Does Better Access to Disclosure Curb CEO Pay? Evidence from a Modern Information Technology Improvement*

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ABSTRACT

We provide evidence that better access to disclosure curbs CEO pay. Using difference-in-differences estimation around the implementation of the SEC EDGAR platform from 1993 to 1996, we find that total CEO pay grows 7-15% less following EDGAR implementation relative to control firms. This effect is more pronounced for highly-paid CEOs, equity-based pay, and firms with unions or those in left-leaning states. Media coverage of executive pay increases following EDGAR adoption, particularly around proxy filing dates. Additionally, we find higher voluntary CEO turnover post-EDGAR, with the market showing a more negative response to turnover announcements, suggesting negative implications for firm value.

Keywords: Executive compensation, disclosure, incentives, CEO turnover
JEL codes: G30, G32

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I. Introduction

The high level of CEO pay in the United States has attracted the attention of media, regulators, investors, and the general public, fueling calls for more regulation and higher transparency in CEO compensation.[1] Yet, there are conflicting predictions regarding the effect of mandated disclosure on CEO pay (see, e.g., Hermalin and Weisbach (2012), Bebchuk and Fried (2004)),[2] and higher transparency may also come at a cost, particularly if the public is averse to inequality (Fehr and Schmidt (1999)). For example, Mas (2017) finds that increasing transparency of city manager pay results in a 75% increase in their quit rates, with cities scrambling to fill the vacancies, Boone, Starkweather, and White (2024) report that the disclosure of high CEO-employee pay ratios lowers employee productivity, and Card, Mas, Moretti, and Saez (2012) find that higher pay transparency lowers job satisfaction. Further, to the extent that high transparency could create more controversy over CEO pay, it could cause directors to make suboptimal decisions in order to avoid it, thereby sacrificing shareholder value (Edmans, Gosling, and Jenter (2023)). In this paper, we study how increased transparency and better access to disclosure, resulting from the introduction of the Electronic Data Gathering, Analysis, and Retrieval (EDGAR) platform, affect CEO pay and examine the implications for firms.

To empirically identify the effect of better access to disclosure on CEO pay, we use the implementation of modern information dissemination technologies by the U.S. Securities and Exchange Commission (SEC). We focus on the introduction of the EDGAR platform that facilitated access to corporate filings in the electronic form. Before EDGAR, it was possible but costly and impractical for investors to access firms' filings (e.g., 10-K, 10-Q, 8-K, or DEF 14A), because investors had to either subscribe to services of commercial data providers or visit one of the SEC's reference rooms in Chicago, New York, or Washington, D.C. (Rider (2001)).[3] To reduce the costs of accessing firm filings,

[1] For example, in SEC Release 2022-149 on the disclosure of the relationships between the executive compensation actually paid and performance measures (Item 402(v), Regulation S-K), SEC Chair Gary Gensler said that "The Commission has long recognized the value to investors of information on executive compensation."

[2] Hermalin and Weisbach (2012) develop a theory showing that, although greater disclosure improves investor monitoring, it imposes costs on executives and increases their equilibrium pay. In contrast, others suggest that greater disclosure can reduce "stealth" pay or provoke public shaming of CEOs, presumably lowering their pay (see, e.g., Bebchuk and Fried (2003), Morse, Nanda, and Seru (2011)).

[3] Chang, Hsiao, Ljungqvist, and Tseng (2022) estimate that obtaining Ford's 10-K from data providers would have cost approximately $145 per page, not counting additional charges. The fact that investors were willing

on February 23, 1993, the SEC announced a plan to require all public U.S. firms to file their mandatory disclosures electronically through the EDGAR system. Firms would join EDGAR in 10 separate waves between April 1993 and May 1996, with the assignment to waves being random, conditional on firm size (Chang, Hsiao, Ljungqvist, and Tseng (2022)). Owing to modern information technologies, the implementation of EDGAR significantly reduced costs of accessing timely firm-specific information for investors and other interested parties and had a profound effect on information production by market participants (see, e.g., Gao and Huang (2020), Chang, Hsiao, Ljungqvist, and Tseng (2022), Chang, Ljungqvist, and Tseng (2023), Goldstein, Yang, and Zuo (2023)), making it an appealing setting for studying how easier access to information affects CEO pay.

Using a stacked difference-in-differences (DiD) design (see, e.g., Cengiz, Dube, Lindner, and Zipperer (2019), Baker, Larcker, and Wang (2022)) around the implementation of SEC EDGAR from 1993 to 1996 and a DiD estimator proposed by de Chaisemartin and d'Haultfoeuille (2020), we find evidence that better access to corporate filings curbs CEO pay. While our sample period is characterized by rapid growth in CEO compensation, we estimate that firms required to post their filings on the EDGAR platform subsequently report 7–15% lower total CEO pay relative to control firms. One interpretation of these results is that modern information technologies allow shareholders to revise their beliefs about firms' pay-setting processes or the level of executive pay, resulting in a more informed decision-making process, pressure on boards, and a lower level of pay relative to other firms. Alternatively, it is possible that easier access to corporate filings galvanizes labor unions, employees, the general public, or the media, who publicly shame highly-paid CEOs and pressure boards to lower CEO pay even if it is to the detriment of their shareholders.[4] Boards, in turn, may yield to public pressure to avoid controversy over CEO pay (Edmans, Gosling, and Jenter (2023)).

Consistent with the idea that disclosure of exceptionally large compensation packages provokes public shaming of CEOs, we find that media coverage of executive compensation increases following EDGAR implementation, particularly around proxy filing dates

to pay providers for disclosure data indicates that acquisition costs were substantial in practice (Blankespoor, deHaan, and Marinovic (2020)).

[4]Studies find that firm employees and the general public tend to significantly underestimate how much CEOs actually earn (Cullen and Perez-Truglia (2022), Larcker, Donatiello, and Tayan (2016)).

(when CEO pay information first becomes available), and that the coverage tends to have a negative tone. Furthermore, the effect of EDGAR on CEO pay increases with the level of reported pay. For example, for CEOs in the top decile of distribution, pay grows approximately 21% less relative to control firms after the implementation of EDGAR, whereas for CEOs in the bottom decile, the effect is more modest, at 10%. Additionally, we find that the pay of executives other than CEOs is less affected by the introduction of EDGAR. These results may reflect the fact that lower-ranked executives lack the celebrity status of CEOs, making their compensation less interesting to the public.

While evidence that firms tend to curb CEO compensation in response to better access to corporate disclosure may be consistent with the narrative of better decision-making by firm shareholders, it is also consistent with politicization of CEO pay and public aversion to high CEO compensation. Specifically, changes in compensation following EDGAR implementation could influence CEOs' incentives to increase firm value. For example, Jensen and Murphy (1990) argue that political forces may often limit firms' ability to use contracts with high pay-performance sensitivity, and prior research finds that the negative press coverage of CEO pay packages tends to focus disproportionally on incentive-based awards, such as stock options (Core, Guay, and Larcker (2008), Kuhnen and Niessen (2012)). We therefore also study how the mix of CEO compensation changes after the implementation of EDGAR. Indeed, we find that CEOs in treated firms receive sharply lower equity-based compensation relative to CEOs in control firms, driven by relatively fewer stock option grants and resulting in a lower share of equity-based incentive pay in overall compensation packages.

We next examine potential mechanisms behind CEO pay changes. Our evidence does not support the conjecture that large institutional shareholders are the primary drivers of these reductions, as firms with high institutional ownership do not reduce CEO pay more than others after EDGAR implementation. We also find only weak support for the hypothesis that increased media coverage contributes significantly to lowering CEO pay. Instead, the evidence aligns more closely with negative sentiment towards excessive CEO compensation from firm stakeholders and the general public. For example, we observe that CEO pay changes are more pronounced in firms with labor unions and those located in left-leaning states, where public aversion to income inequality is presumably higher.

Finally, we investigate whether changes in CEO compensation triggered by EDGAR implementation have implications for firm value. These tests help distinguish between two alternative explanations for the observed decline in CEO pay: improved decision-making by shareholders due to expanded access to information, and public aversion to high executive compensation. To this end, we document that the introduction of EDGAR has a likely unintended effect on CEO turnover. Specifically, CEO turnover increases by more than 2.5% following EDGAR introduction, compared to an average annual CEO turnover rate of 3.5%, and this increase is driven by voluntary CEO turnover. These results support the theoretical predictions by Hermalin and Weisbach (2012), who argue that if it is politically infeasible for firms to increase executive compensation, disclosure can lead to higher CEO turnover rates.

Additionally, we find that the cumulative abnormal returns (CARs) around announcements of CEO turnover are more negative following EDGAR implementation. These results indicate that while increased information access helps curb CEO pay, there may be negative consequences for firm shareholders. Overall, our evidence aligns more closely with the politicization of CEO pay and public aversion to high CEO compensation facilitated by improved disclosure access, echoing arguments by Murphy (2013) and Murphy and Jensen (2018) regarding the effect of disclosure regulations on efficacy of CEO pay.

Our paper contributes to the literature on the relation between disclosure and executive compensation. Previous studies generally suggest that changes in disclosure content do not effectively limit CEO pay (see, e.g., Gipper (2021), Bloomfield (2021), Chang, Dambra, Schonberger, and Suk (2023), Mas (2016), and Faulkender and Yang (2013)). For example, Chang, Dambra, Schonberger, and Suk (2023) find no significant changes in total CEO compensation following the mandated CEO-employee pay ratio disclosure in 2018, and Mas (2016) finds that the 1934 mandated pay disclosure has an upward "ratcheting" effect, whereby lower-paid CEOs experience gains, while well-paid CEOs are unaffected. In contrast to these studies, we focus on a shock to modern information dissemination technologies that significantly improved access to all disclosed information to a large group of people, but at the same time did not require firms to change what is being reported in their filings. This distinction is likely important because improved information access may spark interest in CEO pay among new groups such as employ-

ees, unions, or the general public, who previously had limited access to executive pay information and whose preferences and incentives may differ from those of shareholders.

Two studies by Rose and Wolfram (2002) and Perry and Zenner (2001) examine changes in executive compensation in response to the 1992 disclosure rules and legislation in 1993 capping tax deductibility of management compensation not qualified as "performance-based" at $1 million (Section 162(m) of the IRS Code). They find that affected firms reduce CEO salaries but do not reduce total CEO pay levels. Although EDGAR implementation mostly took place after this period and our identification relies on different groups of treated firms at various times, we take care to ensure that our results are not driven by these regulations. For instance, our findings remain robust when excluding firms that paid CEO salaries near $1 million before Section 162(m).

Finally, our study relates to Mas (2017), who investigates the impact of pay disclosure in the public sector, finding that California's mandate to post municipal wages online decreases city manager pay by about 7% and increases turnover by 75%. In contrast, our focus is on the corporate environment, where a significant portion of managerial compensation comes in the form of incentive-based pay, such as stock and options, and where evaluating managerial performance is arguably more straightforward.

The rest of this paper is organized as follows. Section II describes the setting and research design. Section III describes data sources and presents summary statistics. Section IV reports our main empirical results. Section V concludes.

II. Setting and Research Design

Our identification relies on the phased implementation of the EDGAR system, announced by the SEC on February 23, 1993 and implemented between 1993 and 1996. Before EDGAR, all publicly registered U.S. firms had to physically mail paper copies of their mandatory filings to the SEC's office. The SEC would then review these documents before filing them in reference rooms for public viewing. Investors or other interested parties could access these filings either by subscribing, for a fee, to the services of commercial data vendors such as Mead Data Central or Dialog, or by visiting one of the

three SEC reference rooms in the country.[5] For example, a *New York Times* article describes the problems associated with quickly retrieving the needed information from the SEC's reference rooms as "files are often misplaced or even stolen," "there is the constant noise," and users of the reference room "are left to fend for themselves."[6] These challenges align with arguments by Hirshleifer and Teoh (2003) and Blankespoor, deHaan, and Marinovic (2020) that significant disclosure processing costs may exist in practice. Additionally, Gomez (2024) argues that EDGAR affected information dissemination in two main ways: (i) it decreased the cost of accessing information and (ii) it allowed for faster access to firm filings.[7] Further supporting the notion of limited access to firms' SEC filings prior to EDGAR, Engelberg and Parsons (2011) show that retail investors often learned about firms' earnings announcements from local newspapers in the 1990s.

With the introduction of EDGAR, the SEC required all registered firms to transmit their filings to the SEC's office electronically and assigned firms to one of ten implementation waves over the course of three years. According to Chang, Hsiao, Ljungqvist, and Tseng (2022), based on their private correspondence with the SEC, the assignment of firms to implementation waves was random, conditional on firm size.[8] The preliminary phase-in schedule was provided in SEC Release 33-6977, which essentially fixed the dates for the first four implementation waves,[9] while the finalized phase-in schedule came out in SEC Release 33-7122.[10] The staggered mandatory implementation of the EDGAR

[5]Registered shareholders also received information about CEO pay through mailed proxy statements. However, these statements were not accessible to other interested parties (e.g., the media, employees, or the general public), contained less information about firm performance compared to a compilation of all SEC filings, and did not enable benchmarking of CEO pay against that of other firms. A few university libraries stored SEC filings on microfilm, but the data was typically available only with a delay.

[6]See "S.E.C. Data: Difficult Hunt," the *New York Times*, May 19, 1982.

[7]Gomez (2024) also finds that because of costs of filtering and interpreting information, EDGAR benefitted some investors at the expense of others.

[8]We confirm in our sample that larger firms were assigned to earlier EDGAR waves (Panel A of Table IA.1 in the Internet Appendix). We also compare pre-EDGAR firm and CEO characteristics across different EDGAR implementation waves, controlling for firm size. We find that total CEO pay, CEO tenure, ROA, and stock returns are similar across waves, but leverage and asset tangibility differ. Our results are robust to controlling for potential differential trends in CEO pay across firms with different size, asset tangibility, or leverage.

[9]An outline of the SEC's EDGAR rules said: "The 'significant test group' will be phased in between April 26 and December 1993, in four groups. After the 'significant test group' has successfully filed for at least six months, the Commission will adopt final EDGAR rules modified to reflect the experience gained during that period." Indeed, after the first four waves, SEC did a review and revised dates for waves 5-6 and 8-10.

[10]Firms could apply for temporary or continuing hardship exemptions. Temporary exemptions were granted if a firm experienced unanticipated technical difficulties (e.g., due to a power outage) and allowed for a six-day extension. Continuing hardship exemptions were granted, upon review, if a firm was unable to file electronically without undue burden or expense (e.g., companies under the protection of a bankruptcy court could qualify).

system is helpful for identification purposes because it leaves less scope for omitted firm- or industry-level shocks affecting our results. Furthermore, the relatively short period between the SEC's initial announcement on February 23, 1993 and the phase-in dates for the first four implementation waves (April 26 through December 6, 1993), as well as the remaining uncertainty regarding the phase-in dates for waves 5 through 10, mitigate possible anticipation effects in our setting.[11]

Table 1 presents the finalized phase-in dates for the ten implementation waves, with the first wave taking place on April 26, 1993 and the last on May 6, 1996. For illustrative purposes, Figure 1 also shows the number of firms in the sample covered by EDGAR over time (blue dashed line), as well as the average CEO pay (red solid bars) for firms joining EDGAR in the year of their joining. The first two waves of firms had a somewhat higher CEO pay, averaging $2.7 and $2.6 million, respectively, while eight other waves averaged $1.7 million in CEO pay per year. As we show in the Internet Appendix, however, CEO pay does not differ significantly across waves, conditional on firm size.

To investigate the effect of expanded access to disclosure on CEO compensation, we conduct a stacked DiD analysis, following the approach advocated by Baker, Larcker, and Wang (2022) and implemented, for example, by Cengiz, Dube, Lindner, and Zipperer (2019).[12] The stacked sample helps mitigate potential biases from inadequate control groups that can arise in the presence of heterogeneous or dynamic treatment effects, as highlighted by Goodman-Bacon (2021) and de Chaisemartin and d'Haultfoeuille (2020), among others. Given that all public firms eventually adopted EDGAR, there are no never-treated firms in our setting. Therefore, our stacked sample comprises four stacks of data corresponding to the first four EDGAR implementation waves and uses not-

Gao and Huang (2020) find that only 3% of firms deviated from their assigned phase-in group in the schedule.

[11] It is ex ante unclear what is the best response for a board anticipating EDGAR coverage in the near future, or whether boards are sufficiently forward-looking in setting CEO pay. Anticipatory effects may be muted if the public focuses only on the most recent year of reported compensation, making it unnecessary for boards to act in advance, or if boards are uncertain how much attention the public will pay to disclosures following improved access. If public focuses on pay growth, boards may preemptively raise CEO pay (e.g., by accelerating stock option grants), creating an Ashenfelter-type dip and leading us to overestimate EDGAR's effect. Conversely, if the public focuses on pay levels over several recent years, boards may reduce pay in advance, causing us to underestimate the effect. Finally, there may be "general equilibrium" effects if control firms benchmark their pay against that of treated firms. This, too, would lead us to underestimate the effect of EDGAR on CEO pay. We further discuss anticipatory effects when we present empirical evidence in Section IV.A.

[12] A possible limitation of our setting is that the introduction of EDGAR could affect other firm policies or firm characteristics, such as liquidity. We discuss this issue in Section IV.G.8.

yet-treated firms as controls. We define the control group as firms with phase-in dates at least one year later than those of a given stack, and we include observations for as long as all the control firms remain untreated. Therefore, the coefficients in the stacked DiD analysis are identified from differential changes in CEO pay in treated vs. control firms in 1993 and 1994. Our results remain robust when using the first seven stacks or increasing the gap between phase-in dates for treated and control firms to a minimum of two years. The main specification of the stacked DiD analysis is as follows:

$$Y_{its} = \gamma_{is} + \delta_{ts} + \beta \cdot \ Treated_{is} \times Post_{ts} + X'\Gamma + \varepsilon_{its}, \tag{1}$$

where Y_{its} is the dependent variable of interest (e.g., log of total CEO pay), i indexes firms, t indexes years, and s indexes sub-experiments, which correspond to different EDGAR implementation waves. $Treated_{is}$ is an indicator variable equal to one if a firm is treated in sub-experiment s (i.e., it becomes a mandatory EDGAR filer), $Post_{ts}$ is an indicator variable equal to one if the period t is the post-period in sub-experiment s, X is a vector of control variables, Γ is the vector of corresponding coefficients, γ_{is} is the firm-by-stack fixed effects, δ_{ts} is the year-by-stack fixed effects, and ϵ_{its} is the error term. In tables, we use the shorthand notation, $EDGAR$, to denote $Treated_{is} \times Post_{ts}$, which is equal to one if a firm's fiscal year ends after the phase-in date for its corresponding implementation wave in a given year and zero otherwise.

III. Data and Summary Statistics

We start by obtaining executive compensation data from Execucomp for the period 1992-1999, firm accounting data from Compustat, and stock return data from the Center for Research in Security Prices (CRSP). To enhance data coverage during the pre-EDGAR period, we also collect CEO compensation for fiscal year 1991 (and 1992 if it is missing in Execucomp) from proxy statements.[13] CEO turnover data are from the contributed data on WRDS (Jenter and Kanaan (2015)). The information on firms included in each of the ten phase-in waves for the EDGAR introduction and the finalized phase-in dates

[13]Proxy statements typically contain three years of compensation data (e.g., a proxy filed in 1994 may cover compensation in 1991-1993). Our data collection yields 64 additional observations for 1992 and 567 for 1991. In robustness tests, we further expand our data coverage to year 1990 using compensation data collected by David Yermack, who graciously shared it with us. We do not use his data, described in detail in Yermack (1995), in our main tests because it has a different structure and covers a subset of larger firms.

of implementation come from SEC Releases 33-6977 and 33-7122 and from the Federal Register (Rules and Regulations) published on March 18, 1993.

To obtain media coverage of executive compensation, we follow Core, Guay, and Larcker (2008) and Kuhnen and Niessen (2012) and download compensation articles from major newspapers and newswires for the period 1990-1999 using ProQuest. Specifically, we search for the following phrases in the article title or summary: *CEO* pay*, *executive* pay*, *CEO paid*, *executive paid*, *CEO* bonus*, *executive* bonus*, *CEO* compensat**, *executive* compensat**, *CEO* salar**, *executive* salar**, *CEO* option**, *executive* option**, *CEO* and *million** or *"000"*, *executive** and *million** or *"000"*, and consider only those articles that have the subject of "executives," "chief executive officers," "executive compensation," or "stock options."[14] This procedure yields a sample of 42,991 compensation articles.[15] We then identify firms in our sample that these articles mention and count the total number of articles and article words for each firm-year. We also count the number of negative compensation articles, whereby we define an article as negative if the number of used negative words from the financial dictionary of Loughran and McDonald (2011) is greater than the number of positive words from the same dictionary.

Summary statistics of main variables in our sample are reported in Panel A of Table 2, and variable definitions are provided in Appendix A. Approximately 75.3% of firm-years have EDGAR equal to one. The average CEO earns an annual compensation of $2.62 million during our sample period, out of which approximately 1.60 million is equity-based incentive pay (primarily stock options). Consistent with prior studies, the CEO compensation is positively skewed in our sample, with the average CEO pay being higher than the median of $1.45 million. The average and median annual pay of named executive officers other than the CEO are considerably lower than those of CEOs, at $0.91 million and $0.52 million, respectively. The annual CEO turnover rate is 7.2%, out of which 1.9% is the forced CEO turnover rate (see Peters and Wagner (2014) and

[14] We limit our search to the following news sources: *Wall Street Journal*, *New York Times*, *Washington Post*, *Los Angeles Times*, *Financial Times*, *USA Today*, *American Banker*, *Economist*, *PR Newswire*, *Business Wire*, *Star Tribune*, *Boston Globe*, *Pittsburg Post*, *Vancouver Sun*, *Newsday*, *Orlando Sentinel*, *St. Petersburg Times*, *Chicago Tribune*, *Forbes*, *Salt Lake Tribune*, *Hartford Courant*, *Bloomberg Businessweek*, *Fortune*, and *Guardian*.

[15] While we refer to these articles as compensation articles, they may primarily focus on other issues, such as firm performance, or discuss them in addition to compensation. Table IA.2 in the Internet Appendix lists the ten most commonly used positive and negative words in compensation articles, as classified by the financial dictionary of Loughran and McDonald (2011), and shows that many common words are general rather than compensation-specific.

Jenter and Kanaan (2015) for the construction of this variable). Table 2 also reports on other firm-level characteristics. Owing to the requirement of Execucomp coverage, firms that comprise our sample tend to be large and profitable, with the average book value of assets close to $5.6 billion, ROA of 3.8%, and the annual stock return of 19.7%.

Since we will primarily use the stacked sample for our tests, we also provide the summary statistics for this sample in Panel B. Naturally, the stacked sample is skewed toward the earlier time period, covering the years 1991 to 1995. Consequently, EDGAR adoption is lower in this sample at 23.9%, and the average annual CEO compensation is also lower, averaging $1.60 million.

Panels C and D provide descriptive statistics on the number, length, and tone of compensation articles published in major news outlets during a given year, as well as those published during the three-month period around firms' proxy filing dates (months 3-5 of the fiscal year), since firm proxies are the most relevant SEC filings for CEO pay. In Panel D, 'fiscal year' and 'proxy filing' in parentheses indicate that the statistics are for the relevant variables during a fiscal year or during the three-month period around a firm's proxy filing dates, respectively. The most common source of executive compensation articles is the *Wall Street Journal*, consistent with its primary focus on business and finance. On average, there are approximately 1.73 compensation articles published per firm-year. The total number of words in compensation articles averages 2,441 per firm-year, and this variable is positively skewed. Consistent with the prior literature highlighting the negative media bias (e.g., Core, Guay, and Larcker (2008), Loughran and McDonald (2011), and Kuhnen and Niessen (2012)), more than 70% of all compensation articles in the sample are negative (1.67 negative articles out of 2.33 total articles, on average).

IV. Empirical Results

A. Total CEO Pay

We now turn to our main analysis of the relation between access to disclosure and CEO pay. As EDGAR adoption allowed for free and timely access to electronic firm filings, it enabled broader access to information about the level and structure of CEO pay

and facilitated easier benchmarking of pay against other firms. As a result, investors and other interested parties could pay more attention to executive compensation and potentially revise their beliefs about it. In turn, corporate boards could face increased pressure to curb CEO pay, whether from firm shareholders, the media, labor unions, employees, or other stakeholders. Moreover, boards of firms scheduled to be covered by EDGAR next year might take preemptive action on CEO pay, even if public pressure has not yet materialized, as many directors indicate they would prefer to sacrifice shareholder value rather than face controversy over CEO pay (Edmans, Gosling, and Jenter (2023)).

Table 3 presents the results of the DiD analysis around EDGAR implementation, with the dependent variable being the natural logarithm of total annual CEO pay. In Panel A, we report the results using the stacked sample of data. This sample consists of four stacks corresponding to the first four EDGAR implementation waves (CF-01 to CF-04), with not-yet-treated firms serving as controls. In Panel B, we report the average treatment effects using a DiD estimator of de Chaisemartin and d'Haultfoeuille (2020) on the full 1991-1999 sample. We include firm or firm-CEO fixed effects to account for any possible time-invariant heterogeneity across firms or CEOs, and we include year or industry-year fixed effects to account for the general or industry-specific time trends in CEO pay. Our model also incorporates control variables commonly used in the literature, such as ROA, leverage, stock returns, asset tangibility (measured by the ratio of PP&E to assets), CEO tenure (measured by the logarithm of the number of years in office), and firm size. We exclude variables that may be directly affected by EDGAR implementation, such as Tobin's Q or stock return volatility, because they may serve as 'bad controls' in this setting. Our results are also robust to excluding all control variables.

The results show that better access to corporate disclosure can curb CEO pay. Using a stacked DiD design, where the treatment effect is identified from differential changes in CEO pay between treated and control firms in 1993 and 1994, we find that total CEO pay in treated firms is 13.2% to 15.3% lower than in control firms following EDGAR's introduction, indicating slower pay growth. Results are similar when using a DiD estimator by de Chaisemartin and d'Haultfoeuille (2020), with the magnitude of the main effect varying from 12.9% to 15.4%.[16] Figure 2 plots the corresponding year-by-year

[16] We have also used an alternative DiD estimator proposed by Callaway and Sant'Anna (2021) and find similar

estimates using the DiD estimator of de Chaisemartin and d'Haultfoeuille (2020). The results support the parallel trends assumption in our sample and show a significant negative effect in the first and second years following EDGAR implementation. The absence of pre-trends also provides prima facie evidence that anticipatory effects are unlikely to be driving the results.

Table B.1 of the Appendix further demonstrates the robustness of our results. We find that the results are robust to variations in the construction of the stacked sample, including using four or seven stacks, different minimum gap years between phase-in dates of treatment and control firms, estimating the results without any controls, treating the first four EDGAR waves as a single stack, and estimating the results on a full staggered sample spanning 1991-1999.[17] The magnitude of the negative effect of EDGAR implementation on CEO pay varies slightly across these alternative specifications, ranging from 7.1% (in a staggered sample) to 19.5% (in a stacked sample without time-varying control variables). Additionally, we conduct robustness tests using a propensity score matched sample, where only firms that are never required to report through the EDGAR system are used as control firms (Chang, Hsiao, Ljungqvist, and Tseng (2022)). Our results remain robust, and relevant details are provided in Section IV.G.

To further assess how our estimates are affected by anticipatory actions by boards at control firms, and whether CEO pay declines in absolute terms or only relative to control firms, we examine trends in CEO pay separately for treated and control firms in Figure B.1. Specifically, we use two stacks with implementation years of 1993 and 1995 and measure CEO pay growth relative to the pre-event year. We then plot average CEO pay growth by event year separately for treated and control firms. Consistent with prior literature documenting rising CEO pay in the 1990s, the figure shows increases in CEO pay for both treated and control firms. Nevertheless, pay for treated firms exhibits significantly lower growth beginning in the EDGAR implementation year, with differences statistically significant in each post-event year. Importantly, pre-treatment trends between treated and control groups are similar. A final observation is that control firms show slightly lower pay growth in year $t+2$, which is the last year before their own

results. Note that because of the relative closeness of EDGAR phase-in-dates for different implementation waves, it is not feasible to estimate the effects beyond $t = 2$.

[17] The results are also robust to estimation on a staggered sample covering 1991-1998 or 1991-1997.

EDGAR adoption. This pattern may indicate that boards at control firms preemptively lower CEO pay just before EDGAR, or it may reflect general equilibrium effects, both of which would lead us to underestimate the effect of EDGAR on treated firms.

B. Compensation Distribution for CEOs and Other Executives

So far, we have documented that the implementation of modern information dissemination technologies has a negative effect on the conditional mean of total CEO pay. One potential drawback of this approach, however, is that it assumes a homogenous conditional distribution, while the effect of EDGAR introduction may be heterogenous. This is particularly relevant in our setting, as better access to disclosure may provoke public shaming of the highest-paid CEOs, but not necessarily of the lowest-paid CEOs.

We therefore next use quantile regressions to investigate heterogeneity in CEO pay-disclosure relation. The additional benefit of quantile regressions is their robustness to the presence of outliers in the sample, the importance of which has been highlighted by prior compensation research (Guthrie, Sokolowsky, and Wan (2012)). In particular, we expect CEO pay in the upper tail of the compensation distribution to be more negatively affected by better access to disclosure than CEO pay in the lower tail.

The results reported in Table 4 reveal that the introduction of EDGAR results in approximately 15.0% lower median CEO pay relative to that in control firms, a magnitude similar to those documented in Table 3. Interestingly, the negative effect of EDGAR introduction on the pay of CEOs in the bottom decile of the compensation distribution is smaller, at 9.7%. In contrast, we find a larger negative effect of EDGAR introduction for CEOs in the top decile of the compensation distribution, with their pay dropping by 20.8% following EDGAR adoption. These results suggest that investors and other stakeholders respond differently to disclosure depending on the level of reported CEO pay. Overall, these findings are consistent with the narrative of public shaming of CEOs and potential media sensationalism in response to more information becoming available about CEOs' lavish compensation packages.

In addition to examining changes in CEO pay, we also examine changes in the pay of other firm executives. Table 5 reports the relevant results. We find that the introduction of EDGAR has a more subdued effect on the compensation of named executive officers

other than the CEO, with their pay decreasing by approximately 4.6% relative to that in control firms. These results may reflect the fact that lower-ranked executives typically lack the celebrity status of CEOs, making their compensation less of a focus for the media and the public. Moreover, lower-ranked executives are often paid considerably less than CEOs (with their average pay being approximately three times smaller), and their decisions are less visible to outsiders, resulting in less pressure from the media and the general public for firms to lower the pay of non-CEO executives following improved access to corporate disclosure. Similar to the findings for CEOs, we observe that the compensation of highly-paid executives grows less following EDGAR implementation compared to their less-paid counterparts. However, the difference is relatively small, ranging from -4.3% for executives in the bottom decile of the compensation distribution to -4.9% for executives in the top decile.

C. Components of CEO Pay

Having examined how the total CEO compensation changes post-EDGAR, we next turn our attention to CEO compensation mix. Specifically, we investigate how better access to corporate disclosure via EDGAR affects different components of CEO pay, such as salary, cash bonus, and equity pay, which we further split into stock and option pay.

Table 6 presents the results evaluating the effect of EDGAR introduction on components of CEO pay. Given that individual components of pay may equal zero and "log-like" transformations in regressions may produce difficult-to-interpret estimates in such cases (Cohn, Liu, and Wardlaw (2022), Chen and Roth (2024)), we employ Poisson regressions with fixed-effects (firm-CEO and year). The results indicate a modest effect of EDGAR introduction on CEO salary, with a decrease of 4.2% ($= \exp(-0.043) - 1$)), and no significant effect on CEO cash bonus, calculated as the sum of bonus, non-equity incentive pay, and perquisite pay. These findings could be attributed to overall wage stickiness, the lower monetary value of cash incentive pay and its formulaic nature, and the lesser media attention to these types of pay (Kuhnen and Niessen (2012)).

In contrast, we find an economically and statistically significant effect of EDGAR on CEO equity pay, which includes stock and option awards. For example, results in specification 3 show that CEO equity pay in treated firms is approximately 29.7%

$(= \exp(-0.352) - 1))$ lower relative to that in control firms after EDGAR introduction. This evidence is consistent with the notion that the public and the media tend to focus on large payouts from incentive awards, such as proceeds from stock option exercises (Core, Guay, and Larcker (2008), Kuhnen and Niessen (2012)), and that easier access to disclosure makes it easier for the media, firm stakeholders, or the general public to observe and publicly criticize such awards.

Indeed, when we split equity pay into stock and option pay in the last two specifications,[18] we find that option pay is approximately 22.8% lower in treated firms relative to control firms following the EDGAR implementation. Overall, these results suggest that the introduction of EDGAR has a large negative effect on equity-based incentive pay of CEOs, a smaller negative effect on CEO salary, and no effect on other components of pay, implying a change in CEO compensation mix. These results bring to the fore concerns about a potential negative effect of EDGAR on CEO compensation incentives, which in turn can affect firm policies and value.[19]

One potential concern is that the changes in the value of stock options could arise not only from changes in option-granting practices by the board. Specifically, the value of stock options was not directly provided in proxy statements. Instead, it had to be calculated from the disclosures and other capital markets data, such as stock price volatility, by the proxy statement readers using some option pricing model. Arguably, not all proxy readers were able to perform these calculations. Another issue is that the value of stock options depends on stock return volatility and stock price levels, and to the extent that these quantities are affected by EDGAR, the value of stock options might change as a result. These effects could further be confounded by the board's practice of rolling over the same number of option grants year-to-year (Shue and Townsend (2017a)).

To understand whether the changes in CEO pay are driven by active board decisions, in Panel B, we investigate the extensive margin, i.e., the likelihood of receiving a particu-

[18] Stock pay was not widely used by firms during the sample period, so the results should be interpreted with caution. Approximately 12.9% of firms used this form of compensation, and it accounted for 2.9% of total pay.

[19] Mehran (1995) finds that firm performance is positively related to the percentage of managers' compensation that is equity-based. Most studies find a positive relation between compensation incentives and managerial risk-taking (see, e.g., Guay (1999), Coles, Daniel, and Naveen (2006), Chava and Purnanandam (2010), Armstrong and Vashishtha (2012), Liu and Mauer (2011), Gormley, Matsa, and Milbourn (2013), and Shue and Townsend (2017b)). In contrast, Hayes, Lemmon, and Qiu (2012) find little evidence that the decline in stock options after the adoption of FAS 123R has been accompanied by a decline in firm risk-taking.

lar type of pay.[20] The coefficient on *EDGAR* is insignificant for cash bonuses, suggesting that CEOs are neither more nor less likely to receive bonuses post-EDGAR compared to control firms. In contrast, CEOs are approximately 13.6% less likely to receive equity-based awards, specifically stock options.[21] This difference in the likelihood of option grants alleviates concerns regarding manipulation of option value calculation or changes in value due to other factors, such as volatility.[22] In fact, we find that the value of option grants, conditional on a grant, does not change significantly post-EDGAR, suggesting that our results are largely driven by changes at the extensive margin–specifically, that treated firms are less likely to make new option grants post-EDGAR relative to control firms.[23] Given these findings, a related concern is that option grants can be volatile because of vesting periods. For example, if options are granted for the first time, or if the board accelerates new awards shortly before EDGAR adoption, it is possible that firms are less likely to grant more options until the vesting period for prior grants has lapsed (e.g., for three years). However, it is unlikely that option plans would align exactly with the EDGAR adoption timeline, or that boards would act so quickly, especially considering that the EDGAR announcement came on February 23, 1993, just two months before the first adoption wave on April 26, 1993. Furthermore, we find similar results to those in Tables 3 and 6 when we exclude firms that granted options for the first time in 1992.

Finally, to directly examine how the mix of CEO compensation changes after EDGAR implementation, in Panel C we present the results of OLS regressions, where we normalize all dependent variables from Panel A (individual pay components) by total CEO pay. The results indicate that, as a proportion of total pay, the significance of fixed salary

[20] We omit salary from this panel since over 99.8% of CEOs in our sample receive non-zero salary.

[21] A natural question is when during the year the board makes option grants decisions, and whether it has sufficient time to cut back option awards to avoid CEO pay controversy. To investigate whether option grants are made only at the beginning of the year or are distributed throughout the year, we use data from Thomson Reuters in 1996 (the first year when Thomson Reuters discloses grant dates for a large sample of firms). We find that, while in recent years option grants are more common in the first part of the year, in the late 1990s, option grant dates for CEOs often occurred in the second half of the year, with approximately 57% of grant dates in 1996 falling in the last two quarters of the year.

[22] In Section IV.G, we show that our results are robust to excluding firms later revealed to backdate options. In addition, we verify that the board's propensity to roll over the same number of option grants does not change in a significant way following EDGAR adoption.

[23] Overall, the use of stock options increased over our sample period. Over the three years post-EDGAR, the likelihood of a CEO stock option award increased 18% in treated firms, compared with more than 30% in control firms. In a more detailed analysis (Table IA.3 of the Internet Appendix), we find that the effect is particularly pronounced for firms that had not granted options before EDGAR: treated firms in this group became more conservative in granting options afterward, relative to comparable control firms.

and cash bonuses increases, while that of equity pay, particularly option pay, decreases. Overall, the weaker CEO incentives associated with less equity-based pay following improved access to disclosure align with arguments by Murphy and Jensen (2018) that disclosure can increase the politicization of CEO pay and may prompt concerns about a potential negative effect on firm value. We further explore this aspect later in the paper.

D. Mechanisms: Unions, Media, and Public Aversion to High CEO Pay

We now delve into potential mechanisms underlying the slower growth in CEO pay post-EDGAR. Generally, large institutional investors may have had access to corporate filings prior to EDGAR through their direct engagement with firms or proxy mailings, potentially making them less affected by EDGAR's implementation. Conversely, firm stakeholders, such as employees and unions, and the general public may be more affected. Some investors and stakeholders could acquire information not directly from online filings, but rather from the media, which uses these filings. Additionally, media coverage could directly influence the decisions of boards that prefer to avoid controversy regarding CEO pay (Edmans, Gosling, and Jenter (2023)). Therefore, we next investigate how EDGAR implementation changes media information production regarding executive pay.

Presumably, it is easier for journalists to obtain detailed firm-specific information from the free electronic filings rather than from hard copies available only in three reference rooms in the country. Even if a particular media outlet had a subscription to a commercial data provider prior to EDGAR, the variable costs of accessing information, such as per-page charges, were non-trivial. Given lower information acquisition costs following EDGAR adoption, it is plausible that the media generates more articles on executive compensation, and that these articles help to disseminate information about executive pay. To ascertain whether the introduction of EDGAR prompts greater media scrutiny, we conduct a DiD analysis, focusing in particular on the extent and tone of coverage of executive compensation in major news outlets.

We find that there is greater media coverage of executive compensation for firms joining EDGAR. Specifically, Table 7 shows an increase in coverage of approximately 32.2% (=0.787/2.441) for firms covered by EDGAR. Given that media coverage, on average, tends to be negative (see, e.g., Panel D of Table 2), this evidence suggests

that groups such as labor unions, consumers, or employees likely have more negative information available to them about executive pay. In addition, we find that, conditional on coverage, the published articles tend to have a more negative tone after EDGAR.

In addition to examining media coverage on an annual basis, we also analyze coverage during the three-month period surrounding proxy filings, which is particularly relevant for executive pay. On average, proxy statements are filed 85 days following the fiscal year-end, with an interquartile range of 76 to 99 days. Therefore, we focus on months 3 to 5 (e.g., March through May for December fiscal year-ends). We find that media coverage of executive compensation around proxy filing dates increases by approximately 55.7% (=0.443/0.796) relative to its mean following EDGAR adoption, and the average article length increases by approximately 47.2% (=0.111/0.235). The coefficient on EDGAR for the number of negative articles is positive but statistically insignificant during this period. Overall, the results suggest that the introduction of EDGAR facilitates media coverage of executive compensation.

Besides the media, pressure on boards to lower CEO pay can come from labor unions and employees, the general public, and powerful shareholders. We explore these possibilities next, along with the media channel. Specifically, in Table 8, we regress total CEO pay on *EDGAR* and an interaction between *EDGAR* and particular firm characteristics. Because firm characteristics vary at the firm level and because EDGAR introduction was conditional on firm size, we include firm fixed effects and year-by-firm size fixed effects in all cross-sectional tests in Table 8.

We begin by examining whether the effect of EDGAR on CEO pay is greater in firms with labor unions. It is plausible that employees may take greater interest in executive compensation following the introduction of EDGAR and may become disgruntled. For example, Boone, Starkweather, and White (2024) find that following the disclosure of high CEO-employee pay ratios, employees report greater dissatisfaction with their own pay packages, are more likely to disapprove of their CEOs, and are even less productive. Employee dissatisfaction with CEO pay can also result in significant costs for firms when employees are unionized. Huang, Jiang, Lie, and Que (2017) find evidence consistent with unions being able to affect CEO compensation. Further anecdotal evidence, such as negotiations between General Motors and the United Auto Workers in 1996 or Dow

Jones and the Independent Association of Publishers' Employees in 1993, suggests that unions may raise concerns about executive compensation during their negotiations with firms.[24] It is conceivable that easier access to filings via EDGAR provides unions with better argumentation for their claims. Therefore, we hypothesize that EDGAR adoption has a larger effect on CEO pay in firms with unionized labor.

To explore this possibility, we identify whether a firm has unionized labor by searching for keywords 'unionize,' 'collective bargaining,' and 'unionization' in the firm's first 10-K report available via EDGAR. Firms that use these keywords are classified as unionized. The results in specification 1 of Table 8 indicate that the effect of EDGAR on CEO pay is both statistically and economically greater for firms with labor unions, consistent with the conjecture that better access to information via EDGAR activates unions or that boards lower CEO pay out of concern that unions will target executive pay.

We next examine whether the pressure to lower CEO pay can emanate from the general public. To do so, we create a variable equal to one if a firm's headquarters are located in a state with a Democrat governor as of 1992. The idea is that Democrats generally favor redistributive policies (see, e.g., Ashok, Kuziemko, and Washington (2015)) and are more likely to view income inequality and excessive executive compensation as significant issues.[25] Therefore, firms located in left-leaning states are more likely to face pressure from the general public and local politicians. Consistent with our expectations, the results in specification 2 show that the effect of EDGAR on CEO pay is more pronounced for firms located in states with Democrat governors, suggesting that the attitudes of the general public and local politicians may be important inputs for board decisions regarding CEO pay.

We next study whether greater media scrutiny is one of the mechanisms for lower CEO pay following the introduction of EDGAR. Prior literature on the topic finds that

[24] In the case of Dow Jones, the union aimed to limit CEO pay to no more than 20 times the average Dow Jones salary. Similarly, with GM, the union sought contract language to tie executive compensation increases to those of hourly workers.

[25] According to a survey by Pew Research Center, 78% of Democrats believe there is too much economic inequality in the United States, compared to 41% of Republicans. Anecdotally, Democrats are also more likely to introduce regulations targeting executive compensation. For example, as recently as January 2024, several Democrats (Elizabeth Warren, Ed Markey, Chris Van Hollen, Barbara Lee, Rashida Tlaib) and Bernie Sanders introduced the "Tax Excessive CEO Pay Act," which would raise taxes on firms that pay their top executives at least 50 times more than to a typical worker. Another example is legislation passed in Portland, Oregon (LIC-5.02) that imposes a surtax of 10% on publicly traded companies if their CEO pay ratio is at least 100:1.

media negativity has no effect on total CEO pay (Core, Guay, and Larcker (2008)), but national media negativity lowers the use of stock options by firms (Kuhnen and Niessen (2012)). To examine whether increased media scrutiny is a catalyst that prompts boards to reexamine CEO pay practices, we focus on the cross-sectional heterogeneity in the effect of EDGAR implementation on CEO pay for firms that were already covered by the media prior to the introduction of EDGAR and those that were not. The idea is that for firms with prior coverage, the effect of the increased media scrutiny following the introduction of EDGAR is expected to have a smaller effect on CEO pay.[26] The coefficient on the interaction of no prior compensation coverage and *EDGAR* in specification 3 of Table 8 is negative, but is significant only at the 10% level. Therefore, our results provide only weak support for the media channel.[27]

Finally, to understand whether CEO pay changes following EDGAR adoption are initiated by large institutional investors, in specification 4 we interact *EDGAR* with institutional ownership. If anything, the results are more pronounced for firms with low institutional ownership and, overall, do not support the view that large institutional investors are behind curbing CEO pay following EDGAR adoption. Because the decisions of large institutions are typically more informed than those of retail investors, these results suggest the possibility of politicization of executive pay.

In sum, while our results do not pinpoint an exact mechanism for lower CEO pay, they indicate that the effects are more pronounced in firms with labor unions and those in left-leaning states. These findings suggest that better access to disclosure may have prompted labor unions and the general public to take greater interest in executive compensation and put pressure on boards to lower CEO pay. Of course, this evidence is only suggestive, as we cannot rule out the possibility that firm location or the presence of labor unions may proxy for other firm characteristics unrelated to stakeholder pressure. Given that stakeholders may not share the same preferences as shareholders, it is interesting to

[26] For example, some firms may have been covered because their location was close to the SEC's reference rooms, and journalists tended to cover firms in which local investors took interest (see e.g., Coval and Moskowitz (1999), Grinblatt and Keloharju (2001), and Huberman (2001) on home bias). Other firms may have been covered because of their high public visibility, so journalists were willing to invest in obtaining the SEC filings despite the high cost. For example, a Wall Street journalist conveyed to us that, prior to EDGAR, some newspapers bought a small stake in firms they deemed important, so they would receive their proxy mailings as shareholders.

[27] Our results are not at odds with those of Core, Guay, and Larcker (2008), who find no effect in a regression of residual CEO pay on media negativity. In contrast to them, we examine within-firm changes in CEO pay and focus on a shock to media coverage resulting from easier access to filings.

examine the implications of lower pay for CEO turnover, which we do next.

E. CEO Turnover

We next examine whether access to disclosure via EDGAR has any impact on CEO turnover. There are several potential reasons to expect a relation. First, Hermalin and Weisbach (2012) argue that if firms cannot easily increase executive compensation (and better access to disclosure could make it less politically feasible), this can lead to increases in CEO turnover rates. Second, the introduction of EDGAR can reduce information asymmetry in the labor market for CEOs, making it easier for them to observe how much other firms are paying their CEOs and to search for comparable jobs. For example, better access to corporate disclosure by other firms can enable CEOs to better evaluate the strategies pursued by those firms and determine whether their talents are a good fit for such positions. Third, because the introduction of EDGAR lowers the growth in CEO pay, with particularly significant effects for highly-paid CEOs, it is possible that some of these disgruntled executives may decide to move to other public firms to increase their pay, transition to private firms to enjoy relatively lax disclosure requirements, or even opt for early retirement.[28] Finally, better access to disclosure could motivate investors to exert pressure on boards to replace their CEOs whenever firm performance is subpar, thereby potentially affecting forced CEO turnover.

Table 9 reports the results of OLS regressions, where the dependent variables are total CEO turnover, forced CEO turnover, and voluntary CEO turnover. Specification 1 shows the effect of EDGAR introduction on total CEO turnover. The coefficient on EDGAR is positive and both statistically and economically significant. In particular, the likelihood of CEO turnover increases by 2.5 percentage points, compared to the average CEO turnover of 3.5 percentage points. While the effect is large, it is similar to a city manager turnover increase of 75% upon their pay disclosure, documented by Mas (2017).

To investigate why CEOs are more likely to depart following the introduction of EDGAR, we further split CEO turnover into two categories: forced turnover and volun-

[28] In Table IA.4 of the Internet Appendix, we show that firms that pay more to their CEOs prior to the introduction of EDGAR experience a more negative reaction to the SEC's announcement regarding EDGAR introduction on February 23, 1993. This evidence may indicate that stock market participants anticipated potential negative effects of EDGAR introduction for firm with highly-paid executives.

tary turnover. The identification of forced turnover follows Peters and Wagner (2014) and Jenter and Kanaan (2015). Interestingly, we find that the implementation of EDGAR has no effect on forced CEO turnover (specification 2). These results do not support the conjecture that better access to disclosure allows investors to better evaluate firm performance and push compliant boards to more readily oust underperforming CEOs.[29] In contrast, results in specification 3 demonstrate that the introduction of EDGAR has a positive and statistically significant effect on voluntary CEO turnover, which increases by 2.4 percentage points after EDGAR implementation. The fact that the increase in CEO turnover is concentrated in voluntary turnover is consistent with the hypothesis that CEOs facing lower pay following the introduction of EDGAR are more likely to change jobs or retire. It is also possible that, beyond dissatisfaction with reduced pay, some CEOs found the increased scrutiny or publicity associated with EDGAR implementation unpleasant, which could have led to additional turnover among "high-quality" CEOs. Overall, our results suggest that the implementation of EDGAR may have had an unintended effect on voluntary CEO turnover.

F. Firm Value Implications

Weaker CEO incentives in compensation contracts and higher voluntary CEO turnover are generally unlikely to benefit firm shareholders. For example, prior research finds that higher compensation incentives that come with stock options encourage prudent risk-taking by managers, positively contributing to shareholder value creation (see, e.g., Coles, Daniel, and Naveen (2006), Low (2009)). Higher CEO turnover could also be suboptimal if CEOs have accumulated significant firm-specific human capital and cannot be easily replaced by outsiders or if they can harm the firm by working for its competitors. On the other hand, higher CEO turnover could improve the quality of firm-CEO matches, and lower incentives could benefit firms if there are concerns about manipulation or managerial short-termism (see, e.g., Yermack (1997), Heron and Lie (2007), Bergstresser and Philippon (2006), Burns and Kedia (2006), Peng and Röell (2008), Edmans, Goncalves-Pinto, Groen-Xu, and Wang (2018)).

[29] In Table IA.5 of the Internet Appendix, we also find that CEO turnover does not become more sensitive to firm performance following the introduction of EDGAR, and that the increase in voluntary CEO turnover is more pronounced among CEOs who were highly paid prior to EDGAR.

Because cleanly identifying the effect of changes in compensation incentives on firm value is challenging (see, for example, the survey by Edmans, Gabaix, and Jenter (2017)), and because EDGAR implementation could potentially influence firm value through other channels (see, e.g., Goldstein, Yang, and Zuo (2023), Chang, Ljungqvist, and Tseng (2023), Gomez (2024)),[30] we do not directly examine firm value outcomes here. Instead, we focus on the stock market reaction to announcements of CEO departures as a way to gauge whether additional CEO turnover is detrimental to firm value.

We obtain the dates of CEO turnover from two sources. For forced turnovers, we use the announcement dates compiled by Peters and Wagner (2014) starting in 1993. For voluntary turnovers and forced turnovers prior to 1993, we search for news about chief executive departures on Nexis Uni. In addition to the announcement dates, we gather information on CEO age, whether the departing CEO is a founder, whether the CEO is leaving the firm for another job or intends to pursue other business opportunities, whether the turnover is due to the CEO's death, and whether there are any confounding news events, such as dividend increases or earnings reports on the same day.[31] For each announcement date, we calculate the cumulative abnormal returns in the (-1,0) or (-1,1) windows using the Fama-French three-factor model plus the momentum factor.[32] The summary statistics for this sample are presented in Panel A of Table 10.

Out of 503 CEO turnovers in the sample, 32.0% are classified as forced. In 8.3% of cases, the departing CEO is a founder, and in 1.4% of cases, the cause of CEO turnover is the CEO's death.[33] The average age of the departing CEO is 59, and in 2.2% of cases, the incoming and departing CEOs have the same last name, suggesting family ties. Additionally, 4.4% of CEO turnover announcements mention that the CEO is planning to pursue other business opportunities or is moving to another firm. Finally, the mean and median announcement returns around the dates of CEO turnover are close to zero.

Specifications 1 and 2 in Panel B of Table 10 report the results for CARs around

[30] In particular, Goldstein, Yang, and Zuo (2023) find a decrease in the cost of equity capital and an increase in the level of equity financing following EDGAR implementation, and Goldstein, Yang, and Zuo (2023), Chang, Ljungqvist, and Tseng (2023), and Gomez (2024) find an increase in a firm's stock liquidity.

[31] While we exclude events accompanied by confounding news, our results remain similar if we do not.

[32] We use 100 trading days (with a minimum of 30 valid observations) to estimate the model and stop the estimation 50 days before the event.

[33] In line with existing literature and with a slight abuse of terminology, we classify all turnovers that are not forced as voluntary, including those due to a CEO's death.

the dates of total CEO turnover as the dependent variables, while specifications 3 and 4 report the results for CARs around the dates of voluntary CEO turnover.[34] In general, the stock market reacts more negatively when the incoming CEO has the same last name as the departing CEO. Specifications 1 and 3 show a more positive reaction when a CEO dies. Importantly, we also find larger negative stock market reactions to both voluntary and total CEO turnover following EDGAR implementation. This suggests that CEO departures tend to be more detrimental to firm value following expanded access to corporate disclosure.[35]

Overall, our results suggest that while better access to disclosure can curb CEO pay, there may be negative consequences for shareholders. These results are in line with the arguments by Murphy and Jensen (2018) and Hermalin and Weisbach (2012) that changes in CEO pay may sometimes be politically motivated and that CEO turnover can increase in such cases. They are also related to the survey evidence by Edmans, Gosling, and Jenter (2023), who find that boards may be willing to sacrifice shareholder value in order to avoid controversy on CEO pay.

G. Robustness Tests

In this section, we present robustness tests for our main analysis, including: (1) using alternative stacked, staggered, and propensity score-matched samples; (2) controlling for potential differential pay trends among firms of different sizes, asset tangibility, or leverage; (3) addressing the effects of the 1992 compensation disclosure rules and Section 162(m) of the Internal Revenue Code, which capped the tax deductibility of non-performance-based executive pay at $1 million; (4) excluding early SEC filers, firms in the CF-01 wave, or those affected by early online access to EDGAR filings; (5) excluding firms known to backdate stock options; (6) expanding the sample using David Yermack's compensation data; (7) controlling for early corporate adoption of the World Wide Web (WWW); and (8) addressing potential confounding effects related to stock price infor-

[34] Because CEO turnover is a relatively rare event, we use pooled OLS regressions instead of a stacked DiD.

[35] Anecdotal evidence suggests that investors may be unhappy about CEO turnover even if the media covers these CEOs negatively. For instance, Drew Lewis, the CEO of Union Pacific, had to repeatedly defend his 'exorbitant' pay following the introduction of EDGAR and was covered negatively by the media for taking a leave of absence for alcoholism treatment. Nevertheless, when Union Pacific announced Drew Lewis's plans to step down on November 6, 1996, investors expressed disappointment, with a three-day abnormal return of approximately -1.8% around that date.

mativeness and liquidity. Below, we provide a brief discussion of these additional tests.

G.1. Staggered Sample and Alternative Stacked Samples

Recent econometrics literature shows that, under some conditions, it may be difficult to interpret the coefficients in regular two-way fixed effect (TWFE) regressions with staggered treatment (see, e.g., de Chaisemartin and d'Haultfoeuille (2020), Callaway and Sant'Anna (2021), Baker, Larcker, and Wang (2022)). Nonetheless, for the sake of completeness, we present the results of TWFE regressions in Panel A of Table B.1, using the entire dataset spanning from 1991 to 1999 and employing the same fixed effects as in Table 3. These results indicate that the total CEO pay in treated firms is 7.1% to 10.0% lower following EDGAR introduction compared to that in control firms.

We also report on the tests using the stacked sample but without any control variables, as recommended by Baker, Larcker, and Wang (2022), along with tests using two alternative stacked samples. Panel B presents the results of the robustness tests for our main findings without any covariates. The results indicate that total CEO pay in treated firms is 18.2% to 19.5% lower following the introduction of EDGAR compared to control firms, with all coefficients being statistically significant at the 1% level. Panels C and D display the results of the robustness tests using the first seven stacks and requiring a gap of at least one year between phase-in dates, or using the first four stacks and requiring a gap of at least two years. Panel E shows results where the first four EDGAR waves are combined into a single stack. Across all these robustness tests, total CEO pay in treated firms remains 10.8% to 17.2% lower than in control firms post-EDGAR.

G.2. Matched Sample

As highlighted by Chang, Hsiao, Ljungqvist, and Tseng (2022), the assignment of firms to EDGAR waves was random, conditional on firm size. Relatedly, Figure 1 shows that the average CEO pay is higher for firms that join EDGAR in the first two implementation waves, which is likely a result of larger firm size. Our results are robust to excluding firms that implement EDGAR in the first two waves. Nevertheless, we also use an alternative approach to address the issue of firm size imbalance by constructing a propensity score matched (PSM) sample following Chang, Hsiao, Ljungqvist, and Tseng (2022).

Specifically, to have a clean control group, we require the matched control firms not to experience the shock of EDGAR introduction. This restricts the test sample to end in 1995 because after the last wave in 1996 all firms have been treated. In this setting, the treated firms are those experiencing the EDGAR requirement in 1993, and control firms are those not experiencing the EDGAR introduction any time before 1996. Then the sample period is 1991 to 1995. We match treated firms to control firms in 1992, the year before the EDGAR requirement, based on firm size (either total assets or market capitalization) and industry defined by Fama-French 12 industry classification.

Appendix Table B.2 reports the relevant results. Panel A presents statistics on the match quality prior to EDGAR introduction and shows, in particular, that there are no significant differences in total assets or market capitalization (natural logarithms) of treated firms and matched control firms. Panel B presents the corresponding stacked DiD results using the PSM sample. Specifications 1-2 report the results based on matching on the Fama-French 12 industry and the logarithm of market capitalization, whereas specifications 3-4 report the results based on the Fama-French 12 industry and the log of total assets. The estimated coefficients on *EDGAR* range from -8.1% to -15.5%, consistent with our baseline results reported in Table 3. Therefore, our results are robust to using a matched sample.

G.3. Controlling for Potential Heterogeneous Trends Using Relevant Fixed Effects

To the extent that the assignment of firms to EDGAR waves was related to firm characteristics, concerns may arise about possible heterogeneous compensation trends for firms with different characteristics following the implementation of EDGAR. For instance, CEO pay in large and small firms could evolve differently during our sample period. While our results using PSM matching already help alleviate this concern, we additionally include relevant fixed effects based on year-characteristic combinations. Specifically, we consider the following firm characteristics: firm size (market capitalization or total book value of assets), leverage, and asset tangibility. Firms are classified into quartiles based on the relevant characteristics in the year before EDGAR implementation. The corresponding fixed effects are the year-by-quartile fixed effects. We use the stacked DiD estimator, and the sample comprises four stacks corresponding to the first four

EDGAR implementation waves, as in our main tests. Table IA.6 reports the results, which demonstrate that the total CEO pay in treated firms is 10.0% to 14.6% lower relative to that in control firms after EDGAR, similar to results in Table 3.

G.4. The 1992 Disclosure Rules, Section 162(m), and Option Backdating

Another potential concern in identifying a relation between EDGAR and CEO pay is the presence of confounding events during the same period. In particular, two regulatory changes in 1992 and 1993 could have influenced CEO compensation. First, in 1993, Congress revised the Internal Revenue Code as part of the Omnibus Budget Reconciliation Act, introducing Section 162(m), which limited the tax deductibility of non-performance-based CEO compensation to one million dollars. Second, in February of 1992, the SEC announced and later in the year adopted new executive compensation disclosure rules (see Lo (2003) for the chronology of events). Under the new rules, the firm had to disclose its stock returns compared to an index, present a summary compensation table for the highest paid executives, list executive stock options, stock appreciation rights, and long-term incentive awards, and present a compensation committee report disclosing committee member names and affiliations and highlighting the criteria used in the compensation decisions.

Notably, both the 1993 Section 162(m) of the IRS Code and the 1992 compensation disclosure rules applied to all public firms, while our identification relies on the staggered implementation of EDGAR, with 10 different groups of firms joining the system at different points in time between April 1993 and May 1996. Further, Rose and Wolfram (2002) and Perry and Zenner (2001) find little evidence that the 1992-1993 regulations had a significant effect on total CEO compensation. Nevertheless, to further allay concerns related to these two regulations, we run additional robustness tests. The results of these tests are reported in Appendix Table B.3.

Specifically, to mitigate the concerns related to the 1993 tax deductibility cap on managerial compensation not considered performance-based, we do the following. First, we exclude firms where CEO salary exceeded \$0.9 million in 1992,[36] as these firms may

[36] Following Rose and Wolfram (2002), we use \$0.9 million as a cutoff instead of \$1 million to account for other compensation that is not performance-based. Our results are very similar if we instead exclude firms where CEO salary exceeded \$1 million in 1992.

have been directly affected by the cap. Specifications 1-2 in Panel A show that our main findings remain robust. Second, we exclude firms where the sum of CEO salary and bonus exceeded $0.9 million in 1992. Specifications 3-4 again show consistent results. Finally, instead of excluding firms, we incorporate more granular fixed effects. Specifically, we define an indicator variable, *Salary Below $0.9M*, equal to one if a CEO's salary in 1992 exceeded $0.9 million, and zero otherwise. We then include *Salary Below $0.9M* interacted with year fixed effects (specifications 5-6), and the results remain robust.

To address concerns related to the adoption of the 1992 disclosure rules, we conduct tests for CEO pay using the sample post-1992 (i.e., excluding 1991 and 1992), when the new disclosure rules were already in place and are therefore unlikely to contaminate our findings. However, one disadvantage of this setting is that we cannot use the stacked sample (as there would be no pre-treatment period for treated firms). Therefore, we employ the staggered DiD estimator, and note that these results need to be interpreted with caution. Specifications 1 and 2 in Panel B report these results. The coefficients on EDGAR in both specifications are negative, and the magnitudes are similar to the ones reported in Panel A of Table B.1, but only the coefficient in specification with firm and year fixed-effects is statistically significant.

Another concern may be related to option backdating, specifically, to the potential differential propensity of firms that join EDGAR in different waves to backdate. To address this concern, we exclude firms that were later revealed to backdate options and use a DiD estimation on the stacked sample, as in our main tests.[37] Specifications 3-4 in Panel B of Table B.3 report the results, which show that the total CEO pay in treated firms is about 14% lower following the introduction of EDGAR compared to that in control firms, and both coefficients are statistically significant at the 1% level.

G.5. Early EDGAR Filers, Transitional Filers, and the Online Access to EDGAR

We also run the robustness tests related to the timing of SEC EDGAR implementation. Prior to the adoption of the EDGAR system, the SEC ran a small pilot program, whereby it allowed volunteers to submit their filings electronically. These voluntary filers were then assigned to wave CF-01 in the phase-in schedule, along with some other firms, and

[37]The list of firms that were later revealed to backdate their stock options is available at https://www.wsj.com/public/resources/documents/info-optionsscore06-full.html.

were referred to as "transitional filers."[38] To address concerns that some firms voluntarily self-selected to file early and that some firms appeared in wave CF-01 because of their choice to participate in a pilot program, we therefore run robustness tests excluding early electronic filers (specifications 1-2 in Panel C of Table B.3) or excluding all firms assigned to wave CF-01 (specifications 3-4 in Panel C of Table B.3). The relevant results show that our CEO pay results are not sensitive to removing these firms from the sample.

In 1993, the National Science Foundation (NSF) decided to fund a project to make EDGAR filings available for free online, with all electronic EDGAR filings becoming available online starting January 17, 1994. We thus run robustness tests excluding firms in the first wave and assuming that for waves CF-02, CF-03, and CF-04 the phase-in-date is January 17, 1994 (when all electronic EDGAR filings became freely available online). The EDGAR indicator in the stacked DiD analysis is redefined accordingly. Specifications 5 and 6 report the related results and show that our findings are robust.

G.6. Extending Sample Using David Yermack's Compensation Data

We also run robustness tests on an extended sample, which we construct using the compensation data shared with us by David Yermack, who collected it for firms appearing in Forbes magazine's ranking of the 500 largest U.S. corporations. Specifically, we use the staggered DiD estimator for 1990-1999, the stacked DiD estimator for 1990-1995, and a DiD estimator of de Chaisemartin and d'Haultfoeuille (2020) for 1990-1999. The results are reported in Table B.4 and show that the estimated magnitude of EDGAR adoption on CEO pay ranges from -8.0% to -14.3% in these extended samples, with all estimates being statistically significant at the 1% or 5% levels.

G.7. Early Corporate Adoption of the World Wide Web

While our focus is on the expanded access to information via EDGAR, a potential confounding factor is that, during this period, some firms began adopting the World Wide Web and creating publicly accessible corporate webpages. In particular, although the first proposal for the development of the WWW was introduced by CERN scientist Tim Berners-Lee in 1989, CERN released the software to the public in 1993, fostering WWW

[38] More details on transitional filers are available in Section 3.3.5 of Gao and Huang (2020).

adoption. To address the concern that information could become available through corporate websites, we do the following. First, we remove from the sample any firm-years where 10-Ks contain a reference to 'www.' Second, we identify the name of the corporate website from Compustat and look up the date of the original website registration using www.whois.com. We then keep in the sample only those firms whose websites were registered after the last phase-in date for EDGAR (May 6, 1996). The results in Table IA.7 of the Internet Appendix look very similar to those in Table 3, suggesting that the rollout of corporate webpages is not a significant confounding factor in our setting.

G.8. Alternative Channels: Stock Liquidity and Stock Price Informativeness

One potential limitation of our setting is that the introduction of EDGAR improved public access to all types of financial disclosures, which could affect firms on multiple levels. For example, Goldstein, Yang, and Zuo (2023) and Gomez (2024) find increased stock liquidity after EDGAR adoption, and Gao and Huang (2020) note enhancements in information production by traders and analysts, suggesting changes in stock price informativeness. Of particular relevance to our study is the possibility that CEO compensation may be affected not only by easier access to corporate disclosures, but also by EDGAR-induced changes in stock price informativeness or liquidity.

From a theoretical perspective, it is unclear how changes in stock price informativeness should affect compensation contracts. Classical findings from Holmström (1979) and Lambert and Larcker (1987) suggest that if stock prices become more informative about managerial effort following EDGAR, boards should increase reliance on stock-based compensation. Yet, Chaigneau, Edmans, and Gottlieb (2018) offer more nuanced theoretical insights, indicating that the impact of improved information on managerial incentives may depend on the moneyness of CEO compensation contracts. Empirically, Jayaraman and Milbourn (2012) find that as stock liquidity goes up, boards increase the proportion of equity-based compensation in total compensation. However, our base results reveal the opposite trend, with incentive-based pay decreasing after EDGAR and related improvements in liquidity, mitigating concerns that CEO pay changes are attributed to stock price informativeness or liquidity.

To further explore whether changes in stock price informativeness or liquidity can ex-

plain CEO compensation adjustments post-EDGAR implementation, we split the sample based on pre-EDGAR bid-ask spread and a measure of stock price informativeness used by Durnev, Morck, and Yeung (2004) and Fresard (2012). Results in Table IA.8 of the Internet Appendix indicate a slower growth in CEO pay following EDGAR across samples with both low and high liquidity or informativeness, with slightly larger magnitudes observed for more liquid stocks and those with high stock price informativeness.

V. Conclusion

The explosive growth of CEO pay in the United States has sparked controversy. In this paper, we examine whether better access to corporate disclosure, facilitated by the introduction of modern information technologies, helps curb CEO pay. Using a stacked difference-in-differences design around the implementation of the SEC EDGAR platform from 1993 to 1996, we find that CEO pay grows more slowly following EDGAR implementation. Consistent with the idea that the disclosure of lavish compensation packages provokes public shaming of CEOs, the effect is stronger for highly-paid CEOs. The effect is also stronger for firms with unions and those located in left-leaning states. Media coverage of executive compensation increases following EDGAR introduction, particularly around proxy filing dates. We also find that better access to corporate disclosure provided by EDGAR has a particularly large effect on equity-based incentive pay, supporting the idea that the public tends to disproportionately focus on large incentive-based awards.

Finally, our evidence suggests some negative implications of EDGAR adoption for firm value. First, we document that CEO turnover, particularly voluntary CEO turnover, increases significantly following EDGAR adoption. Second, we find that the stock market reacts more negatively to announcements of CEO turnover after EDGAR adoption, suggesting that such turnover is detrimental to firm value. These results indicate that although easier access to disclosure helps curb CEO pay, it may have negative consequences for shareholders. Overall, our evidence suggests that expanded access to disclosure leads to greater politicization of CEO pay, with groups such as labor unions, employees, the media, and the general public pressuring boards to curb CEO pay without necessarily considering the implications for firm shareholders.

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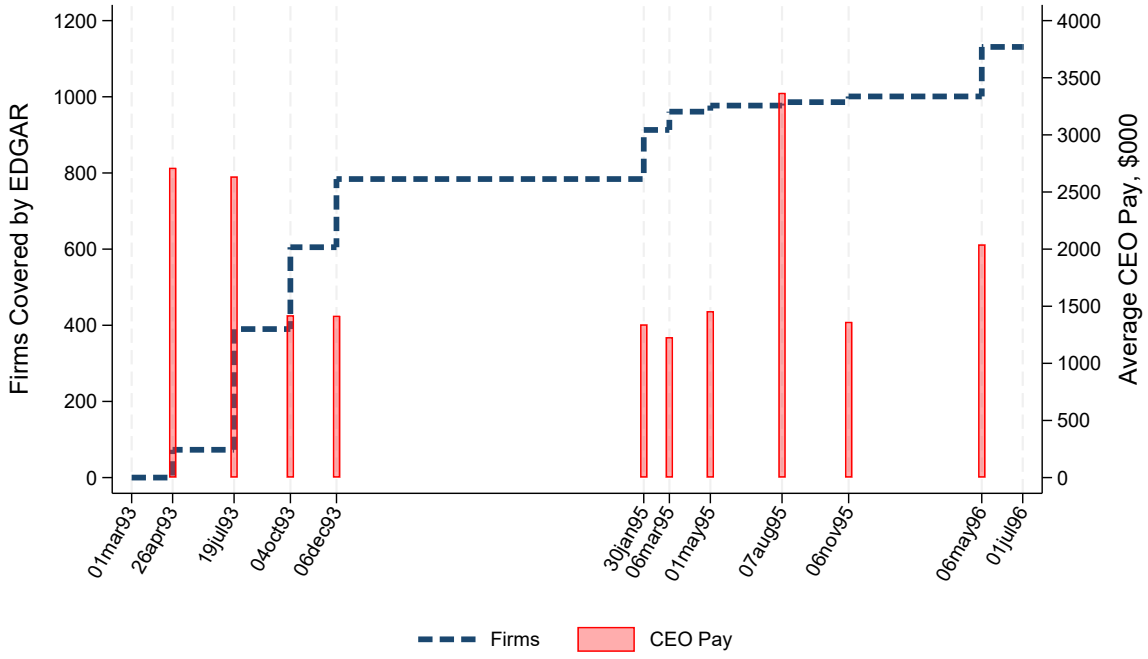

Figure 1. Firms Covered by EDGAR and Average CEO Pay in Joining Firms. The dashed blue line shows the number of firms in the sample covered by EDGAR overtime (see the left axis). The red solid bars show the average CEO pay (in $ thousands) in firms joining EDGAR as of the year of joining (see the right axis).

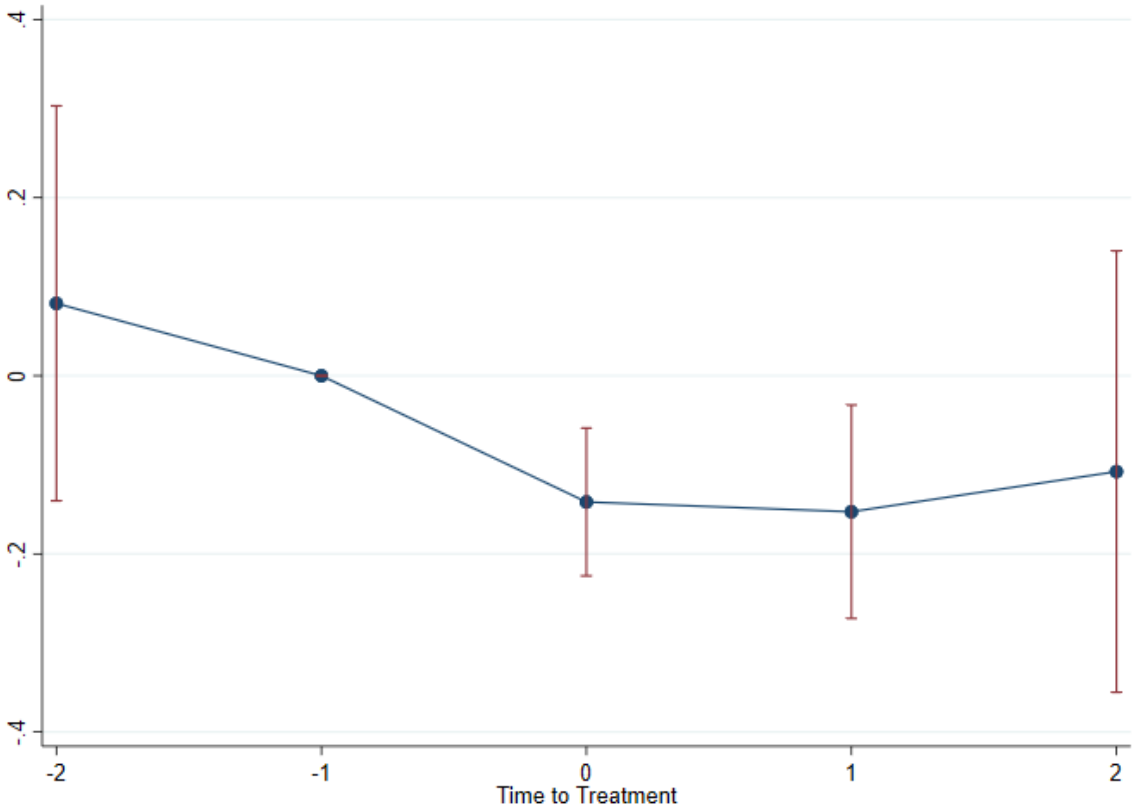

Figure 2. EDGAR Introduction and Total CEO Compensation. The figure shows the effect of mandatory EDGAR filing on CEO pay using DiD estimator of de Chaisemartin and d'Haultfoeuille (2020) with all the estimated treatment effects and placebos. The model includes firm and year fixed effects. Year -1 serves as the baseline year. The x-axis shows the time relative to the EDGAR requirement effective year. The error bars correspond to the 90% confidence intervals constructed using a normal approximation. The confidence intervals are based on standard errors clustered at the firm level.

Appendix A. Variable Definitions

Variable Name	*Description*
EDGAR	Equal to one if a firm is a mandatory EDGAR filer in a given year (i.e., its fiscal year ends after the corresponding phase-in date) and zero otherwise.
Total CEO Compensation	The natural log of total annual CEO pay (TDC1).
Total Executive Compensation (Non-CEOs)	The natural log of total annual executive pay (TDC1) for all executives with reported compensation, with the exception of the CEO.
Log(Assets)	The natural log of the book value of assets.
Debt/Assets	Short-term and long-term debt scaled by the book value of assets.
ROA	Net income scaled by total assets (calculated in the previous year).
PP&E/Assets	Net plant, property, and equipment scaled by the book value of assets.
Stock Return	The firm's annual stock return.
CEO Tenure	The natural log of the number of years the CEO worked at the firm.
Salary	The value of CEO salary, $000s.
Cash Bonus	The value of CEO nonequity incentive pay and bonus, $000s.
Equity Pay	The value of CEO stock and option pay, $000s.
Stock Pay	The value of CEO stock pay, $000s.
Option Pay	The value of CEO option pay, $000s.
%Salary	CEO salary normalized by total CEO compensation.
%Cash Bonus	The sum of CEO nonequity incentive pay, perquisite pay, and bonus divided by total CEO compensation.
%Equity Pay	The value of CEO stock and option pay divided by total CEO compensation.
%Stock Pay	The value of CEO stock pay divided by total CEO compensation.
%Option Pay	The value of CEO option pay divided by total CEO compensation.
Cash Bonus Indicator	An indicator equal to one if CEO receives nonequity incentive pay, perquisite pay, or bonus during a given year.
Equity Pay Indicator	An indicator equal to one if CEO receives stock or option pay during a given year.
Stock Pay Indicator	An indicator equal to one if CEO receives stock pay during a given year.
Option Pay Indicator	An indicator equal to one if CEO receives option pay during a given year.
Total CEO Turnover	Equal to one if a firm has a new CEO in a given year relative to the previous year; zero otherwise.
Forced CEO Turnover	Turnovers classified as forced by Peters and Wagner (2014) and Jenter and Kanaan (2015), zero otherwise.
Voluntary CEO Turnover	Turnovers not classified as forced by Peters and Wagner (2014) and Jenter and Kanaan (2015), zero otherwise.
Institutional Ownership	The number of shares held by institutions divided by the total number of firm outstanding shares.
Union	Equal to one if a firm has unionized labor, as identified by use of the following keywords in the firm's first 10-K report available via EDGAR: 'unionize,' 'collective bargaining,' and 'unionization'; zero otherwise.

Variable Name	*Description*
Comp. Articles	The total number of compensation articles appearing during the fiscal year or during the three-month period around the firm's proxy filing date (i.e., months 3-5 following the fiscal year-end). The compensation articles must appear in major newspapers or newswires and mention a given firm in a given year. The news sources are: *Wall Street Journal*, *New York Times*, *Washington Post*, *Los Angeles Times*, *Financial Times*, *USA Today*, *American Banker*, *Economist*, *PR Newswire*, *Business Wire*, *Star Tribune*, *Boston Globe*, *Pittsburg Post*, *Vancouver Sun*, *Newsday*, *Orlando Sentinel*, *St. Petersburg Times*, *Chicago Tribune*, *Forbes*, *Salt Lake Tribune*, *Hartford Courant*, *Bloomberg Businessweek*, *Fortune*, and *Guardian*. We search for the following phrases in the article summary or title: ceo* pay, executive* pay, ceo paid, executive paid, ceo* bonus, executive* bonus, ceo* compensat*, executive* compensat*, ceo* salar*, executive* salar*, ceo* option*, executive* option*, ceo and (million* or "000"), executive* and (million* or "000"), and pick articles with the subject of "executives," "chief executive officers," "executive compensation," or "stock options."
Negative Articles	The number of compensation articles per firm-year that are classified as negative, i.e., that contain more negative words from the financial dictionary by Loughran and McDonald (2011) than positive words.
Total Words	The total number of words (in 000s) in compensation articles appearing during the fiscal year or during the three-month period around the firm's proxy filing date (i.e., months 3-5 following the fiscal year-end).
Words/Article	The total number of words (in 000s) in compensation articles during the fiscal year or during the three-month period around the firm's proxy filing date (i.e., months 3-5 following the fiscal year-end), divided by the number of compensation articles.
No Articles Pre-EDGAR	Equal to one if there are no compensation articles written about a firm prior to EDGAR; zero otherwise.
Democrat Governor	Equal to one if a firm's headquarters are located in a state when the governor is a Democrat as of 1992; zero otherwise.
CEO Leaves for Another Job	An indicator variable equal to one if turnover is classified as voluntary and the news mention that CEO takes another job or leaves to pursue other opportunities.
CEO Age	The age of the departing CEO.
Founder CEO	An indicator equal to one if the news mention that CEO is one of the firm's founders.
CEO Death	An indicator equal to one if the cause of CEO turnover is CEO death (as reported in the news).
Family CEO Succession	An indicator equal to one if the incoming and departing CEOs have the same last name.

Table 1. EDGAR Phase-In Waves

This table presents the breakdown of SEC Phase-in dates for EDGAR implementation waves.

Wave	SEC Designation	Phase-In Date
1	CF-01	April 26, 1993
2	CF-02	July 19, 1993
3	CF-03	October 4, 1993
4	CF-04	December 6, 1993
5	CF-05	January 30, 1995
6	CF-06	March 6, 1995
7	CF-07	May 1, 1995
8	CF-08	August 7, 1995
9	CF-09	November 6, 1995
10	CF-10	May 6, 1996

Table 2. Summary Statistics

This table reports summary statistics for variables used in the analysis. Panel A reports the summary statistics for the full sample covering the period 1991–1999 that includes firms in Compustat with non-missing CEO pay and accounting data and present. Panel B reports the summary statistics for the stacked sample, which consists of four stacks corresponding to the first four EDGAR implementation waves. Not-yet-treated firms with phase-in dates at least one year later are used as control. Panel C reports the number of compensation articles in major newspapers and newswires by year and source. Panel D reports statistics for the length and tone of compensation articles. All continuous variables are winsorized at the 1st and 99th percentile values. Variable definitions are provided in Appendix A.

Panel A. Summary Statistics: Full Sample

Variable	Mean	SD	p25	p50	p75	N
EDGAR	0.753	0.431	1	1	1	8,786
Total CEO Compensation, $000s	2,624	3,572	758	1,445	2,848	8,786
Total Executive Compensation, $000s	908	1,176	295	521	1,022	40,652
Salary, $000s	537	272	340	493	688	8,786
Cash Bonus, $000s	725	1,107	128	355	815	8,786
Equity Pay, $000s	1,601	3,227	0	468	1,579	8,786
Stock Pay, $000s	154	555	0	0	0	8,786
Option Pay, $000s	1,417	2,990	0	375	1,362	8,786
%Salary	0.394	0.249	0.202	0.345	0.540	8,786
%Cash Bonus	0.284	0.209	0.114	0.261	0.417	8,786
%Equity Pay	0.402	0.377	0	0.348	0.647	8,786
%Stock Pay	0.042	0.110	0	0	0	8,786
%Option Pay	0.359	0.371	0	0.282	0.591	8,786
Cash Bonus Indicator	0.970	0.170	1	1	1	8,786
Equity Pay Indicator	0.724	0.447	0	1	1	8,786
Stock Pay Indicator	0.193	0.395	0	0	0	8,786
Option Pay Indicator	0.678	0.467	0	1	1	8,786
Total CEO Turnover	0.072	0.258	0	0	0	8,786
Forced CEO Turnover	0.019	0.138	0	0	0	8,786
Voluntary CEO Turnover	0.052	0.222	0	0	0	8,786
Log(Assets)	7.088	1.678	5.841	6.913	8.181	8,786
Assets, $M	5,604	15,170	343	1,004	3,572	8,786
Stock Return	0.197	0.487	-0.094	0.127	0.383	8,786
ROA	0.038	0.096	0.013	0.045	0.084	8,786
PPE/Assets	0.325	0.236	0.139	0.271	0.489	8,786
CEO Tenure	1.587	1.161	0.949	1.778	2.390	8,786
Debt/Assets	0.234	0.174	0.090	0.222	0.343	8,786
Union	0.029	0.167	0	0	0	8,786
Democrat Governor	0.486	0.500	0	0	1	8,732
No Articles Pre-EDGAR	0.689	0.463	0	1	1	8,786
Institutional Ownership	0.536	0.183	0.406	0.549	0.675	6,395

Panel B. Summary Statistics: Stacked Sample

Variable	Mean	SD	p25	p50	p75	N
EDGAR	0.239	0.427	0	0	0	5,546
Total CEO Compensation, $000s	1,604	2,244	515	950	1,764	5,546
Total Executive Compensation, $000s	570	616	215	363	662	22,597
Salary, $000s	428	243	251	370	542	5,546
Cash Bonus, $000s	435	757	73	211	469	5,546
Equity Pay, $000s	814	1,854	0	191	822	5,546
Stock Pay, $000s	77	362	0	0	0	5,546
Option Pay, $000s	745	2,311	0	148	713	5,546
%Salary	0.458	0.256	0.257	0.426	0.630	5,542
%Cash Bonus	0.268	0.203	0.095	0.249	0.397	5,542
%Equity Pay	0.318	0.344	0	0.236	0.547	5,542
%Stock Pay	0.029	0.093	0	0	0	5,542
%Option Pay	0.288	0.337	0	0.176	0.502	5,542
Cash Bonus Indicator	0.957	0.203	1	1	1	5,546
Equity Pay Indicator	0.630	0.483	0	1	1	5,546
Stock Pay Indicator	0.129	0.335	0	0	0	5,546
Option Pay Indicator	0.590	0.492	0	1	1	5,546
Total CEO Turnover	0.035	0.183	0	0	0	5,546
Forced CEO Turnover	0.008	0.088	0	0	0	5,546
Voluntary CEO Turnover	0.027	0.162	0	0	0	5,546

Panel C. Compensation Articles by Source

Source	Articles	Source	Articles
Wall Street Journal	29,465	*Boston Globe*	1,120
American Banker	3,519	*Los Angeles Times*	940
New York Times	2,003	*Star Tribune*	813
Business Wire	1,808	*Washington Post*	722
PR Newswire	1,168	*The Economist*	448

Panel D. Compensation Articles' Number, Length, and Tone

Variable	Mean	SD	p25	p50	p75	N
Compensation Articles (fiscal year)	1.730	6.502	0	0	1	5,171
Compensation Articles (proxy filing)	0.541	2.123	0	0	0	5,171
Total Words, 000s (fiscal year)	2.441	9.415	0	0	0.829	5,171
Total Words, 000s (proxy filing)	0.796	3.238	0	0	0	5,171
Words/Article, 000s (fiscal year)	0.375	0.664	0	0	0.633	5,171
Words/Article, 000s (proxy filing)	0.235	0.587	0	0	0	5,171
Comp. Articles (fiscal year), if covered	2.329	7.467	0	0	2	3,842
Comp. Articles (proxy filing), if covered	0.815	2.565	0	0	1	3,437
Negative Articles (fiscal year), if covered	1.673	5.737	0	0	1	3,842
Negative Articles (proxy filing), if covered	0.574	1.999	0	0	0	3,437

Table 3. Better Access to Disclosure and CEO Pay: DiD Analysis

This table reports the treatment effect of EDGAR implementation on total CEO pay (natural logarithm). *EDGAR* is equal to one if a firm is a mandatory EDGAR filer in a given year (i.e., its fiscal year ends after the corresponding phase-in date) and zero otherwise. Panel A reports the stacked difference-in-differences estimation following Cengiz, Dube, Lindner, and Zipperer (2019) and Baker, Larcker, and Wang (2022). The sample consists of four stacks and uses not-yet-treated firms as control. Panel B reports the average treatment effect using a DiD estimator of de Chaisemartin and d'Haultfoeuille (2020) on the 1991-1999 sample. Controls are included but not shown in Panel B. *t*-statistics based on standard errors clustered by firm are reported in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in Appendix A.

Panel A. Stacked DiD Analysis

Dependent Variable:	*Total CEO Compensation*			
	(1)	(2)	(3)	(4)
EDGAR	-0.153***	-0.146***	-0.151***	-0.132**
	[-4.16]	[-3.40]	[-3.43]	[-2.53]
Log(Assets)	0.499***	0.513***	0.509***	0.534***
	[9.27]	[8.15]	[8.51]	[6.91]
Stock Return	0.021	0.000	0.014	0.004
	[0.85]	[0.00]	[0.46]	[0.11]
ROA	0.989***	1.039***	1.140***	1.181***
	[5.10]	[5.18]	[4.65]	[4.55]
PP&E/Assets	-0.218	-0.128	-0.262	-0.027
	[-1.00]	[-0.54]	[-1.14]	[-0.09]
CEO Tenure	-0.094***	-0.095***	-0.005	-0.016
	[-4.08]	[-3.73]	[-0.15]	[-0.38]
Debt/Assets	-0.365**	-0.356**	-0.292	-0.281
	[-2.13]	[-2.11]	[-1.36]	[-1.32]
Observations	5,364	5,166	5,229	5,025
R-squared	0.773	0.799	0.782	0.809
Firm FE	Y	Y	N	N
CEO-Firm FE	N	N	Y	Y
Year FE	Y	N	Y	N
Ind-Year FE	N	Y	N	Y

Panel B. DiD Estimator of de Chaisemartin and d'Haultfoeuille (2020)

Dependent Variable:	*Total CEO Compensation*			
	(1)	(2)	(3)	(4)
EDGAR	-0.129***	-0.154**	-0.136**	-0.136**
	[-2.69]	[-2.17]	[-2.57]	[-2.41]
Observations	3,997	2,863	3,711	2,602
Firm FE	Y	Y	N	N
CEO-Firm FE	N	N	Y	Y
Year FE	Y	N	Y	N
Ind-Year FE	N	Y	N	Y

Table 4. Better Access to Disclosure and CEO Pay: Quantile Regressions

This table reports the estimates from quantile regressions using the 10th, 25th, 50th, 75th, and 90th percentiles of the data. The dependent variable is the log of total annual CEO pay. *EDGAR* is equal to one if a firm is a mandatory EDGAR filer in a given year (i.e., its fiscal year ends after the corresponding phase-in date) and zero otherwise. The sample consists of four stacks corresponding to the first four EDGAR implementation waves. Not-yet-treated firms with phase-in dates at least one year later are used as control. t-statistics based on standard errors clustered by firm are reported in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in Appendix A.

Dependent Variable:	*Total CEO Compensation*				
Quantile:	*p10*	*p25*	*p50*	*p75*	*p90*
	(1)	(2)	(3)	(4)	(5)
EDGAR	-0.097**	-0.113***	-0.149***	-0.191***	-0.209***
	[-2.56]	[-3.15]	[-4.24]	[-4.74]	[-4.77]
Log(Assets)	0.498***	0.501***	0.508***	0.517***	0.521***
	[8.72]	[9.70]	[10.66]	[8.93]	[7.92]
Stock Return	0.035	0.029	0.014	-0.002	-0.009
	[1.54]	[1.27]	[0.60]	[-0.07]	[-0.29]
ROA	0.876***	0.956***	1.132***	1.333***	1.421***
	[3.92]	[4.57]	[5.77]	[6.17]	[6.07]
PP&E/Assets	0.310*	0.137	-0.246	-0.683***	-0.873***
	[1.78]	[0.81]	[-1.35]	[-2.97]	[-3.39]
CEO Tenure	0.019	0.012	-0.005	-0.024	-0.032
	[0.69]	[0.43]	[-0.17]	[-0.73]	[-0.90]
Debt/Assets	-0.550***	-0.472***	-0.299*	-0.103	-0.017
	[-3.10]	[-2.78]	[-1.75]	[-0.51]	[-0.08]
Observations	5,542	5,542	5,542	5,542	5,542
CEO-Firm FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y

Table 5. Better Access to Disclosure and Total Pay of Non-CEO Executives

This table reports the treatment effect of EDGAR implementation on total pay of non-CEO executives. We implement the stacked difference-in-differences estimation following Cengiz, Dube, Lindner, and Zipperer (2019) and Baker, Larcker, and Wang (2022). The sample consists of four stacks corresponding to the first four EDGAR implementation waves. Not-yet-treated firms with phase-in dates at least one year later are used as control. *EDGAR* is equal to one if a firm is a mandatory EDGAR filer in a given year (i.e., its fiscal year ends after the corresponding phase-in date) and zero otherwise. Specification 1 reports the estimates from OLS regressions, and other specifications report estimates from quantile regressions using the 10th, 25th, 50th, 75th, and 90th percentiles, respectively. t-statistics based on standard errors clustered by firm are reported in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in Appendix A.

Dependent Variable:	*Total Executive Compensation (Non-CEOs)*					
	OLS (1)	*p10* (2)	*p25* (3)	*p50* (4)	*p75* (5)	*p90* (6)
EDGAR	-0.046**	-0.043**	-0.044***	-0.045***	-0.048***	-0.049***
	[-1.97]	[-2.45]	[-2.60]	[-2.83]	[-2.77]	[-2.67]
Log(Assets)	0.343***	0.332***	0.335***	0.342***	0.352***	0.355***
	[8.12]	[10.97]	[11.68]	[13.32]	[14.49]	[14.57]
Stock Return	0.006	0.020*	0.016	0.007	-0.004	-0.008
	[0.42]	[1.88]	[1.61]	[0.77]	[-0.38]	[-0.82]
ROA	0.695***	0.478***	0.538***	0.684***	0.854***	0.921***
	[5.27]	[4.87]	[5.73]	[7.90]	[10.07]	[10.73]
PP&E/Assets	0.065	0.137	0.117	0.069	0.012	-0.010
	[0.37]	[1.13]	[1.01]	[0.61]	[0.10]	[-0.08]
Debt/Assets	-0.276**	-0.259***	-0.263***	-0.275***	-0.288***	-0.293***
	[-2.45]	[-3.22]	[-3.44]	[-3.86]	[-3.95]	[-3.87]
Observations	20,160	22,649	22,649	22,649	22,649	22,649
Executive-Firm FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y

Table 6. Components of CEO Pay

This table reports the treatment effect of EDGAR implementation on the components of CEO pay. We implement the stacked difference-in-differences estimation following Cengiz, Dube, Lindner, and Zipperer (2019) and Baker, Larcker, and Wang (2022). The sample consists of four stacks corresponding to the first four EDGAR implementation waves. Not-yet-treated firms with phase-in dates at least one year later are used as control. *EDGAR* is equal to one if a firm is a mandatory EDGAR filer in a given year (i.e., its fiscal year ends after the corresponding phase-in date) and zero otherwise. The dependent variables in Panel A are the CEO salary, CEO cash bonus (nonequity incentive pay, perquisite pay, and bonus), CEO equity pay, CEO stock pay, and CEO option pay; the model is estimated using Poisson regressions. The dependent variables in Panel B are indicator variables equal to one if a particular component of CEO pay is non-zero in a given year; the model is estimated using OLS regressions. The dependent variables in Panel C are the same as in Panel A, but are normalized by total CEO pay; the model is estimated using OLS regressions. Controls are included in all panels, but are shown only in Panel A. t-statistics based on standard errors clustered by firm are reported in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in Appendix A.

Panel A. Components of CEO Pay (Poisson Regressions)

Dependent Variable:	*Salary*	*Cash Bonus*	*Equity Pay*	*Stock Pay*	*Option Pay*
	(1)	(2)	(3)	(4)	(5)
EDGAR	-0.043***	0.096	-0.352***	-0.592	-0.259*
	[-4.54]	[1.34]	[-2.67]	[-1.56]	[-1.91]
Log(Assets)	0.117***	0.336***	0.760***	1.300***	0.706***
	[6.93]	[3.30]	[4.88]	[4.24]	[4.46]
Stock Return	-0.021***	0.191***	-0.119	0.104	-0.132
	[-4.36]	[4.02]	[-1.11]	[0.44]	[-1.20]
ROA	0.071	3.550***	1.813***	-1.482	1.866***
	[1.59]	[7.47]	[3.21]	[-0.95]	[3.26]
PP&E/Assets	0.142**	-0.044	-0.710	-2.431	-0.562
	[2.45]	[-0.11]	[-0.89]	[-0.91]	[-0.70]
CEO Tenure	0.119***	0.073	-0.191**	0.184*	-0.278***
	[11.45]	[1.52]	[-2.33]	[1.91]	[-3.17]
Debt/Assets	-0.028	-0.135	-1.106*	-0.798	-1.161**
	[-0.61]	[-0.34]	[-1.94]	[-0.68]	[-1.98]
Observations	5,546	5,546	5,546	5,546	5,546
R-Squared	0.917	0.860	0.703	0.810	0.715
CEO-Firm FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y

Panel B. Extensive Margin: The Likelihood of Receiving Pay of Particular Type (OLS Regressions)

Dependent Variable:	*Cash Bonus Indicator*	*Equity Pay Indicator*	*Stock Pay Indicator*	*Option Pay Indicator*
	(1)	(2)	(3)	(4)
EDGAR	-0.017	-0.136***	0.002	-0.134***
	[-1.47]	[-4.12]	[0.12]	[-4.04]
Observations	5,233	5,233	5,233	5,233
R-squared	0.640	0.610	0.705	0.619
CEO-Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

Panel C. Components of CEO Pay as a Fraction of Total Pay (OLS Regressions)

Dependent Variable:	*%Salary*	*%Cash Bonus*	*%Equity Pay*	*%Stock Pay*	*%Option Pay*
	(1)	(2)	(3)	(4)	(5)
EDGAR	0.042***	0.055***	-0.076***	-0.005	-0.071***
	[2.65]	[4.40]	[-3.05]	[-0.84]	[-2.91]
Observations	5,229	5,229	5,229	5,229	5,229
R-squared	0.674	0.686	0.548	0.553	0.551
CEO-Firm FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y

Table 7. Media Coverage of Executive Compensation

This table reports the effect of EDGAR implementation on media coverage of executive compensation. We implement the stacked difference-in-differences estimation following Cengiz, Dube, Lindner, and Zipperer (2019) and Baker, Larcker, and Wang (2022). The sample consists of four stacks corresponding to the first four EDGAR implementation waves. Not-yet-treated firms with phase-in dates at least one year later are used as control. *EDGAR* is equal to one if a firm is a mandatory EDGAR filer in a given year (i.e., its fiscal year ends after the corresponding phase-in date) and zero otherwise. The dependent variable in specification 1 (4) is the total number of words in compensation articles mentioning a given firm in a given year (during the three months around the proxy filing date); the variable is set to 0 if there are no compensation articles. The dependent variable in specification 2 (5) is the length of the average compensation article (words/article) mentioning a given firm in a given year (during the three months around the proxy filing date); the variable is set to 0 if there are no compensation articles. The dependent variable in specification 3 (6) is the number of negative compensation articles mentioning a given firm in a given year (during the three months around the proxy filing date); the variable is set to missing if there are no compensation articles. t-statistics based on standard errors clustered by firm are reported in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in Appendix A.

Dependent Variable:	*Total Words*	*Words/ Article*	*Negative Articles*	*Total Words*	*Words/ Article*	*Negative Articles*
	(1)	(2)	(3)	(4)	(5)	(6)
Timing:	*Fiscal Year*			*3 Months Around Proxy Filing*		
EDGAR	0.787***	0.035	0.416**	0.443***	0.111***	0.126
	[2.72]	[0.89]	[2.21]	[2.80]	[3.02]	[1.19]
Log(Assets)	1.404***	0.061	0.861***	0.598***	0.146***	0.358**
	[2.96]	[0.90]	[3.32]	[2.92]	[4.61]	[2.38]
Stock Return	-0.169	-0.007	-0.062	-0.090	-0.022	-0.068
	[-1.20]	[-0.33]	[-0.67]	[-1.32]	[-1.26]	[-1.17]
ROA	-5.262***	-0.244	-2.687***	-1.838***	-0.310*	-1.364***
	[-3.39]	[-1.32]	[-3.51]	[-3.13]	[-1.84]	[-2.79]
PP&E/Assets	-5.858***	-0.262	-2.734***	-1.459**	0.097	-1.693**
	[-3.44]	[-1.40]	[-2.69]	[-2.28]	[0.78]	[-2.21]
CEO Tenure	-0.275	-0.006	-0.276	-0.147	0.008	-0.170
	[-0.87]	[-0.19]	[-1.30]	[-1.21]	[0.29]	[-1.50]
Debt/Assets	-0.785	0.053	-1.402**	-0.833	-0.221*	-0.170
	[-0.52]	[0.31]	[-1.96]	[-1.33]	[-1.93]	[-0.29]
Observations	5,171	5,171	3,842	5,171	5,171	3,437
R-squared	0.914	0.643	0.917	0.830	0.638	0.805
CEO-Firm FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y

Table 8. Cross-Sectional Tests and Mechanisms

This table reports the treatment effect of EDGAR implementation on total CEO pay (natural logarithm). *EDGAR* is equal to one if a firm is a mandatory EDGAR filer in a given year (i.e., its fiscal year ends after the corresponding phase-in date) and zero otherwise. We implement the stacked difference-in-differences estimation following Cengiz, Dube, Lindner, and Zipperer (2019) and Baker, Larcker, and Wang (2022). The full stacked sample consists of four stacks corresponding to the first four EDGAR implementation waves. Not-yet-treated firms with phase-in dates at least one year later are used as control. Firms are classified into quartiles based on their pre-EDGAR market value of equity, and the corresponding year-by-firm-size fixed effects are included in all specifications, along with firm fixed effects. *t*-statistics based on standard errors clustered by firm are reported in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in Appendix A.

Dependent Variable:	*Total CEO Compensation*			
	(1)	(2)	(3)	(4)
EDGAR	-0.115***	-0.078	-0.069	-0.191**
	[-2.59]	[-1.62]	[-1.23]	[-2.16]
EDGAR × Union	-0.216**			
	[-2.16]			
EDGAR × Democrat Governor		-0.085**		
		[-2.06]		
EDGAR × No Articles Pre-EDGAR			-0.075*	
			[-1.70]	
EDGAR × Institutional Ownership				0.086
				[0.57]
Log(Assets)	0.507***	0.530***	0.507***	0.417***
	[9.49]	[9.86]	[9.51]	[5.49]
Stock Return	0.045*	0.049**	0.049**	-0.003
	[1.82]	[1.98]	[1.98]	[-0.12]
ROA	0.851***	0.963***	0.844***	1.034***
	[4.36]	[4.86]	[4.31]	[4.19]
PP&E/Assets	-0.280	-0.158	-0.265	-0.319
	[-1.27]	[-0.70]	[-1.18]	[-1.19]
CEO Tenure	-0.095***	-0.096***	-0.095***	-0.084***
	[-4.19]	[-4.20]	[-4.16]	[-2.95]
Debt/Assets	-0.342**	-0.444**	-0.358**	-0.259
	[-1.97]	[-2.53]	[-2.05]	[-1.21]
Observations	5,077	5,046	5,025	3,584
R-squared	0.773	0.775	0.773	0.787
Firm FE	Y	Y	Y	Y
Year-MVE Firm Size FE	Y	Y	Y	Y

Table 9. CEO Turnover

This table reports the treatment effect of EDGAR implementation on CEO turnover. We implement the stacked difference-in-differences estimation following Cengiz, Dube, Lindner, and Zipperer (2019) and Baker, Larcker, and Wang (2022). The sample consists of four stacks corresponding to the first four EDGAR implementation waves. Not-yet-treated firms with phase-in dates at least one year later are used as control. *EDGAR* is equal to one if a firm is a mandatory EDGAR filer in a given year (i.e., its fiscal year ends after the corresponding phase-in date) and zero otherwise. The dependent variables are CEO turnover in specification 1, forced CEO turnover in specification 2, and voluntary CEO turnover in specification 3. t-statistics based on standard errors clustered by firm are reported in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in Appendix A.

Dependent Variable:	*Total CEO Turnover*	*Forced CEO Turnover*	*Voluntary CEO Turnover*
	(1)	(2)	(3)
EDGAR	0.025**	0.000	0.024**
	[2.30]	[0.06]	[2.47]
Log(Assets)	-0.054***	-0.017***	-0.037***
	[-3.85]	[-3.44]	[-2.86]
Stock Return	-0.018***	-0.002	-0.016***
	[-3.04]	[-1.15]	[-2.80]
ROA	-0.290***	-0.084***	-0.206***
	[-4.27]	[-2.75]	[-3.23]
CEO Age Over 60	-0.084***	0.001	-0.085***
	[-2.94]	[0.22]	[-3.06]
Observations	5,404	5,404	5,404
R-squared	0.358	0.370	0.352
Firm FE	Y	Y	Y
Year FE	Y	Y	Y

Table 10. Abnormal Returns Around CEO Turnover

Panel A reports summary statistics for the sample of CEO turnovers during the period 1991–1999. The announcement dates for forced CEO turnover are from Peters and Wagner (2014) and from the news search (Nexis Uni), and the announcement dates for voluntary CEO turnover are from the news search (Nexis Uni). *CEO Age* is the age of the departing CEO. *Founder CEO* is equal to one if the news mention that CEO is one of the firm's founders and is zero otherwise. *CEO Leaves for Another Job* is equal to one if turnover is classified as voluntary and the news mention that CEO takes another job or leaves to pursue other opportunities; the variable is zero otherwise. *Family CEO Succession* is equal to one if the incoming and departing CEOs have the same last name and is zero otherwise. *CEO Death* is equal to one if the cause of CEO turnover is CEO death and is zero otherwise. Panel B reports the treatment effect of EDGAR implementation on abnormal returns around CEO turnover. The dependent variables are cumulative abnormal returns (CARs) calculated from Fama-French three factor model plus momentum around the announcement date. *EDGAR* is equal to one if a firm is a mandatory EDGAR filer in a given year (i.e., its fiscal year ends after the corresponding phase-in date) and zero otherwise. *t*-statistics based on standard errors clustered by industry (Fama-French 12) and year are reported in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in Appendix A.

Panel A. Summary Statistics

	N	Mean	SD	p25	p50	p75
EDGAR	503	0.817	0.387	1	1	1
Forced CEO Turnover	503	0.320	0.467	0	0	1
CEO Age	503	59	8	54	60	65
Founder CEO	503	0.083	0.277	0	0	0
CEO Death	503	0.014	0.117	0	0	0
Family CEO Succession	503	0.022	0.146	0	0	0
CEO Leaves for Another Job	503	0.044	0.205	0	0	0
Log(Assets)	500	7.059	1.669	5.881	6.926	8.087
Debt/Assets	497	0.236	0.187	0.098	0.217	0.337
Abnormal Returns Around Total CEO Turnover						
CAR(-1,0)	503	0.005	0.058	-0.018	0.003	0.027
CAR(-1,1)	503	0.004	0.063	-0.023	0.004	0.031
Abnormal Returns Around Voluntary CEO Turnover						
CAR(-1,0)	342	0.004	0.038	-0.015	0.003	0.023
CAR(-1,1)	342	0.004	0.049	-0.018	0.003	0.024

Panel B. Abnormal Returns Around CEO Turnover and EDGAR Implementation

Sample:	*CEO Turnover*		*Voluntary CEO Turnover*	
CAR Window:	(-1,0)	(-1,1)	(-1,0)	(-1,1)
	(1)	(2)	(3)	(4)
EDGAR	-0.024***	-0.034***	-0.015*	-0.028**
	[-2.92]	[-3.69]	[-1.84]	[-2.41]
Log(Assets)	0.002	0.003	0.002	0.002
	[1.04]	[1.59]	[1.53]	[1.04]
Debt/Assets	0.027	0.034	0.017	0.043*
	[1.52]	[1.55]	[1.17]	[1.72]
CEO Age	-0.000	0.001	0.000	0.001
	[-0.26]	[1.55]	[0.40]	[1.58]
Founder CEO	0.005	0.014	0.005	0.008
	[0.67]	[1.17]	[0.56]	[0.56]
CEO Leaves for Another Job	-0.010	0.000	-0.008	0.001
	[-1.11]	[0.04]	[-0.85]	[0.06]
Family CEO Succession	-0.018**	-0.030**	-0.024***	-0.033***
	[-2.31]	[-2.39]	[-2.92]	[-2.84]
CEO Death	0.033**	0.033	0.029*	0.033
	[2.28]	[1.31]	[1.76]	[1.17]
Forced CEO Turnover	-0.002	0.003		
	[-0.23]	[0.36]		
Observations	492	492	335	335
R-squared	0.049	0.086	0.131	0.157
Year FE	Y	Y	Y	Y
Ind FE	Y	Y	Y	Y

Appendix B. Robustness Tests and Additional Results

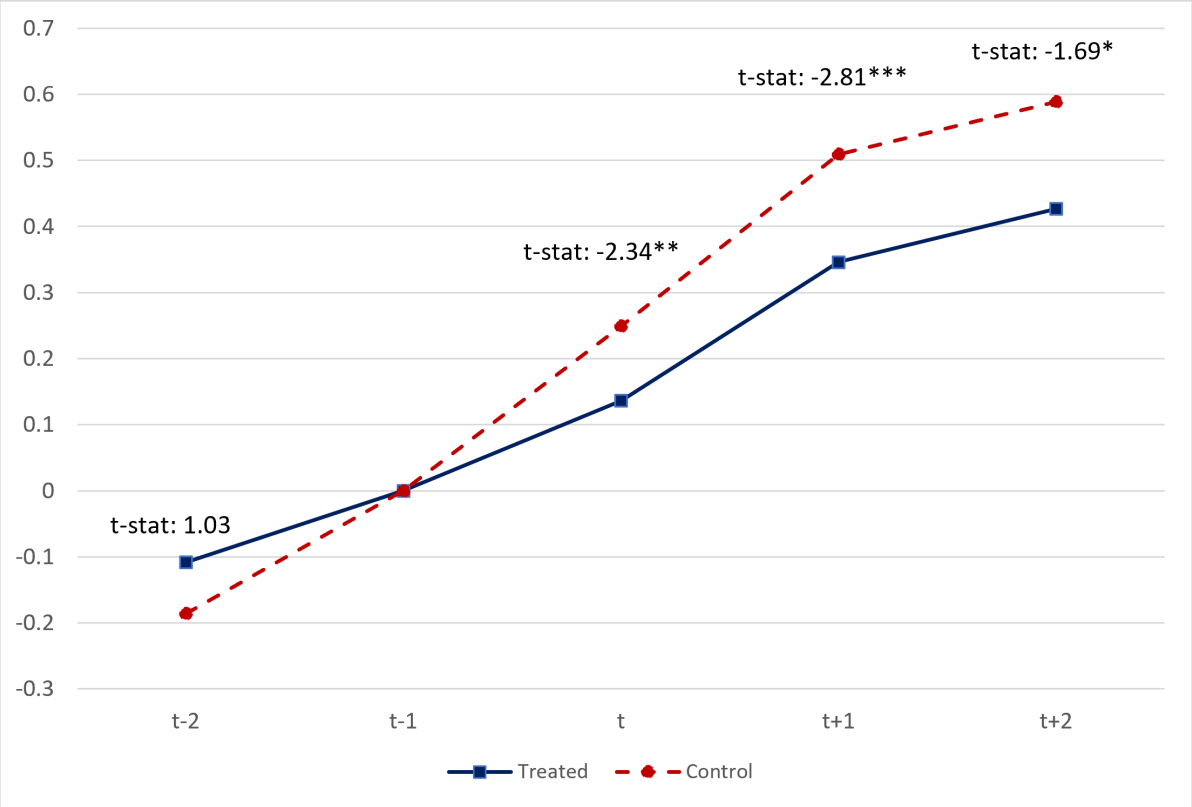

Figure B.1. CEO Pay Growth in Treated and Control Firms Around EDGAR Implementation. This figure shows the evolution of CEO pay for treated and control firms around the implementation of EDGAR. Year t denotes the implementation year. Treated firms are those that implemented EDGAR in year t, and the corresponding control firms are those that had not yet implemented it. Because all firms were treated by 1996, we create two stacks with implementation years of 1993 and 1995. Within each stack, CEO pay growth (relative to the pre-event year $t-1$) is measured as $Y_{i,k,s} - Y_{i,t-1,s}$, where Y is the log of total annual CEO pay, i represents a CEO, $k = t-2, t-1, \ldots, t+2$ denotes the event year, s denotes a stack, and $t-1$ is the base year. The annual average CEO pay growth is then plotted separately for treated firms (solid blue line) and control firms (dashed red line) for each event year. This setup implicitly incorporates stack fixed effects, using year $t-1$ as the base year. t-statistics for the difference in means between treated and control firms are reported in the figure. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table B.1. Robustness of Main Results: Different Samples

This table reports robustness tests for the treatment effect of EDGAR implementation on total annual CEO pay (natural logarithm). *EDGAR* is equal to one if a firm is a mandatory EDGAR filer in a given year (i.e., its fiscal year ends after the corresponding phase-in date) and zero otherwise. Panel A reports the results for a staggered DiD estimation using the full sample of data 1991-1999. Panel B reports the results of the stacked DiD estimation following Cengiz, Dube, Lindner, and Zipperer (2019) and Baker, Larcker, and Wang (2022) without time-varying covariates. Panels C and D report the results of the stacked DiD estimation using either four or seven stacks corresponding to the first four or seven EDGAR implementation waves. As control group, we use not-yet-treated firms that have phase-in dates at least one (Panel C) or two (Panel D) years later than the phase-in date for a given stack. Panel E reports the results of the stacked DiD estimation which combines the first four EDGAR implementation waves (in 1993) in one stack, assuming firms are treated on December 6, 1993. Controls from Table 3 are included in Panels A, C, D, and E but not shown. t-statistics based on standard errors clustered by firm are reported in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in Appendix A.

Panel A. Staggered DiD Analysis

Dependent Variable:	*Total CEO Compensation*			
	(1)	(2)	(3)	(4)
EDGAR	-0.100***	-0.100***	-0.078**	-0.071*
	[-3.10]	[-3.04]	[-2.12]	[-1.90]
Observations	8,786	8,751	8,589	8,557
R-squared	0.738	0.757	0.771	0.791
Firm FE	Y	Y	N	N
CEO-Firm FE	N	N	Y	Y
Year FE	Y	N	Y	N
Ind-Year FE	N	Y	N	Y

Panel B. Stacked DiD Analysis Without Time-Varying Covariates

Dependent Variable:	*Total CEO Compensation*			
	(1)	(2)	(3)	(4)
EDGAR	-0.195***	-0.192***	-0.191***	-0.182***
	[-5.82]	[-4.87]	[-4.69]	[-3.84]
Observations	5,873	5,681	5,728	5,529
R-squared	0.756	0.782	0.768	0.794
Firm FE	Y	Y	N	N
CEO-Firm FE	N	N	Y	Y
Year FE	Y	N	Y	N
Ind-Year FE	N	Y	N	Y

Panel C. Stacked DiD Analysis: 7 Stacks and Not-Yet Treated Firms with 1-Year Gap as Control

Dependent Variable:	*Total CEO Compensation*			
	(1)	(2)	(3)	(4)
EDGAR	-0.108***	-0.115***	-0.123***	-0.113**
	[-3.23]	[-2.83]	[-3.14]	[-2.35]
Observations	6,865	6,526	6,651	6,310
R-squared	0.758	0.789	0.773	0.801
Firm FE	Y	Y	N	N
CEO-Firm FE	N	N	Y	Y
Year FE	Y	N	Y	N
Ind-Year FE	N	Y	N	Y

Panel D. Stacked DiD Analysis: 4 Stacks and Not-Yet Treated Firms with 2-Year Gap as Control

Dependent Variable:	*Total CEO Compensation*			
	(1)	(2)	(3)	(4)
EDGAR	-0.188***	-0.171**	-0.163**	-0.149*
	[-3.50]	[-2.46]	[-2.53]	[-1.80]
Observations	4,039	3,825	3,902	3,697
R-squared	0.781	0.809	0.793	0.823
Firm FE	Y	Y	N	N
CEO-Firm FE	N	N	Y	Y
Year FE	Y	N	Y	N
Ind-Year FE	N	Y	N	Y

Panel E. Stacked DiD Analysis: First 4 EDGAR Waves as One Stack and Not-Yet Treated Firms as Control

Dependent Variable:	*Total CEO Compensation*			
	(1)	(2)	(3)	(4)
EDGAR	-0.113***	-0.116***	-0.122***	-0.124***
	[-2.70]	[-2.59]	[-3.03]	[-2.95]
Observations	4,330	4,308	4,177	4,157
R-squared	0.768	0.787	0.791	0.808
Firm FE	Y	Y	N	N
CEO-Firm FE	N	N	Y	Y
Year FE	Y	N	Y	N
Ind-Year FE	N	Y	N	Y

Table B.2. Robustness Tests: DiD Analysis Using Matched Samples

This table reports the robustness tests for total CEO pay using a propensity score matched (PSM) sample, constructed following Chang, Hsiao, Ljungqvist, and Tseng (2022). The sample is from 1991 to 1995. Treated firms experience the EDGAR requirement in 1993, while control firms are selected from firms experiencing the EDGAR requirement in 1996, after the sample ends (so no control firms experience the treatment at any time in the sample). We match treated firms to control firms in 1992, the year before the EDGAR requirement, based on the Fama-French 12 industry and either the log of the market capitalization or the log of a firm's total assets. Panel A presents statistics on the match quality. Panel B presents the stacked DiD results using the PSM sample. The dependent variable is the log of total annual CEO pay. *EDGAR* is one if a firm is a mandatory EDGAR filer in a given year (i.e., its fiscal year ends after the corresponding phase-in date) and zero otherwise. *t*-statistics based on standard errors clustered at the firm level are reported in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A. PSM Match Quality

	Log(MVE)	*Log(Assets)*
Treated Firms	6.556	7.139
Control Firms	6.466	7.021
Observations	872	1,040
p-value (difference in means)	0.169	0.215

Panel B. DiD Analysis Using PSM Sample

Dependent Variable:	*Total CEO Compensation*			
Matching Variable:	*Log(MVE)*		*Log(Assets)*	
	(1)	(2)	(3)	(4)
EDGAR	-0.081*	-0.155***	-0.086**	-0.147***
	[-1.73]	[-3.57]	[-2.10]	[-3.77]
Observations	3,372	3,223	3,991	3,815
R-squared	0.727	0.768	0.688	0.735
Firm FE	Y	N	Y	N
CEO-Firm FE	N	Y	N	Y
Year FE	Y	Y	Y	Y

Table B.3. Robustness Tests: Other Regulations, Transitional Filers, and the EDGAR Online Access

This table reports robustness tests for the results on total CEO pay. With exception of specifications 1-2 in Panel B, we employ the stacked difference-in-differences estimation method following the approaches of Cengiz, Dube, Lindner, and Zipperer (2019) and Baker, Larcker, and Wang (2022). *EDGAR* equals one if a firm is a mandatory EDGAR filer in a given year (i.e., its fiscal year ends after the corresponding phase-in date) and zero otherwise. Panel A reports the robustness tests related to the introduction of Section 162(m) of the Internal Revenue Code. Specifications 1-2 (3-4) use the stacked sample, but exclude firms with CEO salary (sum of salary and bonus) exceeding $0.9 million in 1992. Panel B reports the robustness tests related to the compensation disclosure rules in 1992 and option backdating practices. Specifications 1-2 report the results for a staggered sample, but exclude observations in 1991-1992. Specifications 3-4 use the stacked sample, but exclude firms that were later revealed to backdate options. Panel C reports the robustness tests related to the timing of SEC EDGAR implementation. Specifications 1-2 use the stacked sample, but exclude firms that had electronic filings prior to their corresponding phase-in date. Specifications 3-4 use the stacked sample, but exclude firms assigned to the first EDGAR implementation wave (CF-01) containing transitional filers (Gao and Huang (2020)). Specifications 5-6 use the stacked sample, but exclude firms that were assigned to the first EDGAR implementation wave (CF-01) and assume that for waves CF-02, CF-03, and CF-04 the phase-in-date is January 17, 1994 (when all electronic EDGAR filings became freely available online). Controls from Table 3 are included in all panels but not shown. *t*-statistics based on standard errors clustered by firm are reported in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in Appendix A.

Panel A. IRC Section 162(m)

Dependent Variable:	*Total CEO Compensation*					
Sample:	*92 Salary<$0.9M*		*92 Salary+Bonus<$0.9M*		*Finer Fixed Effects*	
	(1)	(2)	(3)	(4)	(5)	(6)
EDGAR	-0.144***	-0.148***	-0.107**	-0.125**	-0.148***	-0.150***
	[-3.84]	[-3.28]	[-2.56]	[-2.47]	[-3.99]	[-3.37]
Observations	5,134	5,005	4,277	4,176	5,364	5,229
R-squared	0.754	0.765	0.688	0.699	0.773	0.783
Firm FE	Y	N	Y	N	Y	N
CEO-Firm FE	N	Y	N	Y	N	Y
Year FE	Y	Y	Y	Y	N	N
Salary<$900-Year FE	N	N	N	N	Y	Y

Panel B. Disclosure Rules in 1992 and Option Backdating

Dependent Variable:	*Total CEO Compensation*			
Sample:	*Staggered, Exclude 91-92*		*Stacked, Exclude Option Backdaters*	
	(1)	(2)	(3)	(4)
EDGAR	-0.101***	-0.063	-0.148***	-0.140***
	[-2.83]	[-1.52]	[-4.06]	[-3.20]
Observations	7,350	7,107	5,213	5,084
R-squared	0.749	0.781	0.776	0.785
Firm FE	Y	N	Y	N
CEO-Firm FE	N	Y	N	Y
Year FE	Y	Y	Y	Y

Panel C. Early EDGAR Filers, Transitional Filers, and Edgar Online Access

Dependent Variable:	*Total CEO Compensation*					
Sample:	*Exclude Early E-Filers*		*Exclude Wave CF-01*		*EDGAR Online*	
	(1)	(2)	(3)	(4)	(5)	(6)
EDGAR	-0.152***	-0.150***	-0.171***	-0.167***	-0.082**	-0.076*
	[-4.05]	[-3.35]	[-4.27]	[-3.50]	[-2.34]	[-1.80]
Observations	5,256	5,121	4,424	4,307	4,424	4,307
R-squared	0.768	0.778	0.770	0.781	0.769	0.780
Firm FE	Y	N	Y	N	Y	N
CEO-Firm FE	N	Y	N	Y	N	Y
Year FE	Y	Y	Y	Y	Y	Y

Table B.4. Extended Sample: Using David Yermack's Compensation Data

This table reports the robustness tests for the treatment effect of EDGAR implementation on total CEO pay (natural logarithm). The full sample consists of CEO compensation data obtained from three sources: Execucomp for 1992-1999, online proxies (1991-1992), and David Yermack's compensation data collected from microfilms (1990-1991). *EDGAR* equals one if a firm is a mandatory EDGAR filer in a given year (i.e., its fiscal year ends after the corresponding phase-in date) and zero otherwise. Specifications 1-2 report the results for a staggered difference-in-differences estimation using the full sample of data 1990-1999. Specifications 3-4 report the results using the stacked difference-in-differences estimation method following the approaches of Cengiz, Dube, Lindner, and Zipperer (2019) and Baker, Larcker, and Wang (2022). The sample comprises four stacks corresponding to the first four EDGAR implementation waves. Not-yet-treated firms with phase-in dates at least one year later are used as controls. Panel B reports the average treatment effect using a DiD estimator of de Chaisemartin and d'Haultfoeuille (2020) on the 1990-1999 sample. Controls from Table 3 are included in both panels but not shown. *t*-statistics based on standard errors clustered by firm are reported in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in Appendix A.

Panel A. Extended Sample: DiD Analysis

Dependent Variable:	*Total CEO Compensation*			
	(1)	(2)	(3)	(4)
Sample:	*Staggered (1990-1999)*		*Stacked (1990-1995)*	
EDGAR	-0.093***	-0.080**	-0.087**	-0.112***
	[-2.88]	[-2.43]	[-2.46]	[-3.17]
Observations	9,315	8,986	5,917	5,655
R-squared	0.734	0.773	0.766	0.782
Firm FE	Y	N	Y	N
CEO-Firm FE	N	Y	N	Y
Year FE	Y	Y	Y	Y

Panel B. Extended Sample: DiD Estimator of de Chaisemartin and d'Haultfoeuille (2020)

Dependent Variable:	*Total CEO Compensation*	
	(1)	(2)
EDGAR	-0.116**	-0.143***
	[-2.03]	[-2.98]
Observations	4,585	3,789
Firm FE	Y	N
CEO-Firm FE	N	Y
Year FE	Y	Y

Internet Appendix to *"Does Better Access to Disclosure Curb CEO Pay?"* by Ilona Babenko, Benjamin Bennett, and Zexi Wang

Table IA.1. Firm and CEO Characteristics and EDGAR Implementation Waves

The table provides a comparison of pre-EDGAR firm and CEO characteristics across EDGAR implementation waves. In Panel A, we regress measures of firm size (the log of the firm's market capitalization and the log of the firm's assets) on the EDGAR implementation wave number (1 to 10). In Panel B, we regress each firm or CEO characteristic on the EDGAR implementation wave number (1 to 10), as well as the log of the firm's assets and the log of the firm's market capitalization. The table reports the estimates on EDGAR implementation wave number and their corresponding *t*-statistics based on standard errors clustered by firm. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in Appendix A.

Panel A. Comparing Firm Size Across EDGAR Waves

Pre-EDGAR Characteristic	*Obs.*	*Coefficient*	*t-stat*
Log(MVE)	1,005	-0.312	-18.81***
Log(Assets)	1,024	-0.412	-17.73***

Panel B. Comparing Firm and CEO Characteristics Across EDGAR Waves, Conditional on Size

Pre-EDGAR Characteristic	*Obs.*	*Coefficient*	*t-stat*
Total CEO Compensation	1,004	0.003	0.23
ROA	1,005	-0.000	-0.17
Stock Return	961	0.014	1.50
PP&E/Assets	997	-0.028	-7.16***
Debt/Assets	1,005	-0.006	-2.06**
CEO Tenure	992	-0.014	-0.73

Table IA.2. Additional Descriptive Statistics: Common Positive and Negative Words Used in the Media

This table reports the commonly used positive and negative words in compensation articles. The positive and negative words are classified based on the financial dictionary by Loughran and McDonald (2011).

Common Positive Words	*Common Negative Words*
1. win	1. ill
2. able	2. quit
3. gain	3. harm
4. gains	4. loss
5. benefit	5. cut
6. good	6. bridge
7. perfect	7. lie
8. best	8. lag
9. alliance	9. sue
10. success	10. late

Table IA.3. CEO Option Pay in Subsamples

This table reports the treatment effect of EDGAR implementation on CEO option pay in subsamples sorted by pre-EDGAR option grants. We implement stacked difference-in-differences estimation following Cengiz, Dube, Lindner, and Zipperer (2019) and Baker, Larcker, and Wang (2022). The sample consists of four stacks corresponding to the first four EDGAR implementation waves. Not-yet-treated firms with phase-in dates at least one year later serve as the control group. Column 1 includes treated and control firms that made no CEO option grants in 1992, while Column 2 includes treated and control firms that made some CEO option grants in 1992. *EDGAR* is equal to one if a firm is a mandatory EDGAR filer in a given year (i.e., its fiscal year ends after the corresponding phase-in date) and zero otherwise. The dependent variable is CEO option pay; the model is estimated using Poisson regressions. t-statistics based on standard errors clustered by firm are reported in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in Appendix A.

Dependent Variable:	*Option Pay*	
	(1)	(2)
Treated Firms Options 1992:	N	Y
Control Firms Options 1992:	N	Y
EDGAR	-0.920***	0.132
	[-2.96]	[0.89]
Log(Assets)	0.186	0.553***
	[1.15]	[2.71]
Stock Return	0.339*	0.348***
	[1.78]	[-3.32]
ROA	2.009	0.260
	[1.64]	[0.49]
PP&E/Assets	-2.272	-1.254
	[-1.45]	[-1.07]
CEO Tenure	-0.419**	-0.194*
	[-2.10]	[-1.95]
Debt/Assets	-0.213	-1.429**
	[-0.15]	[-2.35]
Observations	2,707	2,425
R-Squared	0.730	0.726
p-val(difference in coefficients)	0.004	
CEO-Firm FE	Y	Y
Year FE	Y	Y

Table IA.4. Abnormal Returns Around the SEC's Announcement

The dependent variable is the cumulative abnormal return (CAR) calculated from the Fama-French three-factor model plus momentum factor. The window is centered on the announcement date by the SEC regarding EDGAR implementation (February 23, 1993). All explanatory variables are as of the end of fiscal year 1992. t-statistics based on standard errors clustered by firm are reported in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in Appendix A.

Dependent Variable:	*CAR(-1,1)*	
	(1)	(2)
Total CEO Compensation	-0.004**	-0.004**
	[-2.39]	[-2.02]
Log(Assets)	0.003***	0.002*
	[2.76]	[1.85]
Stock Return		-0.000
		[-0.08]
ROA		0.001
		[0.04]
PP&E/Assets		0.009
		[1.47]
CEO Tenure		0.001
		[0.58]
Debt/Assets		0.009
		[0.96]
Observations	1,012	938
R-squared	0.009	0.013

Table IA.5. Voluntary CEO Turnover: Effects of Firm Performance and CEO Compensation

This table reports the treatment effect of EDGAR implementation on voluntary CEO turnover. We implement the stacked difference-in-differences estimation following Cengiz, Dube, Lindner, and Zipperer (2019) and Baker, Larcker, and Wang (2022). The sample consists of four stacks corresponding to the first four EDGAR implementation waves. Not-yet-treated firms with phase-in dates at least one year later are used as control. *EDGAR* is equal to one if a firm is a mandatory EDGAR filer in a given year (i.e., its fiscal year ends after the corresponding phase-in date) and zero otherwise. The dependent variable is voluntary CEO turnover. t-statistics based on standard errors clustered by firm are reported in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in Appendix A.

Dependent Variable:	*Voluntary CEO Turnover*	
	(1)	(2)
EDGAR	-0.098	0.026**
	[-1.45]	[2.44]
EDGAR × Total CEO Compensation	0.016*	
	[1.75]	
EDGAR × Stock Return		-0.013
		[-0.61]
Log(Assets)	-0.041***	-0.036***
	[-3.17]	[-2.77]
Stock Return	-0.018***	-0.015**
	[-3.02]	[-2.51]
ROA	-0.197***	-0.204***
	[-3.05]	[-3.13]
CEO Age Over 60	-0.085***	-0.085***
	[-3.06]	[-3.07]
Observations	5,296	5,404
R-squared	0.354	0.352
Firm FE	Y	Y
Year FE	Y	Y

Table IA.6. Robustness Tests: Controlling for Potential Differential Trends

This table reports the robustness tests for the effect of EDGAR implementation on total annual CEO pay (natural logarithm). We employ the stacked difference-in-differences estimation method following the approaches of Cengiz, Dube, Lindner, and Zipperer (2019) and Baker, Larcker, and Wang (2022). The sample comprises four stacks corresponding to the first four EDGAR implementation waves. Not-yet-treated firms with phase-in dates at least one year later are used as controls. *EDGAR* equals one if a firm is a mandatory EDGAR filer in a given year (i.e., its fiscal year ends after the corresponding phase-in date) and zero otherwise. Controls from Table 3 are included but not shown. In Panel A, firms are classified into quartiles based on their pre-EDGAR market value of equity (specifications 1-2) or total assets (specifications 3-4), and the corresponding year-by-firm-size fixed effects are included as indicated. In Panel B, firms are classified into quartiles based on their pre-EDGAR Debt/Assets (specifications 1-2) or PP&E/Assets (specifications 3-4), and the corresponding year-by-leverage or year-by-asset-tangibility fixed effects are included as indicated. *t*-statistics based on standard errors clustered by firm are reported in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in Appendix A.

Panel A. Year by Firm Size Fixed Effects

Dependent Variable:	*Total CEO Compensation*			
	(1)	(2)	(3)	(4)
EDGAR	-0.121***	-0.120**	-0.100*	-0.113*
	[-2.72]	[-2.21]	[-1.95]	[-1.78]
Observations	5,077	4,960	5,112	4,985
R-squared	0.773	0.782	0.772	0.782
Firm FE	Y	N	Y	N
CEO-Firm FE	N	Y	N	Y
Year-MVE Firm Size FE	Y	Y	N	N
Year-Assets Firm Size FE	N	N	Y	Y

Panel B. Year by Leverage and Year by Asset Tangibility Fixed Effects

Dependent Variable:	*Total CEO Compensation*			
	(1)	(2)	(3)	(4)
EDGAR	-0.141***	-0.146***	-0.135***	-0.139***
	[-3.56]	[-3.08]	[-3.56]	[-3.01]
Observations	5,113	4,986	5,105	4,978
R-squared	0.772	0.782	0.772	0.782
Firm FE	Y	N	Y	N
CEO-Firm FE	N	Y	N	Y
Year-Leverage FE	Y	Y	N	N
Year-Tangibility FE	N	N	Y	Y

Table IA.7. Early Corporate Adoption of World Wide Web

This table reports the robustness tests for the effect of EDGAR implementation on total annual CEO pay. We employ the stacked difference-in-differences estimation method following the approaches of Cengiz, Dube, Lindner, and Zipperer (2019) and Baker, Larcker, and Wang (2022). The sample comprises four stacks corresponding to the first four EDGAR implementation waves. Not-yet-treated firms with phase-in dates at least one year later are used as controls. *EDGAR* equals one if a firm is a mandatory EDGAR filer in a given year (i.e., its fiscal year ends after the corresponding phase-in date) and zero otherwise. Controls from Table 3 are included but not shown. In Panel A, we exclude from the sample any firms that mention 'www' in their 10-Ks. In Panel B, we remove firms with a corporate website listed in Compustat that was registered, as recorded in www.whois.com, before May, 6, 1996. *t*-statistics based on standard errors clustered by firm are reported in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in Appendix A.

Panel A. Excluding Firms with 'www' in 10-Ks

Dependent Variable:	*Total CEO Compensation*			
	(1)	(2)	(3)	(4)
EDGAR	-0.165***	-0.151***	-0.163***	-0.138***
	[-4.37]	[-3.43]	[-3.60]	[-2.58]
Observations	5,172	4,948	5,044	4,814
R-squared	0.770	0.795	0.780	0.805
Firm FE	Y	Y	N	N
CEO-Firm FE	N	N	Y	Y
Year FE	Y	N	Y	N
Ind-Year FE	N	Y	N	Y

Panel B. Excluding Firms with Websites Registered Before May 6, 1996

Dependent Variable:	*Total CEO Compensation*			
	(1)	(2)	(3)	(4)
EDGAR	-0.156***	-0.172***	-0.140**	-0.127*
	[-3.23]	[-2.90]	[-2.37]	[-1.77]
Observations	3,090	2,855	3,007	2,767
R-squared	0.758	0.799	0.771	0.811
Firm FE	Y	Y	N	N
CEO-Firm FE	N	N	Y	Y
Year FE	Y	N	Y	N
Ind-Year FE	N	Y	N	Y

Table IA.8. Alternative Channels: Stock Liquidity and Price Informativeness

This table reports robustness tests for the results on total CEO pay. We employ the stacked difference-in-differences estimation following Cengiz, Dube, Lindner, and Zipperer (2019) and Baker, Larcker, and Wang (2022). The sample consists of four stacks and uses not-yet-treated firms as control. Controls from Table 3 are included but not shown. *EDGAR* equals one if a firm is a mandatory EDGAR filer in a given year (i.e., its fiscal year ends after the corresponding phase-in date) and zero otherwise. The sample is split by the median pre-EDGAR value of bid-ask spread or stock price informativeness measure (see Durnev, Morck, and Yeung (2004) and Fresard (2012)). *t*-statistics based on standard errors clustered by firm are reported in brackets. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are provided in Appendix A.

Dependent Variable:	*Total CEO Compensation*			
	(1)	(2)	(3)	(4)
Sample:	*Low Spread*	*High Spread*	*Low SPI*	*High SPI*
EDGAR	-0.171***	-0.125*	-0.136*	-0.187***
	[-2.66]	[-1.65]	[-1.91]	[-3.33]
Observations	2,393	2,412	2,521	2,462
R-squared	0.827	0.659	0.784	0.769
CEO-Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y