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DISCUSSION PAPER SERIES

IZA DP No. 16366

CEO Stress, Aging, and Death

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Mark Borgschulte
University of Illinois and IZA

Marius Guenzel
Wharton School, University of Pennsylvania

Canyao Liu
Yale University

Ulrike Malmendier
University of California Berkeley, NBER, CEPR, briq and IZA

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ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

CEO Stress, Aging, and Death*

We assess the long-term effects of managerial stress on aging and mortality. First, we show that exposure to industry distress shocks during the Great Recession produces visible signs of aging in CEOs. Applying neural-network based machine-learning techniques to pre- and post-distress pictures, we estimate an increase in so-called apparent age by one year. Second, using data on CEOs since the mid-1970s, we estimate a 1.1-year decrease in life expectancy after an industry distress shock, but a two-year increase when anti-takeover laws insulate CEOs from market discipline. The estimated health costs are significant, also relative to other known health risks.

JEL Classification: G34, I12, M12

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

Corresponding author:
Mark Borgschulte
Department of Economics
University of Illinois, Urbana-Champaign
1407 W Gregory Drive
Urbana, IL 61801
USA

E-mail: markborg@illinois.edu

* We thank Martijn Cremers, Allen Ferrell, Kevin J. Murphy, and Emmanuel Saez for sharing their data. We thank Grigory Antipov, Moez Baccouche, Sid-Ahmed Berrani, and Jean-Luc Dugelay for sharing their age estimation software. We also thank Tania Babina, Xavier Gabaix, Kevin J. Murphy, Owen O'Donnell, Joe Gladstone, Christopher Parsons, Mike Wittry, as well as seminar and conference participants at the 2018 briq Workshop on Skills, Preferences, and Educational Inequality, University of Bonn; the 2018 Berkeley-Stanford Joint Behavioral Economics Mini-Conference; the 2019 NBER Organizational Economics Fall Conference; the 2019 Finance, Organizations and Markets (FOM) Conference; the 2020 American Finance Association Annual Meeting; the 2020 Behavioral Science & Health Symposium; the 2022 NBER Conference on Aging and Health; the 2022 Chicago Booth Behavioral Approaches to Financial Decision-Making Conference; Berkeley Economics; Berkeley Haas; Dartmouth; Harvard Business School; the University of Illinois at Chicago; Texas A&M; the University of Illinois at Urbana-Champaign; the University of Maryland; the University of Minnesota; the University of Pennsylvania; Yale SOM; the London School of Economics; and Arizona State University for helpful comments and suggestions. We thank Nikki Azerang, Clint Hamilton, and Tim Zhang for outstanding research assistance.

I. Introduction

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

In this paper, we document that heightened managerial stress due to industry crises and corporate monitoring predicts significantly accelerated aging and increased mortality among chief executive officers (CEOs) of large U.S. companies. Job demands and work-related stress are increasingly recognized as key determinants of population health and well-being across hierarchy levels in organizations.[1] Yet, there is little quasi-experimental evidence that links health outcomes to variation in job demands and stress across worker contexts. One reason for the lack of causal evidence is that income loss and financial hardship tend to confound the estimation of the causal effects of job stressors. That is, while the work pressures and looming consequences of underperformance for the typical study sample of lower-ranked workers are in many ways harsher as they include (long-term) unemployment, financial hardship, and loss of health insurance, these consequences also make it harder to isolate the health effects of work-related stressors from the effects of low income and financial constraints.

The CEO context helps overcome these identification hurdles as it allows to isolate the effects of high job demands from financial and other confounds. CEOs of large, publicly traded U.S. companies are wealthy and unlikely to be affected by financial hardships even if they lose their job. That said, the role of work-related stressors in the CEO context is of particular interest on its own right for several reasons. First, CEOs work long hours, make high-stakes decisions such as layoffs and plant closures, and face uncertainty in times of crisis (Bandiera et al. 2020, Porter and Nohria 2018). They are closely monitored and criticized when their firm is underperforming, and the media frequently reports on "overworked [and] overstressed" CEOs.[2] Second, CEOs bear the ultimate responsibility for the firm and its employees. Given their overarching importance for firm

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

[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).

performance and job stability of workers, it matters how incentives and performance affect CEOs personally. Third, 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. When CEOs like Tesla's Elon Musk brag about sleeping only a few hours per night and working up to 120 hours,[3] how will these remarks affect selection?

We exploit two sources of variation in work-related stress: periods of industry-wide crises and variation in the intensity of CEO monitoring due to changing corporate-governance regulation. As for the first, prior work has shown that industry shocks and financial distress affect CEO pay and turnover (Bertrand and Mullainathan 2001; Garvey and Milbourn 2006; Jenter and Kanaan 2015). We identify industry distress shocks based on a 30% median firm stock-price decline over a two-year horizon, similar to prior work (Opler and Titman 1994; Acharya et al. 2007; Babina 2020). As for the second, we identify variation in CEO monitoring from the staggered passage of anti-takeover laws across U.S. states in the mid-1980s. The laws shielded CEOs from market discipline by making hostile takeovers more difficult, reducing CEOs' job demands and allowing them to "enjoy the quiet life" (Bertrand and Mullainathan 2003).[4] A large prior literature has used the passage of these laws as proxies for less intense monitoring and shown the effects on CEO behavior.[5] While some studies question whether the passage of anti-takeover laws in fact reduced hostile takeover activity (e. g., Cain et al. 2017), it arguably constituted a significant shift in managers' *perception* of their job environment.[6] Thus, the two sources of variation constitute separate and oppositely-signed changes in job demands. Both build on the notion of distress and protection from stress in the economic literature (and on the popular notion of stress), rather than a biomedical measurement of adrenaline or cortisol levels.[7]

[3] See cnbc.com/2021/02/12/elon-musk-ceo-of-tesla-spacex-on-getting-six-hours-of-sleep.html.

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

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

[6] Consistent with CEOs' perceptions of anti-takeover laws changing job demands, we find suggestive evidence that takeover-protected CEOs remain on the job for longer. We also show, though, that prolonged tenure is unlikely to explain the estimated longevity effects.

[7] Stress arises from experiencing demands without sufficient resources to cope (Lazarus and Folkman 1984).

To investigate the link between work-related stress and health, we assemble two measures of health outcomes: visible signs of aging and life expectancy. Visible signs of aging are widely used by clinicians to assess patient health. They measure how old a person looks, not the actual chronological age. These assessments of so-called "perceived" or "apparent" age have been validated as biological markers predicting mortality and other health outcomes at least since the Baltimore Longitudinal Study of Aging (BLSA) beginning in 1958 (Borkan and Norris 1980). We build on prior medical studies that measure apparent age from facial photographs (e. g., Christensen et al. (2004) and Christensen et al. (2009)), and assess visible aging in photographs of CEOs' faces.

We utilize recent visual machine learning (ML) techniques. Specifically, we employ the ML algorithms from Antipov et al. (2016) that are designed to estimate a person's apparent age, i. e., how old a person looks, rather than a person's chronological age. The software, trained on more than 250,000 pictures, is the winner of the *2016 ChaLearn Looking At People* competition in the *apparent-age estimation* track. We note that these ML techniques are a promising avenue for the assessment of work-induced strains in broader samples and, to the best of our knowledge, we are the first to introduce them into the finance and economics literature.

Our analysis has three main parts. The first part exploits industry-level distress shocks during the Great Recession and relates them to visible signs of accelerated aging, identified by neural-network based ML estimations. In the second part, we continue to exploit industry-level distress shocks, now from longer and earlier time periods, and relate them to CEO mortality. In the third part, we continue to focus on CEO mortality, and study the effect of variation in the intensity of CEO monitoring due to corporate-governance legislation. Across both health outcome measures and both types of variation in managerial stress, we find consistent results of very similar magnitudes.

For the first part of the analysis, we collect a sample of 3,002 facial pictures of the *Fortune 1000* CEOs from the 2006 list (the "CEO Apparent Aging Data"). The pictures are taken at different points during CEOs' tenure, both before and after the 2007/8 financial crisis. This crisis lends itself to the analysis of the effects of managerial stress as the large disruptions associated with the Great Recession caused firms to re-optimize many aspects of their operation including plant closures and layoffs, two of the tasks CEOs most avoid when corporate governance is weakened (Bertrand and Mullainathan 2003). At the same time, the financial crisis affected different sectors differently, providing for a suitable source of variation. It is also recent enough to ensure picture availability.

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).

Using a difference-in-differences design, we estimate that CEOs look one year older post crisis if their industry experienced a distress shock, relative to CEOs in other industries. The estimated difference between distressed and non-distressed CEOs increases over time and amounts to 1–1.2 years for pictures taken five years and more after the onset of the crisis. We include a detailed description of the procedure and examine issues that have been shown to impact the use of visual machine learning in other settings (Wang and Kosinski 2018; Dotsch et al. 2016; Agüera y Arcas et al. 2018). To the best of our knowledge, this represents the first application of visual machine learning to a quasi-experimental research design. Our application illustrates its potential for the study of health and aging to complement standard measures based on mortality, hospital admissions, or survey responses.

In the second and third part of the analysis, we study direct mortality effects associated with industry distress and corporate governance legislation. For these analyses, we extend Gibbons and Murphy's (1992) data of CEOs in the *Forbes* Executive Compensation Surveys from 1975 to 1991. We merge it with hand-collected data on the exact dates of birth and death of 1,900 CEOs of large U.S. firms (the "CEO Mortality Data"), and add information about pay, tenure, and firm characteristics.

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

The estimated mortality effect sizes are large. The effect of experiencing industry distress is equivalent to reducing a CEO's chronological age by 1.1 years. We can also compare the estimated mortality effects to known health threats. For example, smoking until age 30 is associated with a reduction in longevity of roughly one year, and lifelong smoking with a reduction of ten years and more (General 2014; Jha et al. 2013).

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

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

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

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

Our paper contributes to the research examining CEOs and firms. A recent literature sheds light on CEOs' demanding job and time requirements (Bandiera et al. 2020; Bandiera et al. 2018; Porter and Nohria 2018). Working these long hours requires physical stamina, and consistent with our hypothesis, previous work has documented that CEO health is an input into production. Bennedsen et al. (2020) study the negative effect of CEO hospitalizations on firm performance. Keloharju et al. (2020) find that corporate boards in Scandinavia factor CEO health into CEO appointment and retention decisions. None of these papers, however, examines the effect of CEO job demands on CEOs' health trajectories. To the best of our knowledge, we are the first to explore quasi-random variations to establish significant health costs, in terms of mortality and visible aging. The only prior work on executives' health outcomes is Yen and Benham (1986), who compare the age-adjusted mortality rates of 125 executives in the banking industry to those in other industries, and Nicholas (2023), who focuses on mortality patterns across organizational hierarchy levels at one large U.S. firm (white-collar employees at General Electric in the 1930s). Our significantly larger sample and quasi-experimental design allows us to control for industry-specific selection into job environments, and to implement a rigorous survival analysis.

Second, our paper contributes new quasi-experimental evidence to the literature on the health effects of stress, socioeconomic status, and recessions. A vast literature in psychology, medicine, and biology associates chronic stress with changes in hormone levels, brain function, cardiovascular health, DNA, and deleterious health outcomes (McEwen 1998, Epel et al. 2004, Sapolsky 2005). This has led researchers outside of economics to embrace stress, and the damage it causes, as the mechanism underlying many health disparities (Cutler et al. 2006, Cohen et al. 2007, Pickett and Wilkinson 2015, Puterman et al. 2016, Snyder-Mackler et al. 2020). In health and labor economics, stress has been proposed as an explanation for, among other things, the effects of recessions on health (Ruhm 2000; Coile et al. 2014) as well as the association between job loss and higher mortality (Sullivan and Von Wachter 2009).[8] In particular, the latter study examines job displacement in a population of male workers in the 1970s and 1980s (i. e., from similar birth cohorts as the CEOs we study), finding that job displacement at age 40 increases the mortality hazard by 10–15% and reduces life expectancy by 1–1.5 years. The displacement shock is, however, different in nature than the CEO distress shock: it reduces time and effort spent at work (Krueger and Mueller 2012), while industry crises induce CEOs to spend more time and effort at work.

Despite the importance of stress as a potential mediator of health, and the fact that the amount of stress experienced at work has steadily grown since at least the 1950s (Kaplan and Schulhofer-Wohl 2018), few papers examine causal effects of job demands on health. Hummels et al. (2016) document the negative impact of quasi-random trade shocks on workers' stress, injury, and illness. Evolving worker-firm interactions may have eroded protections from competition in the product market, likely exposing workers to higher levels of stress (Bertrand 2004). Outside of economics, stress arising from social hierarchies (sometimes called psychosocial stress), especially in the

[8] Stress has also been linked to the efficacy of child-tax credits (Milligan and Stabile 2011), the health benefits of the Earned Income Tax Credit (Evans and Garthwaite 2014), unemployment insurance (Kuka 2020), access to health care (Koijen and Van Nieuwerburgh 2020), and early-life health disparities (Camacho 2008; Black et al. 2016). Relatedly, guaranteed, job-protected leave has been proposed to reduce adverse effects of mothers' job stress during pregnancy on infant health (Currie and Rossin-Slater 2015). Stress is also implicated in the intergenerational persistence of poverty (Aizer et al. 2016; Persson and Rossin-Slater 2018; East et al. 2017).

workplace, has been examined as an explanation for the strong relationship between socioeconomic status and life expectancy (Marmot et al. 1991, Marmot 2005). Quasi-experimental evidence on a causal effect of workplace promotions is, however, limited and reaches mixed conclusions (Boyce and Oswald 2012, Anderson and Marmot 2012, Johnston and Lee 2013). Turning from the general or poorer populations to wealthier populations, income appears to play a small role in health disparities among the already-wealthy, though Engelberg and Parsons (2016) document a link between stock-market crashes and hospital admissions, especially for anxiety and panic disorders. Social factors, such as the prestige of winning a Nobel prize or an election, may be protective (Rablen and Oswald 2008; Cesarini et al. 2016; Borgschulte and Vogler 2019). Our paper offers complementary evidence that work-related stressors impose long-term health costs, even for successful and wealthy individuals.

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

II. CEO Datasets and Variation in CEO Job Demands

A. CEO Apparent Aging Data

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

We search for five pictures from the beginning of a CEO's tenure and two additional pictures every four years after that. The main challenge is finding *dated* pictures. Pictures from LinkedIn, for instance, do not satisfy this criterion as they generally do not provide date information. 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 such pictures from different points in time during or after their tenure for 453 CEOs. We call this dataset of 3,002 pictures the CEO Apparent Aging Data. We note that the vast majority of CEOs in this sample are male and white.

Table I provides the summary statistics for this dataset, both at the image level (Panel A) and at the CEO level (Panel B). The median picture is from 2009. On average, we found about seven pictures of a CEO, and the average CEO is 57 years old in 2006 and has been in office for eight years. 65% of the CEOs in the data experienced an industry distress shock during the financial crisis. (See Section II.C for details on the definition.) About a third of CEOs experienced pre-crisis industry shocks while in the top position, which we will control for in our analyses. The majority of CEOs head firms in the manufacturing, wholesale and retail, transportation and electric services, and finance industries (Panel C). We will discuss the remaining picture-level characteristics from Panel A in Section III.B.

B. CEO Mortality Data

We collect mortality data for the universe of CEOs included in the *Forbes* Executive Compensation Surveys from 1975 to 1991, as originally collected by Gibbons and Murphy (1992).[9] 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 source of identifying variation in the third part of our analysis, i. e., the timing of anti-takeover laws (see Section II.C), and also in line with prior studies in this literature.[10] We include all firms with a PERMNO identifier in CRSP. The initial sample comprises 2,720 CEOs employed by 1,501 firms.

We manually search for (i) the exact dates of CEOs' birth, (ii) whether a CEO has died, and, if so, (iii) the date of death. All CEOs who did not pass away by the cutoff date of October 1st, 2017 are treated as censored. Our main source of birth and death information is Ancestry.com, which links historical birth and death records from the U.S. Census, the Social Security Death Index, birth certificates, and other historical sources. We validate Ancestry's information with online and newspapers searches, e. g., on birth place, elementary school, or city of residence. Identifying a person as alive turns out to be more difficult as there is little coverage of retired CEOs. We classify a CEO as alive whenever recent sources confirm the alive status, such as newspaper articles or websites that list the CEO as a board member, sponsor, donor, or chair of an organization or event.[11]

We obtain the birth and death information for 2,361 CEOs from 1,374 firms in the post-1975

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

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

[11] We use sources dated 01/2010 or later to infer alive status since recent coverage of a retired CEO makes it very likely that news outlets would also have reported their passing had it occurred by the cutoff date (October 1st, 2017). Our results are robust to restricting the sample period to end in 01/2010 for CEOs classified as alive as of 10/2017, as we discuss in Sections IV.D and V.D).

sample, implying a finding rate of 87%. We call this the CEO Mortality Data. We test and confirm that the availability of birth and death information is not correlated with our measures of variation in work-related stressors, namely, industry distress experience and incorporation in a state that passed a BC law (see Appendix-Tables C.1 and D.1, respectively).

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

We collect tenure information for all individuals in the CEO Mortality Data to fill gaps and correct misrecorded data in the *Forbes* Executive Compensation Surveys. We use Execucomp, online searches, and especially the *New York Times* Business People section, which frequently reports executive changes in our sample firms. When the exact month of a CEO transition is missing, we use the "mid-year convention" (Eisfeldt and Kuhnen 2013) motivated by the relatively uniform distribution of CEO starting months in Execucomp. We further restrict the sample to firms included in CRSP during the time of the CEOs' tenure, which results in a main CEO Mortality sample of 1,900 CEOs.[12]

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

Table II presents the summary statistics. The median CEO in this sample was born in 1927,

[12] 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.

became CEO at age 52, and served as CEO for nine years. The heterogeneity in tenure is relatively large, with an interdecile range of 16.5 years. Non-integer values result from CEOs starting or ending their tenure not at the end of the year. Sixty-six percent of our CEOs have passed away by the censoring date (October 1st, 2017). The median CEO died at age 82, and passed away in 2006. About forty percent of CEOs witness industry distress during their tenure, but multiple distress shocks are rare. More than 80% of CEOs experienced no or at most one distress shock. Throughout, we will focus on a binary rather than a cumulative distress measure.[13] Similarly, about forty percent of CEOs are protected by a BC law at some point during their tenure.

C. Variation in Job Demands

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

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

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

Following prior literature, we focus on the second-generation anti-takeover laws, which states started passing in the mid-1980s after the first-generation laws were struck down by courts in the 1970s and early 1980s (cf. Cheng et al. 2004; Cain et al. 2017). The statutes included Business

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

Combination (BC) laws, Control Share Acquisition, Fair Price, and Directors' Duties laws, and Poison Pills. We follow prior literature and focus on BC laws as the most potent type of statutes, but return to the other types of laws in Section V.D.

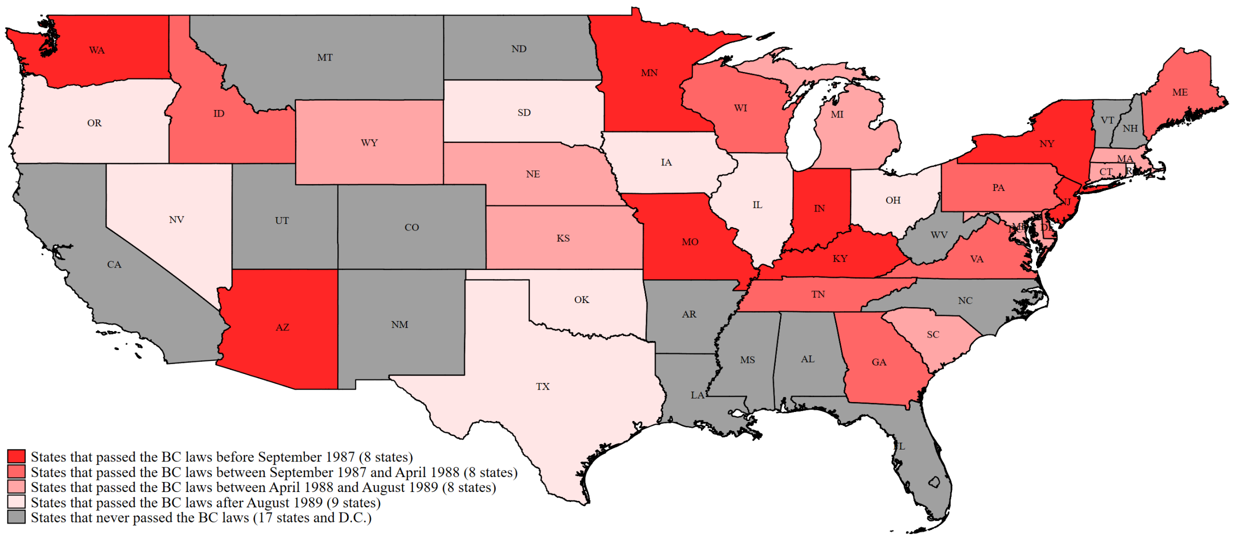

Figure 1. Introduction of Business Combination laws over time. The map omits the states of Alaska and Hawaii, which never passed a BC law.

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. The constitutionality of the laws was established in a 1989 ruling by the U.S. Seventh Circuit Court of Appeals (*Amanda Acquisition Corp. v. Universal Foods Corp.*). From an identification standpoint, an important assumption is that firms' BC law coverage is plausibly exogenous. As Karpoff and Wittry (2018) write on p. 678, "for most firms the laws can be treated as exogenous," since there was no widespread corporate lobbying for the laws. At the same time, Karpoff and Wittry (2018) identify 46 firms who *did* lobby of the adoption of second-generation laws. As we discuss below, our results are robust to the various tests related to lobbying firms and other aspects, such as firm-level defenses and opt-out provisions, proposed in Karpoff and Wittry (2018). We refer the reader to Bertrand and Mullainathan (2003) and Karpoff and Wittry (2018) for a more detailed discussion of the political economy of anti-takeover laws.

An advantage of using anti-takeover laws as identifying variation is that these laws apply based on the state of incorporation, not the state of firms' headquarters or operations. The frequent discrepancies between firms' location and state of incorporation enables us to assess the impact of the laws while controlling for shocks to the local economy. Figure 1 visualizes the staggered introduction of BC laws across states.[14] The map illustrates the variation across both time and states as a source of identification: 33 states passed a BC law between 1985 and 1997, with most laws being passed in 1987-1989. As in prior work, the most common state of incorporation in our data is Delaware. Other common states include New York and Ohio.

III. Industry-Wide Distress Shocks and Apparent Aging

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

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

A. Apparent-Age Estimation

To analyze visible CEO aging, we make use of recent advances in machine learning on estimating people's age. While the earlier generations of age-estimation software focused on a person's *chronological*, i. e., "true" age (Antipov et al. 2016), recent research aims at estimating a person's *apparent* age, i. e., how old a person looks. This distinction is important. As the medical literature shows, the differences between human assessment and chronological age can be large, they are recorded even when the assessing physician knows the chronological age, and they are highly predictive of illness and mortality; cf. Hwang et al. (2011). Correspondingly, we use the apparent-

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

age gap, i. e., the difference between the apparent (perceived) and the chronological age of a person, as our main outcome variable in the first part of the analysis.

The algorithmic implementation of age assessments, and thus the construction of the age gap variable, has been made possible by the development of deep learning and convolutional neural networks (CNNs) and the increased availability of large datasets of facial images with associated true and apparent ages, where the apparent ages are estimated by people. A CNN is a neural network which employs the method of convolution, i. e., of transforming the data by sliding (or, convolving) over it using a slider matrix, to abstractly determine intermediate features about the data such as edges or corners. (We provide a detailed discussion of CNNs and the software training steps in Appendix B.1.) We use a machine-learning based software (Antipov et al. 2016) that has been specifically developed for apparent-age estimation. This software is the winner of the *2016 Looking At People apparent-age estimation* competition. It is based on Oxford's Visual Geometry Group deep CNN architecture.

In a first step, the software has been trained on more than 250,000 pictures with information on people's true age from the Internet Movie Database and from Wikipedia. In a second step, it has been fine-tuned for apparent-age estimation using a newly available dataset of 5,613 facial pictures, each of which was rated by at least ten people in terms of the person's age. The addition of fine-tuning on these human estimates of age is particularly important. It led to the software's largest accuracy improvement (of more than 20%) in the apparent-age estimation of the competition data (see Table 2 in Antipov et al. 2016 and Appendix B.1). Both the distribution of true ages used for training and human age estimations used for software fine-tuning covers people from all age groups, including elderly people.

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

As indicated above, the software has been successfully validated in an apparent-age competition against other ML-trained apparent-aging software. Nevertheless, we have further validated it within our CEO context by comparing it against human assessments. We create 250 random pairs of CEO images such that each pair involves CEOs of similar chronological age (five-year bins), and for each pair ask a team of research assistants to rate which person looks older. The software agrees with the median human assessment in approximately 70% of cases, and in almost 90% of cases when the software-based apparent-age difference between the two images is in the upper tercile of the distribution.

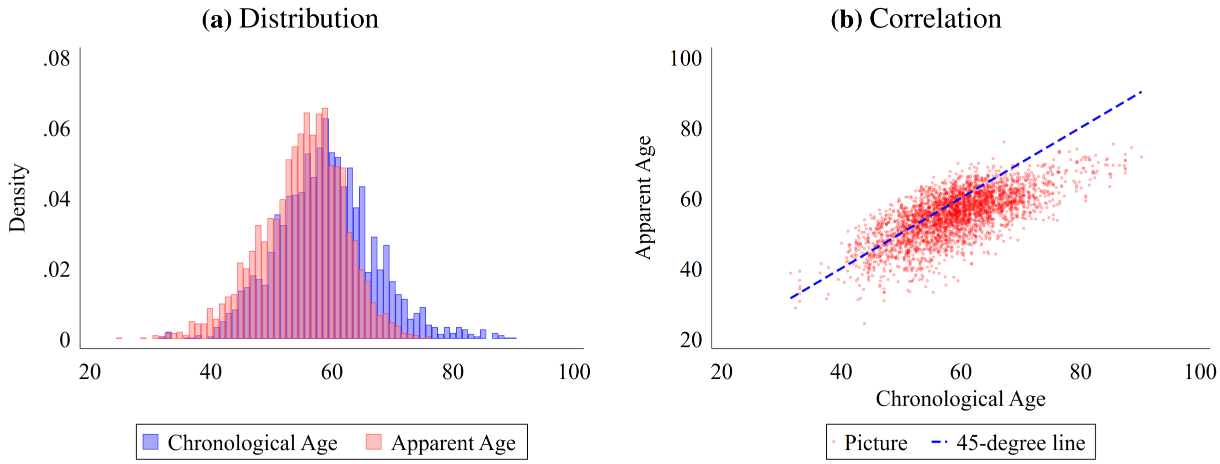

Figure 2. CEO apparent and chronological age. The figure plots apparent and chronological ages for our sample of 3,002 CEO images. Panel (a) shows the CEO apparent-age distribution in red, and the chronological-age distribution in blue, with the overlapping areas appearing as purple. Panel (b) shows a scatter plot of CEOs' apparent age against chronological age. The dotted line represents the 45°-line.

Figure 2 graphs the distributions and correlation of chronological and apparent ages for the 3,002 pictures in the CEO Apparent Aging Data. Panel (a) shows that the distributions of apparent and chronological ages largely overlap, though the apparent-age distribution is shifted to the left. That is, consistent with a large prior literature on the "looks," stature, and health-related measures of CEOs and other high-earning individuals,[15] the software estimates CEOs to look younger than their chronological age. (See also Table II discussed in Section II.B.) We note that our analyses do not rely on comparisons between CEOs and the general population but entail solely *within-CEO* comparisons and account for chronological age.

The scatter plot of CEOs' apparent age against chronological age in panel (b) confirms both the high correlation and the shift in apparent age relative to chronological age, with a greater mass below the 45°-line. In this figure and in the regression analysis below, we trim the sample at the

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

top and bottom 0.5% of the apparent-age gap distribution, i. e., the distribution of the difference between apparent and chronological age, to ensure that outliers in age estimation do not affect the results.

It is worth reiterating that the difference between chronological age and apparent age should not be thought of as a "mistake." By design, the ML software is first trained to predict chronological age and then fine-tuned to match human age assessment. As the medical literature shows, the differences between human assessment and chronological age can be large *and* these differences are highly predictive of illness and mortality.[16]

B. Illustrative Example and Potential Confounds from Picture Heterogeneity

To illustrate the link between industry shocks and aging, we first discuss a specific example. James Donald was the CEO of Starbucks from April 2005 until January 2008, when he was fired after Starbucks' stock had plunged by more than 40% over the preceding year. Figure 3 shows two pictures of Donald: the left one from December 8, 2004, before his appointment at Starbucks, and the right one 4.42 years later, on May 11, 2009, after his dismissal. Donald was 50.76 years old in the first picture, and 55.18 years in the second. The machine-learning based aging software estimates his apparent age in the earlier picture as 53.47 years, and in the later picture as 60.45 years. Thus, the software estimates that he aged by 6.98 years, i. e., 2.5 years more than actual time passed.

The example is also useful to discuss concerns one may have about picture heterogeneity. One noticeable difference between the two images is that Donald smiles in the image in which he looks younger but not in the other one. Hence, one potential identification concern is that photographers and newspaper editors might select different types of images during good and bad times, in a way that is correlated with firm or industry performance. Other differences between the two images include that the lighting in the two pictures seems to be different, and that the left picture might be from a more staged setting. In fact, 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).

The picture processing software is designed to account for such "picture management." The

[16] See, for example, Hwang et al. (2011). Other medical research that documents a positive relationship between apparent age in facial images and life expectancy and health status includes Gunn et al. (2016) and Dykiert et al. (2012).

Figure 3. Sample pictures (James Donald, CEO of Starbucks from 2005 to 2008). The left picture was taken on December 8, 2004. The right picture was taken on Monday, May 11, 2009. Chronological ages based on data from Ancestry.com (date of birth March 5, 1954): 50.76 and 55.18 years, respectively. Apparent ages based on aging software: 53.47 and 60.45 years, respectively.

image pre-processing and *fine-tuning* steps, in particular, help account for image heterogeneity as described in Appendix B.1. Nevertheless, we go one step further and manually assess all pictures in our sample along thirteen dimensions of picture type: *logo*, *side face*, *professional clothes*, *magazine shot*, *magazine quality*, *natural pose*, *natural lighting*, *glasses*, *facial hair*, *smile*, *mood*, *self-confidence*, and *style*. Appendix A provides definitions of these variables, which are all indicator or categorical variables. To highlight a few definitions, *smile* assesses facial expression (smile, frown, or neither). *Mood* assesses mood (happy, grim, or neutral). *Self-confidence* assesses the degree of portrayed self-confidence (not very, normal, or very). *Style* assesses the predicted amount of time the CEO spent getting their face styled and ready in the morning. By controlling for these variables in our estimations, we further alleviate concerns about spurious aging effects. As we will see, our results are not affected (and sometimes strengthened) by the inclusion of the additional controls.

C. Difference-in-Differences Results

We formalize our analysis of job-induced apparent aging in a difference-in-differences design, analyzing differences between CEOs whose industry was in distress during the financial crisis in 2007 and 2008 versus those whose industry was not in distress. As we have detailed in Section II.C, we use three-digit SIC codes and a 30% decline in equity value criterion to identify firms that experienced an industry shock during the crisis years. This approach classifies 78 out of a total of

146 industries represented in the CEO Apparent Aging Data as distressed during at least one of the crisis years 2007 and 2008. Industries classified as distressed during these years include real estate and banking. Non-distressed industries include agriculture, food products, and utilities.

To account for potential selection bias due to CEOs departing from their job during the Great Recession, we identify treated CEOs based on intended exposure. That is, the treatment variable, $\textit{Industry Distress}_j$, is equal to 1 if CEO j's firm operates in an industry that was distressed in 2007, 2008, or both years, regardless of whether the CEO stepped down in 2006–2008. For example, $\textit{Industry Distress}_j$ is 1 for a CEO departing in 2007 and whose industry was distressed in 2008.[17]

Graphical Evidence. We start from plotting the difference in aging trends between distressed and non-distressed CEOs in Figure 4. For this graphical illustration, we bin our picture sample into eight roughly equal-sized and equal-spaced groups over the sample period, $t \in T = \{\text{pre-2001}, \text{2001-04}, \text{..., post-2015}\}$, and estimate the following difference-in-differences model:

$$\textit{Apparent Age Gap}_{i,j,t} = \beta_0 + \sum_{\substack{t \in T,\\ t \neq 2005\text{-}06}} \beta_{1,t}\, \textit{Industry Distress}_j \times \mathbb{1}_t + \boldsymbol{\beta_2'} \boldsymbol{X}_{i,j,t} + \delta_t + \theta_j + \varepsilon_{i,j,t} \quad (1)$$

where i represents a picture, j represents a CEO, and t represents a time bin. $\textit{Apparent Age Gap}_{i,j,t}$ is the difference between CEO j's apparent age in picture i as assessed by the aging software and their chronological age at time t when the picture was taken. $\mathbb{1}_t$ are time indicators, where the t^{th} indicator is equal to 1 for pictures taken in time bin t. They are interacted with $\textit{Industry Distress}_j$, so that the interaction is 1 if the firm of CEO j shown in picture i was distressed in 2007 or 2008. For the graphical presentation, we normalize against the pre-crisis bin ($t = 2005\text{-}2006$).

The vector of control variables $\boldsymbol{X}_{i,j,t}$ includes time-varying controls for a CEO's pre-2007 industry shock experience and pre-2007 CEO tenure as well as the extensive controls for picture setting and characteristics discussed above, and a control for image sharpness. We measure sharpness using an image's Laplacian, i. e., the second spatial derivative of an image, as described in detail in Appendix B.2. We note that for any of these image controls to affect the results in the first place, they would have to systematically affect the software's age estimate and be correlated with industry distress experience. (We extensively discuss such potential concerns in Section III.D.)

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 CEO fixed effects also account,

[17] Regressing actual 2007-2008 industry shock exposure on intended exposure yields a coefficient of 0.92 (t-statistic of 41.17).

for example, for CEO-specific aging and determinants of aging such as risk attitudes and selection into riskier industries. The time fixed effects absorb time trends, such as improving picture quality. While the aging software has been trained on a large number of faces and pictures of differing quality, these fixed effects tighten the identification (and absorb the main effects of the time-industry shock interaction in the regression).

We weight observations by image sharpness, i. e., images in which detail is rendered more clearly receive larger weight. We discuss the robustness to other weighting choices below with the regression results.

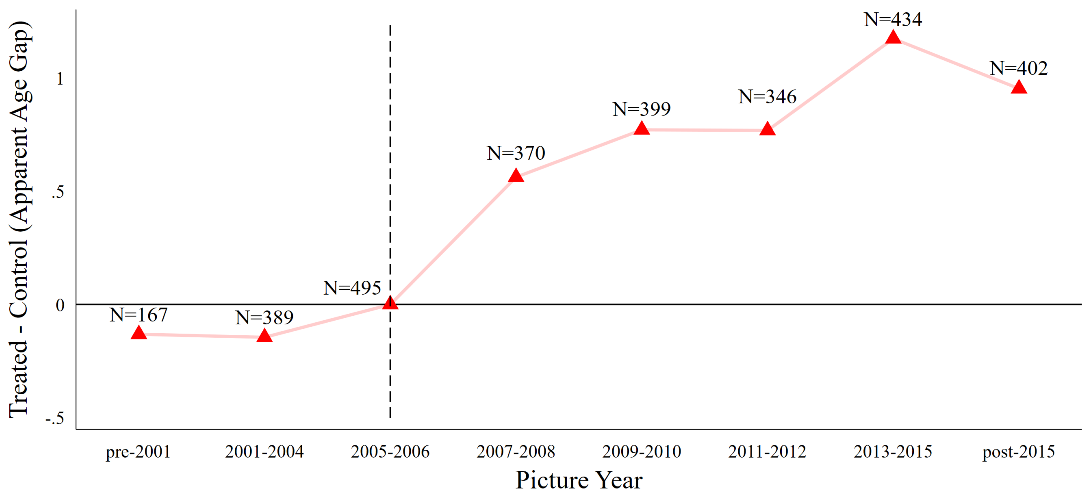

Figure 4. Differences in apparent aging between CEOs with and without industry distress exposure during the Great Recession. This figure depicts the estimated coefficients β_1 of the interaction terms between the time-period indicators and the *Industry Distress* indicator from model (1), where *Industry Distress* is equal to 1 if the CEO's firm was exposed to industry-wide distress during 2007 or 2008. *Apparent-Age Gap* is the difference between apparent and chronological age. Observations are weighted by image sharpness. *N* denotes the number of pictures in each time bin.

Figure 4 plots the estimated components of vector $\boldsymbol{\beta_1} = (\beta_{1,\text{pre-2001}}, \ldots, \beta_{1,t}, \ldots, \beta_{1,\text{post-2015}})$. They capture the differences in the gap between apparent and chronological age between the treated group and the control group at the different points in time, after controlling for covariates and normalized against the pre-crisis bin ($t =$ 2005-06). The difference in apparent-age gap 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. In particular, there is no evidence that CEOs who experience an industry downturn during the Great Recession are aging faster than control CEOs before the crisis. After the onset of the Great Recession, however, the difference in the apparent-age gap increases markedly, first to about half a year, then to about 0.75 years around 2010, then to a full year around 2013. It stabilizes at a level of about one year after 2015. In other words, the graphical evidence indicates that exposure to industry distress accelerates apparent aging starting around the time of exposure and leveling off after a few years to a persistent difference of one year.

We note that the rapid onset of accelerated aging and full realization after a few years are consistent with biological mechanisms linking the hormone responses to cellular damage during periods of chronic stress exposure. For example, a similar time horizon stretching over two to three years has been found for the relationship between stress and mortality in a population with a similar age-cohort distribution to our mortality sample and with access to health care (Nielsen et al. 2008; Rutters et al. 2014).

Regression Model. We turn to estimating the full difference-in-differences regression model, using a specification similar to model (1) above:

$$\textit{Apparent Age Gap}_{i,j,t} = \beta_0 + \beta_1 \textit{Industry Distress}_j \times \mathbb{1}_{\{t>2006\}} + \boldsymbol{\beta_2'} \boldsymbol{X}_{i,j,t} + \delta_t + \theta_j + \varepsilon_{i,j,t}. \quad (2)$$

As before i represents a picture, j represents a CEO, but t now represents a calendar year. All variable definitions remain as as specified for equation (1), and we include the same set of control variables and fixed effects. We also continue to weight observations by image sharpness, and Appendix-Table B.2 shows the robustness to other weighting choices and treatments of outliers. We also test in Appendix-Table B.3 that our measure of image sharpness is uncorrelated with industry distress exposure during the Great Recession. We cluster standard errors at the three-digit SIC code level, at which industry shocks are defined (Abadie, Athey, Imbens, and Wooldridge 2017). The key coefficient of interest is β_1, indicating the change in apparent-age gap after the start of the crisis and during the post-crisis years ($t > 2006$) if the CEO's firm experienced industry shocks during 2007 to 2008.

Table III presents the regression results. In column (1), the coefficient on the interaction term between *Industry Distress* and the post-2006 indicator $\mathbb{1}_{\{t>2006\}}$ is 0.806 (significant at 5%). This indicates that CEOs look about 0.8 years older for a given chronological age if their firm has experienced an industry distress shock between 2007 and 2008. In column (2), we add the extensive set of picture controls for smile vs. frown, mood, portrayed self-confidence, etc. The coefficient on the post-treatment interaction term remains very similar (now 0.882, again significant at 5%).

In columns (3) and (4), we split the post-period into two sub-periods, 2007–2011 and from 2012 onward. The estimates indicate that longer-horizon effects are driving the results. Consistent with the graphical evidence in Figure 4, we estimate a distress-induced apparent-aging effect of around 0.65 years over the earlier five-year horizon that is insignificant or only marginally significant, but an effect of 1-1.2 years over the later horizon, which is significant at 5% or 1%. The estimated effects are similar whether or not we include the additional picture controls.

The long-term persistence of the distress-induced aging effects ameliorates potential concerns about "picture management." If the media or firms engaged in picture selection in a manner that is correlated with distress exposure, it could affect the apparent-age estimates. However, such differential image management is unlikely to occur years after the distress crisis. We further discuss these and other remaining identification concerns in detail in the next subsection.

Overall, the apparent-aging analysis provides robust evidence that increased job demands in the form of industry distress accelerate visual aging. Later, in Section IV, we will find that the effects of industry distress on CEO mortality are very similar in magnitude. Hence, the appearance of aging may presage a shorter lifespan for CEOs whose industries experienced downturns in the Great Recession.

D. Robustness Tests

We perform a series of additional tests, with all tables relegated to Appendix B.5.

CEO Appearance Management. One potential confound of our results is that CEOs might engage in "appearance management." Given their public status, young CEOs may, for example, prefer to grow some facial hair to look older, whereas older CEOs may prefer to dye their hair to look younger.

Such behavior, while plausible, is unlikely to affect our estimates for two reasons. First, there is no difference in chronological age between distressed and non-distressed CEOs as of 2006, the year prior to treatment assignment (Appendix-Table B.4). Thus, general appearance management incentives should not differ by treatment status. Consistent with this, both groups have parallel aging pre-trends, as shown in Figure 4. Second, picture-type control variables such as the facial hair and style controls help alleviate this concern.

Differential Selection of Images During Good and Bad Times. Another potential confound is that newspaper editors might select more grim-faced (and possibly older-looking) CEO images during the crisis, in particular if the firm is in distress. We address this in three ways. First, we point to the stability of the aging effect. Our main result remains stable after five years and throughout the 2010s, when the news about the industry distress and possibly criticism of the CEO would have subsided. In these later pictures such selection is unlikely to be at work (cf. Figure 4). Moreover, the results are robust to including image heterogeneity controls, including assessed smile vs. frown, happy vs. grim, and degree of CEOs' self-confidence.

Second, we show directly that CEOs' facial expression in our sample is not correlated with treatment. That is, when we use the facial expression as the outcome variable (with $+1$ = smiling, 0 = neutral, and -1 = frowning) and test whether industry distress exposure predicts the outcome, we fail to reject the null hypothesis of no significant relationship. This continues to hold when we interact industry distress with a post-dummy to restrict the comparison to pictures from 2007 on (see Panel A of Appendix-Table B.5).

At the same time, we do find that the apparent-age gap is correlated with facial expressions (see Panel B of Appendix-Table B.5). More precisely, we estimate no significant relation when controlling for the full slate of control variables, but there is a significantly positive relation with the frowning indicator when we leave out the mood controls (happy-vs-grim), which are correlated with the smile controls (smiling-vs-frowning).

In light of these findings, we go one step further in addressing the role of smiling-vs-frowning and use AI-based image manipulation techniques to change facial expression. Specifically, we use software that creates natural-looking transformations of faces using machine learning and neural network techniques to change grim expressions towards happier expressions (neutral and even faint smiles). We discuss these manipulations in detail in Appendix B.3. We then include the resulting 341 "fake images" in our analysis and apply the apparent-age software to re-estimate our model. As shown in Appendix-Table B.6, we replicate our findings.

Differential Image Selection Due to CEO Departures. A further concern is that CEO departure rates might differ by industry distress experience, which might then lead to differences in the type of images we can find. For example, news of CEO departures are often accompanied by photos of more grim-faced CEO, and after stepping down, CEOs may assume less prestigious positions with fewer incentives for appearance management. This could drive the differential patterns in apparent aging, even in the long run.

A first amelioration of this concern is that our analysis includes controls for "magazine quality" (assessing the degree to which an image could be used in a magazine) and "style" (assessing time person spent styling their face in the morning). In addition, we show that our results are not driven by differential finding rates of post-crisis images depending on whether CEOs experienced distress during the crisis (Appendix-Figure B.6 and Appendix-Table B.7).

To further address the concern, we collect information on the departure dates and post-departure career trajectories for each CEO in the sample. We then re-estimate our results on the sub-sample of CEOs remaining in office until 2010 or later.[18] As we show in more detail in Appendix B.4, in this subsample CEO departure rates and image "types" (in-office, post-departure but non-retired, retired) are similar across distressed and non-distressed CEOs, muting concerns related to departure-driven picture heterogeneity and selection. Moreover, our aging results are robust (Appendix-Table B.8).

Alternative Empirical Specification. We have also replicated our results with apparent age as the outcome variable and chronological age as an additional control variable (Appendix-Table B.9), rather than using the apparent-age gap as the outcome variable.

Alternative Combination of Trained Neural Networks. Lastly, our results are not sensitive to how exactly we combine the eleven trained neural nets (cf. Section III.A and Appendix B.1). Appendix-Table B.10 shows that the results are unchanged when using the median, rather than the average, apparent-age estimate across all trained models in the regressions.

E. Possible Mechanisms

As discussed earlier, the rapid onset of visible signs of aging is consistent with the medical literature on stress responses. Cortisol and other immediately-triggered "flight-or-fight" hormonal reactions to chronic stress are plausibly linked to our finding of accelerated aging. In particular, Harvanek et al. (2021) find that higher insulin resistance (which can be caused by excess cortisol) positively predicts a person's epigenetic age, which in turn is a strong predictor of mortality (see Föhr et al. 2021).

In light of these plausible biomedical underpinnings of our results, we have explored ways to tease out related physical changes from the picture sample. First, we have considered changes in body mass index (BMI). Biochemically, chronic stress increases cortisol levels (Van der Valk et al. 2018), which slows down metabolism and triggers a desire for "comfort foods." Both can lead to weight increases. In addition, excessive work demands triggered by the crisis might by themselves induce unhealthy eating due to time constraints, and adversely affect exercise behavior. These behavioral changes can evolve into unhealthy habits in the long run, even after heightened

[18] Of course, whether CEOs remain in office until 2010 is endogenous. The selection from this conditioning likely works against identifying the effect of distress as distressed CEOs who remain in office are presumably more "resilient."

job demands subside.

Correspondingly, we have retrieved machine-learning based estimations of the BMI from facial images (Sidhpura et al. 2022). Applying these estimation techniques to our CEO image data, we find suggestive, albeit insignificant evidence of higher BMI levels among treated CEOs over time (Appendix-Table B.11). While more data and less noisy estimates would be needed to draw conclusive evidence, the estimates are suggestive of a slow-moving, long-run behavior change.

In addition to poor diet and lack of exercise, stress might induce other behavior changes that can accelerate aging, including lack of sleep and excess alcohol consumption. To test for those channels, we have constructed measures of the degree of puffy eyes, red eyes, dark areas under eyes, and red face. We find no evidence that these facial aspects are significantly related to industry distress exposure (Appendix-Table B.12). While it is possible that a larger sample and refined definitions of the above features would allow for the detection of a statistically significant relationship, the null effect is consistent with such features more likely reflecting very recent sharp changes in behavior.

In summary, our CEO Apparent Age dataset does not allow to identify specific biomedical or behavioral channels beyond the main result on visible signs of aging, which are a widely used indicator in medicine. Our additional results here are suggestive of changes in BMI, which are consistent with hormonal responses to stress as well as worse diet and exercise behavior.

IV. Industry-Wide Distress Shocks and Life Expectancy

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

A. *Empirical Strategy*

We employ stratified Cox (1972) proportional hazards models to estimate the effect of variation in the exposure to industry distress 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 estimate

$$\ln \lambda(t | \mathit{Industry\ Distress}_{i,t}, X_{i,t}) = \ln \lambda_{0,j}(t) + \beta\, \mathit{Industry\ Distress}_{i,t} + \boldsymbol{\delta}' \boldsymbol{X}_{i,t}, \tag{3}$$

where λ is the hazard rate and $\lambda_{0,j}$ is the baseline hazard rate, which we allow to vary across the Fama and French (1997) 49 industries j. *Industry Distress*$_{i,t}$ is an indicator variable equal to 1 if CEO i has experienced distress by year t, i. e., if in year t or a prior year during the CEO's tenure, the forward-looking two-year stock return of the median firm in the industry is less than -30%. Note that, when a CEO steps down, the value of the distress indicator remains constant at its value at departure. $\boldsymbol{X}_{i,t}$ is a vector of control variables. In our baseline specifications, the controls include a CEO's chronological age, time trends (linear or fixed effects), and location fixed effects. The location fixed effects are based on firms' state of headquarters and absorb state-level characteristics such as general business conditions, pollution, and eating habits to the extent that these are time-invariant. We also present specifications with birth-cohort fixed effects. We cluster standard errors at the three-digit SIC code level, at which industry shocks are defined (Abadie, Athey, Imbens, and Wooldridge 2017).

B. Graphical Evidence

Before presenting the main estimation results, we provide graphical evidence on the mortality effects of CEOs' exposure to industry distress. Figure 5 plots Kaplan-Meier survival graphs, with the vertical axis showing the survival rate and the horizontal axis the time elapsed (in years) since becoming CEO, split by whether CEOs experienced industry distress during their tenure. The non-parametric Kaplan-Meier estimator discretizes time into intervals $t_1, ..., t_J$, and is defined as $\widehat{\lambda_j^{KM}} = \frac{f_j}{r_j}$, where f_j is the number of spells ending at time t_j and r_j is the number of spells that are at risk at the beginning of time t_j.

For the graphical presentation, we restrict the sample to CEOs who were appointed no later than ten years prior to the retirement age of 65 and who stepped down no later than ten years after the retirement age.[19] This restriction increases similarity with respect to age among CEOs and thus accounts indirectly for age being a strong predictor of mortality and of treatment (due to the fact that once treated, CEOs remain classified as treated in subsequent years). In the univariate Kaplan-Meier survival graphs, we cannot control for age directly since these graphs plot survival unadjusted for covariates. In our Cox (1972) regressions below, we will be able to directly control for age as a covariate in the model.

Figure 5 shows that the survival line for CEOs who were exposed to industry distress during

[19] Appendix-Figure C.1 plots the retirement hazard for CEOs in our main sample, confirming a large spike in retirements at age 65, consistent with the evidence in Jenter and Lewellen (2015). While CEOs may continue to work after stepping down, few (32 in total) become CEO at another firm in our sample.

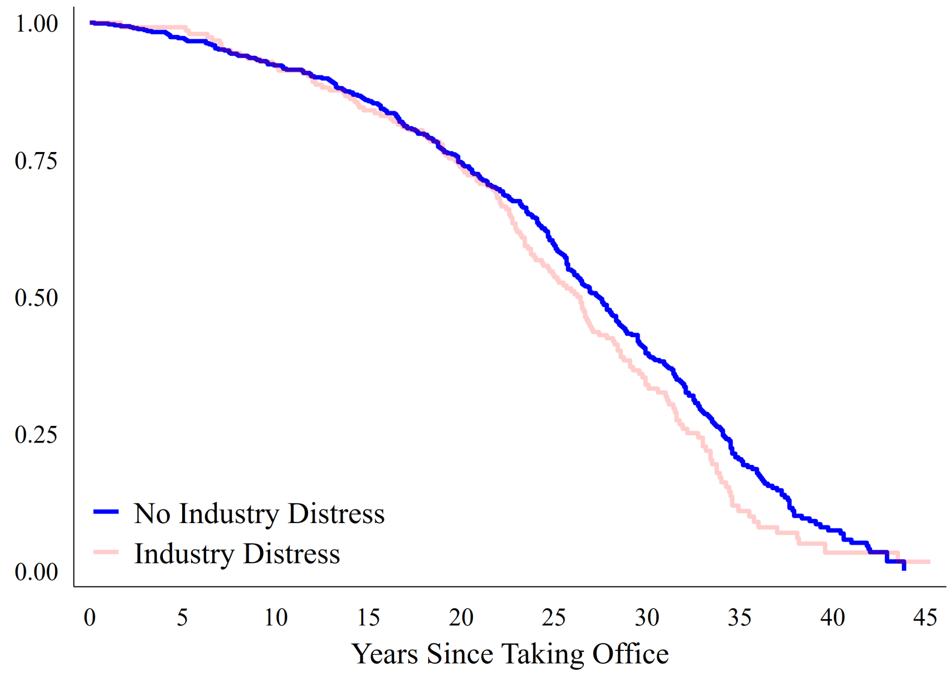

Figure 5. Kaplan-Meier survival estimates. This figure shows Kaplan-Meier survival plots of the relation between industry distress experience and longevity. The vertical axis shows the fraction of CEOs who are still alive. The horizontal axis reflects time elapsed (in years) since a person became CEO.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

their tenure is visibly left-shifted at longer horizons. That is, distressed CEOs have significantly worse long-run survival patterns than non-distressed CEOs. For example, about 67 percent of distressed CEOs have passed away 30 years after their appointment, whereas it takes closer to 32 years for the non-distressed CEO group. The hazard coefficient on industry distress corresponding to the survival patterns in Figure 5 is 0.178, which is in the ballpark of the Cox (1972) regression estimates we discuss next. The survival plot offers first, suggestive evidence that experiencing industry distress as CEO is associated with adverse consequences in terms of life expectancy. Our hazard analysis below formalizes the observed patterns.

C. Main Results

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

The specification in column (1) estimates the model with the industry-distress indicator, a linear control for age, a linear control for time trends, and location fixed effects based on firms' state of headquarters, which absorb state-level characteristics such as general business conditions, pollution, and eating habits to the extent that these are time-invariant. In column (2), we replace the linear time control with year fixed effects, and in column (3), we include CEO birth-year fixed effects instead of year fixed effects. Columns (4) to (6) re-estimate columns (1) to (3), including as additional control variables CEO pay (from Gibbons and Murphy 1992) and firm size (assets and employees from Compustat). These controls are available for 96% of sample observations.

Across specifications, we estimate a robust, statistically and economically significant effect of industry distress on the mortality hazard. Averaging the estimates across columns, distress experience increases CEOs' log mortality hazard by 0.136, i.e., given a hazard ratio of $\exp(0.136) = 1.145$, the mortality hazard increases by 14.5%.

Turning to the control variables, the effect of age is significantly positive across specifications, reflecting that older people have a higher risk of dying. Note that the linear age term is motivated by the Gompertz (1825) "law of mortality," i. e., the empirical regularity that the risk of dying follows a geometric increase after middle age (Olshansky and Carnes 1997). In untabulated results, we obtain virtually identical estimates when including higher-order age terms. The linear time control in columns (1) and (4) is close to zero and insignificant, suggesting no general time trends in the survival of CEOs over the sample period.

Economic Significance. One way to evaluate the magnitude of the estimated distress effect on longevity is relative to other predictors, in particular relative to age: What increase in chronological age does the effect of distress exposure on mortality correspond to? Averaging across all columns, the estimated effect of age on the log mortality hazard is 0.120. This means that the effect of industry distress exposure corresponds to the effect of being 1.1 years older ($0.136/0.120 \approx 1.1$).

This "in-sample" comparison has the advantage that it is based on data from the sample CEOs, who have a higher baseline life expectancy. Alternatively, we can compare the estimated mortality hazard with mortality statistics of the general U.S. population. For example, at age 57 (the median

CEO age in our sample), the one-year mortality rate of a male American born in 1927 (the median birth year in our sample) is 1.337% (Human Mortality Database 2019). Industry distress experience as CEO pushes this rate up to 1.532%, which is roughly the mortality rate of a male born in 1927 at age 59, i. e., when two years older.

Finally, we can compare our mortality estimates to the estimated effect of other economic stressors in different populations. For example, in Sullivan and Von Wachter (2009), job displacement increases the mortality hazard by 10–15% and reduces life expectancy by 1–1.5 years. This effect is similar to our industry-distress point estimates, though as noted, job displacement reduces time and effort spent at work (Krueger and Mueller 2012) whereas industry distress induces CEOs to spend more time and effort at work. Nicholas (2023) estimates larger mortality differences in his sample of General Electric employees, finding that working as an executive was associated with a 3–5-year reduction in longevity compared to lower-hierarchy employees.

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

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

D. Robustness Tests and Mechanisms

We perform several additional robustness tests, with all tables relegated to Appendix C.

First, to further examine potential confounds around heterogeneity in life expectancy over time, and this heterogeneity being correlated with industry distress exposure, Appendix-Table C.2 estimates alternative specifications that allow the effect of age on mortality to be cohort-specific. Specifically, we sort CEOs into quintiles based on birth year, and allow for separate age estimates. While there are small differences in age effects across CEO cohorts, the industry distress coefficients are barely affected and remain statistically and economically significant.

Second, we re-estimate the effect of industry distress when varying the censoring year for CEOs' alive status. This check alleviates concerns that we may have failed to identify some deaths, hence granting "extra years" of life to these CEOs (cf. footnote 11). The coefficients remain stable as we gradually move the censoring date for CEOs identified as alive as of 10/2017 from Oct. 1, 2017 to

Dec. 31, 2010 (Appendix-Figure C.2).

Third, we have explored specific recession periods similar to the analysis in Section III, such as the 1987 stock-market downturn or the 1981-82 recession. However, fewer than five percent of the CEOs in our sample experienced either of these shocks so that we lack statistical power when applying the same methodology. The corresponding estimates indicate that CEOs who experienced these specific shocks tend to have a higher mortality hazard, though they are generally not statistically significant.

We also explored the availability of data on the causes of death as they might elucidate the mechanisms relating industry downturns to mortality. We found that cause of death is sometimes reported in CEOs' obituaries and in news articles, albeit less commonly than date of death, and that it is never reported on ancestry.com, one of our primary sources for death. We were able to extract cause of death information for 493 of the 1,247 CEOs who passed away in our sample. Within this relatively small and possibly selected sample, industry distress is associated with a higher frequency of deaths due to heart disease or stroke, and a lower frequency of deaths due to cancer or Alzheimer's disease, though we lack power to rule out that these differences are due to chance.

In summary, while the results are robust to a wide range of mortality specifications, the data does not allow to pin down specific crises or specific medical causes of mortality.

V. Corporate Monitoring and Life Expectancy

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

A. *Empirical Strategy*

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

$$\ln\, \lambda(t|BC_{i,t}, \boldsymbol{X}_{i,t}) = \ln\, \lambda_{0,j}(t)\ + \beta I(BC_{i,t}) + \boldsymbol{\delta}'\boldsymbol{X}_{i,t}, \tag{4}$$

where $I(BC_{i,t})$ is an indicator variable equal to 1 if CEO i in industry j has been exposed to a BC law by year t. As in Section IV.A, in the baseline models $\boldsymbol{X}_{i,t}$ includes chronological age, time trends (or fixed effects), and state-of-headquarters fixed effects. We also verify again that all of our results are robust to specifications that account for CEO birth cohorts. Furthermore, we add as an additional control an indicator variable capturing a CEO's exposure to first-generation anti-takeover laws to all specifications. Karpoff and Wittry (2018) emphasize the importance of accounting for the historical political economy of the BC laws and therefore include a control for first-generation laws in their empirical tests. In Section V.D, we also implement a host of further robustness tests proposed by Karpoff and Wittry (2018).

Second, we test for differential effects depending on BC law exposure intensity. Given that the laws led to a permanent corporate governance regime change, rather than a temporary exposure (as in the case of industry distress), we replace the indicator $I(BC_{i,t})$ with a measure $BC_{i,t}$ that counts the exposure length in years until year t:[20]

$$\ln\ \lambda(t|BC_{i,t},\boldsymbol{X}_{i,t}) = \ln\ \lambda_{0,j}(t)\ + \beta BC_{i,t} + \boldsymbol{\delta}'\boldsymbol{X}_{i,t}. \tag{5}$$

Relative to the binary measure, the cumulative exposure measure is more prone to endogeneity concerns for long-serving CEOs. We address this concern in Section V.D, where we examine initial vs. incremental exposure as well as *predicted* exposure length.

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

B. *Graphical Evidence*

We again start by plotting Kaplan-Meier survival graphs. As in Figure 5, we focus on CEOs who started and ended their tenure within +/- ten years around the retirement age of 65, so that the univariate plots account for CEO age to some degree.

Panel (a) of Figure 6 plots the survival lines of this set of CEOs comparing those who became CEO in the 1970s and were never shielded by a BC law, those who became CEO in the 1980s and were not shielded by a BC law, and those who became CEO in the 1980s and were insulated by BC law protection during their tenure.[21]

Two results emerge. First, the survival patterns of the 1970s and 1980s cohorts without BC

[20] In these specifications, we also replace the indicator for first-generation law exposure with an exposure-length measure. In both cases, we calculate exposure length up to daily precision. For example, Delaware's BC law was adopted on 2/2/1988, and a CEO's exposure in Delaware in 1988 is calculated as $BC_{i,1988} = \frac{365-\text{doy}(2/2/1988)}{365} = 0.92$.

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

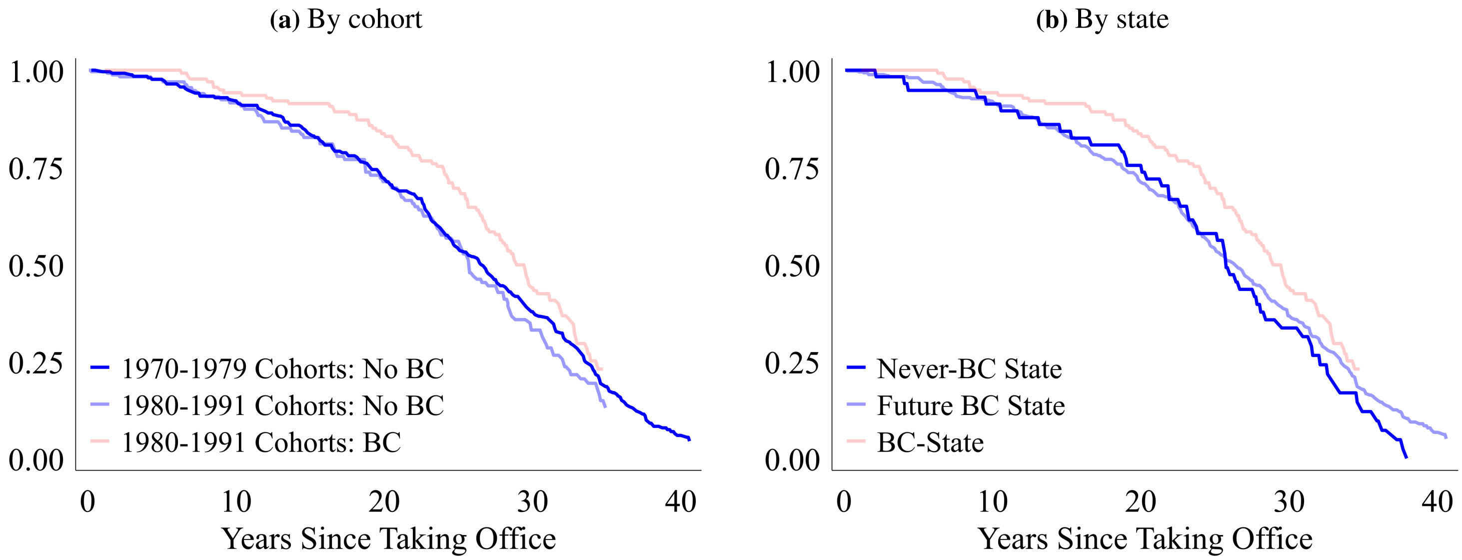

Figure 6. Kaplan-Meier survival estimates. This figure shows Kaplan-Meier survival plots for the relation between anti-takeover law protection and longevity. The vertical axis shows the fraction of CEOs who are still alive. The horizontal axis reflects time elapsed (in years) since a person became CEO. Panel (a) compares the survival of CEOs starting in the 1970s who never served under a BC law (dark blue) to those who became CEO in the 1980s and never served under a BC law (light blue) and those who became CEO in the 1980s and were eventually exposed to a BC law (light red). Panel (b) splits the CEOs from Panel (a) based on whether their state never passed a BC law (dark blue), passed a BC law after the CEO stepped down (light blue), or passed a BC while in office (light red). As in Figure 5, both panels focus on CEOs appointed up to ten years prior to the retirement age of 65 and stepping down up to ten years after the retirement age.

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

To address concerns about systematic differences between BC and non-BC states affecting the results, Panel (b) reshuffles CEOs in Panel (a)'s No-BC-cohorts, grouping them by whether their state eventually enacted a BC law after the CEO stepped down (light blue) or not (dark blue). The survival lines for these groups are virtually identical. Only CEOs in BC states *with* BC exposure (light red) 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 estimations will include location fixed effects and are robust to using state-of-incorporation fixed effects.

The survival plots suggest significant adverse consequences in terms of life expectancy associated with serving under more stringent corporate governance regimes. The underlying hazard coefficient on BC law exposure is -0.228, which is very similar to the Cox (1972) hazard model results we turn to next.

C. *Main Results*

Table V shows the hazard estimates based on empirical model (4) in the first four columns, and based on model (5) in the last four columns. That is, in columns (1) to (4), we summarize the total effect of BC law exposure with the indicator $I(BC_{i,t})$, which is equal to 1 if CEO i has been exposed to a BC law by time t. In columns (5) to (8), we estimate the linear effect in years of exposure, $BC_{i,t}$. We include fixed effects and controls as in the Table IV specifications with year controls (linear or fixed effects), in addition to the control for first-generation law exposure.

Across all columns, we estimate a statistically and economically strong effect of BC law protection on mortality. For the BC indicator in columns (1) to (4), the hazard coefficient ranges from -0.198 to -0.234. For the cumulative BC exposure measure in columns (5) to (8), the coefficient ranges from -0.037 to -0.040. Averaging the latter estimates across columns, a one-year increase in exposure to more lenient governance is estimated to reduce a CEO's mortality risk by 3.8%. For a CEO with a typical BC law exposure, both BC exposure measures imply very similar effects on longevity.[22]

The estimated effects of control variables on mortality rates mirror those in Table IV, with the linear time control being insignificant and age strongly predicting mortality rates. The coefficients on the first-generation anti-takeover controls are negative throughout, pointing to health benefits arising also from first-generation law protection. These estimates are, however, not reliably significant and their magnitudes fluctuate across specifications.

Economic Significance. The estimates pertaining to the BC exposure variables imply meaningful effect sizes. First, we use again the "in-sample" comparison to other CEOs. Based on the average hazard coefficient estimates from the specifications with the BC indicator variable, -0.217 for BC exposure and 0.112 for age, the effect of BC law protection on mortality is equivalent to being about two years younger. This effect size is of a similar order of magnitude as the 1.1 year increase estimated for exposure to industry distress. The somewhat larger mortality effect, compared to the

[22] The cumulative measure estimates a 16% shift in mortality hazard associated with the median BC exposure of 4.41 years, close to the 18-21% shift estimated with the BC indicator.

industry-shock experience, might reflect the more permanent nature of the BC law experience.

Second, we use again the general U.S. population life tables to assess the magnitude of the estimate. Here, the median exposure to lenient governance of 4.4 years pushes the 1.337% mortality rate of males born in 1927 rate down to 1.128%, which is roughly the mortality rate of males born in 1927 at age 54.5, i. e., when two and a half years younger.

As with the industry-distress analysis, we also collected data on the causes of death. We were able to locate information for 450 of the pre-BC law CEOs, about 35 percent of passed CEOs in the pre-BC-law sample. The data does not allow to detect a specific cause of death as the driver of the relationship between BC laws and lifespan, though the frequency of strokes is somewhat higher in the high-job-demands (i.e., non-BC) subsample, as we also saw in the industry distress analysis.

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

D. Robustness Tests

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

Other Specifications and Sample Choices. We first test and confirm that our results are robust to using birth-year fixed effects, using the same specification as in columns (3) and (6) in the industry-distress regressions in Table IV, cf. Appendix-Table D.2. Mirroring the robustness test in Section IV.D, the results also hold with age-by-birth-cohort controls (Appendix-Table D.3).

Next, as in Section IV.D, our results are robust to different censoring date choices (Appendix-Figure D.2).

Finally, while it is standard in the BC law literature to assign location fixed effects based on firms' headquarters (cf. Gormley and Matsa 2016), the results are also robust to using state-of-incorporation fixed effects (Appendix-Table D.4). The stability of the estimates suggests that firm locations do not affect the relationship between governance and longevity. Alternatively, the results are robust to keeping state of headquarters fixed effects but dropping CEOs who stepped down significantly before the passage the BC laws (Appendix-Figure D.3), which further ameliorates potential concerns specific to the BC law results around differences between BC-protected and non-protected CEOs.

Other Anti-Takeover Laws. The results are also robust to using the first-time enactment of any of the five second-generation anti-takeover laws as identifying variation (Appendix-Table D.5). 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 the extensive robustness checks proposed in Karpoff and Wittry (2018). We account for firms lobbying for the passage of BC laws or opting-out, as well as confounding effects of firm-level defenses, and further restrictions based on the first-generation anti-takeover laws (Appendix-Tables D.6 and D.7). Additionally, the results are robust to data cuts based on state of incorporation and industry affiliation (Appendix-Table D.8).

Nonlinear Exposure Effects and Predicted Length of Exposure. The final two robustness checks address concerns that the cumulative BC specification in equation (5) picks up CEOs' endogenous selection into a long tenure. Note that this concern does not apply to the indicator strategy, and thus does not threaten our main findings in Table V, but merely the magnitude of the per-year estimates in columns (5) to (8).

We first address this by examining separately the mortality effects of initial years and incremental years of BC law exposure. Columns (1) and (2) in Appendix-Table D.9 re-estimate columns (5) and (6) of Table V, but with $BC_{i,t}$ split into below- and above-median exposure, $BC_{i,t}^{(\text{min-}p50)}$ and $BC_{i,t}^{(p51\text{-max})}$. Here, the below-median exposure variable counts exposure years up to the sample median (conditional on exposure) of 4.41 years, and the above-median exposure variable records incremental exposure.[23] In both columns, the hazard coefficient on below-median BC exposure is strongly significant. By contrast, the coefficient on above-median BC exposure is close to zero and insignificant. These results imply that the estimated survival gains are driven by the initial years of reduced monitoring, rather than by the tails of long-tenured CEOs.

Second, we estimate a hazard model using a CEO's predicted, rather than true length of BC exposure. The prediction model only uses information from prior to the BC law passage to predict CEOs' remaining tenure over time, from which predicted BC exposure is derived; see Appendix D for full details. The results are reported in columns (3) and (4) of Appendix-Table D.9. The hazard coefficient estimates of -0.042 in both columns are very similar to those in Table V, while the

[23] 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 4.41, and $BC_{i,t+1}^{(p51\text{-max})}$ to 0.59. In year $t+2$, $BC_{i,t+2}^{(\text{min-}p50)}$ remains at 4.41, and $BC_{i,t+2}^{(p51\text{-max})}$ increases to 1.59.

standard errors, now bootstrapped (since we use a generated regressor), are larger.[24]

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

E. Business Combination Laws and CEO Pay

The permanent corporate governance regime changes induced by BC laws also lend themselves to studying the extent to which managers account for the health implications of their jobs, for example in negotiating pay. We conduct a simple calibration exercise that builds on the literature on the value of a statistical life (Viscusi and Aldy 2003). (See Appendix D for details.) We calculate that, if CEO pay reflects working conditions, then a reduction in mortality risk of 4.0% per year of BC exposure (as estimated in column (5) of Table V) would imply a CEO pay change between -2.5% and -10%.[25] By contrast, when we turn from the theoretical calibration to the empirical relationship between BC law protection and pay, we estimate a positive, albeit statistically insignificant effect of BC law passage on pay (Appendix-Table D.10). The estimates indicate a pay increase of around 4.5–7.6%.[26] The apparent lack of a compensating differential casts doubt on whether all parties fully account for the health implications of different governance regimes.

VI. Conclusion

In this paper, we assess the health consequences of being exposed to increased job demands and a more stressful work environment while in a high-profile CEO position. We analyze the consequences for CEOs' aging and mortality using two sources of variation in job demands, industry-wide distress shocks and the staggered introduction of anti-takeover laws.

We first show that industry distress is reflected in short- to medium-term signs of adverse health consequences, namely faster visible aging. To the best of our knowledge, we are the first to collect and utilize panel data of facial images and apply machine-learning based apparent-age estimation software in social-science research. Implementing a difference-in-differences design that exploits

[24] A regression of true BC exposure on predicted exposure yields a coefficient of 1.215, which indicates that the prediction well approximates the true exposure. The estimated effects remain sizable if we divide them by 1.215, amounting to $-0.042/1.215 = -0.035$.

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

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

variation in industry distress during the Great Recession, we estimate that CEOs who experienced industry distress during the 2007-2008 financial crisis look roughly one year older than those whose industry did not suffer the same level of distress. The effect of distress on aging becomes slightly larger over time, increasing to 1.18 years if we analyze pictures from 2012 and afterwards.

We then document, using an earlier CEO sample, more long-term adverse health outcomes associated with strenuous job demands. CEOs who experienced periods of industry-wide distress during their tenure die significantly earlier. We estimate a mortality effect corresponding to that of a 1.1-year increase in chronological age. In line with these results, we observe significant improvements in life expectancy for CEOs who became shielded by an anti-takeover law during their tenure.

In sum, our results indicate that financial distress and stricter corporate governance regimes—the latter of which are generally viewed as desirable and welfare-improving—impose significant personal health costs on CEOs. While we lack direct physical or medical measures of heightened stress, the evidence implies a substantial personal cost for CEOs in terms of their health and life expectancy. As such, our findings also contribute to the literature on the trade-offs between managerial incentives and private benefits arising from the separation of ownership and control. We document and quantify a previously unnoticed yet important cost—personal health cost—associated with serving under strict corporate governance.

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

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

Finally, another promising avenue is the more fine-grained identification of stressors. Which aspects of individual job situations and which decisions tend to have the largest adverse health consequences, for either management or regular employees? In the context of CEOs, our results on the mortality-improving effects of takeover protection point to a role for shareholder scrutiny and disciplining mechanisms. Our results on accelerated aging and mortality in response to industry distress suggest that recession-triggered "tough" decisions such as downsizings and layoffs may also be particularly relevant.[27] Consistent with a mechanism related to layoffs, Guenzel et al. (2023) find evidence of a personal cost for CEOs associated with firing employees. Furthermore, heightened workplace stress can also adversely affect other aspects of life, including marriage, divorce rates, and parenting. We leave these topics for future research.

[27] We have also explored whether the adverse health outcomes in response to industry-wide equity-value declines vary with CEO compensation structures. We find no evidence that the mortality or aging results differ by the degree of CEOs' cash compensation or salary relative to performance-contingent pay, which speaks against a channel related to changes in material well-being induced by distress shocks as an important driver of our findings.

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Table I
Summary Statistics of CEO Apparent Aging Data

Panel A presents image-level summary statistics. Panel B presents CEO-level statistics. Panel C contains information on the industry distribution in our sample. *Industry Distress* during 2007-2008 is an indicator variable for distress experience during these years. *Industry Distress* pre-2007 is an indicator variable for distress experience prior to 2007. All variables are defined in Appendix A.

Panel A: Image Statistics						
	N	Mean	SD	P10	P50	P90
Picture Year	3,002	2008.86	5.68	2003	2009	2016
Chronological Age	3,002	59.02	8.09	48.96	58.88	69.04
Industry Distress (2007-2008)	3,002	0.67	0.47	0	1	1
Apparent Age	3,002	55.14	7.00	45.71	55.85	63.35
Picture Sharpness	3,002	2.57	0.74	1.55	2.58	3.54
Logo	3,002	0.19	0.39	0	0	1
Side Face	3,002	0.23	0.42	0	0	1
Professional Clothes	3,002	0.81	0.40	0	1	1
Magazine Shot	3,002	0.00	0.04	0	0	0
Magazine Quality	3,002	2.80	1.04	1	3	4
Natural Pose	3,002	0.67	0.47	0	1	1
Natural Lighting	3,002	0.18	0.38	0	0	1
Glasses	3,002	0.32	0.47	0	0	1
Facial Hair	3,002	1.19	0.56	1	1	2
Smile	3,002	1.69	0.67	1	2	3
Mood	3,002	1.76	0.69	1	2	3
Self-Confidence	3,002	2.22	0.65	1	2	3
Style	3,002	3.22	0.70	2	3	4

Panel B: CEO Statistics						
	N	Mean	SD	P10	P50	P90
No. of Pictures per CEO	453	6.63	4.36	2	6	12
Chronological Age in 2006	453	56.68	6.96	48	57	65
Tenure (Pre-2007)	453	8.35	7.89	2	6	18
Industry Distress (2007-2008)	453	0.65	0.48	0	1	1
Industry Distress (pre-2007)	453	0.38	0.48	0	0	1

Panel C: Industry Distribution	
Industry (No. of CEOs)	Manufacturing (175); Finance, Insur, Real Estate (66); Wholesale and Retail (70); Services (44); Other (30); Trans.; Commns.; Elec., Gas, and Sanitary Services (69)

Table II
Summary Statistics of CEO Mortality Data

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

	N	Mean	SD	P10	P50	P90
Birth Year	1,900	1926.87	9.48	1915	1927	1940
Passed Away (by October 2017)	1,900	0.66	0.48	0	1	1
Year of Death	1,247	2003.72	9.83	1989	2006	2015
Age at Death	1,247	80.89	9.91	67	82	92
Age Taking Office	1,900	51.87	6.87	43	52	60
Year Taking Office	1,900	1978.74	8.04	1969	1979	1989
Tenure	1,900	10.04	6.70	2.5	8.75	19
Industry Distress	1,900	0.39	0.49	0	0	1
BC	1,605	2.21	4.20	0	0	8.24
BC | BC> 0	616	5.75	5.05	0.75	4.41	12.37

Table III
Industry Distress and CEO Aging

This table shows OLS estimates of the effect of industry distress during the Great Recession on CEO apparent aging, identified from CEOs' facial images. The dependent variable is the apparent-age gap, i. e, the difference between apparent and chronological age. *Industry Distress* is equal to 1 if the CEO's firm was exposed to industry-wide distress during 2007 or 2008. Observations are weighted by image sharpness. 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.

	(1)	(2)	(3)	(4)
Industry Distress $\times$ $\mathbb{1}_{\{t>2006\}}$	0.806**	0.883**		
	[0.382]	[0.382]		
Industry Distress $\times$ $\mathbb{1}_{\{2006<t<2012\}}$			0.634	0.670*
			[0.386]	[0.396]
Industry Distress $\times$ $\mathbb{1}_{\{t\geq 2012\}}$			1.049**	1.183***
			[0.462]	[0.448]
CEO FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
CEO Controls	Y	Y	Y	Y
Picture Controls		Y		Y
No. of CEOs	453	453	453	453
Observations	3,002	3,002	3,002	3,002

Table IV
Industry Distress and Mortality

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

	(1)	(2)	(3)	(4)	(5)	(6)
Industry Distress	0.108**	0.103*	0.147**	0.140**	0.137**	0.178***
	[0.053]	[0.055]	[0.058]	[0.059]	[0.061]	[0.063]
Age	0.119***	0.119***	0.121***	0.117***	0.117***	0.125***
	[0.006]	[0.006]	[0.007]	[0.006]	[0.006]	[0.007]
ln(Pay)				−0.005	−0.011	0.011
				[0.045]	[0.046]	[0.047]
ln(Assets)				−0.078	−0.084*	−0.091*
				[0.047]	[0.048]	[0.052]
ln(Employees)				0.029	0.033	0.038
				[0.049]	[0.049]	[0.051]
Year	−0.003			0.001		
	[0.005]			[0.006]		
FF49 Strata	Y	Y	Y	Y	Y	Y
Location FE	Y	Y	Y	Y	Y	Y
Year FE		Y			Y	
Birth Year FE			Y			Y
No. of CEOs	1,900	1,900	1,900	1,818	1,818	1,818
Observations	58,034	58,034	58,034	55,796	55,796	55,796

Table V
Business Combination Laws and Mortality

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
I(BC)	−0.234***	−0.229***	−0.208**	−0.198**				
	[0.086]	[0.083]	[0.085]	[0.081]				
BC					−0.040***	−0.039***	−0.038***	−0.037***
					[0.006]	[0.006]	[0.008]	[0.008]
I(FirstGen)	−0.035	−0.101**	−0.036	−0.109**				
	[0.048]	[0.047]	[0.042]	[0.043]				
FirstGen					−0.016	−0.024*	−0.012	−0.020*
					[0.014]	[0.014]	[0.011]	[0.011]
Age	0.111***	0.112***	0.111***	0.112***	0.109***	0.109***	0.109***	0.109***
	[0.004]	[0.004]	[0.005]	[0.005]	[0.004]	[0.004]	[0.005]	[0.005]
ln(Pay)			0.001	0.001			−0.009	−0.008
			[0.035]	[0.035]			[0.040]	[0.040]
ln(Assets)			−0.030	−0.040			−0.007	−0.016
			[0.035]	[0.035]			[0.032]	[0.032]
ln(Employees)			0.004	0.008			−0.013	−0.011
			[0.032]	[0.032]			[0.033]	[0.034]
Year	−0.001		−0.001		−0.003		−0.002	
	[0.005]		[0.005]		[0.006]		[0.006]	
FF49 Strata	Y	Y	Y	Y	Y	Y	Y	Y
Location FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE		Y		Y		Y		Y
No. of CEOs	1,605	1,605	1,553	1,553	1,605	1,605	1,553	1,553
Observations	50,530	50,530	49,052	49,052	50,530	50,530	49,052	49,052

Internet Appendix

Appendix A Variable Definitions

Variable Name	Definition
	Panel A: CEO Apparent Aging Analysis
Apparent Age$_{i,t}$	How old CEO in picture i taken in year t looks. The apparent age is estimated using a machine-learning based software by Antipov et al. (2016) that has been specifically developed for apparent-age estimation. See Appendix B.1 for additional detail.
Chronological Age$_{i,t}$	CEO i's age in year t.
Apparent-Age Gap$_{i,t}$	Difference between CEOs' apparent age in picture i taken in year t and their chronological age in year t.
Industry Distress$_{i,t}$	Indicator equal to 1 if CEO i is exposed to industry distress, defined as in Panel B, in 2007 and/or 2008.
Image Sharpness	Sharpness of image measured as detailed in Appendix B.2.
Logo	Indicator equal to 1 if there is a logo (e.g., the GettyImages logo) on the CEO's face in the picture.
Side Face	Indicator equal to 1 for images showing a side face instead of a front face.
Professional Clothes	Indicator equal to 1 if CEO is in "work mode," say wearing business clothes, and 0 if in "casual mode," say wearing a T-shirt.
Magazine Shot	Indicator equal to 1 if image is taken from a magazine cover.
Magazine Quality	Five-point categorical variable assessing the degree to which an image could be used in a magazine (no chance, likely not, maybe, yes, definitely).
Natural Pose	Indicator equal to 1 if CEO did not expect the picture and 0 if CEO expected the picture (e.g., photo call).
Natural Lighting	Indicator equal to 1 if lighting appears natural and 0 if lighting appears unusual, e.g., black-and-white image, stage lighting, etc.
Glasses	Indicator equal to 1 if CEO wears glasses.
Facial Hair	3-point categorical variable assessing the degree of facial hair (none, very little or stubble, regular).
Smile	3-point categorical variable assessing facial expression (smile, frown, neither).
Mood	3-point categorical variable assessing mood (happy, grim, neutral).
Self-Confidence	3-point categorical variable assessing the degree of portrayed self-confidence (not very, normal, very).
Style	5-point categorical variable assessing time person spent getting ready (with their face) in the morning (none, less than usual, normal, more than usual, celebrity-level effort).

Panel B: CEO Mortality Analysis	
$Age_{i,t}$	CEO i's (chronological) age in year t.
Birth Year	CEO's year of birth.
Dead (by Oct. 2017)	Indicator for whether a CEO has passed away by October 1st, 2017.
Year of Death	CEO's year of death, calculated up to monthly level.
Age Taking Office	CEO's age when appointed as CEO.
Year Taking Office	Year in which a CEO is appointed.
$Tenure_{i,t}$	CEO i's cumulative tenure (in years) at time t.
$Industry\ Distress_{i,t}$	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 code across CRSP, Compustat, and CBSD, and the latter in case of a tie.
$I(BC_{i,t})$	Indicator equal to 1 if CEO i is insulated by a BC law in year t; remains at 1 in all subsequent years $\tau > t$, including after CEO departure.
$BC_{i,t}$	CEO i's cumulative exposure to a BC law during tenure up to time t (in years); remains constant after CEO departure.
$BC_{i,t}^{(\text{min-}p50)}$	CEO i's below-median (4.4 years) cumulative BC law exposure during tenure up to time t (in years); remains constant after CEO departure.
$BC_{i,t}^{(p51\text{-max})}$	CEO i's above-median (4.4 years) cumulative BC law exposure during tenure up to time t (in years); remains constant after CEO departure.
$I(FL_{i,t})$	Indicator equal to 1 if CEO i is insulated by the first-time enactment of a 2nd generation anti-takeover law (FL) in year t; constant after CEO departure.
$FL_{i,t}$	CEO i's cumulative exposure to the first-time enactment of a 2nd gen. anti-takeover law (FL) during tenure up to time t (in years); constant after CEO departure.
$Year_{i,t}$	Year of a subspell; used in hazard models when linearly controlling for time.
$Pay_{i,t}$	CEO i's total pay in year t (Gibbons and Murphy 1992); missing data is interpolated.
$Assets_{j,t}$	Firm j's total assets in year t (Compustat); missing data is interpolated.
$Employees_{j,t}$	Firm j's number of employees in year t (Compustat); missing data is interpolated.

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

1. Apparent-Age Estimation

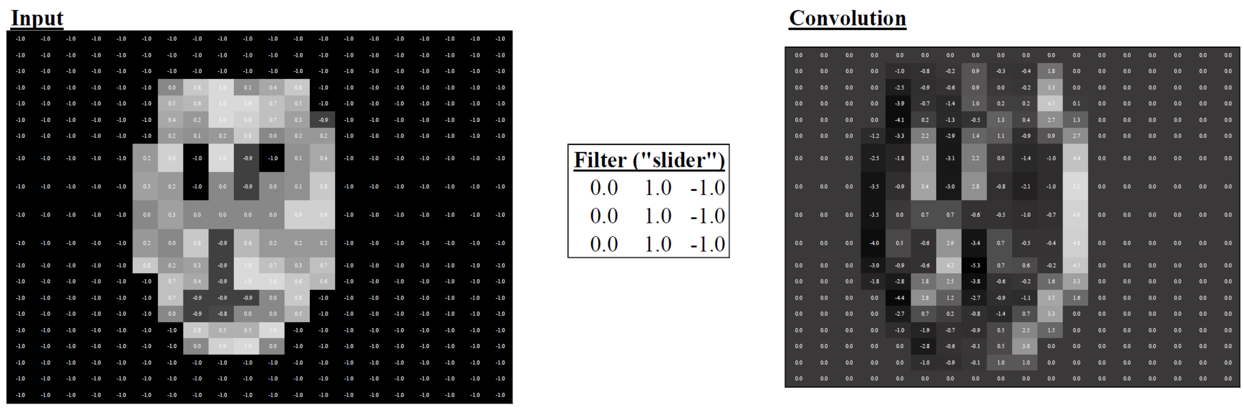
We use machine learning based software by Antipov et al. (2016), henceforth referred to as ABBD. This software was developed for the purpose of apparent-age estimation, and was the winning solution of the second edition of the *ChaLearn Looking At People* competition in the *apparent-age estimation*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

At the core of ABBD's apparent-age estimation tool is the training of a *convolutional neural network* (CNN). A CNN is a special class of *neural networks* that is particularly useful for image recognition and computer vision problems. A neural network is a system that learns to perform a task by studying training data.[1] It is architectured with three classes of layers: input, output, and hidden layers. The input layer receives the external data being evaluated, and the output data contains the network's response to the input. The hidden layers in between abstractly determine intermediate features of the data. A CNN is a neural network in which some of the hidden layers employ the method of convolution, i. e., of transforming the input by sliding (or, convolving) over it, to detect patterns (such as edges or corners), which are then passed on to the next layer.

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

CNNs have become widely used over the past ten to twenty years, with numerous applications, in particular to image recognition and classification. In an influential article on *deep learning*[2] 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."

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



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

ABBD's apparent-age estimation software starts from a pre-trained version of a state-of-the-art CNN for face recognition called VGG-16,[3] 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 chronological age of the person. The training step is implemented by minimizing the mean absolute error between predicted age and chronological age. In a second step, the software is fine-tuned for apparent-age estimation on a unique dataset of 5,613 facial images that also contains information on people's *apparent* age, consisting of at least 10 human age estimates (per picture), which were specifically collected for the *ChaLearn Looking At People* competition. The fine-tuning step is implemented by minimizing a metric that penalizes deviations from the average (human) age estimate more when the disagreement about the person's apparent age is low.[4] Training and fine-tuning essentially mean that the software learns to estimate the age of the people in the two

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

[4] 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.

datasets using the information on chronological and apparent age by adapting learning parameters in the hidden layers.

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

Age distribution in training datasets. Both the IMDb-Wikipedia data and the dataset employed for human-based fine-tuning include people from all age groups, and in particular people aged 50 and above. This ensures that the software is trained and fine-tuned on data that includes people with similar facial characteristics as our CEOs, such as with regard to baldness patterns, hair color, and wrinkle development. For reference, the CEO at the 10$^{\text{th}}$ (50$^{\text{th}}$, 90$^{\text{th}}$) percentile in our dataset is 48 (57, 65) years old in 2006 (see Table I).

Image pre-processing. Before feeding the pictures into the CNN for training and fine-tuning, ABBD "standardize" them, a process they label picture pre-processing. Specifically, they use existing software solutions to detect, scale, and align the face in each image, and resize each image to 224×224 pixels. Intuitively, standardizing images reduces the noise present when training and fine-tuning the software and improves performance (cf. Table 2 in Antipov et al. 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 et al. 2016).

ABBD's trained software does not include image pre-processing code (and can, in fact, be applied to "raw images" so long as they are resized). We nonetheless replicate some of their pre-processing steps in order to increase the similarity between our CEO images and the images used for software training. Before pre-processing a picture, we make sure that the image contains only the face of the CEO. If a picture contains multiple faces, such as a CEO with their partner, other managers, or a journalist, we first manually crop the picture and keep only the portion that shows the CEO. We then use the Python-based "face_recognition" package[5] 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.

Appendix-Figure B.2 shows several examples of pre-processed facial images. Panel (a) shows pre-processed images used to train ABBD's software. One can see that they differ in terms of "tint"

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

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 our sample. Again, there are differences in terms of tint and background, so it is worth reiterating that these are image features that the software can learn to take into account in its estimation during the training stage. Furthermore, comparing images across the two panels illustrates that our implementation of the image pre-processing step indeed leads to similar results compared to ABBD's original implementation on the training datasets.

(a) Training sample

(b) CEO sample

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

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

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

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

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

2. Measuring Image Sharpness

We follow the computer science and imaging literature[6] and measure image sharpness based on the image Laplacian, which is the second spatial derivative of an image. We first transform each image to grayscale. We then calculate the Laplacian of the image. The Laplacian of an image with pixel intensities $I(x,y)$ is

$$L(x,y) = \frac{\delta^2 I}{\delta x^2} + \frac{\delta^2 I}{\delta y^2}.$$

The Laplacian determines the extent to which there are rapid changes in grayscale images. Sharp images tend to be associated with more rapid changes, as in these images detail is rendered more clearly and there are more "edges." As a result, sharp images have, relatively speaking, large positive and negative Laplacian values.

The *variance of the Laplacian* method for image sharpness calculation makes use of this feature. For our purposes, we calculate image sharpness as the continuous measure $sharpness = \ln \sigma(L(x,y))$, i. e., we take the natural logarithm of the standard deviation of an image's Laplacian values.

As reported in Table I, the mean and median image sharpness values in our sample are 2.57 and 2.58, respectively. Appendix-Figure B.3 shows two illustrative images from a CEO in our sample (Rex W. Tillerson, former CEO of ExxonMobil). The left image has a sharpness value of 2.14, which is close to the 25th percentile of the image sharpness distribution in our sample. The right image has a sharpness value of 3.28, which is close to the 75th percentile of the sample distribution.

Appendix-Figure B.3. Image sharpness in sample images.

We test that our measure of image sharpness is uncorrelated with treatment, i.e., with industry distress exposure during the Great Recession. These results are shown in Appendix-Table B.3, included in Internet Appendix Section B.5.

[6] See, e. g., Pech-Pacheco et al. (2000) and Bansal et al. (2016). See also pyimagesearch.com/2015/09/07/blur-detection-with-opencv/.

3. Manipulating Facial Expression in Images

As discussed in the main text, one potential concern with the aging analysis is image heterogeneity, in particular differential smiling versus frowning. In addition to testing how facial expression correlates with treatment and to controlling for facial expression in our analyses (see Section III.D for a detailed discussion), we implement an AI-based robustness test.

Specifically, for images classified as *frown*, we use the app FaceApp (https://www.faceapp.com/) and create "fake" images from the original images, in which we reduce frowning towards a more neutral expression. FaceApp is *not* a basic photo editing application, but uses machine learning and neural network techniques to create natural-looking transformations of faces.[7] We manipulate images "from frowning to neutral" rather than "from smiling to neutral" since the latter would require significant changes to a person's face, e.g., removing teeth visible while smiling. Thus the former approach is closer to, loosely speaking, taking a derivative of the image with respect to facial expression.

We apply the AI-based expression manipulation application to all 354 images in our dataset that are classified as frowning, and hand-verify for each image that the manipulated version shows a more neutral expression, even including a faint smile, which is the case for 341 (or 96%) of the frowning images. For each of these images, we create three versions of "fake" images, with a progressively more neutral/faint smile expression. We present examples of these image transformations in Appendix-Figure B.4, again for Rex W. Tillerson. The left-most images are the original, real images (these images are the same as those in Appendix-Figure B.3). The remaining three images in each row are the three manipulated "fake" images with a progressively more neutral expression.

We then apply the apparent-aging software to each of the 341 successfully manipulated images, and re-estimate our main regressions using the apparent age estimate from the manipulated instead of the original images. We report the results in Appendix-Table B.6, included in Internet Appendix Section B.5. We refer to the degree of expression manipulation as none, 1/3, 2/3, and 3/3, respectively, and present the results without image controls, since some of these assessments may change with image manipulation (e.g., self-confidence). Panel A in Appendix-Table B.6 contains the results for the simple pre-vs. post split, as in columns (1) and (2) of Table III. Panel B splits the post-period into two sub-periods, as in columns (3) and (4) of Table III. In both panels, column (1) corresponds to the results shown in Table III (column (1) and (3), respectively), and columns (2) to (4) present the results using the face-manipulated apparent-age estimates.

[7] See, e.g., forbes.com/sites/haroldstark/2017/04/25/introducing-faceapp-the-year-of-the-weird-selfies/.

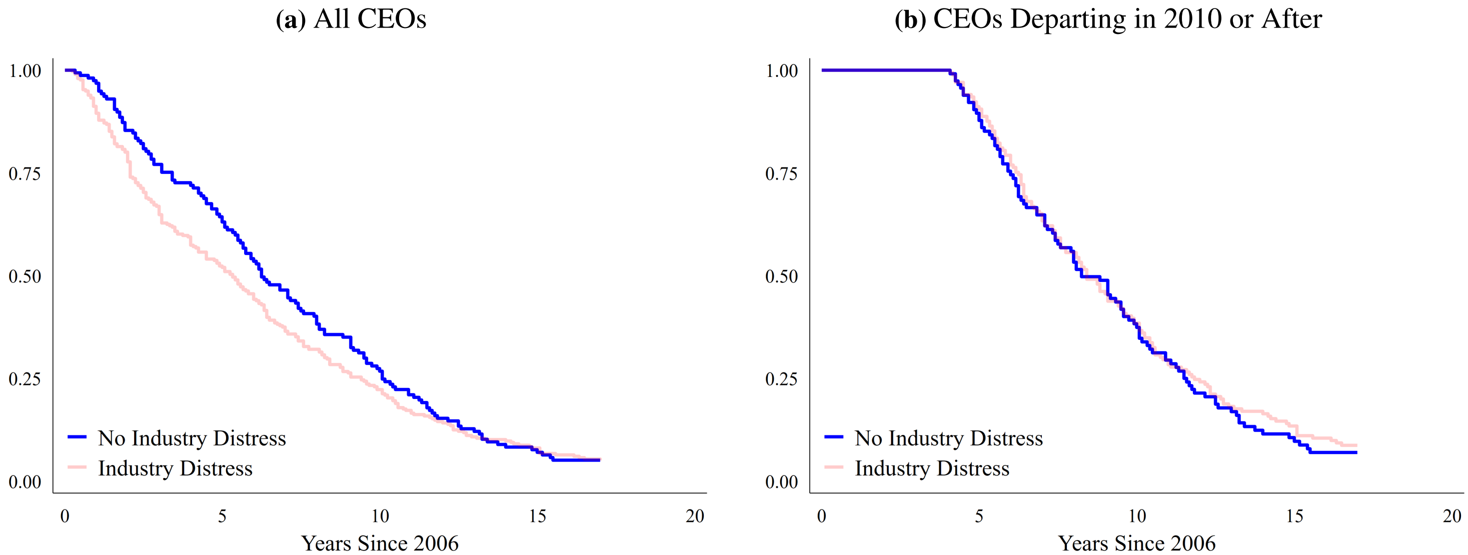
Appendix-Figure B.4. Manipulating facial expression using FaceApp's Artificial Intelligence techniques.

Comparing the estimates in Appendix-Table B.6 across columns, our results barely change when using the manipulated-face apparent-age estimates, which further alleviates concerns around picture heterogeneity confounds.

4. Robustness Related to Differential CEO Departures

This section provides additional details addressing the concern about differential selection of images due to CEO departures. In particular, if differential CEO departure rates by industry distress lead to heterogeneity in the type of images we can find, this could explain the observed apparent aging patterns, even in the long run. For example, news of CEO departures are often accompanied by photos of more grim-faced CEOs. Additionally, after stepping down, CEOs may assume less prestigious positions or retire, accompanied with less management of public perceptions and lower incentives for appearance management. It is thus important to examine differential CEO turnovers as a potential mechanism.

Panel (a) of Appendix-Figure B.5 plots CEO departure rates since 2006 (since we focus on the 2006 cohort of *Fortune* firms). Consistent with prior work (Jenter and Kanaan 2015), it is indeed the case that distressed CEOs leave the CEO position significantly sooner, in particular during the crisis, which reinforces the resulting concern of image heterogeneity as a potential confound.



Appendix-Figure B.5. Kaplan-Meier estimates for CEO departures. This figure shows Kaplan-Meier plots for the relation between industry distress during the Great Recession and CEO departures. The vertical axis shows the fraction of CEOs who are still in office. The horizontal axis reflects time elapsed (in years) since 2006. Survival lines are adjusted for CEOs' median elapsed tenure until 2006. Panel (a) compares CEO departure rates of all CEOs in our sample. Panel (b) compares CEO departure rates conditional on leaving office in 2010 or afterwards.

To address the concern, we start by again plotting CEOs' departure rates in Panel (b) of Appendix-Figure B.5, now conditioning on CEOs who remain in office during the crisis and depart in 2010 or afterwards. (Of course, whether CEOs remain in office until 2010 is endogenous, though the selection from this conditioning likely works against identifying distress effects. Distressed CEOs who remain in office are presumably more "resilient.") As panel (b) reveals, in the conditional sample CEO departure rates do not differ by whether CEOs were exposed to industry distress during 2007–2008.

We then test for differences in the "status" of CEOs (and hence the type of images available) in this subsample of CEOs. Despite their similar departure rates, distressed and non-distressed CEOs might still differ in what they do after leaving office, for example, retire versus continue in high-level positions. In Appendix-Table B.1, we test for differences in the fraction of images associated with five "types" of status: (i) CEO is still in office, (ii) CEO has left the CEO position and no longer appears in the Execucomp database,[8] (iii) CEO has left the CEO position but still

[8] Execucomp contains the full S&P 1500 companies plus companies that were once part of the S&P 1500 index and are still trading, and some of the largest 2,500 firms per year.

appears in Execucomp in a non-CEO role, (iv) CEO has left the CEO position but still appears in Execucomp in a CEO role, and (v) CEO has left the CEO position and the CEO departure is classified as "retirement" by Gentry et al. (2021).

Appendix-Table B.1
Fraction of Images By CEO Departure Status and Industry Distress Exposure

This table shows the fraction of CEO images by CEO departure status and industry distress exposure, conditional on CEOs leaving office in 2010 or afterwards.

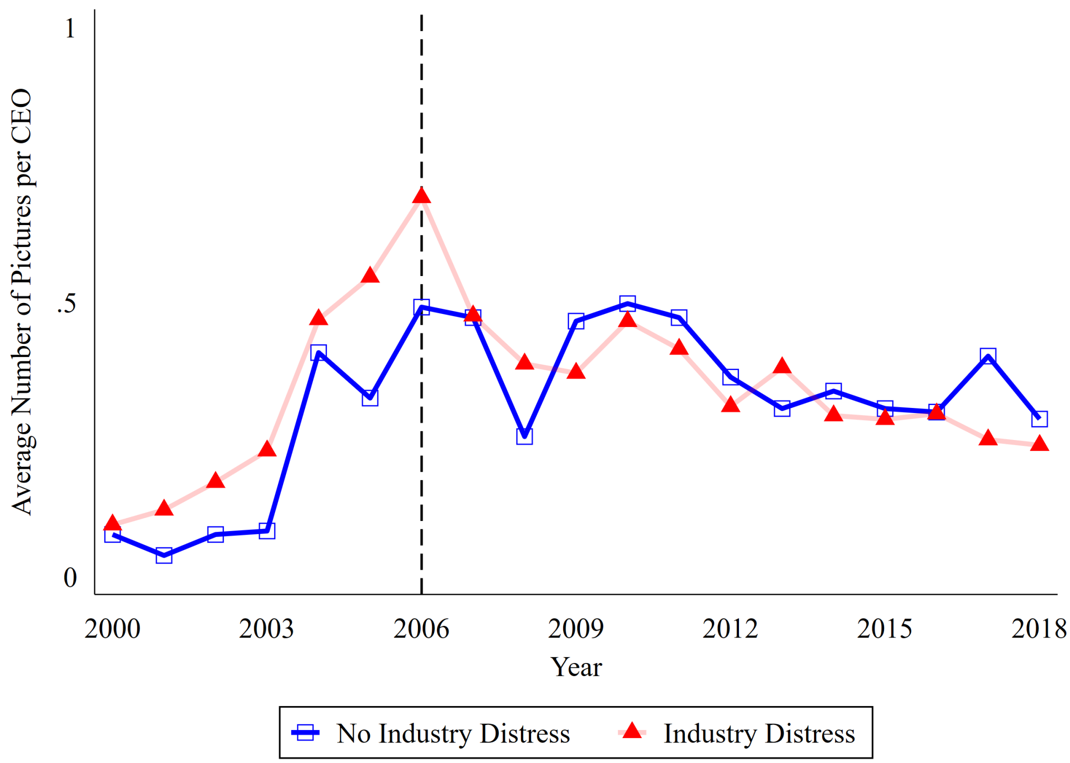
	Industry Distress	No Industry Distress	*p*-Value for Differences
In Office	89 %	89 %	0.77
Not Retired – Non-Execucomp	4 %	3 %	0.21
Not Retired – Execucomp Non-CEO	0 %	0 %	0.94
Not Retired – Execucomp CEO	2 %	2 %	0.60
Retired	5 %	6 %	0.35

As Appendix-Table B.1 reveals, within the subsample of CEOs departing in 2010 or afterwards, there are no significant differences in status and associated fraction of images. In other words, in this subsample, both the timing of when CEOs depart and CEOs' appearance management incentives from in-office versus post-departure positions are similar for distressed and non-distressed CEOs. As a result, repeating our analysis on this subsample of CEOs departing in 2010 or later is a useful way to "mute" the potential confound of heterogeneous CEO incentives (and resulting heterogeneous picture types) due to heterogeneous departure rates.

We do so in Appendix-Table B.8, included in Internet Appendix Section B.5, where we repeat the main analysis from Table III conditional on CEOs leaving office in 2010 or afterwards. We continue to find strong apparent aging effects in this subsample, which if anything are stronger than the baseline results. These results on the subsample of CEOs for which departure rates and images types do not significantly vary by treatment ameliorate concerns that our effects are driven by confounds related to differential picture management incentives.

5. Additional Figures and Tables

This section contains additional figures and tables related to the analysis on industry-wide distress shocks and CEO apparent aging.



Appendix-Figure B.6. Average number of pictures per CEO across years. 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 black vertical line indicates the year 2006.

Appendix-Table B.2
Industry Distress and CEO Aging – Robustness

	Winsor (0.5; 99.5) Weight by Sharpness		No trim/winsor Weight by Sharpness		Trim (1; 99) Weight by Sharpness		Winsor (1; 99) Weight by Sharpness		Trim (0.5; 99.5) No weighting		Trim (0.5; 99.5) Weight by Sharpness $\times 1/N_{CEO}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Industry Distress $\times \mathbb{1}_{\{t>2006\}}$	0.825^{**}		0.806^{**}		0.894^{**}		0.844^{**}		0.759^{**}		1.146^{***}	
	[0.375]		[0.386]		[0.380]		[0.371]		[0.359]		[0.413]	
Industry Distress $\times \mathbb{1}_{\{2006<t<2012\}}$		0.628		0.607		0.703^{*}		0.651^{*}		0.562		0.985^{**}
		[0.387]		[0.397]		[0.393]		[0.384]		[0.383]		[0.432]
Industry Distress $\times \mathbb{1}_{\{t\geq 2012\}}$		1.100^{**}		1.082^{**}		1.166^{**}		1.113^{**}		1.020^{**}		1.407^{***}
		[0.443]		[0.455]		[0.453]		[0.439]		[0.417]		[0.528]
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
CEO Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Picture Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
No. of CEOs	454	454	454	454	452	452	454	454	453	453	453	453
Observations	3,034	3,034	3,034	3,034	2,970	2,970	3,034	3,034	3,002	3,002	3,002	3,002

Appendix-Table B.3
Image Sharpness and Industry Distress

This table relates image sharpness to industry distress exposure during the Great Recession. The dependent variable is image sharpness as defined in Appendix B.2. *Industry Distress* is an indicator variable, defined at the CEO level, capturing whether a CEO was exposed to industry distress during the Great Recession. Columns (3) and (4) add interactions with the post-indicator. Standard errors are clustered at the industry level. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

	(1)	(2)	(3)	(4)
Industry Distress	0.053	0.026		
	[0.055]	[0.054]		
Industry Distress × $\mathbb{1}_{\{t>2006\}}$			−0.025	−0.035
			[0.065]	[0.061]
Year FE		Y	Y	Y
CEO FE			Y	Y
CEO Controls			Y	Y
Picture Controls				Y
No. of CEOs	453	453	453	453
Observations	3,002	3,002	3,002	3,002

Appendix-Table B.4
Treatment Status and Chronological Age

	Industry Distress		No Industry Distress		*p*-Value for Differences	
	Mean	Median	Mean	Median	*t*-test	Wilcoxon test
Chronological Age in 2006	56.60	56.00	56.81	57.00	0.77	0.28

p-value for test of equality of distributions (Wilcoxon rank-sum test): 0.68

Appendix-Table B.5
Image Facial Expression, Industry Distress, and CEO Aging

This table examines image facial expression, industry distress exposure during the Great Recession, and CEO aging. In Panel A, the dependent variable is the assessed facial expression, coded as -1 = frowning, $+1$ =smiling, and 0 =neutral. *Industry Distress* is an indicator variable, capturing whether a CEO was exposed to industry distress during the Great Recession. Columns (3) and (4) of Panel A add interactions with the post-indicator. In Panel B, the dependent variable is the apparent-age gap, which is the difference between apparent age, as assessed from CEOs' facial images, and chronological age. The specification in column (1) of Panel B is the same as in column (2) of Table III. The specification in column (2) is the same as that in column (1) except that it omits the control variable assessing mood (happy, grim, neutral). Standard errors are clustered at the industry level. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

Panel A: Image Facial Expression and Industry Distress

	(1)	(2)	(3)	(4)
Industry Distress	0.023	0.032		
	[0.050]	[0.048]		
Industry Distress × $\mathbb{1}_{\{t>2006\}}$			0.102	0.053
			[0.070]	[0.055]
Year FE		Y	Y	Y
CEO FE			Y	Y
CEO Controls			Y	Y
Picture Controls				Y
No. of CEOs	453	453	453	453
Observations	3,002	3,002	3,002	3,002

Panel B: Industry Distress and CEO Aging – Smiling Control

	(1)	(2)
Industry Distress × $\mathbb{1}_{\{t>2006\}}$	0.883**	0.876**
	[0.382]	[0.382]
Neutral	0.003	0.112
	[0.285]	[0.191]
Frown	0.381	0.589**
	[0.394]	[0.281]
CEO FE	Y	Y
Year FE	Y	Y
CEO Controls	Y	Y
Mood Control	Y	
Other Picture Controls	Y	Y
No. of CEOs	453	453
Observations	3,002	3,002

Appendix-Table B.6
Industry Distress and CEO Aging – Image Manipulation Robustness

This table shows OLS estimates of the effect of industry distress exposure during the Great Recession on CEO apparent aging as in Table III, using the sample including expression-manipulated images. Panel A contains the results for the split into pre- and post-period (as in columns (1) and (2) of Table III). Panel B splits the post-period into two sub-periods (as in columns (3) and (4) of Table III). We refer to the degree of face manipulation of frowning images as none (i.e., original sample), 1/3, and 2/3, and 3/3, indicating progressively more neutral and faint smile expressions. As before, *Industry Distress* is equal to 1 if the CEO's firm was exposed to industry-wide distress during 2007 or 2008, and observations are weighted by image sharpness. 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.

	(1)	(2)	(3)	(4)
Panel A: Pre- Vs. Post Period				
Industry Distress × $\mathbb{1}_{\{t>2006\}}$	0.806**	0.812**	0.821**	0.834**
	[0.382]	[0.371]	[0.367]	[0.364]
Panel B: Short- Vs. Long-Horizon in Post-Period				
Industry Distress × $\mathbb{1}_{\{2006<t<2012\}}$	0.634	0.642*	0.633*	0.629*
	[0.386]	[0.375]	[0.375]	[0.376]
Industry Distress × $\mathbb{1}_{\{t\geq 2012\}}$	1.049**	1.051**	1.084**	1.122**
	[0.462]	[0.459]	[0.450]	[0.441]
Degree of Face Manipulation	None	1/3	2/3	3/3
Year FE	Y	Y	Y	Y
CEO FE	Y	Y	Y	Y
CEO Controls	Y	Y	Y	Y
No. of CEOs	453	453	453	453
Observations	3,002	3,002	3,002	3,002

Appendix-Table B.7
Industry Distress and CEO Aging – Drop Post-2016 Years

This table shows OLS estimates of the effect of industry distress exposure during the Great Recession on CEO apparent aging as in Table III, restricting our sample to years prior to and including 2016. This robustness test is motivated by the slight divergence in picture finding rates by industry distress exposure after 2016 (Appendix-Figure B.6). As before, *Industry Distress* is equal to 1 if the CEO's firm was exposed to industry-wide distress during 2007 or 2008, and observations are weighted by image sharpness. 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.

	(1)	(2)	(3)	(4)
Industry Distress × $\mathbb{1}_{\{t>2006\}}$	0.760*	0.806**		
	[0.386]	[0.392]		
Industry Distress × $\mathbb{1}_{\{2006<t<2012\}}$			0.565	0.598
			[0.392]	[0.406]
Industry Distress × $\mathbb{1}_{\{t\geq 2012\}}$			1.133**	1.204**
			[0.502]	[0.504]
CEO FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
CEO Controls	Y	Y	Y	Y
Picture Controls		Y		Y
No. of CEOs	433	433	433	433
Observations	2,721	2,721	2,721	2,721

Appendix-Table B.8
Industry Distress and CEO Aging – CEOs Departing in 2010 or After

This table shows OLS estimates of the effect of industry distress exposure during the Great Recession on CEO apparent aging as in Table III, conditional on CEOs leaving office in 2010 or afterwards. As before, *Industry Distress* is equal to 1 if the CEO's firm was exposed to industry-wide distress during 2007 or 2008, and observations are weighted by image sharpness. 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.

	(1)	(2)	(3)	(4)
Industry Distress × $\mathbb{1}_{\{t>2006\}}$	1.116**	1.126**		
	[0.503]	[0.499]		
Industry Distress × $\mathbb{1}_{\{2006<t<2012\}}$			0.768	0.750
			[0.533]	[0.544]
Industry Distress × $\mathbb{1}_{\{t\geq 2012\}}$			1.500***	1.539***
			[0.569]	[0.551]
CEO FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
CEO Controls	Y	Y	Y	Y
Picture Controls		Y		Y
No. of CEOs	282	282	282	282
Observations	1,929	1,929	1,929	1,929

Appendix-Table B.9
Industry Distress and CEO Aging – Alternative Specification

This table shows OLS estimates of the effect of industry distress exposure during the Great Recession on CEO apparent aging as in Table III, but modifies the regression specification. The dependent variable is now the CEO's apparent age (rather than the apparent-age gap), and the CEO's chronological age is included as an additional covariate in the model. As before, *Industry Distress* is equal to 1 if the CEO's firm was exposed to industry-wide distress during 2007 or 2008, and observations are weighted by image sharpness. 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.

	(1)	(2)	(3)	(4)
Industry Distress $\times$ $\mathbb{1}_{\{t>2006\}}$	0.806**	0.882**		
	[0.381]	[0.379]		
Industry Distress $\times$ $\mathbb{1}_{\{2006<t<2012\}}$			0.634	0.670*
			[0.385]	[0.393]
Industry Distress $\times$ $\mathbb{1}_{\{t\geq 2012\}}$			1.048**	1.181***
			[0.462]	[0.447]
Chronological Age	0.849***	0.734**	0.850***	0.735**
	[0.282]	[0.283]	[0.283]	[0.284]
CEO FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
CEO Controls	Y	Y	Y	Y
Picture Controls		Y		Y
No. of CEOs	453	453	453	453
Observations	3,002	3,002	3,002	3,002

Appendix-Table B.10
Industry Distress and CEO Aging – Median Apparent-Age Estimate Across Trained CNNs

This table shows OLS estimates of the effect of industry distress exposure during the Great Recession on CEO apparent aging as in Table III, using the median rather than average estimated apparent age across the trained neural nets as the apparent-age estimate. As before, *Industry Distress* is equal to 1 if the CEO's firm was exposed to industry-wide distress during 2007 or 2008, and observations are weighted by image sharpness. 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.

	(1)	(2)	(3)	(4)
Industry Distress × $\mathbb{1}_{\{t>2006\}}$	0.798**	0.875**		
	[0.381]	[0.381]		
Industry Distress × $\mathbb{1}_{\{2006<t<2012\}}$			0.623	0.658
			[0.388]	[0.399]
Industry Distress × $\mathbb{1}_{\{t\geq 2012\}}$			1.045**	1.179***
			[0.461]	[0.447]
CEO FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
CEO Controls	Y	Y	Y	Y
Picture Controls		Y		Y
No. of CEOs	453	453	453	453
Observations	3,002	3,002	3,002	3,002

Appendix-Table B.11
Industry Distress and CEO Aging – Possible Mechanisms (1)

This table shows OLS estimates of the effect of industry distress exposure during the Great Recession on CEOs' estimated body mass index (BMI). BMI is estimated from CEOs' facial images based on the deep learning approach by Sidhpura et al. (2022), and using the estimated BMIs from their Inception-v3 and Xception based models, the two models with the best performance on the most relevant test data involving celebrities. The dependent variable is an indicator variable equal to one if both estimated BMIs are 25 or above, i. e., in the overweight range as per CDC definitions (cdc.gov/obesity/basics/adult-defining.html), and zero otherwise. *Industry Distress* is equal to 1 if the CEO's firm was exposed to industry-wide distress during 2007 or 2008, and observations are weighted by image sharpness. Standard errors, clustered at the industry level, are shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

	(1)	(2)
Industry Distress × $\mathbb{1}_{\{2006<t<2012\}}$	0.020	0.022
	[0.059]	[0.058]
Industry Distress × $\mathbb{1}_{\{t\geq 2012\}}$	0.067	0.071
	[0.052]	[0.049]
Control Mean (Pre-Period)	0.22	0.22
Control Mean (Post-Period)	0.19	0.19
CEO FE	Y	Y
Year FE	Y	Y
CEO Controls	Y	Y
Picture Controls		Y
No. of CEOs	444	444
Observations	2,831	2,831

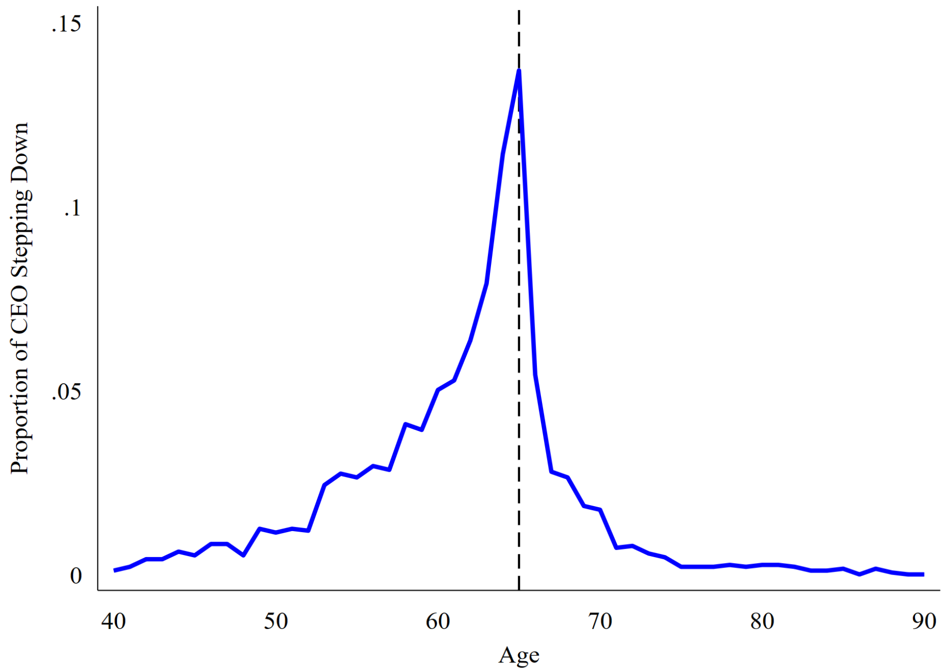
Appendix-Table B.12
Industry Distress and CEO Aging – Possible Mechanisms (2)

This table shows OLS estimates of the effect of industry distress exposure during the Great Recession on various facial conditions. The dependent variables are indicator variables equal to one if the assessed degree of puffy eyes, red eyes, dark under eyes, and red face, respectively, is very noticeable, and zero otherwise. With respect to red eyes, the indicator is also set to one if assessed degree of red eyes is minor, as there are very few images ($< 0.5\%$) assessed as having very noticeable red eyes. *Industry Distress* is equal to 1 if the CEO's firm was exposed to industry-wide distress during 2007 or 2008, and observations are weighted by image sharpness. Standard errors, clustered at the industry level, are shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

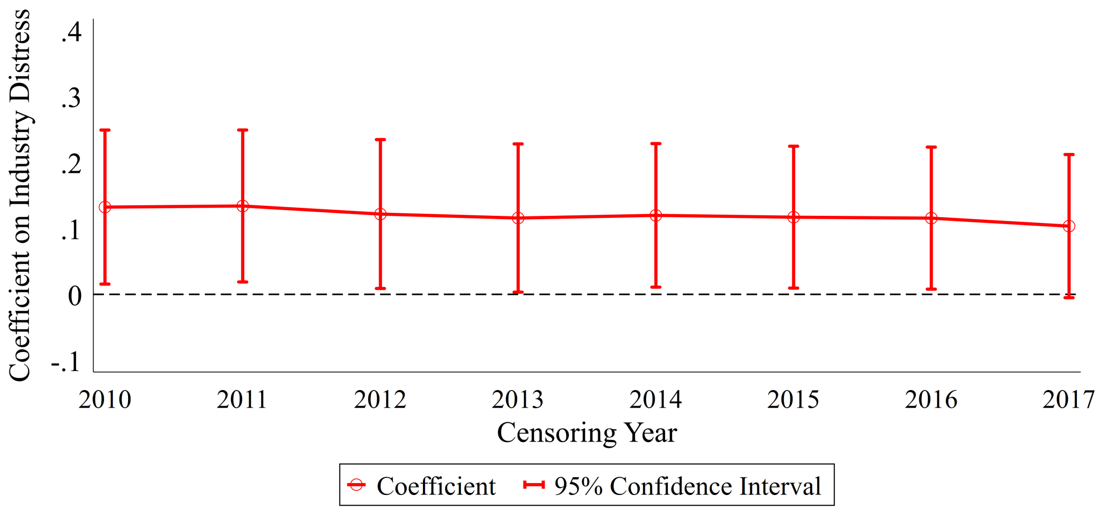
	Puffy Eyes		Red Eyes		Dark Under Eyes		Red Face	
Industry Distress × $\mathbb{1}_{\{2006<t<2012\}}$	0.014	0.012	−0.006	−0.003	−0.016	−0.021	−0.014	−0.012
	[0.034]	[0.034]	[0.030]	[0.029]	[0.030]	[0.030]	[0.022]	[0.022]
Industry Distress × $\mathbb{1}_{\{t\geq 2012\}}$	−0.008	−0.012	−0.028	−0.020	−0.027	−0.037	0.014	0.012
	[0.047]	[0.047]	[0.029]	[0.028]	[0.025]	[0.026]	[0.029]	[0.029]
Control Mean (Pre-Period)	0.13	0.13	0.04	0.04	0.03	0.03	0.06	0.06
Control Mean (Post-Period)	0.21	0.21	0.02	0.02	0.06	0.06	0.07	0.07
CEO FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
CEO Controls	Y	Y	Y	Y	Y	Y	Y	Y
Picture Controls		Y		Y		Y		Y
No. of CEOs	453	453	453	453	453	453	453	453
Observations	3,002	3,002	3,002	3,002	3,002	3,002	3,002	3,002

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

This section contains additional figures and tables, and presents further robustness tests referenced in Section IV, related to the analysis on industry-wide distress shocks and CEO life expectancy.



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



Appendix-Figure C.2. Estimated effect of industry distress when varying the censoring year. This figure shows the estimated coefficients on the industry distress indicator variable using the specification from Table IV, column (2), but varying the censoring date for CEOs identified as alive as of 10/2017. In the main analysis, the cutoff date is Oct. 1, 2017, i. e., CEOs who did not pass away before this date are treated as censored. The alternative censoring dates are Dec. 31, 2016; Dec. 31, 2015; ...; down to Dec. 31, 2010. The number of CEOs in the sample ($N = 1,900$) remains unchanged when varying the cutoff.

Appendix-Table C.1
Availability of Death Status and Industry Distress Exposure

This table examines the correlation between the availability of death/alive information and CEOs' exposure to industry distress. The dependent variable is an indicator variable that equals one if we are able to determine the death/alive status of a CEO. The sample is all CEOs in our initial sample (which, as described in the main text, is a panel but has gaps and does not have full tenure histories) with data on the firm's industry affiliation (SIC codes). Since we did not collect full tenure histories for CEOs without death/alive information, in *Model 1* the *Industry Distress* indicator is equal to one if the firm experienced industry distress (as defined in Appendix A) in a ten-year window starting from the first year in which a CEO is included in our initial sample. In *Model 2*, the *Industry Distress* indicator is equal to one if the firm experienced industry distress in a symmetric ten-year window around the first year in which a CEO is included in our initial sample ($\pm$5 years). As in the main regressions, both models account for firms' location (HQ state) and industry affiliation. Standard errors are clustered at the industry level. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

	Coefficient	S.E.	*p*-Value	No. of Obs
Model 1:				
Industry Distress	0.008	0.014	0.557	2,631
Model 2:				
Industry Distress	0.010	0.016	0.537	2,631

Appendix-Table C.2
Age-by-Birth-Cohort Robustness

This table reports hazard coefficients estimated as in Table IV, but allowing the effect of (chronological) age to vary by CEOs' birth cohort. 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.

	(1)	(2)	(3)	(4)
Industry Distress	0.106**	0.101*	0.137**	0.134**
	[0.054]	[0.056]	[0.058]	[0.060]
Age × Birth Cohort 1 (oldest)	0.118***	0.118***	0.116***	0.116***
	[0.009]	[0.010]	[0.009]	[0.010]
Age × Birth Cohort 2	0.118***	0.118***	0.116***	0.116***
	[0.009]	[0.011]	[0.009]	[0.011]
Age × Birth Cohort 3	0.118***	0.118***	0.116***	0.116***
	[0.010]	[0.012]	[0.010]	[0.012]
Age × Birth Cohort 4	0.117***	0.118***	0.115***	0.115***
	[0.011]	[0.013]	[0.011]	[0.013]
Age × Birth Cohort 5 (youngest)	0.118***	0.119***	0.116***	0.117***
	[0.011]	[0.014]	[0.012]	[0.014]
ln(Pay)			−0.004	−0.011
			[0.047]	[0.048]
ln(Assets)			−0.077	−0.083*
			[0.048]	[0.048]
ln(Employees)			0.028	0.032
			[0.049]	[0.049]
Year	−0.003		0.003	
	[0.009]		[0.009]	
FF49 Strata	Y	Y	Y	Y
Location FE	Y	Y	Y	Y
Year FE		Y		Y
No. of CEOs	1,900	1,900	1,818	1,818
Observations	58,034	58,034	55,796	55,796

Appendix D Corporate Monitoring and Life Expectancy: Robustness Tests

This section contains additional figures and tables, and presents further robustness tests referenced in Section V, related to the analysis on anti-takeover laws and CEO life expectancy. We first present additional details on BC-specific robustness tests and then report additional figures and tables.

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

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

In addition to BC laws, four other types of anti-takeover laws were passed by individual states since the 1980s: (1) Control Share Acquisition laws prohibited acquirers of large equity stakes from using their voting rights, making it more difficult for hostile acquirers to gain control. (2) Fair Price laws required acquirers to pay a fair price for shares acquired in a takeover attempt. Fair could mean, for example, the highest price paid by the acquirer for shares of the target within the last 24 months (cf. Cheng, Nagar, and Rajan 2004). (3) Directors' Duties laws extended the board members' duties to incorporate the interests of non-investor stakeholders, even if not necessarily maximizing shareholder value. (4) Poison Pill laws guaranteed that the firms had the right to use poison pill takeover defenses. We refer to the first of these five laws (including BC laws) passed by a state as the *First Law* (*FL*). Anti-takeover law exposure is similar when jointly looking at all five second-generation laws. For example, conditional on any *FL* exposure, the median CEO experiences 4.12 years, close to the 4.41 years in the BC-based analysis.

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

Appendix-Table D.5 re-estimates Table V 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 columns (3) and (4), based on cumulative law exposure, the hazard coefficients range from -0.040

to -0.041, compared to -0.037 to -0.040 in Table V.

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

Karpoff and Wittry (2018) propose several robustness tests to address endogenous firm responses to anti-takeover laws, which we implement in Appendix-Table D.6. For all sample restrictions, we follow the procedure suggested in Karpoff and Wittry (2018). In Panel A, we remove the 46 firms identified by these authors as having lobbied for the passage of the second-generation laws. In Panel B, we use Institutional Shareholder Services (ISS) Governance (formerly, RiskMetrics) data from 1990 to 2017 to identify firms that opted out of coverage by the laws and exclude them from the analysis. In Panel C, we exclude firm-years in which firms had adopted firm-level anti-takeover defenses. We identify firms with firm-level defenses combining ISS data with data provided to us by Cremers and Ferrell (2014), which extends the Gompers et al. (2003) G-index backwards to 1977-1989. We back out whether firms used firm-level defenses in 1977-1989 by "subtracting" the state-wide laws from the G-index, which combines firm- and state-level defenses. Firm-level defenses include Golden Parachutes and Cumulative Voting (cf. Gompers et al. (2003) for details).

In all subsamples, the hazard coefficient on BC exposure remains significant at 5% or 1%, and the hazard coefficient estimates are nearly unchanged, ranging from -0.195 (Panel B, column 2) to -0.253 (Panel A, column 1) for the indicator version, and from -0.038 (Panel B, columns 3 and 4) to -0.041 (Panel C, column 3) 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 lost their effect only starting from June 1982 after the *Edgar v. MITE* ruling. As discussed in the main text, we account for the possible confound of first-generation laws in all our regressions by adding a control variable for first-generation law exposure, as in Table IV in Karpoff and Wittry (2018).

We further address this concern in Appendix-Table D.7 through three cuts of the data. In subsample A, we we drop all sample-years prior to 1983 for CEOs with first-generation law exposure. In subsample B, we drop all CEOs with first-generation law exposure who stepped down prior to 1983, i. e., we restrict the sample to CEOs without first-generation law exposure and, for CEOs with exposure, to those who who served during the "post-first-law period." Note that in terms of number of CEOs remaining, subsample B is more restrictive than subsample A, and note that both subsamples still include CEOs with first-generation law exposure. In subsample C, we restrict the sample to CEOs without any first-generation law exposure, thus fully removing any potential remaining concerns about insufficient accounting for first-generation laws. In all subsamples, we continue to estimate negative hazard coefficients for both the indicator and cumulative BC exposure variables, similar in size and statistical significance to those in the main table.

Finally, in a last set of robustness checks, we move beyond the tests suggested in Karpoff and Wittry (2018) and create sub-samples based on firms' state of incorporation and industry affiliation, inspired by similar robustness checks in Giroud and Mueller (2010) and Gormley and Matsa (2016). In Appendix-Table D.8, we exclude firms that are incorporated in Delaware or in New York, the two most common states of incorporation in our sample (Panel A); firms in the Banking industry (Panel B); or firms in the Utilities industry (Panel C). In all three panels, the hazard coefficient estimates on binary and cumulative BC exposure are barely affected by these data cuts.

Predicted Length of Exposure: Prediction Model Details

To purge the per-year estimates in the right four columns of Table V of possible endogeneity in the length of exposure, we construct a measure of predicted BC law exposure, and relate predicted, rather than true exposure, to CEO mortality rates.[9] To this end, we proceed in three steps. First, we estimate a prediction model for CEO tenure; we then construct predicted BC exposure; and finally we re-estimate the hazard regressions using predicted BC exposure as the independent variable.

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

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

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

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

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

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

$\widehat{BC}_{i,t}$ 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:

$$\ln\ \lambda(t|\widehat{BC}_{i,t}, X_{i,t}) = \ln\ \lambda_{0,j}(t) + \beta\widehat{BC}_{i,t} + \boldsymbol{\delta}'\boldsymbol{X}_{i,t}. \tag{D.3}$$

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

Business Combination Laws and CEO Pay

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

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

Before estimating the empirical relation, it is useful to first calibrate what effect size we would expect if compensation for health ramifications were the primary channel empirically. In their meta-analysis of the literature on the value of a statistical life (VSL), Viscusi and Aldy (2003) report an estimate around \$6.7 million (in 2000 dollars) for a person with income of around \$26,000, and an income elasticity for the VSL of around 0.5. Applied to our CEO sample, this translates into a VSL of around \$47.3 million.[10] Given a baseline mortality rate of 1.706% for 60-year-olds born in 1927 (Human Mortality Database 2019), a reduction in mortality risk of 4.0% per year of BC exposure (column (5) in Table V) implies a CEO pay change between -2.5% and -10%, depending on whether the wage adjustment reflects the entire BC-induced mortality risk shift over

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

the expected remaining lifespan or solely the shift over the remaining years while serving as CEO.[11]

With these calibrated effects in mind, Appendix-Table D.10 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 controls and fixed effects as in the hazard analyses (now including industry fixed effects as opposed to stratifying by industry). In column (2), we add the control variables used in Bertrand and Mullainathan (1998): tenure, firm assets, and employees, also used in some of our hazard specifications. 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):

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

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

We estimate a positive, albeit statistically insignificant treatment effect. The estimates indicate a pay increase between 4.5–7.6%. In comparing the results to the earlier work, which estimated a (more significant) 5.4 percent pay increase, it is important to note that our analysis is conducted on a CEO-level sample, and restricts the sample to incumbent, pre-BC CEOs.

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

Business Combination Laws and CEO Tenure

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

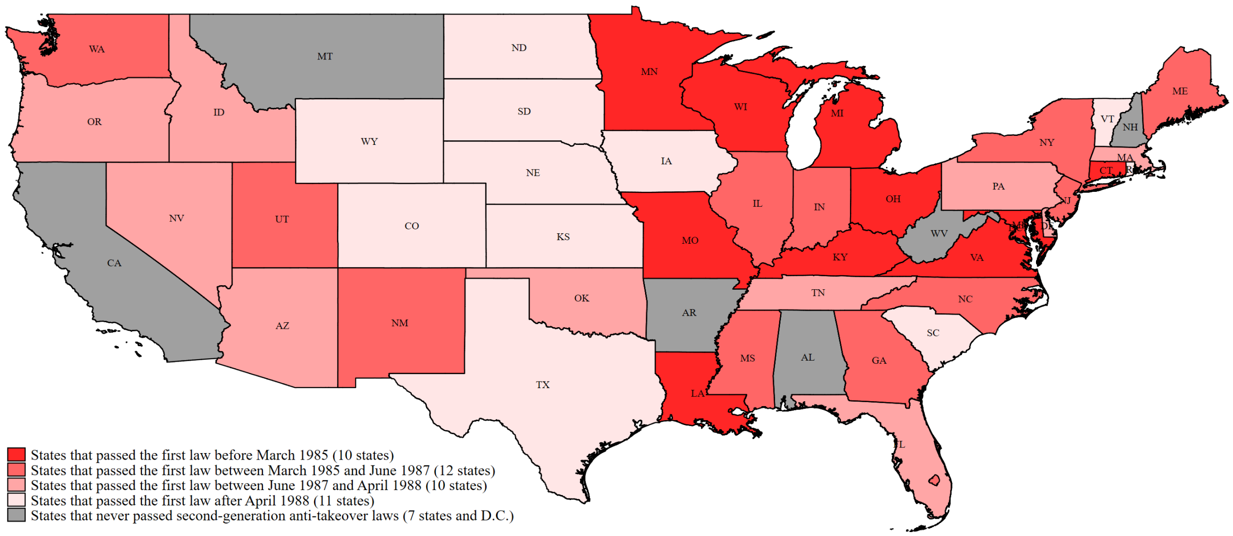
Similar to the case of CEO pay, theory does not provide a clear prediction as to how tenure should respond to the anti-takeover laws. On the one hand, CEOs may become entrenched and

[11] The calculations are based on an average length of BC exposure of 5.75 years (Table II), an average time of 23.88 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 $(-23.88/5.75) \times (4.0\% \times 1.706\% \times \$47.3\text{mn})/\$1.3\text{mn} = -10\%$.

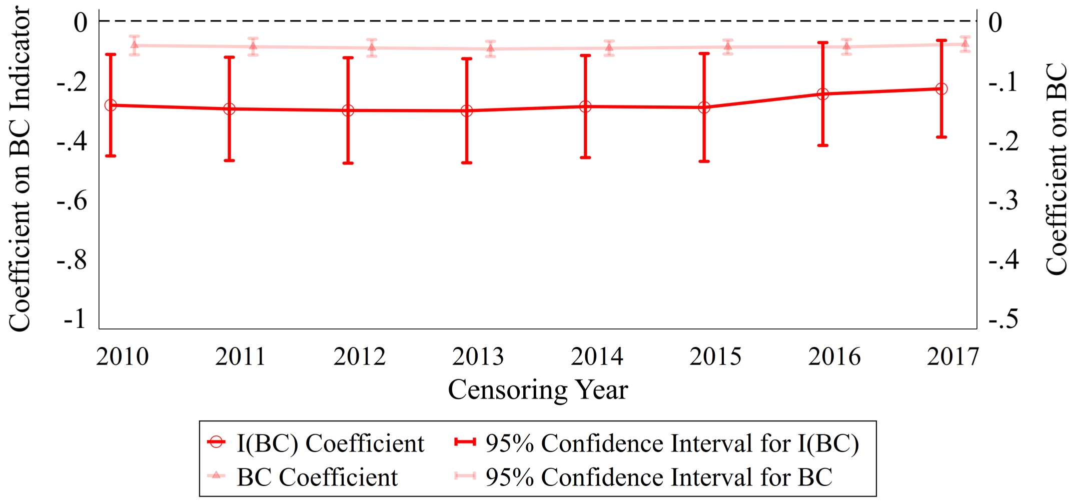
stay on the job longer. On the other hand, CEOs who reduce effort on the job might be fired more frequently. We again estimate hazard models, now analyzing CEO departure as the outcome variable. The results in columns (1) and (2) of Appendix-Table D.11 indicate that BC law treatment, $I(BC)$, decreases the separation hazard by around 19–22%, but the effect is only marginally significant with year fixed effects in column (2), with standard errors nearly doubling. In the specifications using the length of exposure variable *BC* (in columns 3 to 4), the estimated separation hazard falls by 5–9%, but again the effect sizes are quite sensitive to controlling for time trends linearly versus via fixed effects. Overall, the results are suggestive of increases in tenure in response to BC law passage.

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

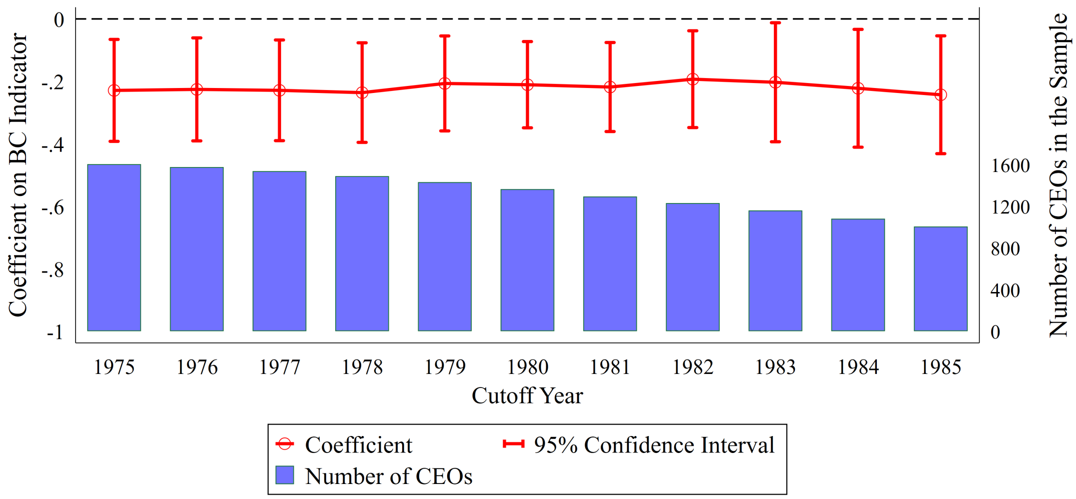
An increase in tenure (or delayed retirement) as a result of anti-takeover insulation is also unlikely to be the channel for the estimated increase in longevity. To begin with, prior research has found small or even beneficial effects of retirement on health in the general population (Hernaes et al.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



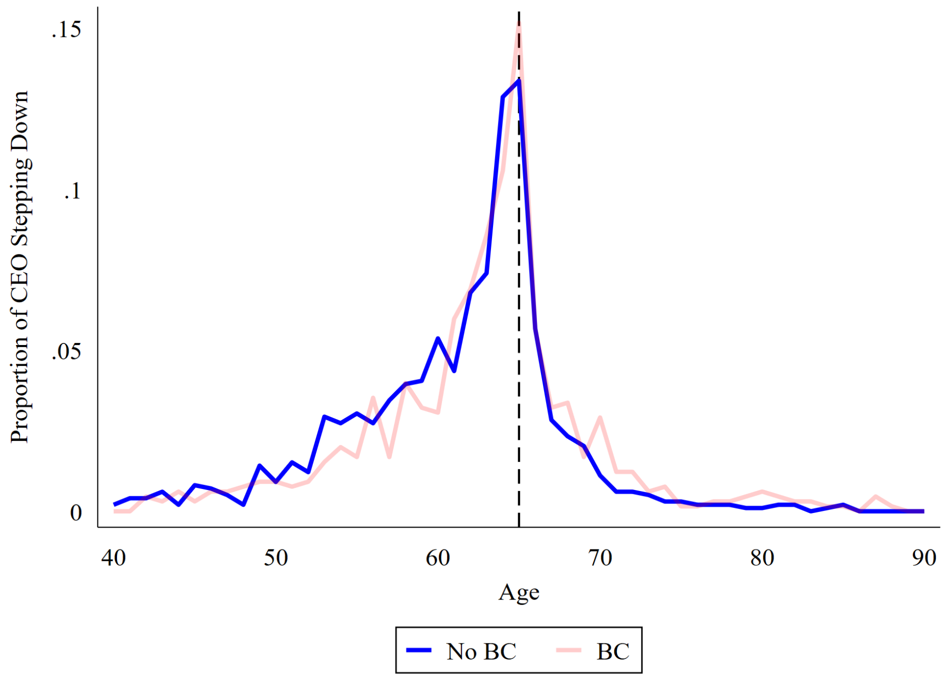
Appendix-Figure D.1. First-time introduction of second-generation anti-takeover laws over time. This figure visualizes the distribution of first-time enactments of any of the five most common second-generation anti-takeover laws over time, i. e., business combination (BC), fair price (FP), control share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



Appendix-Figure D.2. Estimated effect of the BC law exposure when varying the censoring year. 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 V, columns (2) and (6), but varying the censoring date for CEOs identified as alive as of 10/2017. In the main analysis, the cutoff date is Oct. 1, 2017, i. e., CEOs who did not pass away before this date are treated as censored. The alternative censoring dates are Dec. 31, 2016; Dec. 31, 2015; ...; down to Dec. 31, 2010. The number of CEOs in the sample ($N = 1,605$) remains unchanged when varying the cutoff.



Appendix-Figure D.3. Estimated effect of the BC law exposure when varying the sample cutoff year. This figure shows the estimated coefficients on the BC indicator variable $I(BC)$ when using the specification from Table V, column (2), 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 V, column (2).



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

Appendix-Table D.1
Availability of Death Status and Business Combination Law Exposure

This table examines the correlation between the availability of death/alive information and BC law passage. The dependent variable is an indicator variable that equals one if we are able to determine the death/alive status of a CEO. The sample is all CEOs in our initial sample with data on the firm's industry affiliation (SIC codes) and state of incorporation. The *I(BC)* indicator is equal to one if the firm is incorporated in a state that passed a BC law. As in the main regressions, we account for firms' location (HQ state) and industry affiliation. Standard errors are clustered at the state of incorporation level. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

	Coefficient	S.E.	*p*-Value	No. of Obs
I(BC)	0.014	0.019	0.459	2,514

Appendix-Table D.2
Birth-Year Fixed Effects

This table reports hazard coefficients estimated as in Table V, but with birth year fixed effects instead of year controls (linear or fixed effects). All variables are defined in Appendix A. Standard errors, clustered at the state-of-incorporation level, are shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

	(1)	(2)	(3)	(4)
I(BC)	-0.233^{**}	-0.211^{*}		
	[0.114]	[0.119]		
BC			-0.039^{***}	-0.038^{***}
			[0.007]	[0.007]
Age	0.114^{***}	0.115^{***}	0.109^{***}	0.111^{***}
	[0.006]	[0.006]	[0.006]	[0.006]
ln(Pay)		0.022		0.018
		[0.036]		[0.040]
ln(Assets)		−0.041		−0.021
		[0.043]		[0.036]
ln(Employees)		0.014		−0.003
		[0.039]		[0.036]
FirstGen Law Control	Y	Y	Y	Y
FF49 Strata	Y	Y	Y	Y
Location FE	Y	Y	Y	Y
Birth Year FE	Y	Y	Y	Y
No. of CEOs	1,605	1,553	1,605	1,553
Observations	50,530	49,052	50,530	49,052

Appendix-Table D.3
Age-by-Birth-Cohort Robustness

This table reports hazard coefficients estimated as in Table V, but allowing the effect of (chronological) age to vary by CEOs' birth cohort. 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.

	(1)	(2)	(3)	(4)
I(BC)	-0.226^{**}	-0.215^{**}		
	[0.105]	[0.104]		
BC			-0.034^{***}	-0.032^{***}
			[0.006]	[0.006]
Age × Birth Cohort 1 (oldest)	0.090^{***}	0.086^{***}	0.086^{***}	0.082^{***}
	[0.009]	[0.010]	[0.008]	[0.009]
Age × Birth Cohort 2	0.089^{***}	0.085^{***}	0.085^{***}	0.081^{***}
	[0.009]	[0.010]	[0.008]	[0.009]
Age × Birth Cohort 3	0.088^{***}	0.083^{***}	0.083^{***}	0.079^{***}
	[0.010]	[0.011]	[0.009]	[0.010]
Age × Birth Cohort 4	0.085^{***}	0.080^{***}	0.080^{***}	0.075^{***}
	[0.011]	[0.013]	[0.010]	[0.012]
Age × Birth Cohort 5 (youngest)	0.081^{***}	0.074^{***}	0.076^{***}	0.070^{***}
	[0.011]	[0.013]	[0.010]	[0.012]
Year	0.019^{**}		0.018^{*}	
	[0.009]		[0.009]	
FF49 Strata	Y	Y	Y	Y
Location FE	Y	Y	Y	Y
Year FE		Y		Y
No. of CEOs	1,605	1,605	1,605	1,605
Observations	50,530	50,530	50,530	50,530

Appendix-Table D.4
State-of-Incorporation Fixed Effects

This table reports hazard coefficients estimated as in Table V, but including state-of-incorporation fixed effects instead of state-of-headquarters fixed effects. All variables are defined in Appendix A. Standard errors, clustered at the state-of-incorporation level, are shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

	(1)	(2)	(3)	(4)
I(BC)	-0.234^{***}	-0.235^{***}		
	[0.088]	[0.086]		
BC			-0.042^{***}	-0.041^{***}
			[0.006]	[0.007]
Age	0.112^{***}	0.112^{***}	0.109^{***}	0.109^{***}
	[0.004]	[0.004]	[0.005]	[0.005]
Year	-0.001		-0.004	
	[0.005]		[0.007]	
FirstGen Law Control	Y	Y	Y	Y
FF49 Strata	Y	Y	Y	Y
Location (Incorp.) FE	Y	Y	Y	Y
Year FE		Y		Y
No. of CEOs	1,605	1,605	1,605	1,605
Observations	50,530	50,530	50,530	50,530

Appendix-Table D.5
First-Time Second-Generation Anti-Takeover Laws and Mortality

This table reports hazard coefficients estimated as in Table V, 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). 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.

	(1)	(2)	(3)	(4)
I(FL)	-0.171^{**}	-0.164^{**}		
	[0.070]	[0.065]		
FL			-0.041^{***}	-0.040^{***}
			[0.006]	[0.006]
Age	0.111^{***}	0.112^{***}	0.106^{***}	0.106^{***}
	[0.004]	[0.004]	[0.004]	[0.004]
Year	-0.003		-0.002	
	[0.004]		[0.006]	
FirstGen Law Control	Y	Y	Y	Y
FF49 Strata	Y	Y	Y	Y
Location FE	Y	Y	Y	Y
Year FE		Y		Y
No. of CEOs	1,510	1,510	1,510	1,510
Observations	47,994	47,994	47,994	47,994

Appendix-Table D.6
Excluding Lobbying Firms, Opt-Out Firms, and Firm-Years with Firm-Level Defenses

This table reports hazard coefficients estimated as in Table V, 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 the 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 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.

	(1)	(2)	(3)	(4)
	Panel A: Excluding Lobbying Firms			
I(BC)	-0.253^{***}	-0.249^{***}		
	[0.090]	[0.088]		
BC			-0.040^{***}	-0.039^{***}
			[0.008]	[0.008]
No. of CEOs	1,530	1,530	1,530	1,530
Observations	48,106	48,106	48,106	48,106
	Panel B: Excluding Opt-Out Firms			
I(BC)	-0.200^{**}	-0.195^{**}		
	[0.080]	[0.078]		
BC			-0.038^{***}	-0.038^{***}
			[0.007]	[0.007]
No. of CEOs	1,532	1,532	1,532	1,532
Observations	48,180	48,180	48,180	48,180
	Panel C: Excluding Firm-Level Defenses			
I(BC)	-0.235^{***}	-0.228^{***}		
	[0.086]	[0.088]		
BC			-0.041^{***}	-0.040^{***}
			[0.006]	[0.006]
No. of CEOs	1,599	1,599	1,599	1,599
Observations	43,400	43,400	43,400	43,400
Linear Age Control	Y	Y	Y	Y
FirstGen Law Control	Y	Y	Y	Y
FF49 Strata	Y	Y	Y	Y
Location FE	Y	Y	Y	Y
Linear Year Control	Y		Y	
Year FE		Y		Y

Appendix-Table D.7
Restrictions Based on First-Generation Laws

This table re-estimates columns (2) and (6) of Table V with sample restrictions based on 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 sample-years prior to 1983 for CEOs with first-generation law exposure. In subsample B, we drop all CEOs with first-generation law exposure who stepped down prior to 1983, i. e., we restrict the sample to CEOs without first-generation law exposure and, for CEOs with exposure, to those who who served during the "post-first-law period." 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 without any first-generation law exposure. 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.

	Subsample A: Drop FirstGen-CEO Pre-1983 Sample Years		Subsample B: Drop FirstGen-CEOs Stepping Down Pre-1983		Subsample C: Drop FirstGen-CEOs	
I(BC)	-0.242^{***}		-0.178^{**}		-0.310^{***}	
	[0.086]		[0.083]		[0.085]	
BC		-0.043^{***}		-0.034^{***}		-0.044^{**}
		[0.006]		[0.009]		[0.018]
Age	0.112^{***}	0.108^{***}	0.122^{***}	0.118^{***}	0.121^{***}	0.115^{***}
	[0.005]	[0.005]	[0.006]	[0.007]	[0.012]	[0.014]
FirstGen Law Control	Y	Y	Y	Y	–	–
FF49 Strata	Y	Y	Y	Y	Y	Y
Location FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
No. of CEOs	1,574	1,574	1,234	1,234	548	548
Observations	40,790	40,790	39,511	39,511	15,629	15,629

Appendix-Table D.8
Excluding DE or NY Incorporated, Banking, or Utility Firms

This table reports hazard coefficients estimated as in Table V with the sample restricted based on firms' state of incorporation or industry affiliation. In Panel A, we exclude firms that are incorporated in Delaware or New York (the two most common states of incorporation in our sample). 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.

	(1)	(2)	(3)	(4)
	Panel A: Excluding DE/NY Firms			
I(BC)	-0.314^{**}	-0.303^{**}		
	[0.132]	[0.132]		
BC			-0.042^{*}	-0.039^{*}
			[0.022]	[0.022]
No. of CEOs	738	738	738	738
Observations	22,103	22,103	22,103	22,103
	Panel B: Excluding Banking Firms			
I(BC)	-0.292^{***}	-0.289^{***}		
	[0.079]	[0.074]		
BC			-0.055^{***}	-0.054^{***}
			[0.007]	[0.007]
No. of CEOs	1,328	1,328	1,328	1,328
Observations	42,322	42,322	42,322	42,322
	Panel C: Excluding Utility Firms			
I(BC)	-0.220^{***}	-0.216^{***}		
	[0.077]	[0.075]		
BC			-0.038^{***}	-0.037^{***}
			[0.006]	[0.005]
No. of CEOs	1,422	1,422	1,422	1,422
Observations	45,017	45,017	45,017	45,017
Linear Age Control	Y	Y	Y	Y
FirstGen Law Control	Y	Y	Y	Y
FF49 Strata	Y	Y	Y	Y
Location FE	Y	Y	Y	Y
Linear Year Control	Y		Y	
Year FE		Y		Y

Appendix-Table D.9
Nonlinear Effects and Predicted Exposure

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

	(1)	(2)	(3)	(4)
$\mathrm{BC}^{\min-p50}$	−0.075***	−0.071***		
	[0.022]	[0.022]		
$\mathrm{BC}^{p51-\max}$	−0.015	−0.016		
	[0.016]	[0.015]		
$\widehat{\mathrm{BC}}$			−0.042	−0.042
			[0.037]	[0.039]
Age	0.108***	0.108***	0.110***	0.110***
	[0.004]	[0.004]	[0.007]	[0.007]
Year	−0.001		−0.004	
	[0.006]		[0.007]	
FirstGen Law Control	Y	Y	Y	Y
FF49 Strata	Y	Y	Y	Y
Location FE	Y	Y	Y	Y
Year FE		Y		Y
No. of CEOs	1,605	1,605	1,605	1,605
Observations	50,530	50,530	50,530	50,530

Appendix-Table D.10
Business Combination Laws and CEO Pay

The table shows OLS estimates where the dependent variable is the logarithm of a CEO's total pay in a given year. In column (1), "Age Controls" includes linear age, and in columns (2) and (3) it includes linear and quadratic age. "Tenure Controls" includes linear and quadratic tenure. "Firm Characteristics" includes logarithms of asset size and the number of employees. Standard errors, clustered at the state-of-incorporation level, are shown in brackets. *, **, and *** denote significance at the 10, 5, and 1 percent level, respectively.

	(1)	(2)	(3)
I(BC)	0.071	0.076	0.045
	[0.057]	[0.047]	[0.051]
FirstGen Law Control	Y	Y	Y
Age Controls	Y	Y	Y
Tenure Controls		Y	Y
Firm Characteristics		Y	Y
FF49 FE	Y	Y	
Location FE	Y	Y	
Year FE	Y	Y	Y
Firm FE			Y
No. of CEOs	1,553	1,553	1,553
Observations	17,719	17,719	17,719

Appendix-Table D.11
Business Combination Laws and Tenure

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

	(1)	(2)	(3)	(4)
I(BC)	-0.244^{***}	-0.205^{*}		
	[0.067]	[0.110]		
BC			-0.090^{***}	-0.049^{**}
			[0.024]	[0.023]
Age	0.106^{***}	0.104^{***}	0.107^{***}	0.105^{***}
	[0.013]	[0.014]	[0.013]	[0.014]
Year	0.083^{***}		0.098^{***}	
	[0.008]		[0.013]	
FirstGen Law Control	Y	Y	Y	Y
FF49 Strata	Y	Y	Y	Y
Location FE	Y	Y	Y	Y
Year FE		Y		Y
No. of CEOs	1,605	1,605	1,605	1,605
Observations	17,864	17,864	17,864	17,864