluster standard errors at the state-of-incorporation level, given that the BC laws applied based on the state of incorporation (Abadie, Athey, Imbens, and Wooldridge 2017). As pointed out in Section 2.1, we restrict the sample to CEOs who were appointed prior to the enactment of a BC law to alleviate selection concerns.

In the specification of columns (1) and (4), the estimated hazard ratio on the BC indicator is 0.764, and the ratio on the BC law exposure is 0.955, both significant at 1%. The indicator captures the total effect of BC exposure, while the cumulative exposure measure is the effect of an additional year of exposure: a one-year increase in exposure to more lenient governance is estimated to reduce a CEO's mortality risk by 4.5%. For a CEO with a typical BC law exposure, both measures imply very similar effects on longevity.[18]

The results do not change when we make comparisons within industry or include a more flexible control for time. The inclusion of industry fixed effects in columns (2) and (5) addresses the possibility that certain industries are differentially incorporated in BC-law states. The resulting estimates of the hazard ratio on BC law exposure are almost unchanged, 0.769 and 0.958, both significant at 1%. Similarly, year fixed effects in column (3) and (6), instead of the linear time control, have virtually no effect on the estimates.

Turning to the interpretation of the control variables, the linear time control is close to one and insignificant, suggesting no general time trends in the survival of CEOs over

[17] Estimates are 0.776 for $I(BC)$ and 0.955 for BC, which are very close to the estimates in the Table III.

[18] The cumulative measure estimates a 17-18% shift in the mortality hazard associated with the median BC exposure of 4.4 years ($\exp(4.4 \times \ln(0.955)) = 0.817$), very close to the 22-24% shift estimated in the BC indicator measure.

the sample period. The effect of *Age* is significantly positive, reflecting that older people have a higher estimated risk of dying.[19] One potential concern is that the treatment group is younger on average and is more likely to still be alive, and that the model may have difficulty separating the effect of age from treatment in older age ranges. To address this, we test and confirm the robustness of our results to including birth-year fixed effects, CEO appointment-year fixed effects, and age-cohort interactions (see Section 3.5 for details) and, alternatively, higher-order age terms. Across these robustness tests, the estimated hazard coefficients remain significant and are remarkable stable in magnitude.

Economic Significance. One way to evaluate the magnitude of the estimated effect on longevity is relative to other predictors of CEO life expectancy in our hazard model, in particular CEO age. This "in-sample" approach has the advantage that it is directly based on data from the sample CEOs. The estimated effect of age on death hazard from column (3) is 1.124, i. e., a 12.4% increase per year of age. This means that the life-extending effects of BC law protection corresponds to the effect of a two-year shift in CEO age.[20]

Alternatively, we can compare our estimated hazard with mortality statistics of the general U.S. population, acknowledging that statistics derived for high SES groups would be ideal. 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). The median exposure to lenient governance of 4.4 years pushes this rate down to 1.119%, which is roughly the mortality rate of a male born in 1925 at age 54, i. e., when three years younger. The implied three- year gain in remaining life expectancy is in fact close to the difference in age at death using the simple within-cohort comparison of treated and non-treated CEOs in Table II for the 1921-1925 cohort.

Yet 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 gain in life expectancy from BC law exposure is thus twice as large as the gain from not smoking in the first three decades of one's life.

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

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[19] We note that the Gompertz (1825) "law of mortality," i. e., the empirical regularity that the risk of dying follows a geometric increase after middle age, motivates a linear age term (Olshansky and Carnes 1997).

[20] Using equation (1) from the Cox (1972) estimation to calculate how much older a CEO needs to be to offset the estimated BC effect of 0.777, we solve $(\frac{1}{1.124})^x = 0.777$ and obtain $x = 2.16$.

3.4. *Alternative Specifications*

Nonlinear Effects. The survival plots in Figure 2(b) suggested that the incremental effects of a more lenient governance regime on survival rates diminish over time. The first few years of BC law exposure appear to have the largest effect, possibly because CEOs adapt to the new business environment and exhaust their opportunities to adjust their activities.

To examine this possibility empirically, we estimate a modified version of (2) where we split the cumulative BC exposure variable into below- and above-median exposure, $BC_{i,t}^{(\text{min-}p50)}$ and $BC_{i,t}^{(p51\text{-max})}$, with above-median exposure variable picking up incremental exposure, in addition to initial exposure.[21]

Columns (1) to (3) in Table IV present the results, with controls and fixed effects as before. Across columns, the hazard ratio on below-median BC exposure is strongly significant (at 1%) and ranges from 0.908 to 0.916. These estimates imply that initial insulation from market discipline yields substantial reductions in mortality risk, corresponding to a 9% higher survival rate. By contrast, the coefficient on above-median BC exposure is close to one and insignificant. Thus, in line with the survival plots, the estimated survival gains are concentrated in the first few years of exposure to reduced monitoring.

Predicted Length of Exposure. Our second alternative specification uses CEOs' predicted rather than true BC-law exposure. This estimation purges the per-year estimates of possible endogeneity in the length of exposure. We note that this concern does not apply to the indicator strategy, and thus the endogeneity concern does not threaten our main findings in Table III, but merely the magnitude of the per-year estimates. 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:

$$RemainTenure_{i,t} = \boldsymbol{X}'_{i,t}\boldsymbol{A} + e_{i,t}. \tag{3}$$

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 (3) to construct CEOs' predicted exposure to

[21] 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.

BC laws,

$$\widehat{BC}_i^* = I(BCLawPassed_{s(i),t}) \times \widehat{RemainTenure}_{i,t^*}, \tag{4}$$

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

$$\lambda(t|\widehat{BC}_{i,t}, X_{i,t}) = \lambda_0(t)\exp\{\beta\widehat{BC}_{i,t} + \boldsymbol{\delta' X}_{i,t}\} \tag{5}$$

Columns (4) to (6) in Table IV present the results, with controls and fixed effects as in Table III. Since this approach involves a generated regressor, we use the block bootstrap method (a block is a state of incorporation cluster) with 500 iterations for the standard errors.

The results corroborate our baseline findings. Predicted BC exposure is estimated to significantly affect CEOs' mortality rates. The estimated hazard ratios range from 0.943 to 0.952 and are very similar to those in Table III. While the bootstrapped standard errors are larger than those in Table III, 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 (3) of Table IV, $\exp(\ln(0.952)/1.21) = 0.960$. Point estimates from a non-linear predicted-exposure model are also similar in magnitude to those reported in Columns (1) to (3) of Table IV, though less precisely estimated.

3.5. *Robustness Tests*

Our results are robust to a series of additional tests. For brevity, we only provide a brief overview of these tests here and present a detailed discussion in Appendix B.

CEO Cohorts. We estimate various alternative specifications involving cohort effects. These specifications address concerns arising from more recent CEOs being shielded more often by BC laws. Our results are virtually unchanged when we include birth-year fixed effects (Panel A of Appendix-Table B.2). Relatedly, the results are very similar when we keep the year fixed effect setup but allow the effect of age on mortality to vary across birth cohorts (Panel B of Appendix-Table B.2). Additionally, our results are unchanged when adding appointment-year fixed effects to the model (Panel C of Appendix-Table B.2), and when dropping CEOs who stepped down significantly before the passage the BC laws (Appendix-Figure B.2).

Other Specifications and Sample Choices. Our results are robust to including additional CEO and firm controls, in particular CEO pay and firm size measures (Panel A of Appendix-Table B.3), and to specifications with state of incorporation fixed effects (Panel B of Appendix-Table B.3). They are also robust to different censoring date choices (Appendix-Figure B.3).

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 B.4). 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 B.5 and B.6). Additionally, the results are robust to data cuts based on state of incorporation and industry affiliation (Appendix-Table B.7).

Linear Probability Model. To address any concerns regarding the usage of the hazard model, we estimate a linear probability model (LPM) at the CEO level. The dependent variable is an indicator variable for whether a CEO has passed away by October 1st, 2017, and the main independent variable of interest is an indicator for BC exposure during a CEO's tenure (Appendix-Table B.8). Even though the LPM discards all time-series variation, as it does not take into account how soon CEOs pass away after being appointed, the results support the hazard analysis. The estimated coefficient on the BC exposure indicator is negative and significant at conventional levels, indicating that BC-protected CEOs are less likely to die before the censoring date. In terms of magnitudes, the effect of being protected by BC laws on survival likelihood corresponds to that of a two and a half year increase in CEO age at appointment in the LPM, similar to the hazard model.

3.6. *Intermediate Outcomes: Tenure, Retirement, and Pay*

In addition to health benefits, we observe several other sources of private benefits, namely pay and tenure as CEO. These outcomes are of interest themselves and may also provide insights regarding why CEOs live longer when facing a less stressful work environment.

We begin with an analysis of CEO tenure. Theory does not provide a strong 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 the hazard model from the survival analysis. The results in columns (1)-(2) of Panel A in Table V indicate that BC law treatment, $I(BC)$, decreases the separation hazard by 20-21 percent, but the effect halves in magnitude and becomes statistically insignificant after controlling for year effects (column 3), with standard errors nearly doubling. In the specifications using the length of exposure variable *BC* (in columns 4 to 6), the estimated separation hazard falls by 4 to 9 percent.

Further analyses of CEOs' age at the end of their tenure suggest that increases in tenure—if there are any—would be driven by fewer CEOs stepping down in their 50s and early 60s. Appendix-Figure B.4 plots the retirement hazard separately for CEOs with and without BC law exposure.[22] 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 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 business combination laws.

Longer tenure (or delayed retirement) as a result of anti-takeover insulation—if there is any such effect— 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 3.4 on nonlinearities point to initial exposure effects, with prolonged exposure (from prolonged tenure) having no incremental impact on life expectancy. Consistent with these arguments, we find quantitatively very similar longevity effects of BC exposure when we focus on CEOs who leave office at or shortly before age 65.[23]

We next turn to CEO pay. Here, too, the theoretical prediction is unclear, as also noted by Bertrand and Mullainathan (1998). On the one hand, a model of compensating differentials

[22] CEOs may continue to work after they separate, however, we find few (34 in total) cases in which a CEO steps down and then becomes CEO at another firm in our sample.

[23] The finding in Appendix-Figure B.4 that a disproportional fraction of CEOs in our sample steps down near this retirement age is consistent with the evidence in Jenter and Lewellen (2015).

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.[24] 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.[25] Given a baseline 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 6 in Table III) 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.[26]

With these calibrated effects in mind, Panel B of Table V 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):

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

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

[25] 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 $VSL_{CEO} = \exp(0.5 \times (\ln(\$1.3\text{m}) - \ln(\$26\text{k})) + \ln(\$6.7\text{m})) = \$47.3\text{m}$.

[26] The calculations are based on an average length of BC exposure of 5.68 years (Table I), 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 $(-24.77/5.68) \times (4.1\% \times 1.366\% \times \$47.3\text{mn})/\$1.3\text{mn} = -9\%$.

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 around 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.

Taken together, the evidence speaks against a compensating reduction in pay, but is instead suggestive of additional rents (higher pay). Combined with the evidence on an increase in tenure, the estimates imply that lifetime compensation rises as a result of exposure to the laws. 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 also casts doubt on whether all parties fully account for the health implications of different governance regimes.

4. INDUSTRY-WIDE DISTRESS SHOCKS AND LIFE EXPECTANCY

Our second source of identification for variation in CEOs' job demands exploits the occurrence of industry-wide distress shocks. We will utilize this source of identification both for an alternative approach to estimating mortality effects and for the apparent-age estimation.

For the mortality analysis, we continue to use the CEO sample collected for the BC analysis and described in Table I, which allows us to compare effect sizes across the two approaches. We also retain the key features of the BC law analysis, including the hazard specification, control variables, and primary robustness checks, albeit with a new independent variable: the experience of industry-wide distress shocks. As discussed in Section 2.4, the industry shock definition is based on observing an industry-wide 30%-decline in equity value over a two-year horizon. In our sample, 648 out of the 1,605 CEOs, or 40% of CEOs, witness at least one period of industry distress during their tenure (see Table I). However, fewer than twenty percent of CEOs experience two or more industry shocks, and fewer than ten percent experience three or more. Given that industry shocks are infrequent, we specify industry distress exposure as an indicator variable; any cumulative or incremental effects would be estimated off of very few and long-serving CEOs.

We use the Cox (1972) proportional hazards model to estimate a modified version of (1):

$$\lambda(t|BC_{i,t}, X_{i,t}) = \lambda_0(t)\ \exp(\beta Industry\ Distress_{i,t} + \boldsymbol{\delta}' \boldsymbol{X}_{i,t}) \tag{6}$$

where $Industry\ Distress_{i,t}$ is an indicator equal to 1 if CEO i has experienced distress by year t. In addition to the controls and fixed effects from Table III, we also control for BC law exposure, given our evidence that these laws significantly affect a CEO's lifespan. We cluster standard errors at the three-digit SIC code level, at which industry shocks are defined.

Table VI reports the estimation results. Across specifications, the estimated hazard ratios of *Industry Distress* reveal substantial adverse effects of industry-shock exposure on CEOs' long-term health. The coefficient estimates are very similar across models, ranging from 1.179 to 1.190, and are significant at the 5% or 1% level.

The estimated coefficients on the control variables are similar to above. The coefficients on *Age* continue to be positive, with hazard ratios ranging from 1.115 to 1.125. The hazard ratios on *Year*, in the linear time controls specifications, are again close to 1 and insignificant or only marginally significant, indicating no strong time trends in mortality.

The estimates point to meaningful effect sizes. Applying the approach from Section 3.3 to the hazard ratio estimates from the most conservative specification in column (3), 1.179 for *Industry Distress* and 1.125 for *Age*, we calculate that the effect of industry distress on mortality is equivalent to being 1.4 years older, as calculated by solving $1.125^x = 1.179$. Compared to the estimated effect size of exposure to BC laws, which corresponded to 2.14 years in CEO age, the effect of industry distress is of similar order of magnitude but smaller. The smaller magnitude might reflect the more temporary nature of industry-shock experiences relative to variations corporate-governance regimes. Overall, both approaches to estimating the effect of variation in job demand on CEO mortality reveal substantial effect sizes, also compared to other determinants of longevity and known health risks.

Robustness. We present several robustness checks, with all tables relegated to Appendix C. First, we re-estimate equation (6) with additional CEO and firm controls (CEO pay, firm assets, and employees), mirroring the first robustness check of the BC analysis. The estimated hazard ratios become slightly larger and remain highly significant (Panel A of Appendix-Table C.1). In terms of economic magnitude, it is now equivalent to being 1.6 years older.

Second, we re-estimate the model on an extended sample that includes the 295 CEOs we had dropped from the analysis as they were appointed after the introduction of BC laws. As shown in Panel B of Appendix-Table C.1, the estimated coefficients remain similar. The estimated effect here is equivalent to being 1.1 years older.

We have also explored specific recession periods, such as the 1987 stock-market downturn or the 1981-82 recession. However, fewer than 5% of the CEOs in our sample experienced *either* of these shocks so that we lack statistical power when applying the same methodology of comparing CEOs who did and did not experience an industry downturn in their firms. While the corresponding estimates indicate that CEOs who experienced these shocks tend to have a higher mortality hazard, they are not statistically significant. We have also considered a more restrictive distress definition requiring, in addition, negative industry sales growth, as in the robustness tests in Acharya et al. (2007), and in Opler and Titman (1994) and Babina (2020). This definition classifies fewer than five percent of CEOs in our sample as distressed and substantially increases standard errors. Nonetheless, we estimate similar effect sizes as above, corresponding to an age effect between 0.8 and 1.7 years.

We also estimate a linear probability model instead of the hazard model. As in the LPM of Section 3.5, the dependent variable captures whether a CEO has passed away by the cutoff date. The main independent variable is now an indicator that is 1 if a CEO ever experienced industry distress during her tenure. We include control variables and fixed effects as in the BC-based LPM in Appendix-Table B.8, and following the main Table VI again control for a CEO's BC exposure. The results in Appendix-Table C.2 show that industry-shock exposure is estimated to increase the likelihood of death by 3.6% to 6.2%, with the effect being significant at 5% in the more flexible specifications with age or birth-year fixed effects. The economic significance of the estimates is comparable to those in the hazard models. For example, the midpoint of the estimate range implies an industry-shock effect that corresponds to assuming the CEO position when 1.8 years older, which is similar to the hazard-based age comparisons above. Thus, the LPM approach corroborates the hazard-based findings despite it discarding the time-series variation in CEOs' lifespan.

Differently from the analysis of BC law exposure, we do not implement robustness checks using exposure length, non-linear effects, and predicted exposure. The reason is that the dynamics of industry shocks and selection into the sample of multi-distress-year CEOs complicate the implementation and interpretation of such analyses. Related evidence can be found in the extensive literature on the effects of industry shocks.[27] In the next section, we focus on industry downturns generated by the Great Recession in a difference-in-difference research design which sidesteps issues related to dynamics and sample selection.

All together, the industry-shock analysis provides evidence that significant and unexpected changes in the work environment and job demands of CEOs have strong effects on their health in terms of life expectancy.

[27] See, for example, Jenter and Lewellen (2015) on CEO turnover, including during recessions; and Bertrand and Mullainathan (2001) and Garvey and Milbourn (2006) for the effects of industry performance on CEO pay.

5. INDUSTRY-WIDE DISTRESS SHOCKS AND APPARENT AGING

In the final step of our analysis, we move from the focus on longevity to more immediate, non-fatal manifestations of CEOs' health associated with demanding job environments. Research in medicine and biology has established links between stress and signs of visible aging, such as hair whitening (Zhang et al. 2020) and inflammation, which in turn accelerates skin aging (Heidt et al. 2014, Kim et al. 2013). We ask whether experiencing industry distress translates into accelerated apparent aging of CEOs.

We use a more recent sample of 1,000 firms in the 2006 *Fortune 500* list for this analysis since picture availability and quality have substantially improved over time. It allows us to exploit CEOs' differential exposure to industry shocks during the Great Recession.

5.1. *Apparent-Age Estimation Software*

To analyze visible CEO aging, we make use of recent advances in machine learning on estimating people's age. Most of the earlier age estimation software focused on a person's *biological*, i. e., "true" age (Antipov, Baccouche, Berrani, and Dugelay 2016). Recent research has started to aim at estimating a person's *apparent* age, i. e., how old a person looks. The progress in this area has been made possible by the development of deep learning 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.

For our analysis, we use a machine-learning based software (Antipov et al. (2016)) that has been specifically developed for the problem of apparent-age estimation. This software is the winner of the *2016 Looking At People apparent-age estimation* competition. We provide a detailed discussion of CNNs and the training steps associated with the software in Appendix D and give a brief summary here. The software is based on Oxford's Visual Geometry Group deep convolutional neural network architecture. In a first step, it 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 this apparent age data is particularly important; this step led to the software's largest accuracy improvement (amounting to more than 20%) in the apparent age estimation of the competition data by far (see Table 2 in Antipov et al. 2016 and Appendix D).

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 100×1 vector of probabilities associated with all apparent ages from 0 to 99 years. The apparent age point estimate is derived by multiplying each apparent age with its probability. The software also 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 estimation of the eleven models.

5.2. *Apparent-Age Distribution and Summary Statistics*

We first document the estimated apparent-age distribution and provide summary statistics for our sample of 3,086 collected pictures, described in Section 2.2.

Figure 3 provides graphical evidence of the distributions and correlations of biological and apparent ages. Panel (a) shows that the distributions of apparent and biological ages largely overlap, though the apparent age distribution is somewhat shifted to the left. That is, on average, the software estimates CEOs to look younger than their biological age. This reflects that CEOs have high SES, have better access to health care, can afford healthier food, and live longer than the average population (see Table I, and cf. Chetty et al. 2016). Our results below on the effect of industry shocks on CEO aging do not rely on comparisons between CEOs and the general population but entail solely *within-CEO* comparisons.

Panel (b) shows a scatter plot of CEOs' apparent age against biological age, confirming a high correlation between the two concepts of age, but also a greater mass below than above 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.

Table VII provides the summary statistics for the 463 CEOs for whom we are able to collect at least two dated pictures. On average, we are able to find about 7 pictures of a CEO (conditional on finding at least two pictures). The average CEO is 56.35 years old in 2006, and the mean pre-2006 tenure is 8 years. The majority of CEOs head firms in the manufacturing, transportation, communications, electricity and gas, and finance industries.

To illustrate the proposed channel from industry shocks to aging, we first discuss a specific example. James Donald was the CEO of Starbucks from April 2005 until January 2008, when he was fired after Starbucks' stock had plunged by more than 40% over the preceding year. The top of Figure 4 shows two pictures of Donald: the left one was taken on 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 predicts his 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.

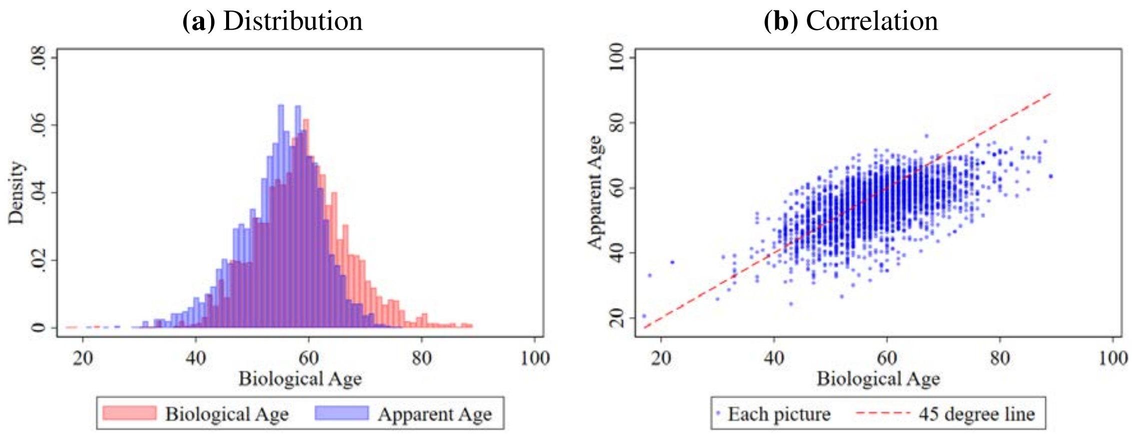

FIGURE 3.—CEO apparent and biological age. *Notes*: The figure plots apparent and biological ages for our sample of 3,086 CEO images. Panel (b) shows CEOs' apparent-age distribution in blue (medium grey), and their biological-age distributions in red (light grey), with the overlapping areas appearing as purple (dark grey). 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.

Turning to the full set of 20 pictures of Donald that we are able to collect for the period 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 and increases to 4.97 years from 2007 on. The bottom half of Figure 4 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 points to concerns one may have regarding picture heterogeneity. 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. More broadly, 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 D help account for such image heterogeneity, we go one step further and

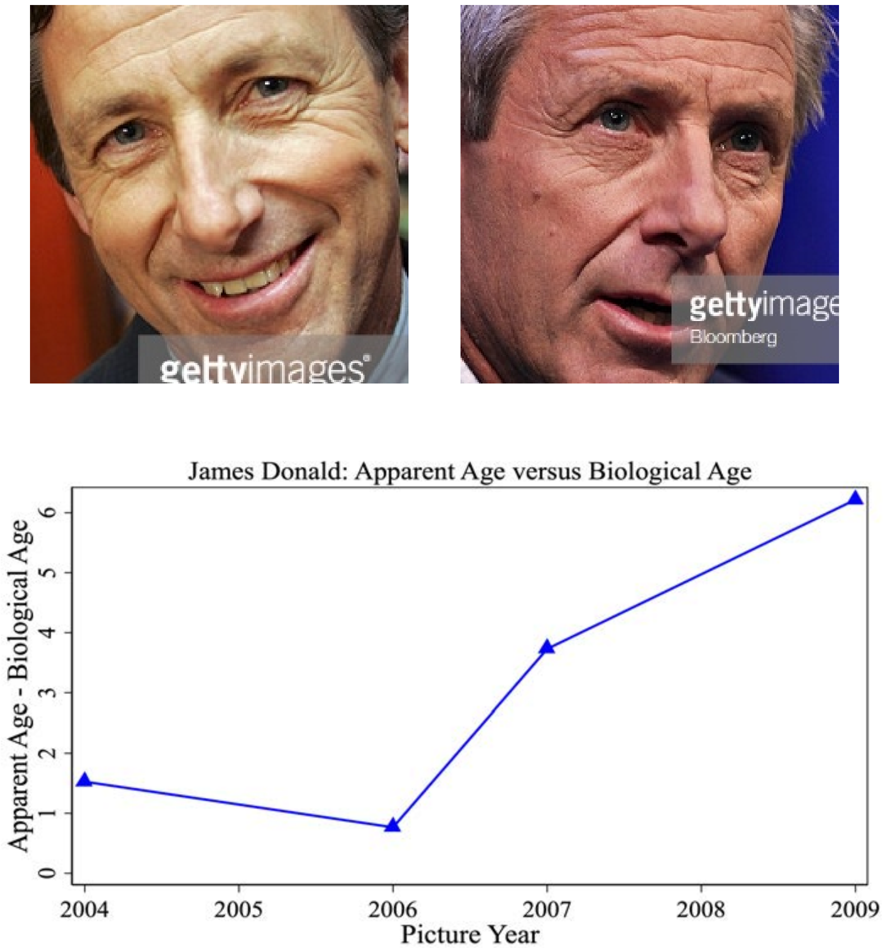

FIGURE 4.—Sample pictures (James Donald, CEO of Starbucks from 2005 to 2008). *Notes*: The first two pictures show James Donald, CEO of Starbucks from 2005 to 2008. Based on data from Ancestry.com, Donald was born on March 5, 1954. The picture on the left was taken on December 8, 2004, that on the right on Monday, May 11, 2009. Biological ages: 50.76 and 55.18 years, respectively. Apparent ages based on aging software: 53.47 and 60.45 years, respectively. The figure at the bottom shows how James Donald's apparent age compares to his true age over time based on 20 pictures collected for the period from 2004 to 2010.

manually assess all pictures along seven dimensions: *logo*, *side face*, *professional*, *magazine*, *natural*, *natural lighting*, and *glasses*. For *logo*, we construct an indicator variable that takes value 1 if there is a logo (for instance, the "gettyimages" logo) on the face in the picture. For *side face*, the indicator is 1 if the CEO in the picture shows a side face instead of front. For *professional*, the indicator takes on 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. For *magazine*, the variable takes on 1 if the picture is from a magazine cover. For *natural*, the variable reflects whether the CEO expects the picture or not, i. e., whether it is natural posing or a photo call. For *natural lighting*, the variable reflects whether the lighting feels natural (with light from all directions) or unusual, e.g., black and white, stage lighting, etc. The variable *glasses* takes on 1 if the CEO in the picture wears glasses.

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

5.3. *Difference-in-Differences Analysis*

We formalize our analysis of job-induced apparent aging in a difference-in-differences design. Following the approach from the mortality analysis, we continue to use three-digit SIC codes and a 30% decline in equity value criterion to identify firms that experienced an industry shock during the financial crisis. This approach classifies 79 out of a total of 149 industries as being in distress 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.

We analyze differences in visible signs of aging between CEOs whose company was in distress during the crisis years versus those whose company was not in distress. To account for 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*, as equal to 1 if the CEO'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. In particular, *Industry Distress* is encoded as 1 for a CEO departing in 2007 and whose firm's industry was distressed in 2008.[28]

We start from plotting the difference in aging trends between the two groups of CEOs in Figure 5. For this graphical illustration, we bin our data into nine 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:

$$\begin{aligned} Apparent\ Age_{i,j,t} = \; & \beta_0 + \beta_1 \, Biological\ Age_{i,j,t} + \sum_{t \in T} \beta_{2,t} \, Industry\ Distress_j \times \mathbb{1}_t \\ & + \boldsymbol{\beta_3'} \boldsymbol{X}_{i,j,t} + \delta_t + \theta_j + \varepsilon_{i,j,t} \end{aligned} \tag{7}$$

where i represents a picture, j represents a CEO, and t represents a time bin. $\mathbb{1}_t$ are time indicators, where the t^{th} indicator is equal to 1 for pictures taken at time t. They are interacted with $Industry\ Distress_j$, so that the interaction is 1 if the firm of CEO j shown in picture i

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

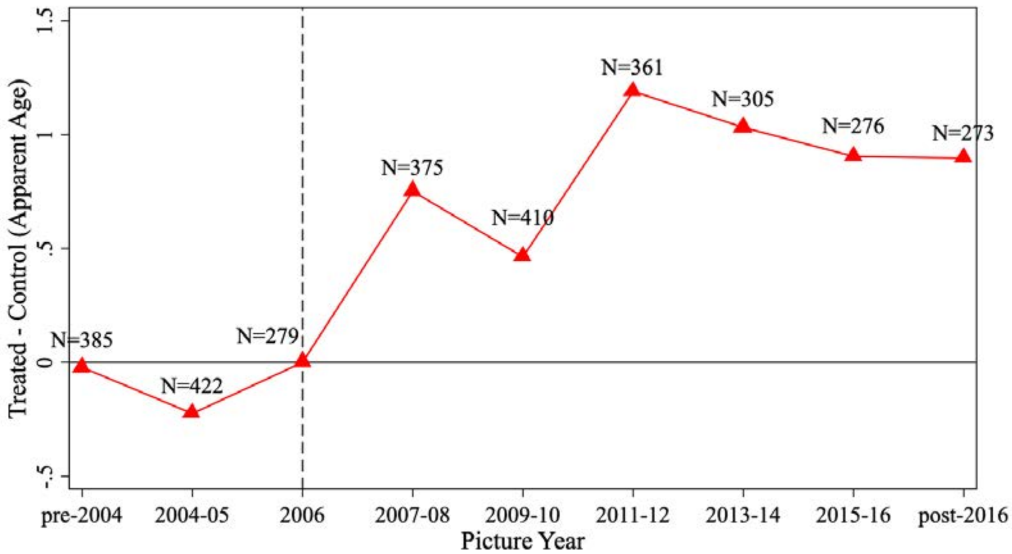

FIGURE 5. —Differences in apparent aging between CEOs with and without industry distress exposure during the Great Recession. *Notes*: This figure depicts the estimated coefficients β_2 of the interaction terms between the time-period indicators and the *Industry Distress* indicator from estimating equation (7), 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.

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 θ_j and time fixed effects δ_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 further (and absorb the main effects of the time-industry shock interaction in the regression). We note that for either of these variables to potentially affect the estimation 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. As discussed above, we additionally include extensive controls for picture setting and characteristics.

Figure 5 plots the estimates 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:

$$\begin{aligned} \textit{Apparent Age}_{i,j,t} = & \beta_0 + \beta_1 \textit{Biological Age}_{i,j,t} + \beta_2 \textit{Industry Distress}_j \times \mathbb{1}_{\{t>2006\}} \\ & + \boldsymbol{\beta_3'} \boldsymbol{X}_{i,j,t} + \delta_t + \theta_j + \varepsilon_{i,j,t} \end{aligned} \tag{8}$$

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 7, and δ_t and θ_j capture the year and CEO fixed effects, respectively. The key coefficient of interest is β_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 VIII 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 during and post-crisis if they experienced industry distress shocks 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, capturing pictures taken between 2007 and 2011 and since 2012, respectively. Our estimates imply a distress-induced apparent aging effect of around 0.8 years over a five-year horizon that increases to about 1.2 years over longer horizons. 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 that our results may be confounded by firms engaging in "picture management" or "CEO appearance management." Such efforts by firms could in principle affect the apparent aging estimates if they are correlated with distress exposure. However, by 2012, more than 50% of CEOs have departed from their position. Arguably, firms have little incentives or ability to manage the appearance of former

CEOs who have stepped down.

We perform a series of additional tests. First, we verify that all results are similar when we estimate the difference-in-differences model on the non-winsorized sample (Panel A of Appendix-Table C.3). Second, we again explore using the more restrictive distress definition that requires negative industry sales growth. One advantage of focusing on the Great Recession period is that around 29% of the CEOs are still classified as experiencing distress under this more stringent definition. This is also reflected in the results, which continue to show economically and statistically significant aging effects (Panel B of Appendix-Table C.3). If anything, the estimated effect of industry distress on apparent aging is slightly larger under the more severe distress definition, 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). Unsurprisingly given the results above, experiencing severe distress is also estimated to significantly affect aging patterns in the long run when splitting the post-period into sub-periods in columns (3) and (4).

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 C.1 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 Panel C of Appendix-Table C.3. The size and significance of the coefficients on the interaction terms remain similar across all columns.

All together, the apparent aging analysis provides additional evidence that increased job demands in the form of industry distress diminish the health of CEOs. Given our other results, the appearance of visual aging may presage a shorter lifespan for CEOs whose industries experienced downturns in the Great Recession.

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

We document that CEOs who serve under stricter governance die significantly earlier. We estimate a four to five percent difference in mortality rates as result of one year of exposure to less stringent corporate governance. The effect is driven by the initial years of reduced monitoring. Incremental health benefits taper off at higher levels of exposure to more lenient governance. In line with these results, we observe significantly reduced life expectancy for CEOs who experienced periods of industry-wide distress during their tenure.

We then show that industry distress is also reflected in more immediate 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. We implement a difference-in-differences design that exploits variation in industry distress exposure during the financial crisis. 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. Mirroring (inversely) the effect of more lenient corporate governance over time, the effect of distress on aging becomes slightly larger over time, increasing to 1.178 years if we analyze pictures from 2012 and afterwards.

In sum, our results indicate that stricter corporate governance regimes—which are generally viewed as desirable and welfare-improving—and financial distress impose significant personal health costs to CEOs. While we lack direct physical or medical measures of heightened stress, the evidence implies that stricter governance and economic downturns constitutes 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—shorter life expectancy and faster aging of the CEOs—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 costs as they progress in their careers and how these costs affect selection into service as a CEO. Are there other dimensions of compensation? Are some high-ability candidates for a Forbes-level CEO career more aware of these consequences than others and select out? Additionally, which jobs and hierarchy levels come with the largest adverse health consequences, also in light of looming financial hardships?

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[a]

	Main CEO Sample					
	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
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
Industry Distress	1,605	0.40	0.49	0	0	1

[a]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. All variables are defined in Appendix A.

TABLE II

MORTALITY BY COHORT AND BUSINESS COMBINATION LAW EXPOSURE[a]

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

[a]This table splits the sample of CEOs by cohort and BC law exposure. Each cell shows the percentage deceased by October 1, 2017 and the age at death conditional on having passed away.

TABLE III

EXPOSURE TO BUSINESS COMBINATION LAWS AND MORTALITY[a]

Dependent Variable: $Death_{i,t}$						
	(1)	(2)	(3)	(4)	(5)	(6)
I(BC)	0.764***	0.769***	0.777***			
	[0.062]	[0.068]	[0.067]			
BC				0.955***	0.958***	0.959***
				[0.005]	[0.005]	[0.005]
Age	1.113***	1.123***	1.124***	1.111***	1.121***	1.122***
	[0.006]	[0.005]	[0.004]	[0.007]	[0.005]	[0.005]
Year	1.005	1.002		1.005	1.001	
	[0.004]	[0.005]		[0.004]	[0.004]	
Location FE (HQ)	Y	Y	Y	Y	Y	Y
FF49 FE		Y	Y		Y	Y
Year FE			Y			Y
Number of CEOs	1,605	1,605	1,605	1,605	1,605	1,605
Observations	50,530	50,530	50,530	50,530	50,530	50,530

[a]This table shows hazard ratios 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 three columns and a count variable of years of exposure, BC, in the right three 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 IV

NONLINEAR EFFECTS AND PREDICTED EXPOSURE[a]

Dependent Variable: $Death_{i,t}$						
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathrm{BC}^{(\min\text{-}p50)}$	0.908***	0.913***	0.916***			
	[0.021]	[0.024]	[0.023]			
$\mathrm{BC}^{(p51\text{-}\max)}$	0.992	0.993	0.992			
	[0.015]	[0.017]	[0.017]			
$\widehat{\mathrm{BC}}$				0.943***	0.951**	0.952**
				[0.018]	[0.023]	[0.023]
Age	1.111***	1.122***	1.122***	1.110***	1.120***	1.120***
	[0.010]	[0.009]	[0.009]	[0.007]	[0.005]	[0.005]
Year	1.005	1.001		1.007*	1.004	
	[0.006]	[0.007]		[0.004]	[0.004]	
Location FE (HQ)	Y	Y	Y	Y	Y	Y
FF49 FE		Y	Y		Y	Y
Year FE			Y			Y
Number of CEOs	1,605	1,605	1,605	1,605	1,605	1,605
Observations	50,530	50,530	50,530	50,530	50,530	50,530

[a]This table shows hazard ratios 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 three columns is $\widehat{BC}$, a count variable of years of predicted cumulative exposure to a BC law. The main independent variables in the right three columns are $BC_{i,t}^{(\min\text{-}p50)}$ 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 three columns, we present bootstrapped standard errors, using the block bootstrap method with 500 iterations, in brackets. For the right three columns, we present standard errors clustered at the state-of-incorporation level, in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

TABLE V

BUSINESS COMBINATION LAWS, RETIREMENT, AND CEO PAY[a]

	Panel A: Business Combination Laws and Retirement					
Dependent Variable: *CEO Departure*$_{i,t}$						
	(1)	(2)	(3)	(4)	(5)	(6)
I(BC)	0.788***	0.801***	0.911			
	[0.055]	[0.054]	[0.097]			
BC				0.910***	0.907***	0.957**
				[0.019]	[0.019]	[0.020]
Age	1.100***	1.105***	1.104***	1.100***	1.107***	1.104***
	[0.010]	[0.011]	[0.012]	[0.011]	[0.012]	[0.012]
Year	1.069***	1.072***		1.096***	1.100***	
	[0.008]	[0.008]		[0.015]	[0.015]	
Location FE (HQ)	Y	Y	Y	Y	Y	Y
FF49 FE		Y	Y		Y	Y
Year FE			Y			Y
Number of CEOs	1,575	1,575	1,575	1,575	1,575	1,575
Observations	49,556	49,556	49,556	49,556	49,556	49,556

	Panel B: 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

[a]Panel A shows hazard ratios estimated from a Cox (1972) proportional hazards model. The dependent variable is an indicator that equals one if the CEO leaves their position in a given year. Panel B shows OLS estimates. 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) "Age Controls" 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.

TABLE VI
INDUSTRY DISTRESS AND MORTALITY[a]

Dependent Variable: $Death_{i,t}$			
	(1)	(2)	(3)
Industry Distress	1.189***	1.190**	1.179**
	[0.076]	[0.083]	[0.084]
Age	1.115***	1.124***	1.125***
	[0.006]	[0.007]	[0.007]
Year	1.010*	1.007	
	[0.006]	[0.006]	
BC Exposure Control	Y	Y	Y
Location FE (HQ)	Y	Y	Y
FF49 FE		Y	Y
Year FE			Y
Number of CEOs	1,605	1,605	1,605
Observations	50,530	50,530	50,530

[a]This table shows hazard ratios 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 VII
SUMMARY STATISTICS FOR APPARENT AGING ANALYSIS[a]

	Panel A: CEO Characteristics					
	N	Mean	SD	P10	P50	P90
Biological Age in 2006	463	55.54	6.55	47	56	63
Industry Distress (2007-2008)	463	0.65	0.48	0	1	1
Industry Distress (Pre-2006)	463	0.54	1.13	0	0	2
Tenure (Pre-2006)	463	8.00	7.73	2	6	17
No. of Pictures per CEO	463	7.35	4.51	3	6	13
	Panel B: 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)					

[a]Summary statistics of CEOs with at least two pictures from different times during their tenure. *Industry Distress* during 2007-2008 is an indicator for distress exposure during these years. *Industry Distress* pre-2006 counts the number of industry distress experiences prior to 2006.

TABLE VIII
INDUSTRY DISTRESS AND CEO AGING[a]

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

[a]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.

ONLINE APPENDIX

APPENDIX A VARIABLE DEFINITIONS

Variable Name	Definition
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
$Age_{i,t}$	CEO i's age in year t
$Tenure_{i,t}$	CEO i's cumulative tenure (in years) at time t
$I(BC_{i,t})$	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.
$BC_{i,t}^{(\min\text{-}p50)}$	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.
$Industry\ Distress_{i,t}$	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.
$Year_{i,t}$	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 CORPORATE MONITORING: ROBUSTNESS TESTS

This appendix presents the robustness tests of the relation between anti-takeover laws and CEOs' life expectancy referenced in Section 3.5.

CEO Cohorts

We implement a series of robustness tests addressing possible cohort effects, in light of the fact that BC laws disproportionately protected more recent CEOs who are younger on average. First, Panel A of Appendix-Table B.2 directly includes CEO birth-year fixed effects. Coefficients and levels of significance are very similar in all specifications, whether using the BC indicator, linear, or non-linear BC variables. Next, Panel B of Appendix-Table B.2 reverts to year fixed effects, but allows 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 three BC law variables are barely affected and remain statistically and economically significant. We also consider CEO cohorts based on the year of their appointment to the top position. In Panel C of Appendix-Table B.2, we augment the main models with appointment-year fixed effects. The BC law estimates are virtually unaffected. Finally, we consider estimation subsamples with later start years or earlier end years. In Appendix-Figure B.2, we move forward the starting year of the sample one year at a time. The results are stable across the different sample year cutoffs. We then vary the censoring date for defining the death or alive status of the CEOs to address the concern that CEOs with information in more recent years may have characteristics that are correlated with longevity. Appendix-Figure B.3 shows that the estimated coefficients for both $I(BC)$ and BC remain stable with different censoring years.

Additional CEO and Firm Controls

Panel A of Appendix-Table B.3 contains the results when we include CEO pay (from Gibbons and Murphy 1992) and firm size (assets and employees from CRSP and Compustat) as additional control variables. Our main specification excludes these variables as they may themselves be affected by the passage of the BC laws.[29] We linearly interpolate any missing data. (Nonetheless, the number of observations decreases, as there are observations where data on one of the three additional controls is missing in all years.) Two findings emerge. First, the coefficients on the BC law exposure variables are very similar to those in Table III

[29] See Section 3.6 for how CEO pay responds to BC laws.

and remain significant at 1%. Second, in none of the specifications, any of the additional control variables is significant. This might reflect endogenous selection on observables. The (non-)results on pay and size are also in line with the notion that income in the very upper tail of the distribution is no longer correlated with health outcomes (Chetty et al. 2016).

As another variation in firm-level controls, we use fixed effects for state of incorporation instead of headquarters state. The results (in Panel B of Appendix-Table B.3) are barely affected.

First-Time Enactment 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 B.1 visualizes the *FL* enactment by states over time.

Appendix-Table B.4 re-estimates Table III 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 ratios range from 0.955 to 0.957, compared to 0.955-0.959 in Table III. 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 B.3.

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 B.5. 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 ratio on BC exposure remains significant at 1%, both when using the indicator and the count variable for BC experience. In addition, the hazard ratio estimates are nearly unchanged, ranging from 0.752 (Panel A, column 1) to 0.806 (Panel B, column 3) for the indicator version, and from 0.954 (Panel C, column 4) to 0.960 (Panel A, column 3; Panel B, columns 5 and 6) 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 B.6 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 hazard ratios substantially below one 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 B.7, 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 ratio estimates on binary and cumulative BC exposure are barely affected by these data cuts.

Linear Probability Model

To address any potential concerns about the hazard model, we also estimate a linear probability model, using the same 1,605 CEOs as in the hazard analysis:

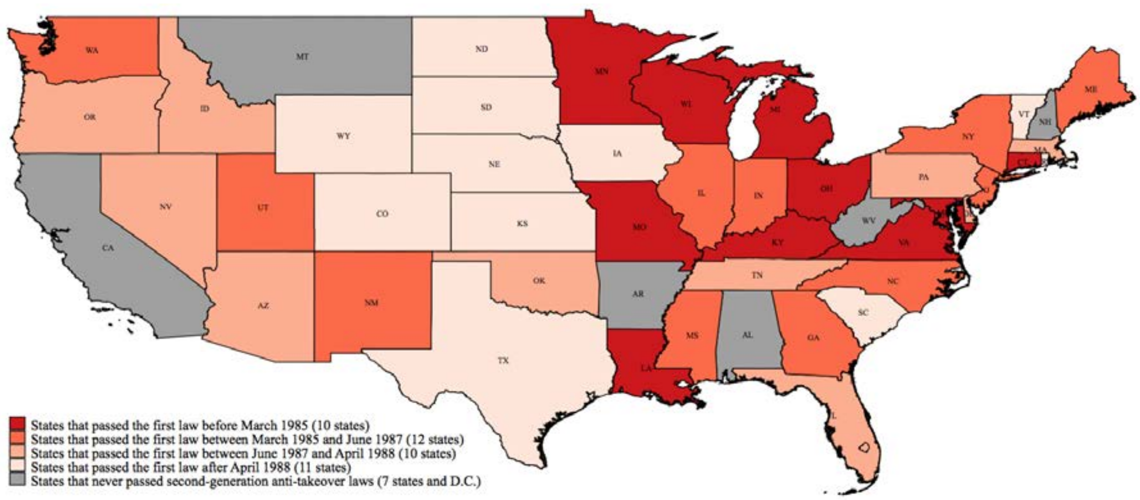
$$Y_{i,j,k,m,t} = \beta_0 + \beta_1 X_i + \theta_j + \delta_k + \phi_m + \eta_t + \varepsilon_{i,j,k,m,t}$$

where i represents a CEO, j represents a headquarters state, k represents an industry, m represents tenure-start age, and t either represents tenure-start year or birth year. The dependent variable $Y_{i,j,k,m,t}$ is an indicator variable that takes value one if the CEO has died by October 1st, 2017. The main independent variable of interest, X_i, is an indicator variable that takes value one if the CEO has ever been protected by a BC law and zero otherwise.

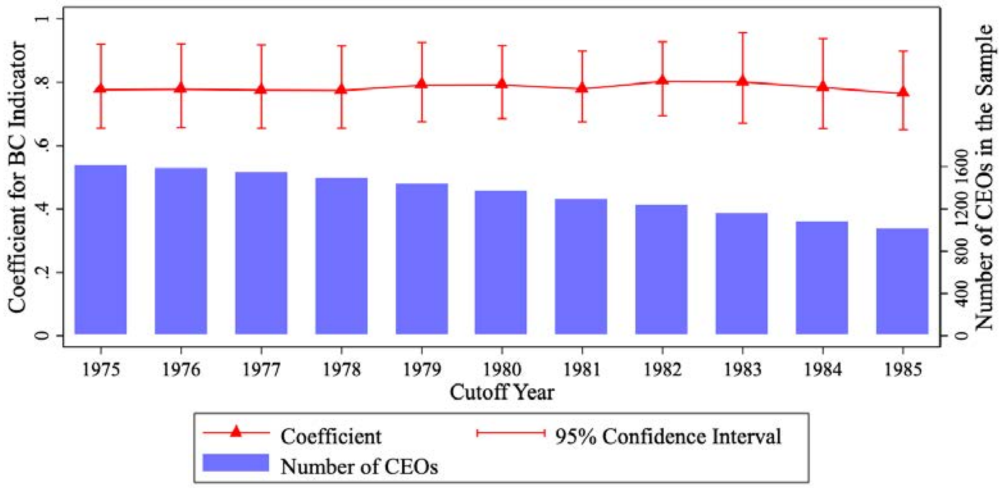
The results are shown in Appendix-Table B.8. In column (1), we linearly control for tenure-start age, and also include tenure-start year, industry, and headquarters-state fixed effects.[30] In column (2), we add tenure-start age fixed effects instead of the linear term.

[30] Since this analysis no longer uses CEO-year data, the industry classification is from the last year of a CEO's tenure.

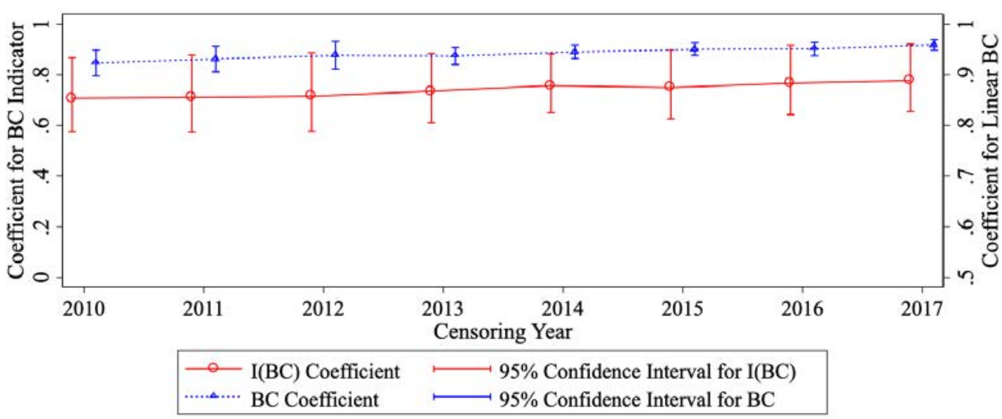
In column (3), we include birth-year fixed effects instead of tenure start-year fixed effects. All specifications are constructed to map closely to the specifications in the hazard model analysis. In all three columns, the estimated coefficients on the BC experience indicator are similar, ranging from -0.063 to -0.069, statistically significant at conventional levels. To interpret the economic magnitude of these estimates, we compare them to the coefficient on the linear age term in column (1): the effect of being protected by BC laws corresponds to that of assuming the CEO position when two and a half years younger ($0.063/0.027 = 2.56$). Hence, the age-based effect size comparisons are very close to those estimated in the hazard model.



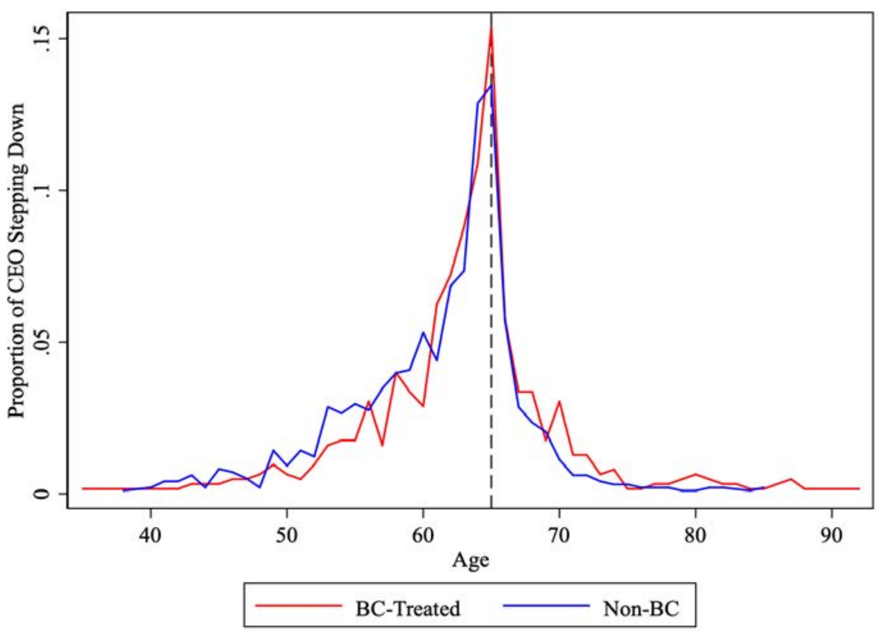
APPENDIX-FIGURE B.1.—First-time introduction of second-generation anti-takeover laws over time. *Notes*: This figure visualizes the distribution of first-time enactments of any of the five most common second-generation anti-takeover laws over time, i. e., business combination (BC), fair price (FP), control 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 B.2.—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 III, column 3, 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 III, Column (3).



APPENDIX-FIGURE B.3.—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 III, columns 3 and 6, but varying the censoring date defining death or alive status. 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, 2010; Dec. 31, 2011; ...; Dec. 31, 2016; and Oct. 1, 2017. The number of CEOs in the sample remains unchanged when varying the cutoff, i.e. $N = 1,605$.



APPENDIX-FIGURE B.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 B.1
ADDITIONAL SUMMARY STATISTICS[a]

Panel A: Summary Statistics for Different CEO Sub-Groups

	No BC Exposure (N=980)					Below-Median BC Exposure (N=320)					Above-Median BC Exposure (N=305)				
	Mean	SD	P10	P50	P90	Mean	SD	P10	P50	P90	Mean	SD	P10	P50	P90
Birth Year	1922	8.48	1913	1921	1934	1927	6.90	1921	1926	1938	1933	6.51	1926	1933	1942
Dead (by 10/2017)	0.82	0.38	0	1	1	0.68	0.47	0	1	1	0.38	0.49	0	0	1
Year of Death	2002	10.24	1987	2003	2015	2008	8.05	1994.08	2010	2016	2009	7.05	1997	2012	2017
Age at Death	82.30	10.10	68.00	83.83	94.00	81.89	9.52	68.00	84.17	92.42	79.64	9.13	66.83	81.17	90.42
Age Tak. Office	52.88	6.69	44	53	61	51.47	6.94	42	52	60	47.79	6.34	40	48	56
Year Tak. Office	1975	7.08	1966	1974	1984	1979	6.60	1971	1980	1986	1981	5.89	1972	1982	1987
Tenure	8.70	5.72	2	7.50	16	10.83	6.48	4	9.04	20.08	16.54	7.21	8.42	15.08	27.33
BC	0.00	0.00	0	0	0.00	1.93	1.24	0.5	1.86	3.82	9.61	4.52	5.41	8.33	14.74

Panel B: Most Common Industries and Incorporation States

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

[a]This table presents additional summary statistics for our main sample covering 1,605 CEOs, with splits based on CEOs' exposure to BC laws. All variables are defined in Appendix A.

APPENDIX-TABLE B.2
BUSINESS COMBINATION LAWS AND MORTALITY –
CEO BIRTH-YEAR FIXED EFFECTS, AGE-BY-COHORT CONTROLS, AND APPOINTMENT-YEAR FIXED EFFECTS[a]

Dependent Variable: $Death_{i,t}$									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Panel A: Birth Year FE			Panel B: Age-By-Cohort Controls			Panel C: Appointment Year FE		
I(BC)	0.772**			0.794**			0.774**		
	[0.089]			[0.086]			[0.077]		
BC		0.958***			0.965***			0.956**	
		[0.007]			[0.006]			[0.006]	
$BC^{(\text{min-}p50)}$			0.918**			0.923**			0.910**
			[0.033]			[0.029]			[0.026]
$BC^{(p51\text{-max})}$			0.989			0.997			0.990
			[0.018]			[0.018]			[0.017]
Age	1.130***	1.128***	1.129***				1.127***	1.124***	1.123***
	[0.006]	[0.005]	[0.006]				[0.005]	[0.006]	[0.005]
Age × Birth Cohort 1 (oldest)				1.095***	1.092***	1.093***			
				[0.011]	[0.011]	[0.011]			
Age × Birth Cohort 2				1.094***	1.090***	1.091***			
				[0.011]	[0.010]	[0.011]			
Age × Birth Cohort 3				1.092***	1.088***	1.089***			
				[0.012]	[0.011]	[0.012]			
Age × Birth Cohort 4				1.088***	1.085***	1.086***			
				[0.014]	[0.013]	[0.013]			
Age × Birth Cohort 5 (youngest)				1.082***	1.079***	1.081***			
				[0.014]	[0.013]	[0.013]			
Location FE (HQ)	Y	Y	Y	Y	Y	Y	Y	Y	Y
FF49 FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE				Y	Y	Y	Y	Y	Y
Birth Year FE	Y	Y	Y						
CEO Appointment Year FE							Y	Y	Y
Number of CEOs	1,605	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	50,530

[a]Columns (1), (4), and (7) show re-estimates of the regressions from column (3) of Table III. Columns (2), (5), and (8) show re-estimates of the regressions from column (6) of Table III, and column (3), (6), and (9) show re-estimates of the regression from column (3) of Table IV. Regressions in Panel A include birth-year fixed effects instead of year fixed effects. Regressions in Panel B allow for birth cohort-specific age effects. Birth cohorts are defined by sorting CEOs into quintiles by birth year. Regressions in Panel C add CEO appointment year fixed effects. 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 B.3

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

Dependent Variable: $Death_{i,t}$						
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Additional Controls					
I(BC)	0.770***	0.786***	0.799***			
	[0.056]	[0.068]	[0.066]			
BC				0.956***	0.961***	0.962***
				[0.007]	[0.007]	[0.007]
ln(Pay)	0.986	1.008	1.003	0.977	0.988	0.985
	[0.035]	[0.040]	[0.040]	[0.043]	[0.048]	[0.048]
ln(Assets)	1.015	0.968	0.960	1.023	0.986	0.979
	[0.026]	[0.041]	[0.040]	[0.024]	[0.036]	[0.034]
ln(Employees)	0.990	1.017	1.023	0.988	1.008	1.012
	[0.021]	[0.039]	[0.038]	[0.021]	[0.037]	[0.038]
Location FE (HQ)	Y	Y	Y	Y	Y	Y
Number of CEOs	1,503	1,503	1,503	1,503	1,503	1,503
Observations	49,052	49,052	49,052	49,052	49,052	49,052
	Panel B: State-of-Incorporation Fixed Effects					
I(BC)	0.767***	0.760***	0.768***			
	[0.064]	[0.067]	[0.066]			
BC				0.953***	0.955***	0.956***
				[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
Observations	50,530	50,530	50,530	50,530	50,530	50,530
Year (Linear Control)	Y	Y		Y	Y	
Age (Linear Control)	Y	Y	Y	Y	Y	Y
FF49 FE		Y	Y		Y	Y
Year FE			Y			Y

[a]Panel A reports hazard ratios estimated as in Table III, with additional controls for CEO pay, assets, and employees. Panel B reports hazard ratios estimated as in Table III, 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 B.4

FIRST-TIME SECOND-GENERATION ANTI-TAKEOVER LAWS AND MORTALITY[a]

Dependent Variable: $Death_{i,t}$						
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Baseline Results			
I(FL)	0.802***	0.802***	0.807***			
	[0.053]	[0.061]	[0.061]			
FL				0.955***	0.957***	0.957***
				[0.006]	[0.006]	[0.006]
Number of CEOs	1,510	1,510	1,510	1,510	1,510	1,510
Observations	47,994	47,994	47,994	47,994	47,994	47,994
			Panel B: Additional Controls			
I(FL)	0.827***	0.844**	0.855**			
	[0.051]	[0.059]	[0.058]			
FL				0.957***	0.961***	0.962***
				[0.008]	[0.007]	[0.008]
ln(Pay)	0.977	1.005	1.000	0.984	1.001	0.998
	[0.036]	[0.037]	[0.037]	[0.042]	[0.045]	[0.045]
ln(Assets)	1.014	0.944	0.937*	1.026	0.976	0.970
	[0.026]	[0.035]	[0.033]	[0.025]	[0.032]	[0.031]
ln(Employees)	0.995	1.045	1.050	0.987	1.019	1.022
	[0.020]	[0.036]	[0.036]	[0.019]	[0.036]	[0.037]
Number of CEOs	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
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 III, 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 B.5

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

Dependent Variable: $Death_{i,t}$						
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Excluding Lobbying Firms					
I(BC)	0.752***	0.756***	0.762***			
	[0.067]	[0.069]	[0.069]			
BC				0.955***	0.958***	0.959***
				[0.006]	[0.007]	[0.007]
Number of CEOs	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
	Panel B: Excluding Opt-out Firms					
I(BC)	0.784***	0.797***	0.806***			
	[0.064]	[0.065]	[0.064]			
BC				0.956***	0.960***	0.960***
				[0.006]	[0.006]	[0.006]
Number of CEOs	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
	Panel C: Excluding Firm-level Defenses					
I(BC)	0.762***	0.765***	0.774***			
	[0.060]	[0.066]	[0.067]			
BC				0.954***	0.957***	0.957***
				[0.005]	[0.005]	[0.005]
Number of CEOs	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
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 III, 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 B.6

RESTRICTION TO YEARS AFTER THE END OF THE FIRST-GENERATION LAWS[a]

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)	0.766***		0.803***		0.659***	
	[0.063]		[0.059]		[0.059]	
BC		0.957***		0.960***		0.965
		[0.005]		[0.006]		[0.027]
Age	1.124***	1.122***	1.128***	1.124***	1.132***	1.125***
	[0.005]	[0.006]	[0.006]	[0.006]	[0.015]	[0.020]
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

[a]This table re-estimates columns (3) and (6) of Table III 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 B.7

EXCLUDING DE OR NY INCORPORATED, BANKING, OR UTILITY FIRMS[a]

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 III 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 I). 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 B.8

LINEAR PROBABILITY MODEL AT THE CEO LEVEL[a]

Dependent Variable: *Death*$_i$			
	(1)	(2)	(3)
BC Treatment	-0.069***	-0.068**	-0.063*
	[0.024]	[0.026]	[0.031]
Tenure Start Age	0.027***		
	[0.002]		
Location FE (HQ)	Y	Y	Y
FF49 FE	Y	Y	Y
Tenure Start Year FE	Y	Y	
Tenure Start Age FE		Y	Y
Birth Year FE			Y
Observations	1,605	1,605	1,605

[a]This table reports regression results of a linear probability model at the CEO level instead of a hazard model. Each observation represents one CEO in our dataset. The dependent variable is an indicator that is one if the CEO passed away by October 1st, 2017, and zero otherwise. "BC Treatment" is an indicator variable that is one if the CEO has ever been protected by a BC law and zero otherwise. Fixed effects are indicated at the bottom of the table. Standard errors, clustered at the state-of-incorporation level, are shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

APPENDIX C INDUSTRY-WIDE DISTRESS SHOCKS: ROBUSTNESS TESTS

This appendix contains all robustness figures and tables on industry-wide distress shocks.

APPENDIX-FIGURE C.1.—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 C.1
INDUSTRY DISTRESS AND MORTALITY –
ADDITIONAL CONTROLS AND CEOs[a]

Dependent Variable: $Death_{i,t}$						
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Additional Controls			Panel B: Additional CEOs		
Industry Distress	1.188***	1.219***	1.207***	1.130**	1.147**	1.138**
	[0.076]	[0.085]	[0.085]	[0.065]	[0.065]	[0.065]
Age	1.115***	1.125***	1.125***	1.118***	1.125***	1.126***
	[0.006]	[0.006]	[0.006]	[0.006]	[0.007]	[0.007]
Year	1.008	1.008		1.009	1.007	
	[0.006]	[0.007]		[0.006]	[0.006]	
ln(Pay)	0.987	1.020	1.015			
	[0.036]	[0.045]	[0.045]			
ln(Assets)	1.010	0.950	0.944			
	[0.032]	[0.048]	[0.048]			
ln(Employees)	0.990	1.028	1.033			
	[0.035]	[0.057]	[0.058]			
BC Exposure 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
Number of CEOs	1,553	1,553	1,553	1,900	1,900	1,900
Observations	49,052	49,052	49,052	58,034	58,034	58,034

[a]Panel A reports hazard ratios estimated as in Table VI but with additional controls for CEO pay, assets, and employees. Panel B reports hazard ratios estimated as in Table VI but using an extended sample that includes CEOs who were appointed after the passage of anti-takeover laws. 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 C.2

LINEAR PROBABILITY MODEL AT THE CEO LEVEL[a]

Dependent Variable: $Death_i$			
	(1)	(2)	(3)
Industry Distress	0.036	0.056**	0.062**
	[0.025]	[0.026]	[0.025]
BC Treatment	-0.074**	-0.076**	-0.073**
	[0.029]	[0.030]	[0.029]
Tenure Start Age	0.027***		
	[0.002]		
Location FE (HQ)	Y	Y	Y
FF49 FE	Y	Y	Y
Tenure Start Year FE	Y	Y	
Tenure Start Age FE		Y	Y
Birth Year FE			Y
Observations	1,605	1,605	1,605

[a]This table reports the regression results of a a linear probability model at the CEO level instead of a hazard model. Each observation represents one CEO in our dataset. The dependent variable is an indicator that is one if the CEO has passed away by October 1st, 2017, and zero otherwise. “Industry Distress” is an indicator variable that is one if the CEO has ever experienced industry distress. “BC Treatment” is an indicator variable that is one if the CEO has ever been protected by a BC law and zero otherwise. Fixed effects are indicated at the bottom of the table. 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.3
INDUSTRY DISTRESS AND CEO AGING –
NO WINSORIZATION, MORE RESTRICTIVE INDUSTRY DISTRESS DEFINITION, AND PRE-2016 SAMPLE[a]

Dependent Variable: *Apparent Age*$_{i,j,t}$												
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Panel A: Not Winsorized				Panel B: Restrictive Distress Definition				Panel C: Pre-2016 Sample			
Industry Distress × $\mathbb{1}_{\{t>2006\}}$	0.940*	0.978**			1.173**	1.064**			0.816*	0.839*		
	[0.496]	[0.491]			[0.469]	[0.463]			[0.487]	[0.485]		
Industry Distress × $\mathbb{1}_{\{2006<t<2012\}}$			0.807	0.841			1.102**	1.007*			0.604	0.622
			[0.536]	[0.531]			[0.504]	[0.510]			[0.535]	[0.525]
Industry Distress × $\mathbb{1}_{\{t\geq 2012\}}$			1.135*	1.178**			1.261**	1.135**			1.323**	1.360**
			[0.576]	[0.564]			[0.563]	[0.551]			[0.566]	[0.568]
Biological Age	0.912***	0.908***	0.944***	0.940***	1.272***	1.274***	1.268***	1.269***	0.952***	0.943***	0.983***	0.974***
	[0.093]	[0.093]	[0.095]	[0.095]	[0.021]	[0.022]	[0.027]	[0.028]	[0.095]	[0.096]	[0.094]	[0.093]
CEO FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Picture Controls		Y		Y		Y		Y		Y		Y
Number of CEOs	463	463	463	463	463	463	463	463	463	463	463	463
Observations	3,086	3,086	3,086	3,086	3,086	3,086	3,086	3,086	3,086	3,086	3,086	3,086

[a]Panel A shows OLS estimates of the effect of industry distress exposure during the Great Recession on CEO apparent age without winsorizing apparent age. Panel B uses a more restrictive industry distress definition requiring negative industry sales growth in addition to the 30%-equity-decline criterion, as in the robustness tests in Acharya et al. (2007), and in Opler and Titman (1994) and Babina (2020). Panel C restricts the sample to only pictures taken prior to 2016. In all the three panels, we weight observations by the inverse of the number of pictures collected 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.

APPENDIX D APPARENT-AGE ESTIMATION

Our goal is to trace visible signs of aging in CEOs' faces. That is, we are interested in how old a person *looks*, which is referred to as the person's *apparent age*. By contrast, biological age describes how old a person is (time elapsed since birth) and will in general differ from a person's apparent age. To implement this analysis, we use machine learning based software by Antipov et al. (2016), henceforth referred to as the ABBD software. This software was specifically developed for the purpose of apparent-age estimation, and it was the winning solution of the second edition of the *ChaLearn Looking At People* competition in the *apparent-age estimation* track.

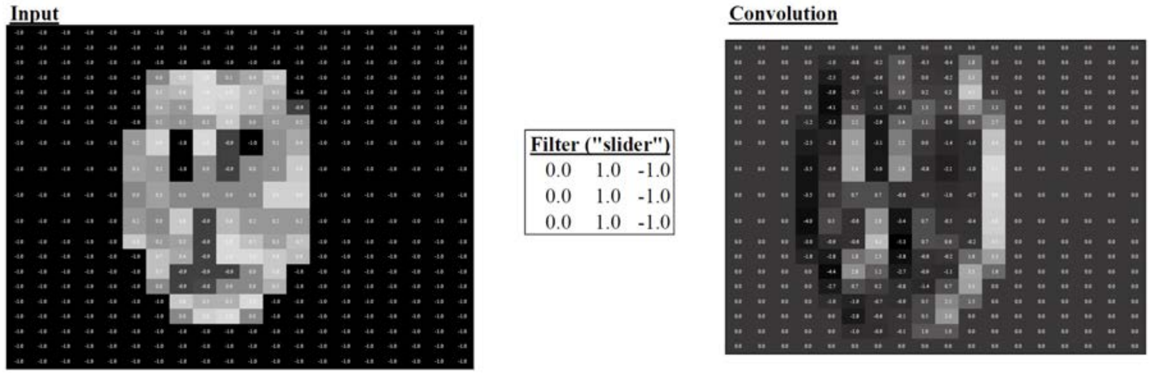
At the core of ABBD's apparent-age estimation tool is the training of a *convolutional neural network* (CNN). A CNN is a special class of *neural networks* that is particularly useful for image recognition and computer vision problems. A neural network is a system that learns to perform a task by studying training data.[31] 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 in-between layers are the hidden layers, which abstractly determine intermediate features about 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 D.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 $3x3$ 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 popular over the past ten to twenty years, with numerous applications, in particular to image recognition and classification. In an influential article on *deep learning*[32] 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."

[31] The task is referred to as *supervised learning* if the data is labeled (annotated), as is our training data.

[32] A neural network is considered *deep* if it has multiple hidden layers.



APPENDIX-FIGURE D.1.—Simplified example of convolution. *Notes*. This figure shows a simplified example of convolution. The fictional input image (left) is roughly recognizable as a face. Each cell (pixel) is encoded with a number that determines its color (between -1.0-black and $+1.0$-white). The output image (right) is obtained through convolution. The 3×3 filter matrix (center) slides over each possible 3×3 region in the input image and outputs the sum of element-wise matrix multiplication of these $3x3$ 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,[33] 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, which also contains information on the *apparent* age of the person in each picture. The information on people's apparent age consists of at least 10 age estimates (per picture) by humans, 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.[34] 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 hidden layers.

[33] Introduced by Simonyan and Zisserman (2014), VGG-16 is a deep CNN. 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.

[34] 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 μ and σ are the image-level mean and standard deviation of across the human-based age estimates.

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 VII).

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 224×224 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. We use the Python-based "face_recognition" package[35] to detect the picture region showing the CEO's face, extract the face, center it in the image, and resize the image to 224×224 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. 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.

Appendix-Figure D.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. 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

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

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 D.2.—Examples of pre-processed images. *Notes*. This figure shows examples of pre-processed facial images. Panel (a) shows examples of 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. As described above, ABBD's software development includes a fine-tuning step using a dataset on human-based age estimates. Across all training and image pre-processing steps, the fine-tuning on this apparent age led to the biggest accuracy improvement on the competition data, amounting to more than 20% (cf. Table 2 in Antipov, Baccouche, Berrani, and Dugelay 2016). This underscores the importance of using a software specifically trained for apparent-age estimation, rather than an "off-the-shelf" software solely trained on images annotated with people's biological age, for our study of CEO visual aging.

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 100×1 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.

Data augmentation. In the fine-tuning step of the software development, ABBD use five-times data augmentation to reduce overfitting. 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 in our application, suppose that among the fine-tuning sample of 5,613 images, people who look older happen to look slightly to the upper right. 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.[36] 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.

[36] These specific image modifications assume that there is *no* intrinsic relation between apparent age and camera angle. This appears reasonable but highlights that data augmentation choices involve judgment.