Essays in Financial Economics

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Abstract

My thesis consists of three individual papers, covering topics on corporate social responsibility, public policies, corporate governance, and econometric methodology. The first paper explores the interaction between public policies and private-sector prosocial motives. Using the case of greenhouse gas emissions, I show that state-level emissions reduction targets adopted by nine U.S. states lower shareholder pressure on firms to cut emissions, as evidenced by fewer emission-related shareholder proposals and lower voting support rates. Furthermore, these state-level targets do not reduce corporate emissions as intended. These findings suggest that public policies can sometimes crowd out private-sector prosocial motives and thus undermine the intended policy effects.

The second paper addresses a methodological question in empirical corporate finance: when and why the outcome variable should be log-transformed in reduced-form regressions. My analysis highlights an important but often overlooked distinction in the economic meaning: using logged outcome estimates average proportional changes, while using non-logged outcome captures average level changes. I demonstrate that different functional specifications can sometimes even produce estimated coefficients with opposite signs. Consequently, I argue that the functional form of the outcome variable should not be solely based on statistical characteristics but also on the research question.

The last paper explores the role of CEO power in uncertain times. Conventionally, regulators and researchers focus on the cost of excessive CEO power. I examine whether powerful CEOs can be beneficial in uncertain times. I measure CEO power by CEO chair duality and uncertainty by industry-level stock volatility. I find that, during uncertain time, powerful CEOs are less likely to be dismissed, and they are associated with better performance. To mitigate endogeneity concerns surrounding CEO power, I exploit the onset of the COVID-19 pandemic as an unexpected, drastic, and sudden surge in uncertain. Firms are unlikely to replace their CEO or change CEO power during such a short period. I find that powerful CEOs are associated with significantly

higher stock returns during the one-month period at the beginning of the pandemic. I find suggestive evidence for two mechanisms that contribute to powerful CEOs' effectiveness during uncertainty: they are more capable of taking swift action, and they are more willing to share information with the board. Overall, this paper challenges the conventional view that CEO power is always manipulative and detrimental.

Overall, my thesis examines the factors that influence corporate decision-making and performance, and discusses a common methodology issue in empirical research.

Contents

Chapter 1

Public Policy and Private-Sector Prosocial Motives: The Case of Greenhouse Gas Emissions

Public Policy and Private-Sector Prosocial Motives: The Case of Greenhouse Gas Emissions*

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Abstract

Do public policies always promote corporate prosocial behaviour? I propose that their effectiveness depends on their interactions with private-sector intrinsic motives. Using a difference-in-differences approach, I examine how shareholder pressure to reduce emissions responds to the adoption of greenhouse gas reduction targets in nine U.S. states. I find that firms in adopting states experience reduced shareholder pressure, as reflected in fewer emission-related shareholder proposals and lower voting support. Furthermore, I find no evidence that these policies reduce corporate emissions. These findings suggest that, under certain circumstances, public interventions can inadvertently crowd out private-sector prosocial efforts, thereby undermining their intended effects.

Keywords: Prosocial motives, Public policy, GHG emissions, Shareholder engagement, Corporate social responsibility

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1.1 Introduction

Since the concept of externalities was introduced over a century ago (Marshall, 1890; Pigou, 1920), economists have proposed both public and private solutions to mitigate the negative externalities of business activities and to promote the provision of public goods. While extensive research has examined public policies and private-sector prosocial efforts separately, less attention has been given to how they interact.

Public policies, such as regulations, subsidies, and taxes, are intended to create extrinsic incentives for private-sector prosocial behaviour. However, I argue that their effects may be more complex than anticipated, as private-sector prosocial behaviour is shaped not only by extrinsic incentives but also by intrinsic motives such as reputation and altruism (Riedl and Smeets, 2017; Bauer et al., 2021; Giglio et al., 2023). How these intrinsic motives respond to public policies remains an open question.

In this paper, I investigate how public policies influence the private sector's intrinsic motives (hereafter also referred to as "prosocial motives") and how these interactions moderate the effectiveness of public policies in promoting private-sector prosocial behaviour. I use the case of greenhouse gas (GHG) emissions reduction, a shared objective of both public and private efforts, as my empirical setting. For private-sector motives, I focus on emission-related shareholder proposals, as firms have a fiduciary duty to maximize shareholder welfare and shareholders engage with firms on climate issues by submitting and voting on proposals (Krueger et al., 2020). For public policies, I study state-level emissions reduction targets adopted by nine U.S. states in 2019. These targets mandate percentage reductions in state-level GHG emissions over defined periods and are supported by measures designed to incentivise firms to reduce emissions. This empirical setting presents several advantages for studying the public-private interaction: GHG emissions are objectively measurable, target adoption by a subset of states in the sample allows for causal inference, and the equivalent climate impact of one unit of emissions by any emitter (i.e., interchangeability) enables an assessment of aggregate policy effect.

My firm- and facility-level samples include 2,057 firms and 4,763 facilities in the U.S. from 2016 to 2021. I use a difference-in-differences (DID) framework to leverage the policy adoption by a subset of states in the sample. Firms headquartered in and facilities located within the nine adopting states constitute the treatment group, while those in the twenty-five states that have never adopted such targets serve as the control group. I examine private-sector prosocial motives by comparing the likelihood of firms receiving emission-related shareholder proposals and the voting outcomes on these proposals before and after policy adoption. I then use the same DID framework to evaluate the effectiveness of these policies in shaping firm behaviour, specifically emissions. Finally, recognising that the interaction between public policy and private-sector prosocial motives may vary across policies, I examine the Paris Agreement as an additional case for comparison.

As the effects of public policies on private-sector prosocial motives have not been explicitly modelled, I extrapolate hypotheses from existing theories of individuals' prosocial behaviour. Bénabou and Tirole (2006) categorize individuals' motives for prosocial behaviour as altruistic, extrinsic, and reputational, proposing that extrinsic incentives can crowd out reputational motives by making voluntary actions less distinguishable from those driven by external rewards. They also suggest that increased publicity may strengthen reputational motives. Andreoni (1988, 1990) argues that public provision can crowd out individuals' altruistic motives by reducing the perceived need for their contributions. In my context, public policies simultaneously create extrinsic incentives for firms, act as a public provision of pressure, and probably increase publicity surrounding firm behaviour. Extending these theories, I hypothesize that public policies may result in both crowding-in and crowding-out effects on private-sector prosocial motives, and thus the net effect of a given policy is an empirical question.

I show that after the adoption of state-level emissions reduction targets, the probability a firm receives emission-related shareholder proposals in a given year decreases by 0.81 percentage points, a significant decline from the sample mean of 2.16% to 1.35%. The proportion of emission-related proposals among all shareholder proposals decreases by 5.44 percentage points, from a sample mean of 10.77% to 5.33%, indicating that these targets disproportionately crowd out emission-related proposals.

Beyond submitting proposals, shareholders also convey their preferences to the management through voting. My results show that the average support rate for GHG emission-related proposals decreases by 12.78 percentage points post-treatment, falling from a sample mean of 31.38% to 18.60%. These findings indicate that state-level emissions reduction targets crowd out private-sector intrinsic motives to reduce emissions.

Given the crowding-out effect on private-sector prosocial motives, the overall effectiveness of these policies in altering firm behaviour (i.e., reducing emissions) depends on whether their direct impact outweighs this crowding-out effect. Applying the DID framework to facility-level emissions, I find no evidence that these targets achieve their intended reductions. State-level analyses further corroborate this finding, similarly revealing no reduction in aggregate corporate emissions.

Recognizing that the effectiveness of state-level targets depends on the balance between their direct impact and crowding-out effect, I hypothesize that facilities under higher pre-policy shareholder pressure to cut emissions are more likely to experience stronger crowding-out effects, leading to increased emissions. Conversely, facilities under lower ex-ante shareholder pressure are more likely to reduce emissions following policy adoption. I classify my sample of facilities using three measures of pre-policy shareholder pressure: the number of emission-related shareholder proposals received, the number of analysts covering their parent firms, and whether they are privately or publicly owned. The emissions regression results across subsamples align with my hypothesis. Furthermore, facilities under greater shareholder pressure have higher average emissions, indicating that shareholders focus their emissions-reduction pressure on large emitters.

Climate policies are not inherently ineffective. They can be effective when their direct impact is strong, their crowding-out effect on private-sector prosocial motives is moderate, or they enhance private-sector prosocial motives. To explore this, I examine another public policy—the Paris Agreement—to assess whether it influences private-sector prosocial motives and behaviour differently. As the first universal, legally binding global climate agreement, the Paris Agreement raises public attention to corporate emissions and thereby likely crowds in private-sector prosocial motives via the "publicity" channel. Additionally, its ambitious 1.5-degree target necessitates much more stringent measures than previous accords, indicating a strong direct impact. Consistent with these hypotheses, pre-post analyses show that following the signing of the Paris Agreement in 2016, firms become 1.08 percentage points more likely to receive GHG emission-related proposals in a given year, with the proportion of such proposals increasing by 6.04 percentage points and support rates rising by 10.77 percentage points. This increased shareholder pressure is accompanied by an average reduction of 0.04 million metric tons of carbon dioxide equivalent (CO_2e) in facility emissions annually. While these pre-post comparisons cannot be interpreted causally, they provide suggestive evidence that stringent public policies that raise public attention can crowd in private-sector prosocial motives and effectively reduce emissions.

Although this paper focuses on GHG emissions reduction as its empirical setting, it has broader implications for the interaction between public policies and private-sector prosocial motives more generally. The findings suggest that public policies can sometimes fall short of their objectives by inadvertently crowding out private-sector prosocial efforts.

This paper makes three contributions to the literature. First, despite the extensive research on the separate roles of public policy and private-sector prosocial efforts, their interaction remains relatively understudied. In the context of curbing GHG emissions, the growing literature on the public-private interaction is mostly theoretical and focuses on how firms' commitments can influence public climate policy-making (Biais and Landier, 2022; Acharya et al., 2023; Allen et al., 2023; Carlson et al., 2023; Heeb et al., 2023). This paper examines the reverse interaction, namely, how public policies influence private-sector prosocial motives.

Second, while the research and practice of public policies conventionally focus on the provision of extrinsic incentives, this paper reveals their unintended effects on private-sector intrinsic motives, which can substantially impact policy effectiveness.

Finally, this paper contributes to the vast literature on corporate social responsibility (CSR). In contrast to Friedman (1970)'s focus on shareholder value maximization, recent research distinguishes between shareholder "value" and shareholder "welfare" or "values" (Hart and Zingales, 2017; Starks, 2023), suggesting that some prosocial behaviours are driven by considerations beyond profit maximization. As Starks (2023) highlights, a key challenge in this field is to disentangle the diverse motives involved. This paper contributes to this ongoing discussion by shedding light on the complex interplay between the extrinsic and intrinsic motives behind corporate social responsibility.

1.2 Background

There are several challenges in studying the interaction between public policies and private-sector prosocial efforts. First, private-sector prosocial motives and behaviours are inherently difficult to observe and measure. For instance, ESG ratings exhibit substantial divergence across data providers (Chatterji et al., 2016; Gibson Brandon et al., 2021; Christensen et al., 2022; Berg et al., 2022). Second, social and environmental performances are often not interchangeable, complicating the assessment of the aggregate policy effect. For example, if equality improves in some companies but deteriorates in others following a public policy, it is unclear whether the policy achieves a net positive effect. Third, many public policies are adopted at the national level, making causal inference challenging.

This paper primarily focuses on state-level GHG emission reduction targets, a setting that presents three key advantages. First, corporate emissions are objectively measurable, providing reliable data for analysis. Second, emission reduction efforts are interchangeable, as one unit of reduction by any firm has the same impact on the climate. This perfect substitutability allows for an evaluation of the aggregate effect of public policies, even when some firms increase emissions while others reduce theirs. Third, these targets have been adopted by only a subset of U.S. states, creating a natural setting for applying the DID method and facilitating causal inference. To study private-sector prosocial motives, this paper focuses on shareholders and analyses trends in emission-related shareholder proposals. Shareholder proposals are formal recommendations submitted by shareholders to companies, urging specific actions that reflect their priorities and concerns. According to a survey by Krueger et al. (2020), institutional investors engage with portfolio firms on climate risks through three primary strategies: private discussions with management, submitting shareholder proposals, and voting on proposals. Since data on private discussions are proprietary, this paper focuses on the latter two channels to examine shareholder preferences on climate-related issues.

U.S. climate policy is characterized by a multi-tiered approach comprising federal, regional, and state components. At the federal level, climate policy has experienced significant shifts across different administrations (The White House, 2015a,b, 2017, 2021, 2022). The U.S. signed the United Nations Framework Convention on Climate Change in 1992 and the Kyoto Protocol in 1997 but did not ratify the latter. During the Obama administration, the U.S. introduced the Clean Power Plan (CPP) and played a pivotal role in negotiating the Paris Agreement in 2015, committing to reduce emissions by 26-28% below 2005 levels by 2025. The Trump administration reversed many of these policies, dismantling the CPP and announcing the U.S. withdrawal from the Paris Agreement in 2017. However, the Biden administration shifted the course once again, rejoining the Paris Agreement in 2021 and setting new targets to reduce emissions by 50-52% from 2005 levels by 2030 and to achieve net zero by 2050. In 2022, the Biden administration further reinforced its climate strategy by signing the Inflation Reduction Act, which allocates $369 billion to help build a clean energy economy.

While federal policies often fluctuate with changes in administration, regional and state-level policies tend to remain more stable and complement federal efforts. For example, the Regional Greenhouse Gas Initiative (RGGI), an interstate program aimed at reducing CO_2 emissions from the power sector, was launched in 2009 and currently includes eleven member states (Chan and Morrow, 2019; Kumar and Purnanandam, 2024). California implemented a carbon cap-and-trade program in 2013, targeting plants that emit at least 25,000 tons of CO_2e annually (Bartram et al., 2022).

As of September 2023, twenty-six states have adopted economy-wide GHG reduction targets, typically specifying a percentage reduction by a certain year relative to a baseline year. These targets vary across states in terms of reduction percentage, baseline year, time frame, and legal status (established either through legislation or executive orders). The information on these targets is sourced from Korganbekova (2024) and cross-verified with original laws, mandates, and additional references (Shields, 2023; Center for Climate and Energy Solutions, 2023). A summary of these state-level targets is presented in Table A1.1.

State-level GHG reduction targets are supported by specific regulations and programs designed to achieve them. For instance, New York state passed the Climate Leadership and Community Protection Act in 2019, setting a series of emission reduction goals: a 40% reduction from 1990 levels by 2030 and an 85% reduction by 2050 (New York State, 2019). The state subsequently developed specific regulations to meet these targets, including expanding the Clean Energy Standard in 2020, which set goals for 70% renewable electricity by 2030 and 100% by 2040. In 2021, the state enacted Zero Emission Vehicle (ZEV) Requirements, mandating all new light-duty vehicles be

ZEVs by 2035 and all other new vehicles by 2045. Additionally, the state established the Advanced Building Codes, Appliance and Equipment Efficiency Standards Act of 2022 to reduce GHG emissions associated with buildings and appliances (New York State, 2020, 2021, 2022). Similarly, after the state of Colorado adopted a series of emission reduction targets in 2019, it introduced industry-specific regulations and programs that either raised energy standards for firms or provided them with financial assistance for green transitions.

This paper focuses on emissions reduction targets rather than specific regulations for two reasons. First, these targets are economy-wide, whereas specific regulations typically apply to only one or a limited number of industries, making the targets more comprehensive in scope. Second, states generally set their targets before adopting specific measures to achieve them. As a result, the private sector is likely to begin making adjustments as soon as these targets are adopted, in anticipation of subsequent regulations and programs. Consequently, the adoption of these targets serves as the initial step in a series of regulatory actions and is treated as the timing of the intervention in this study.

1.3 Related literature and hypotheses development

There has been a long-standing debate on whether the responsibility for advancing the common good should rest with the government or the private sector. Over the half-century since Friedman (1970), the mainstream view regards the government as the social planner responsible for maximizing social welfare, while companies are expected to focus exclusively on shareholder value. This view is grounded in three key arguments. First, companies lack sufficient motives to achieve optimal social welfare in the presence of externalities (Pigou, 1920; Samuelson, 1954). Second, the government is better equipped to enhance social well-being through tools such as regulation (Glaeser and Shleifer, 2003), taxation (Pigou, 1920), redistribution (Mirrlees, 1971), and the authority to define and enforce property rights (Coase, 1960). Third, from a fiduciary duty perspective, "a corporate executive is an employee of the owners of the business", and thus, the only responsibility of a business is to increase profits (Friedman, 1970).

In contrast, stakeholder theory contends that firms should account for the broader impacts of their activities and proactively balance the interests of all stakeholders (Freeman, 1984; Carroll, 1991). Over the past two decades, this perspective has gained significant attention and recognition in both industry and academia. Empirical studies supporting stakeholder theory suggest that investors may undertake social responsibility for either pecuniary or nonpecuniary motives. When driven by pecuniary motives, these prosocial efforts can be described as "doing good to do well", serving as a means to enhance long-term shareholder value. For example, investors who engage with firms on CSR (or ESG) issues expect higher returns or lower risks (Dimson et al., 2015, 2023; Krueger et al., 2020; Hoepner et al., 2024). However, social interests do not always align with shareholder value, and in such cases, investors' prosocial behaviours can be justified by non-financial motives (Hart and Zingales, 2017; Starks, 2023). Surveys and experiments conducted by Riedl and Smeets (2017), Hartzmark and Sussman (2019), Bauer et al. (2021), Guenster et al. (2022), and Giglio et al. (2023) provide evidence that investors' preferences are indeed partially influenced by non-financial factors like altruism and social signalling. Similarly, Baker et al. (2022) demonstrate through a revealed preference approach that investors are willing to forgo some financial returns for sustainable investments.

In the specific context of climate change and corporate GHG emissions, the literature has also explored the respective roles of the government and private sector. On the public policy front, Chan and Morrow (2019) and Kumar and Purnanandam (2024) show that entity-level CO_2 emissions decline following the adoption of RGGI. Korganbekova (2024) finds that state-level emission targets are also effective in reducing entity-level emissions. In contrast, Bartram et al. (2022) find that the California cap-and-trade program is ineffective for financially unconstrained firms and even counterproductive for constrained firms due to spillover effects. Inderst and Opp (2024) analyse the potential impact of a mandatary taxonomy for sustainable investment. Huang and Kopytov (2024) model how increased regulation stringency may paradoxically result in higher pollution levels by reshaping shareholder compositions, which in turn alter firms' decisions. Oehmke and Opp (2022) analyse the effectiveness of banks' green capital requirements. On the private sector side, investors can influence firms through either portfolio composition or direct engagement (Broccardo et al., 2022; Jagannathan et al., 2023; Berk and Van Binsbergen, 2024; Green and Roth, 2024; Oehmke and Opp, 2024). Azar et al. (2021) show that the "Big Three" institutional investors focus their engagement efforts on large firms with high CO_2 emissions, leading to subsequent emission reductions. However, Atta-Darkua et al. (2023) raise doubts on the effectiveness of investor-led initiatives, finding that institutional investors primarily decarbonize their portfolios through re-weighting rather than engagement.

There is a burgeoning, primarily theoretical literature on the interaction between public- and private-sector efforts for reducing GHG emissions. One strand of this literature suggests that public policies can be endogenous to firms' prosocial efforts. Biais and Landier (2022) and Acharya et al. (2023) show firms' investments in green technologies can increase the credibility of government commitments to cap emissions and incentivise transitions, respectively. Acemoglu and Rafey (2023) suggest technology advancements might reduce the stringency of climate policies: geoengineering breakthroughs lower the negative externalities of emissions and thereby reduce the equilibrium carbon tax. Allen et al. (2023), Carlson et al. (2023), and Heeb et al. (2023) study how sustainable investing affects political support for climate policies. The reverse interaction—how private-sector efforts might be influenced by public policy—remains underexplored. Piatti et al. (2023) model how public policies (taxes and subsidies) crowd out the private provision of public goods by consequentialist investors, showing that the impacts of these policies on the total provision of public goods depend on the comparative inefficiencies of the government versus the private sector. To the best of my knowledge, no empirical research has specifically examined the impact of climate policies on private-sector intrinsic motives for emissions reduction.

Bénabou and Tirole (2006) develop a model that examines the interaction between various motives behind individuals' prosocial behaviours. According to their framework, prosocial behaviours are driven by three types of motives: altruism, extrinsic incentives, and reputation. Applied to the context of corporate GHG emissions reduction, the private sector may be motivated by: (1) Extrinsic incentives, such as subsidies, taxes, or industry standards tied to emissions, often mandated by climate policies; (2) Altruism, where firms willingly sacrifice partial profits to contribute to a more sustainable future; and (3) Reputation, which can carry both affective and instrumental value. Examples of the latter include attracting investment flows for institutional investors or enhancing employee and customer satisfaction for portfolio firms.

The model also offers a framework for understanding how climate policies might influence these motives. First, climate policies create extrinsic incentives to reduce emissions. Second, such extrinsic incentives may crowd out reputational motives by making genuine commitments to sustainability less distinguishable from incentivised actions, thereby reducing reputational benefits. On the other hand, if climate policies increase publicity surrounding firms' climate impacts and the greenness of investors' portfolios, they could amplify reputational motives. Thus, while climate policies create extrinsic incentives for emissions reduction, they may either crowd in or crowd out reputational motives.

The interaction between public policy and altruism, another type of prosocial motive, can be examined through the models developed by Andreoni (1988) and Andreoni (1990). These models represent altruism through a utility function dependent on the aggregate provision of a public good, where private contributions decrease as public provision increases, maintaining an optimal total level of provision. Applied to my context, if shareholders optimise emission levels for each firm, climate policies can be viewed as the public provision of pressure on firms to reduce emissions, potentially reducing shareholders' perceived need to engage with firms on this issue.

Building on the models by Bénabou and Tirole (2006), Andreoni (1988), and Andreoni (1990), I hypothesize that (1) climate policies provide extrinsic incentives for corporate emissions abatement but may simultaneously crowd out private-sector altruistic and reputational motives. However, climate policies may also crowd in private-sector reputational motives through the publicity channel. Consequently, (2) the overall effectiveness of such policies in reducing corporate emissions is uncertain and depends on the balance between their direct impacts and the extent of potential crowding-out effects. Figure 1.1 illustrates these motives and how they may be influenced by climate policies.

1.4 Data and Sample

1.4.1 Shareholder Proposals

I obtain shareholder proposal data from the ISS - Voting Analytics - Shareholder Proposals database ("ISS Proposal database"), which tracks shareholder proposals received by Russell 3000 firms from 2006 onwards. Using this database, I calculate the number of proposals each firm receives annually and merge this information with the CRSP/-Compustat Merged - Fundamentals (CCM) database.

Since the coverage of the ISS Proposal database does not fully overlap with that of CCM, a firm's absence from the ISS database in a given year could mean either that it receives no shareholder proposals or that it is not covered by the database. To account for this ambiguity, I create a subsample that includes only firms with at least one recorded shareholder proposal in the ISS Proposal database, excluding those with no proposals. Both the main sample and this subsample are used in the analysis of shareholder proposals to ensure robustness.

I also identify shareholder proposals that are related to GHG emissions and count their numbers. A proposal is classified as GHG emission-related if its "resolution" in the ISS Proposal database (i.e., description) contains any of the following terms (case insensitive): "ghg", "2 degree", "two degree", "climate", "global warming", "renewable energy", "carbon", "paris agreement", "net zero", "net-zero", "energy efficiency", "coal", "greenhouse", "fossil fuel", "methane", "scope 1", "scope 2", and "scope 3". I define a dummy variable, *GHG Proposal Dummy*, which equals one if a firm receives one or more GHG emission-related shareholder proposals in a given year, and zero otherwise. For firm-years with a positive number of shareholder proposals, I also calculate the *GHG Proposal Ratio*, which represents the proportion of GHG emission-related shareholder proposals relative to the total number of shareholder proposals a firm receives in that year.

Following these steps, I construct a firm-year-level sample (and a subsample) with the variables *GHG Proposal Dummy*, *GHG Proposal Ratio*, and the number of emission-related proposals. Then I match this sample (and the subsample) with control variables constructed using data from the CCM and CRSP Monthly Stock databases. Table A1.3 summarises the categories of emission-related shareholder proposals, organised by sponsors and content.

1.4.2 Voting Results

The voting results on GHG emission-related shareholder proposals are obtained from the ISS - Voting Analytics - Company Vote Results US database ("ISS Results database"), which covers proposals received by Russell 3000 firms from 2003 onwards. I focus on GHG emission-related shareholder proposals. A proposal is classified as GHG emission-related if its "AgendaGeneralDesc" in the database (i.e., description) contains any of the terms listed in Section 1.4.1.

As noted by Bach and Metzger (2019), the method of counting votes varies across firms and is typically outlined in each firm's corporate code or charter. Specifically, some firms treat abstentions and/or nonparticipating shares as votes against the proposal, while others do not. To account for this variability, I follow their approach and calculate the *Support Rate* as the percentage of votes in favor of the proposal, based on the denominator specified in the company's bylaws (i.e., the "base" variable in the ISS Results database).

I match this proposal-level voting results dataset with control variables constructed using data from the CCM and CRSP Monthly Stock databases.

1.4.3 GHG Emissions

The facility-level emissions are sourced from the Greenhouse Gas Reporting Program (GHGRP), an emissions data collection program introduced by the U.S. Environmental Protection Agency (EPA). Since 2010, all U.S. facilities that emit more than 25,000 metric tons of CO_2e per year are required to report their emissions and other relevant information to the program administrator annually.[1] Once a facility falls under these reporting requirements, it must continue reporting annually unless its emissions have fallen below 25,000 metric tons of CO_2e for five consecutive years or 15,000 metric tons of CO_2e for three consecutive years. EPA specifies the methodologies for calculating and reporting GHG emissions and verifies the reported data through a multi-step process, which ensures the data are accurate, consistent, and thus suitable for cross-facility and longitudinal comparisons.

This study focuses on direct emissions reported in the GHGRP database, corresponding to Scope 1 emissions as defined by the World Resources Institute Greenhouse Gas Protocol. This scope of facility-level emissions measurement is aligned with state-level emissions reduction policies that focus on total emissions on the state level.

[1] CO_2e, or carbon dioxide equivalent, represents the number of metric tons of CO_2 emissions with an equivalent global warming potential to one metric ton of another greenhouse gas. In addition to the 25,000 metric tons of CO_2e threshold, GHGRP includes other conditions under which a facility is required to report its emissions, such as specific product categories or emission sources. More details about the requirements can be found in U.S. Environmental Protection Agency Office of Atmospheric Protection (2024).

Aggregate direct emissions reported through GHGRP amount to about three billion metric tons of CO_2e per year, representing about half of total U.S. emissions. Because only facilities emitting over 25,000 metric tons of CO_2e are required to report, the database predominantly includes facilities in high-GHG-emission industries such as chemicals, metals, minerals, petroleum, natural gas, and power generation. I match facility-year-level emissions data to their parent firms using the linking table provided by GHGRP, and then manually match the parent firm names with company names in CCM database. For uncertain name matches, I use additional information such as city, ZIP code, and company website to ensure accuracy. Finally, I merge the emission dataset with control variables constructed using data from the CCM and CRSP Monthly Stock databases.

For state-level analyses, I measure emissions in two ways. First, I aggregate facility-level emissions from GHGRP to the state level. While this approach leaves out most smaller facilities that emit less than 25,000 metric tons of CO_2e annually, the included facilities account for the majority of corporate emissions, making the dataset sufficiently representative for evaluating climate policies. Nonetheless, to address any concerns regarding the omission of smaller emitters, I also use an alternative data source: state-level energy-related CO_2 emission data provided by U.S. Energy Information Administration (2023) (EIA), which is estimated based on energy consumption. Unlike GHGRP, this database does not include greenhouse gases other than CO_2. However, CO_2 alone accounts for more than 85% of all GHG emissions measured in CO_2e in the U.S. This database is available from 1970 onwards.

It should be noted that the EIA database reports CO_2 emissions from both firms and households. Since this paper focuses on the interaction between public policy and corporate-sector prosocial behaviours, I exclude household emissions. The EIA's "sectoral specific emission tables by state" categorize emissions into five sectors: commercial, electric power, industrial energy, residential, and transportation. Given that a substantial portion of emissions from the residential and transportation sectors originate from households, I treat the sum of emissions from the commercial, electric power, and industrial energy sectors as the aggregate corporate emissions.

1.4.4 Control Variables

Control variables, including *Asset (ln)*, *Leverage*, *ROA*, and *MB*, are constructed using data from CCM database. *AR* is calculated based on the Fama-French-Carhart four-factor model using data from the CRSP monthly database and Kenneth French's Website. State-level *GDP* data is sourced from U.S. Bureau of Economic Analysis (2024). The number of analyses following each firm is obtained from IBES - Detail History - Detail File with Actuals. Detailed information on the construction of these variables is presented in Table A1.2.

1.4.5 Samples and summary statistics

As detailed above, I construct multiple samples at different levels of observation to accommodate various analyses. A firm-year-level sample is used to analyze the likelihood of receiving emission-related shareholder proposals. Voting results are examined using a proposal-level sample. Emissions are analyzed using both facility-year-level and state-year-level samples. The time frame during which all databases are available spans from 2010 to 2021.

The majority of my analyses focus on state-level emission reduction targets, with the detailed timeline presented in Table A1.1. Given the data availability from 2010 to 2021 and an event window of [t-3, t+2], only targets adopted between 2013 and 2019 can be used for analyses. Among the eleven states (with 15,025 facilities) that meet this criteria, nine (with 14,655 facilities) adopted targets in 2019. Therefore, I concentrate on the treatments in 2019, designating companies and facilities in these nine states as the treatment group, while those in the twenty-five states that have never adopted such targets as the control group. Observations from states that adopted state-level targets in years other than 2019 are excluded from the samples used in the analyses.

Descriptive statistics for the variables over the event window from 2016 to 2021 are shown in Panel A of Table 1.1.

My samples for examining state-level targets include 2,057 firms, 4,763 facilities, and 281 emission-related shareholder proposals, of which 100 are voted on. Table A1.3 categorizes these 281 proposals based on their sponsors and content. On average, 2.16% of firms receive at least one emission-related proposal each year. Although this represents a small proportion of firms, those that received at least one emission-related proposal during the sample period are responsible for 66.4% of total emissions from all covered firms, indicating that shareholder engagement on emissions primarily targets large emitters. Therefore, shareholder proposals serve as a sufficiently representative measure of shareholder efforts to reduce corporate emissions. The average proportion of emission-related shareholder proposals is 10.8%, the average voting support rate for these proposals is 31.4%, and the average facility emits 0.45 million metric tons of CO_2e per year.

The sample for Table 1.7, which examines the impacts of the Paris Agreement, covers the period from 2013 to 2018, with 2016 as the event year. The corresponding descriptive statistics are presented in Panel B of Table 1.1.

1.5 Empirical Analyses and Results

1.5.1 Emission-Related Proposals and Voting Results

In this section, I examine the impact of state-level targets on shareholder engagement with firms regarding GHG emissions reduction. I begin by analyzing the likelihood of firms receiving emission-related proposals and then examine the voting outcomes on these proposals.

The following regression is estimated on the firm-year-level sample:

$$Y_{it} = \beta_0 + \beta_1 Treated_{it} + B_2 X_{it} + d_i + d_t + \varepsilon_{it} \tag{1.1}$$

where Y_{it} represents the *GHG Proposal Dummy*, an indicator equal to one if firm i receives at least one emission-related proposal in year t and zero otherwise. $Treated_{it}$ is a dummy variable set to 1 if the state where firm i is headquartered has adopted a state-level target by year t, and 0 otherwise. X_{it} is a vector of firm characteristics, d_i is the firm fixed effect, d_t is the year fixed effect, and ϵ_{it} is the error term adjusted for heteroskedasticity and clustered at the state level. For the subsample of firm-years with at least one shareholder proposal, an additional regression is estimated using the Equation (2.11), with the dependent variable Y_{it} replaced by *GHG Proposal Ratio*, the proportion of emission-related shareholder proposals relative to the total number of shareholder proposals received by firm i in year t.

The results are presented in Panel A of Table 1.2. Columns 1-2 report the results from the regressions of *GHG Proposal Dummy* using the main sample. The estimated coefficient of $Treated$ is significantly negative, implying that firms become less likely to receive an emission-related shareholder proposal after their headquartered states adopt an emissions reduction target. Considering that the average probability of receiving such a proposal across the full sample is 2.16%, the treatment effect of 0.81 percentage points (based on the specification of Column 2) is economically significant. For robustness purposes, I also run the regressions on the subsample of firms with at least one recorded shareholder proposal in the ISS Proposal database, and the results are reported in Columns 3-4. Although the estimate is less statistically significant on this subsample, its magnitude is larger, corroborating that firms are less likely to receive an emission-related proposal after the adoption of state-level targets.

To assess whether the decrease is specific to emission-related proposals or exists in all types of shareholder proposals, I estimate regressions of the *GHG Proposal Ratio* on the subsample of firm-years with at least one shareholder proposal (so that the denominator is nonzero). As shown in Columns 5-6, the proportion of GHG emission-related shareholder proposals relative to all shareholder proposals decreases by 5.44 percentage points after the treatment, which is both statically and economically significant when compared to the sample mean of 10.77%. These findings indicate that state-level emissions reduction targets disproportionally reduce the number of emission-related shareholder proposals firms receive related to proposals on other issues.

The DID framework estimates the comparative changes in the outcome variable between the treatment group and the control group. To further investigate the respective changes within each group, I perform pre-post comparisons centered around 2019 for each group separately. The following regression is estimated on the firm-year-level subsamples of the treatment group and the control group:

$$Y_{it} = \beta_0 + \beta_1 Post_{it} + B_2 X_{it} + d_i + \varepsilon_{it} \tag{1.2}$$

where $Post_{it}$ is a dummy variable equal to 1 if the given year is 2019 or later, and 0 otherwise. All other variables in the equation retain the same definitions as in Equation (2.11).

Panel B presents the results for the treatment group, revealing significant decreases in both the likelihood of receiving an emission-related proposal and the proportion of such proposals following the treatment year 2019. In contrast, the results for the control group, shown in Panel C, suggest a much smaller and insignificant decrease in the likelihood of receiving an emission-related proposal and no significant change in the proportion of emission-related proposals. Therefore, the pre-post comparisons suggest that the treated firms indeed become less likely to be targeted by emission-related shareholder proposals. On the other hand, there is no evidence of spill-over effects or general trends among the control firms.

Panels (a) and (b) of Figure 1.2 display the estimated dynamic treatment effects by year and their 95% confidence intervals from the difference-in-differences regressions of *GHG Proposal Dummy* and *GHG Proposal Ratio*. These figures reveal significantly negative treatment effects in both *GHG Proposal Dummy* and *GHG Proposal Ratio*. These treatment effects are dynamic and enlarge over the three years post-treatment (e.g., the treatment effect in year 3 is greater than those in years 1 and 2). This might be caused by the fact that emission-reduction targets are typically followed by specific regulations and programs that are gradually rolled out in the few years after the adoption of the target, as illustrated by the example of New York state described in Section 1.2. Additionally, the figures serve as tests for the parallel trend assumption, showing no evidence of a pre-existing trend before the treatment. If anything, the estimated coefficients slightly slope upward before the treatment, contrasting with the downward slope in the post-treatment period. Thus, the treatment effects of state-level targets are not driven by pre-existing trends.

I conduct two robustness tests on the likelihood of receiving emission-related shareholder proposals. First, some pension funds might directly implement government mandates, creating a potential link between their behaviour and climate policies. To address the concern that my results may be driven by this mechanism, I exclude proposals submitted by pension funds. Table A1.4 confirms that my findings remain robust.

Second, special-interest organisations and individuals may submit proposals reflecting idiosyncratic agendas, which are not necessarily representative of broader shareholder preferences (Gantchev and Giannetti, 2021). To disentangle whether my results are influenced by such behaviour or reflect the actions of more sophisticated investors, I categorise proposals into two groups: those submitted by individual and special-interest investors and those by institutional investors. Results in Table A1.5 indicate that the crowding-out effect is predominantly driven by institutional investors, alleviating concerns that the findings are due to "noisy" behaviour from individual or special-interest investors.

In addition to submitting proposals, shareholders can also exert influence on firms by voting on these proposals. In fact, some types of investors, like passive funds, tend to vote on proposals but not submit them directly. Table 1.3 analyses voting results on emission-related shareholder proposals using the proposal-level sample. Columns 1 and 2 report the results of a DID analysis of the voting outcomes, obtained by running the regression specified in Equation (2.11) with *Support Rate* as the outcome variable. According to the specification in Column 2, the average support rate on emission-related shareholder proposals decreases by 12.78 percentage points after the adoption of state-level targets. This treatment effect is economically significant, given the sample mean of 31.38%. The estimated dynamic treatment effects by year and their 95% confidence intervals are presented in Figure 1.2, Panel (c).

Columns 3-6 present pre-post comparisons of support rates within the treatment and control groups around 2019, respectively. The evidence indicates a decrease in the support rate within the treatment group following the adoption of state-level targets. In contrast, the support rate within the control group increases after 2019, possibly reflecting growing awareness and concerns about climate issues.

I conduct a series of additional tests. First, I examine the voting behaviour of different types of investors. In Table A1.6, I interact the treatment dummy with *Socially Responsible Fund*, which indicates whether a mutual fund family is socially responsible based on fund names. I find that the state-level target adoption reduces voting support from both socially responsible mutual funds and other mutual funds, although the former exhibits a more moderate change. In Table A1.7, I test whether my results are driven by different state-level COVID-19 policies, which may be correlated with state-level climate policies. After controlling for two alternative measures of lockdown durations, my results remain robust, and the COVID-19 policy measures show no correlation with emission-related shareholder proposals. In Table A1.8, I examine whether state-level emissions reduction targets have spill-over effects on shareholder proposals related to other social and environmental issues. I find no changes in social proposals, whereas the proportion of proposals related to non-emission-related environmental issues significantly increases. This result may be explained by either shareholders re-allocating their focus to other environmental issues or shifting to other environmental issues to signal their pro-environmental commitments. Finally, to address the concern that some firms have facilities located in different states from their headquarters, exposing them to local climate policies, I re-conduct my analyses excluding firms with facilities in different states from their headquarters. The results are robust, as shown in Table A1.9.

Overall, the results indicate that state-level emissions reduction targets seem to crowd out private-sector intrinsic motives for reducing corporate emissions. This aligns with the theoretical predictions by Bénabou and Tirole (2006) and Andreoni (1988), which suggest that extrinsic incentives and public provision can crowd out reputational and altruistic motives for prosocial behaviours.

1.5.2 Emissions

Given the crowding-out effect of state-level emissions reduction targets on private-sector prosocial motives, the overall effectiveness of these policies in reducing corporate emissions remains uncertain. The outcome depends on the comparative strengths of the direct policy effect on corporate emissions and the crowding-out effect on private-sector motives. To assess the overall effectiveness of state-level targets in reducing corporate emissions, I conduct a difference-in-differences analysis of facility-level GHG emissions using Equation (2.11), with the dependent variable replaced by emissions and firm fixed effects replaced by facility fixed effects.

Table 2.1 presents the regression results of facility-level GHG emissions. The estimated coefficient of $Treated$ is insignificantly positive, suggesting that state-level climate policies are ineffective in reducing corporate emissions.

For robustness, I also conduct DID analyses of emissions at the state level. The two proxies for state-level corporate emissions, *Emission GHGRP* and *Emission EIA*, are regressed on the treatment variable, state-level GDP, year fixed effects, and state fixed effects. The results, reported in Table 1.5, show that the treatment effect is insignificantly positive, further supporting the conclusion that state-level emissions reduction targets are ineffective in reducing corporate emissions.

Panels (a) and (b) in Figure 1.3 show the estimated dynamic treatment effects by year and their 95% confidence intervals from the DID regressions of facility-level emissions. Panels (c) and (d) show those of state-level emissions. The parallel trend assumption is satisfied for all tests as there is no evidence of a pre-existing trend, especially an upward one, before the treatment. Consistent with the notion of policy ineffectiveness, all four panels show no evidence of emissions reduction post-treatment. On the opposite, post-treatment emissions are slightly higher than pre-treatment, although the differences are insignificant.

Collectively, these findings suggest that there is no evidence of an overall decline in aggregate emissions after the adoption of state-level targets, indicating that the intended goal of these policies is not achieved.

As theory predicts and the results above suggest, the treatment effect of state-level targets arises from two opposing forces—their direct impact and their crowding-out effect—so the net effect should vary among individual facilities based on their relative sensitivities to regulatory and shareholder pressures. The evidence shown in Table 1.6, although mostly insignificant, seems to support this prediction. First, I categorize facilities owned by public parent firms into two groups based on ex-ante shareholder pressure, measured by the number of GHG emission-related proposals their parent firms receive in the three years preceding 2019. Consistent with the notion that facilities under greater ex-ante shareholder pressure experience a stronger crowding-out effect, facilities receiving zero proposals before 2019 reduce emissions post-treatment, while those receiving at least one proposal increase emissions. Notably, the average emission level of the latter group is about three times higher than that of the former, implying that shareholder engagement on emissions-related issues tends to focus on high emitters.

Second, I use an alternative measure of ex-ante shareholder pressure—the average number of analysts following the parent firm over the three years before the treatment—to divide the public firm-owned facilities into halves. Consistent with prediction, facilities whose parent firms are followed by more analysts increase emissions post-treatment, while the remaining facilities reduce emissions. Finally, I compare facilities owned by public parent firms with those owned by private firms, for which the theoretical prediction is less clear. The results show that facilities owned by private firms reduce emissions, while those owned by public firms increase emissions. This finding aligns with the idea that managers of private firms, who have stronger incentives to maximize firm value, are more sensitive to environmental liability risks (Bellon, 2024) and thus more responsive to policy changes. Overall, these heterogeneity tests demonstrate how the net effect of state-level targets reflects the balance between their direct impact and their crowding-out effect.

1.5.3 The Impacts of Paris Agreement

Climate policies, or any other public policies, are not inherently ineffective. Whether a public policy can achieve its intended goal is determined by how much extrinsic incentives it provides and how it interacts with the existing reputational and altruistic incentives of shareholders. A policy is likely to be effective if the extrinsic incentives dominate any crowding-out effects or if the public policy actually crowds in private-sector prosocial motives. For the last set of tests in this paper, I analyse how the Paris Agreement interacts with private-sector motives and the policy's effectiveness in reducing corporate emissions. Since the Paris Agreement applies to the entire economy, a DID analysis for causal inference is unviable. I perform pre-post comparisons between the three years after the signing of the Paris Agreement (2016-2018) with the three years before (2013-2015) for suggestive evidence.

First, I examine the changes in private-sector intrinsic motives for emissions reduction following the signing of the Paris Agreement in 2016. As shown in Panel A of Table 1.7, the likelihood of firms receiving an emission-related shareholder proposal in a given year increases by 1.08 percentage points. Furthermore, the proportion of emission-related proposals among all shareholder proposals rises by 6.04 percentage points, and the voting support rate for these proposals increases by 10.77 percentage points (reported in Panel B). These results are both statistically and economically significant. Overall, the evidence suggests that the Paris Agreement has a crowding-in effect on private-sector prosocial motives, consistent with the "publicity" channel described in Bénabou and Tirole (2006). Given that the Paris Agreement is a landmark accord bringing together nearly all nations for the first time to combat climate change, it is unsurprising that the Paris Agreement raises firms' awareness of climate issues and their intrinsic motives for reducing corporate emissions.

Second, I assess the changes in actual emissions following the Paris Agreement. Since the Agreement appears to crowd in private-sector motives, I hypothesize that facilities would reduce emissions after its signing. Consistent with this hypothesis, as shown in Panel C, the average facility reduces emissions by 0.04 million metric tons of CO_2e, approximately one-tenth of the sample mean of 0.42 million metric tons.

While these pre-post comparisons are not necessarily causal due to the potential influence of confounding factors, they nonetheless provide suggestive evidence that public policies, such as the Paris Agreement, can crowd in private-sector motives and reduce externalities.

1.6 Conclusion

This paper examines the relationship between public policy and private-sector prosocial motives and behaviours, focusing on the impact of state-level GHG emissions reduction targets. By analysing firms' likelihood of receiving emission-related shareholder proposals and voting support rates, the findings reveal a crowding-out effect of these state-level policies on private-sector intrinsic motives for reducing corporate emissions. Additionally, I find no evidence that these state-level policies reduce corporate emissions as intended.

By showing the crowding-out effect of public policy on private-sector motives, this paper underscores the complexity of interactions between public and private efforts toward the common good. However, it does not imply that public policies are inherently ineffective or unwarranted. Rather, it serves as a caution that public policies may prove ineffective or even counter-productive if their direct impact is weak relative to any unintended side effects. In other words, the effectiveness of public policies does not always follow the simple logic that "every little bit helps". Hypothetically, if the state-level emissions reduction targets were more ambitious and created stronger extrinsic incentives that outweighed their crowding-out effect, or if they enhanced the visibility of firms' actions and thus bolstered private-sector reputational motives through the publicity channel, these policies could potentially reduce corporate emissions effectively.

This paper focuses on the interaction between public policy and private-sector commitments in the context of GHG emissions. Extending this analysis to other common goods remains a promising area for research. Additionally, it should be noted that this paper does not specifically identify whether the crowding-out effect of state-level targets pertains to reputational or altruistic motives, nor does it distinguish between the motives of shareholders and those of decision-makers within firms, such as managers and directors. Moreover, the interplay of prosocial motives between different parties may also shed light on other phenomena, such as the recent ESG backlash. Although these questions are inherently complex and demand innovative methods to disentangle and measure specific motives, they present exciting directions for future research.

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Figure 1.1: Firms' Motives for Emission Reduction

This diagram illustrates the various motives driving firms to reduce GHG emissions and how these motives are influenced by climate policy. Firms may be driven by three types of motives: (1) Extrinsic incentives, such as subsidies or taxes tied to emissions mandated by climate policy; (2) Reputation, which can hold both affective and instrumental values; and (3) Altruism, where firms are willing to sacrifice some financial returns for the benefit of a livable future for all. Climate policy influences these motives through several channels. First, it directly provides extrinsic incentives for reducing emissions. Second, it may either crowd out reputational motives by obscuring the true intent behind emissions reduction efforts (i.e., increasing the noise-to-signal ratio) or crowd them in by raising the publicity of firms' actions. Lastly, for altruistic motives, if shareholders expect climate policies to compel their portfolio firms to cut emissions, their altruistic efforts may be crowded out, as the perceived need for their engagement decreases.

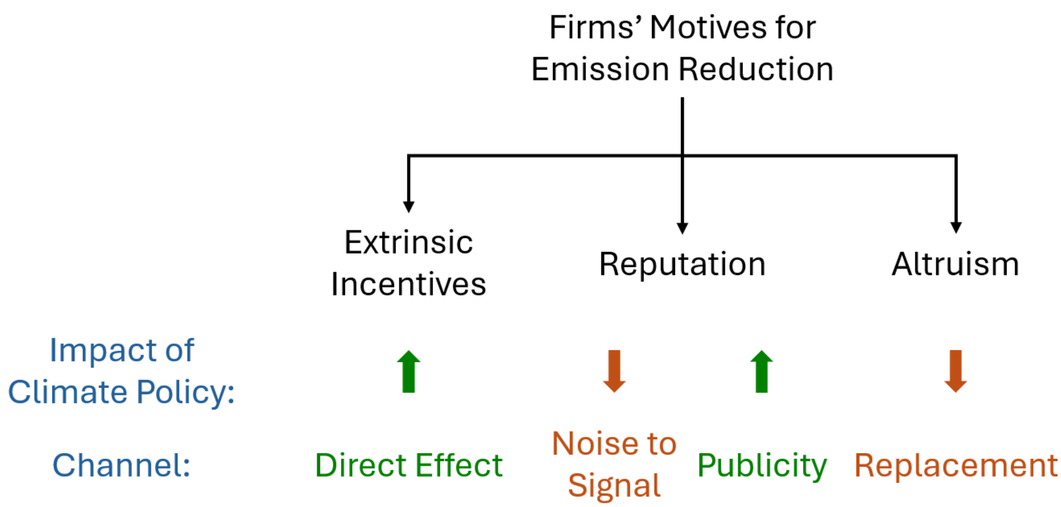
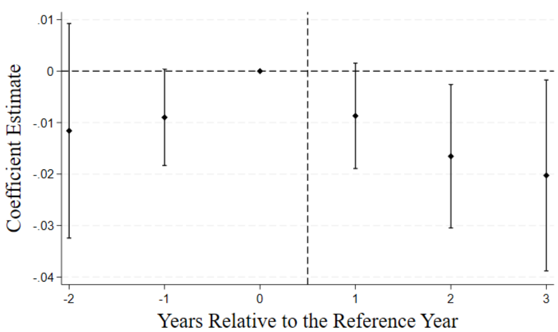


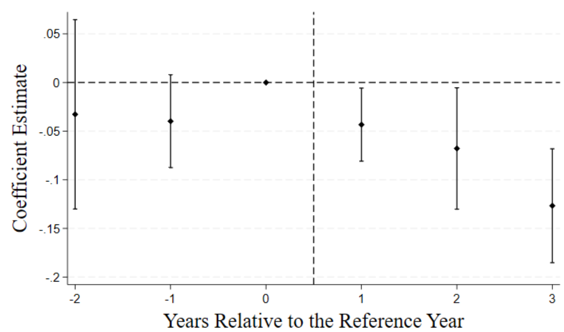
Figure 1.2: The Likelihood of Receiving Emission-Related Proposals and Support Rates

These graphs depict the coefficients and their 95% confidence intervals from the TWFE difference-in-differences regressions of *GHG Proposal Dummy*, *GHG Proposal Ratio*, and *Support Rate*. The *GHG Proposal Dummy* equals 1 if a firm receives one or more GHG emission-related shareholder proposals in a given year, and 0 otherwise. The *GHG Proposal Ratio* represents the proportion of GHG emission-related shareholder proposals relative to the total number of shareholder proposals a firm receives in a given year. The *Support Rate* is the voting result rate on an emission-related proposal. Firms headquartered in the nine states that adopted state-level targets in 2019 constitute the treatment group, while the control group includes firms headquartered in the twenty-five states that have never adopted such targets. The sample period spans from 2016 to 2021, with 2018 set as the reference year.

(a) GHG Proposal Dummy



(b) GHG Proposal Ratio



(c) Support Rate

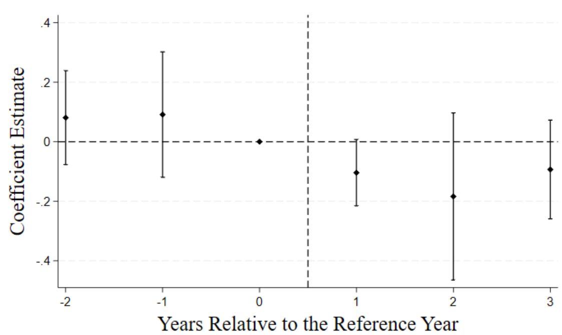
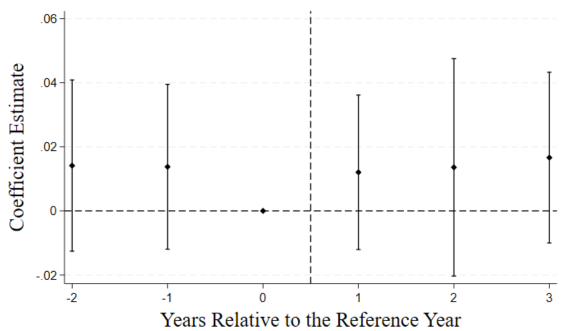


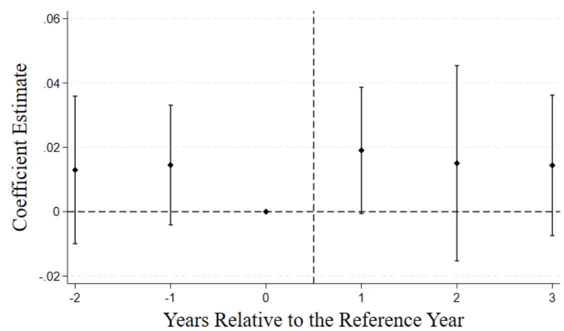
Figure 1.3: Facility-Level and State-Level GHG Emissions

These graphs depict the coefficients and their 95% confidence intervals from the TWFE difference-in-differences regressions of facility-level and state-level GHG emissions. In the facility-level analyses, the treatment group consists of facilities located in the nine states that adopted state-level targets in 2019, while the control group includes facilities in the twenty-five states that have never adopted such targets. For the state-level analyses, these nine states and twenty-five states serve as the treated and control groups, respectively. The sample period spans from 2016 to 2021, with 2018 set as the reference year. Panels (a) and (b) present results for facility-level emissions using GHGRP data, with Panel (b) focusing on a balanced subsample of facilities that are consistently present throughout the entire period. Panels (c) and (d) show results for state-level emissions, using data from either GHGRP or EIA sources.

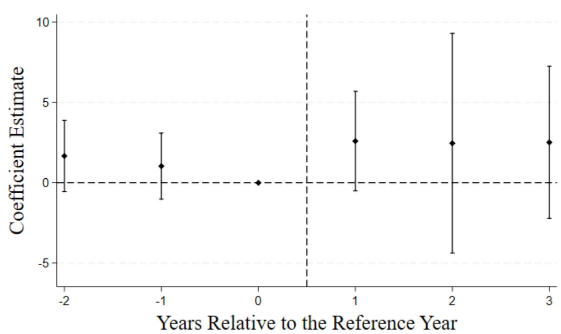
(a) Facility-Level Emission



(b) Facility-Level Emission (Balanced)



(c) State-Level Emission by GHGRP



(d) State-Level Emission by EIA

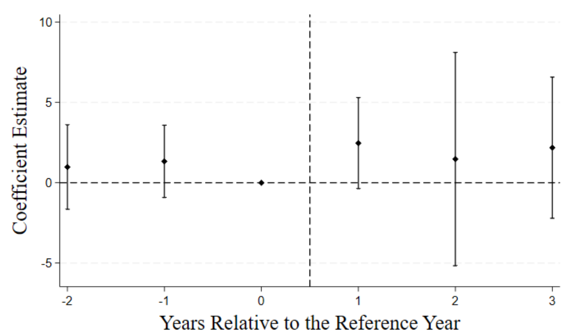
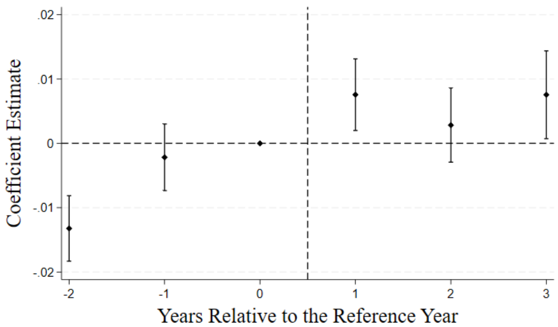


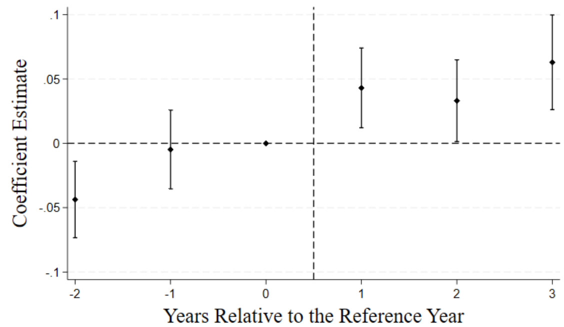
Figure 1.4: Pre-Post Comparisons Around the Paris Agreement

These graphs illustrate the coefficients and their 95% confidence intervals from the pre-post comparisons around the Paris Agreement signed in 2016. The sample period is from 2013 to 2018, with 2015 set as the reference year. Panels (a)-(d) present results for *GHG Proposal Dummy*, *GHG Proposal Ratio*, *Support Rate*, and *Facility-Level Emission*, respectively.

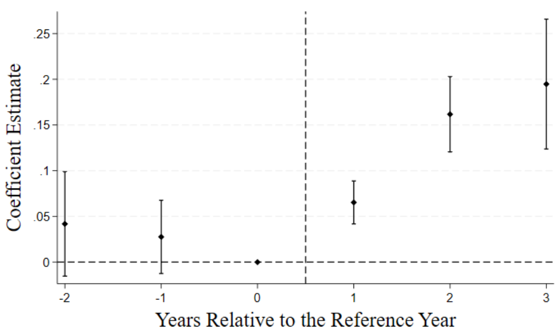
(a) GHG Proposal Dummy



(b) GHG Proposal Ratio



(c) Support Rate



(d) Facility-Level Emission

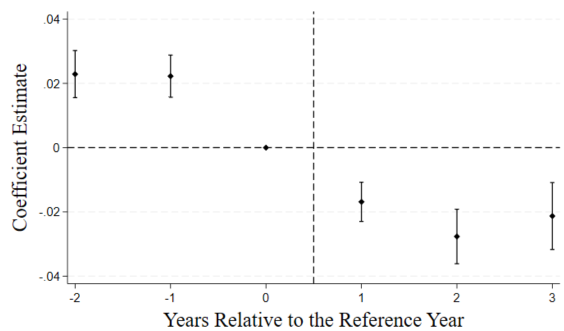


Table 1.1: Descriptive Statistics

This table reports the descriptive statistics for outcome and control variables. Panel A covers the period from 2016 to 2021, which corresponds to the primary datasets used for analyzing the impacts of state-level emission reduction targets. Panel B spans from 2013 to 2018, the period used for pre-post analyses of the Paris Agreement. Detailed descriptions and data sources for these variables are provided in Table A1.2.

Panel A: Descriptive statistics for the primary sample (2016 - 2021)

	Mean	Median	SD	25th Perc.	75th Perc.	Min	Max	Skewness	Observations	Level	Unit
Governance-Related Variables											
GHG Proposal Dummy	0.0216	0	0.1454	0	0	0	1	6.5804	10274	Firm-Year	Dummy
GHG Proposal Ratio	0.1077	0	0.2708	0	0	0	1	2.5489	1140	Firm-Year	Ratio
Support Rate	0.3138	0.3022	0.1860	0.147	0.4301	0.01264	0.7937	0.3462	100	Proposal	Ratio
Emissions											
Emission	0.4525	0.06846	1.3576	0.03359	0.2013	0	21	6.0894	26637	Facility-Year	Million $MTCO_2e$
Emission GHGRP	59.1566	41.08	66.2584	26.99	75.57	0.2288	389.7	3.3567	204	State-Year	Million $MTCO_2e$
Emission EIA	63.4879	47.26	74.5430	29.19	73.65	0.8974	450.4	3.7194	204	State-Year	Million $MTCO_2e$
Control Variables											
AR	-0.0230	-0.06451	0.6892	-0.2661	0.1295	-5.181	34.3	16.9058	10274	Firm-Year	Ratio
Asset (ln)	7.0782	7.244	2.2033	5.687	8.614	-1.324	13.71	-0.2204	10274	Firm-Year	ln(Million $)
Leverage	0.3168	0.2876	0.3005	0.1038	0.459	0	9.207	6.0895	10274	Firm-Year	Ratio
ROA	0.0185	0.02556	1.9749	-0.02847	0.06549	-9.726	125.2	49.9629	10274	Firm-Year	Ratio
MB	3.8718	1.168	128.4750	0.8223	1.87	0.01366	12253	87.0028	10274	Firm-Year	Ratio
GDP	0.3227	0.185	0.3888	0.07637	0.3338	0.03627	1.815	2.2243	204	State-Year	Trillions of Chained 2012 $

Panel B: Descriptive statistics for the Paris Agreement sample (2013 - 2018)

	Mean	Median	SD	25th Perc.	75th Perc.	Min	Max	Skewness	Observations	Level	Unit
Governance-Related Variables											
GHG Proposal Dummy	0.0178	0	0.1322	0	0	0	1	7.2965	16754	Firm-Year	Dummy
GHG Proposal Ratio	0.0855	0	0.2349	0	0	0	1	2.9820	1750	Firm-Year	Ratio
Support Rate	0.2415	0.2394	0.1398	0.1087	0.3286	0.005155	0.657	0.4029	168	Proposal	Ratio
Emissions											
Emission	0.4229	0.06263	1.3490	0.03109	0.1766	-0.001196	22.29	6.5669	40866	Facility-Year	Million $MTCO_2e$
Control Variables											
AR	-0.0155	-0.04947	0.5119	-0.254	0.1508	-7.823	7.484	2.5732	16754	Firm-Year	Ratio
Asset (ln)	6.8121	6.878	2.1535	5.354	8.286	-0.8604	14.45	-0.0064	16754	Firm-Year	ln(Million $)

Continued on next page

Table 1.1 continued

Leverage	0.2626	0.2246	0.2705	0.0362	0.4032	0	9.207	4.5937	16754	Firm-Year	Ratio
ROA	0.0316	0.02941	3.0316	-0.02372	0.06864	-9.726	226.4	62.0711	16754	Firm-Year	Ratio
MB	3.5810	1.254	105.8765	0.8641	2.046	0.02403	12253	97.7820	16754	Firm-Year	Ratio

Table 1.2: The Likelihood of Receiving GHG Emission-Related Proposals

This table presents regression estimates on how a firm's likelihood of receiving GHG emission-related shareholder proposals is affected by the adoption of state-level GHG emissions reduction targets. Firms headquartered in the nine states that adopted state-level targets in 2019 constitute the treatment group, while those in the twenty-five states that have never adopted such targets serve as the control group. In Columns 1 through 4, the dependent variable *GHG Proposal Dummy* equals 1 if a firm receives one or more GHG emission-related shareholder proposals in a given year, and 0 otherwise. Columns 1 and 2 use the full sample, while Columns 3 and 4 use a subsample limited to firms that received at least one shareholder proposal during the ISS Proposal database period (2006–2021). The dependent variable in Columns 5 and 6, *GHG Proposal Ratio*, represents the proportion of GHG emission-related shareholder proposals relative to the total number of shareholder proposals a firm receives in a given year. These columns use a subsample of firm-years with at least one shareholder proposal, as this condition is necessary for the dependent variable as a fraction to be meaningful. Panel A presents the results from the difference-in-differences estimation over the 2016–2021 period. The variable *Treated* is a dummy variable set to 1 if the firm's headquartered state has adopted a state-level target by the given year, and 0 otherwise. Panels B and C show within-group pre-post comparisons around 2019 for the treatment and control groups, respectively. While untabulated, Columns 2, 4, and 6 in Panels B and C include the same control variables as in Panel A. In these panels, *Post* is a dummy variable set to 0 for years prior to 2019 and 1 for 2019 and later. Standard errors, adjusted for clustering at the state level, are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. All control variables are defined in Table A1.2.

Dep. Var. =	*GHG Proposal Dummy*				*GHG Proposal Ratio*	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Difference-in-differences estimation for treatments in 2019						
Treated	-0.0083**	-0.0081**	-0.0207*	-0.0212*	-0.0541***	-0.0544***
	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)	(0.02)
AR_{t-1}		-0.0013		-0.0102		-0.0254*
		(0.00)		(0.01)		(0.01)
$Asset_{t-1}$		0.0082**		0.0434**		-0.0087
		(0.00)		(0.02)		(0.04)
$Leverage_{t-1}$		0.0061		0.0381		-0.0284
		(0.01)		(0.06)		(0.10)
ROA_{t-1}		-0.0002**		0.0027		-0.1409
		(0.00)		(0.05)		(0.11)
MB_{t-1}		0.0000***		0.0040**		-0.0011
		(0.00)		(0.00)		(0.01)
Observations	10274	10274	3529	3529	1140	1140
Adj. R^2	0.2493	0.2493	0.2381	0.2393	0.4313	0.4298
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full Sample		$\#(\text{Proposals})_i \geq 1$		$\#(\text{Proposals})_{it} \geq 1$	
Panel B: Pre-post comparison within the treatment group around 2019						
Post	-0.0108***	-0.0110***	-0.0276***	-0.0292***	-0.0371**	-0.0318***
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	4083	4083	1464	1464	473	473
Adj. R^2	0.2345	0.2334	0.2334	0.2319	0.5172	0.5156
Controls	No	Yes	No	Yes	No	Yes

Continued on next page

Table 1.2 continued

Dep. Var. =	*GHG Proposal Dummy*				*GHG Proposal Ratio*	
	(1)	(2)	(3)	(4)	(5)	(6)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full Sample		#(Proposals)$_i$ ≥1		#(Proposals)$_{it}$ ≥1	
Panel C: Pre-post comparison within the control group around 2019						
Post	-0.0026	-0.0052	-0.0070	-0.0209*	0.0160	0.0148
	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)	(0.01)
Observations	6191	6191	2065	2065	667	667
Adj. R^2	0.2558	0.2560	0.2382	0.2410	0.3793	0.3752
Controls	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full Sample		#(Proposals)$_i$ ≥1		#(Proposals)$_{it}$ ≥1	

Table 1.3: Voting Results on GHG Emission-Related Proposals

This table presents regression estimates on how the voting results on a firm's GHG emission-related shareholder proposals are influenced by the adoption of state-level GHG emissions reduction targets. Firms headquartered in the nine states that adopted state-level targets in 2019 constitute the treatment group, while those in the twenty-five states that have never adopted such targets serve as the control group. The dependent variable, *Support Rate*, is the support rate for a proposal. Columns 1 and 2 show results from the difference-in-differences estimation over the period from 2016 to 2021. The variable *Treated* is a dummy variable set to 1 if the firm's headquarter state has adopted a state-level target by the given year, and 0 otherwise. Columns 3-4 and 5-6 present within-group pre-post comparisons around 2019 for the treatment and control groups, respectively. In these columns, *Post* is a dummy variable set to 0 for years prior to 2019 and 1 for 2019 and later. Standard errors, adjusted for clustering at the state level, are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. All control variables are defined in Table A1.2.

Dep. Var. =	*Support Rate*					
Sample =	*Full Sample*		*Treatment Group*		*Control Group*	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.1878**	-0.1278***				
	(0.08)	(0.04)				
Post			-0.1444**	-0.2062**	0.2084***	0.2196***
			(0.05)	(0.05)	(0.01)	(0.02)
AR_{t-1}		-0.0183		0.1585		-0.1530*
		(0.03)		(0.22)		(0.07)
$Asset_{t-1}$		-0.1996		-0.1169		-0.0803
		(0.16)		(0.07)		(0.33)
$Leverage_{t-1}$		-0.4684		2.2818		0.6706
		(0.32)		(1.42)		(0.66)
ROA_{t-1}		-0.4086		0.3039		-0.3822
		(0.33)		(1.35)		(0.51)
MB_{t-1}		-0.1875**		-0.3093**		0.1793
		(0.07)		(0.10)		(0.12)
Observations	100	100	25	25	75	75
Adj. R^2	0.5482	0.5401	0.5884	0.6437	0.2653	0.2497
Year FE	Yes	Yes	No	No	No	No
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.4: Facility-level GHG Emissions

This table presents regression estimates on the impact of state-level GHG emissions reduction targets on facility-level GHG emissions. Facilities located in the nine states that adopted state-level targets in 2019 are designated as the treatment group, while the control group consists of facilities in the twenty-five states that have never adopted such targets. The sample period spans from 2016 to 2021. The dependent variable is *ln(Emission)* in Columns 1-3 and *Emission* in Columns 4-6. The variable *Treated* is a dummy variable set to 1 if the state where the facility is located has adopted a state-level target by the given year, and 0 otherwise. Columns 1 and 4 include all facilities with available data, while the remaining columns use a balanced subsample of facilities that are consistently present throughout the period. Standard errors, adjusted for clustering at the state level, are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. All control variables are defined in Table A1.2.

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var. =	*ln(Emission)*			*Emission*		
Treated	-0.0780***	-0.0797***	-0.0512	0.0049	0.0070	0.0381
	(0.02)	(0.02)	(0.03)	(0.01)	(0.01)	(0.03)
AR_{t-1}			-0.0058			-0.0014
			(0.01)			(0.01)
$Asset_{t-1}$			0.0476***			0.0230*
			(0.02)			(0.01)
$Leverage_{t-1}$			0.1673			0.0974
			(0.15)			(0.06)
ROA_{t-1}			0.0069			-0.1707*
			(0.09)			(0.09)
MB_{t-1}			0.0002***			0.0000
			(0.00)			(0.00)
Observations	26558	23683	8289	26637	23718	8300
Adj. R^2	0.9052	0.9183	0.9383	0.9589	0.9674	0.9662
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Facility FE	Yes	Yes	Yes	Yes	Yes	Yes
Balanced	No	Yes	Yes	No	Yes	Yes

Table 1.5: State-Level GHG Emissions

This table presents difference-in-differences regression estimates on the effect of state-level GHG emissions reduction targets on state-level aggregate GHG emissions. The treatment group consists of the nine states that adopted state-level targets in 2019, while the control group comprises the twenty-five states that have never adopted such targets. The sample period spans from 2016 to 2021. The dependent variable in Columns 1 and 2 is *Emission GHGRP*, calculated by aggregating facility-level emissions from the Greenhouse Gas Reporting Program (GHGRP) of the US EPA. In Columns 3 and 4, the dependent variable is *Emission EIA*, which measures state-level energy-related carbon emissions reported by the US EIA. The variable *Treated* is a dummy variable set to 1 if the state has adopted a state-level target by the given year, and 0 otherwise. Standard errors, adjusted for clustering at the state level, are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The control variable is defined in Table A1.2.

	(1)	(2)	(3)	(4)
Dep. Var. =	*Emission GHGRP*		*Emission EIA*	
Treated	1.6217	1.5658	1.2719	1.2575
	(2.15)	(2.16)	(1.80)	(1.79)
GDP		-41.3423**		-10.6589
		(18.33)		(23.72)
Observations	204	204	204	204
Adj. R^2	0.9964	0.9965	0.9974	0.9974
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes

Table 1.6: Heterogeneity Tests on Facility-Level GHG Emissions

This table presents the results of heterogeneity tests on how a facility's ex-ante investor pressure moderates the effect of state-level GHG emissions reduction targets on the facility's emissions. Facilities located in the nine states that adopted state-level targets in 2019 are designated as the treatment group, while the control group consists of facilities in the twenty-five states that have never adopted such targets. The sample period spans from 2016 to 2021. The dependent variable is *Emission* in all three panels. The variable *Treated* is a dummy variable set to 1 if the state where the facility is located has adopted a state-level target by the given year, and 0 otherwise. In Panel A, facilities of public parent firms are categorized into two groups based on *#(Proposals)*, the total number of GHG emission-related shareholder proposals received by the parent firm during the three years before the treatment, i.e., 2016-2018. Columns 1 and 2 report results for facilities whose parent firms received zero or at least one such proposal, respectively. This panel also reports the means and standard deviations of *Emission* and *#(Proposals)* for each group. In Panel B, facilities of public parent firms are divided into two groups according to *#(Analysts)*, the average number of analysts following each parent firm over the three years before the treatment, i.e., 2016-2018. Columns 1 and 2 display results for facilities with parent firms followed by below- or above-median numbers of analysts, respectively. This panel also reports the means and standard deviations of *Emission* and *#(Analysts)* for each group. In Panel C, facilities are categorized based on whether their parent firms are public or private. This panel also reports the mean and standard deviation of *Emission* for each group. Standard errors, adjusted for clustering at the state level, are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Facilities of public parent firms categorized by the number of GHG emission-related proposals

Dep. Var. =	*Emission*	
Group =	*#(Proposals)=0*	*#(Proposals)*$\geq$ 1
	(1)	(2)
Treated	-0.0353*	0.0945
	(0.02)	(0.06)
Observations	5017	3229
Adj. R^2	0.9589	0.9636
Year FE	Yes	Yes
Facility FE	Yes	Yes
Mean(*#(Proposals)*)	0	4.3267
SD(*#(Proposals)*)	(0)	(2.47)
Mean(*Emission*)	0.2630	1.0041
SD(*Emission*)	(0.84)	(2.41)

Panel B: Facilities of public parent firms categorized by the number of analysts following

Dep. Var. =	*Emission*	
Group =	*#(Analysts) Below Median*	*#(Analysts) Above Median*
	(1)	(2)
Treated	-0.0094	0.0689*
	(0.02)	(0.04)
Observations	4284	3984
Adj. R^2	0.9645	0.9636
Year FE	Yes	Yes
Facility FE	Yes	Yes
Mean(*#(Analysts)*)	48.5489	87.0731
SD(*#(Analysts)*)	(22.27)	(31.38)
Mean(*Emission*)	0.5235	0.5835

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Table 1.6 continued

SD(*Emission*)	(1.63)	(1.74)

Panel C: Facilities of private versus public parent firms

Dep. Var. =	*Emission*	
Group =	*Facilities of Private Parent Firms*	*Facilities of Public Parent Firms*
	(1)	(2)
Treated	-0.0018	0.0215
	(0.01)	(0.03)
Observations	15450	8268
Adj. R^2	0.9709	0.9640
Year FE	Yes	Yes
Facility FE	Yes	Yes
Mean(*Emission*)	0.4442	0.5524
SD(*Emission*)	(1.22)	(1.68)

Table 1.7: Pre-Post Comparisons Around the Paris Agreement

This table presents pre-post analyses of how shareholder pressure and facility-level GHG emissions respond to the Paris Agreement, agreed upon in December 2015 and formally signed in April 2016. The sample period spans from 2013 to 2018, with 2016 considered the treatment year. For all three panels, *Post* is a dummy variable set to 0 for years prior to 2016 and 1 for 2016 and later. In Panel A, the dependent variable in Columns 1 through 4, *GHG Proposal Dummy*, equals 1 if a firm receives one or more GHG emission-related shareholder proposals in a given year and 0 otherwise. Columns 1 and 2 use the full sample, while Columns 3 and 4 use a subsample limited to firms that received at least one shareholder proposal during the ISS Proposal database period (2006–2021). The dependent variable in Columns 5 and 6, *GHG Proposal Ratio*, represents the proportion of GHG emission-related shareholder proposals relative to the total number of shareholder proposals a firm receives in a given year. These columns use a subsample of firm-years with at least one shareholder proposal, as this condition is necessary for the dependent variable as a fraction to be meaningful. The dependent variables in Panel B and Panel C are the voting *Support Rate* on emission-related shareholder proposals and facility-level *Emissions*, respectively. As noted in the table, some specifications include untabulated control variables, consistent with those in Panel A of Table 1.2. Standard errors, adjusted for clustering at the state level, are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. All control variables are defined in Table A1.2.

Panel A: The likelihood of receiving GHG emission-related proposals

Dep. Var. =	*GHG Proposal Dummy*				*GHG Proposal Ratio*	
	(1)	(2)	(3)	(4)	(5)	(6)
Post	0.0108***	0.0101***	0.0286***	0.0258***	0.0604***	0.0611***
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	16754	16754	5757	5757	1750	1750
Adj. R^2	0.3445	0.3443	0.3426	0.3422	0.3537	0.3517
Controls	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full Sample		#(Proposals)$_i$ $\geq$1		#(Proposals)$_{it}$ $\geq$1	

Panel B: Voting results on GHG emission-related proposals

Dep. Var. =	*Support Rate*	
	(1)	(2)
Post	0.1077***	0.1141***
	(0.02)	(0.02)
Observations	168	168
Adj. R^2	0.3905	0.3788
Controls	No	Yes
Firm FE	Yes	Yes

Panel C: Facility-level GHG emissions

Dep. Var. =	*Emission*		
	(1)	(2)	(3)
Post	-0.0367***	-0.0380***	-0.0640***
	(0.00)	(0.00)	(0.01)
Observations	40866	34890	9851
Adj. R^2	0.9630	0.9646	0.9704
Controls	No	No	Yes
Firm FE	Yes	Yes	Yes
Balanced	No	Yes	Yes

Table A1.1: State-Level GHG Emissions Reduction Targets

This table summarizes state-level GHG emissions reduction targets, primarily sourced from Korganbekova (2024) and cross-verified with original laws, mandates, and additional references (Shields, 2023; Center for Climate and Energy Solutions, 2023). As of September 2023, twenty-six states have adopted economy-wide targets for reducing GHG emissions.

#	State	State Code	Adoption Year	Title and Number of the Statute/Order	Legal Status	Target
1	California	CA	2006	Global Warming Solutions Act (AB 32)	Statute	10% below 1990 levels by 2020, 80% below 2001 levels by 2050
2	Iowa	IA	2007	Generation Performance Standards (455B)	Executive Order	50% -90% below 2005 levels by 2050
3	Minnesota	MN	2007	Next Generation Energy Act (HF436)	Statute	15% below 2005 levels by 2015, 30% below 2005 levels by 2025, 80% below 2005 levels by 2050
4	New Jersey	NJ	2007	Global Warming Response Act (C.26:2C-37)	Statute	to 1990 levels by 2020, 80% below 2006 levels by 2050
5	Oregon	OR	2007	Global Warming Actions (Act HB 3543)	Statute	10% below 1990 levels by 2020, 75% below 1990 levels by 2050
6	Hawaii	HI	2007	A Bill for an Act Relating to Greenhouse Gas Emissions (Act 234)	Statute	to 1990 levels by 2020
7	Connecticut	CT	2008	Global Warming Solutions Act (N 08-98)	Statute	10% below 1990 levels by 2020, 80% below 2001 levels by 2050
8	Massachusetts	MA	2008	Global Warming Solutions Act (Chapter 298)	Statute	10-25% below 1990 levels by 2020, 80% below 1990 levels by 2050
9	Maryland	MD	2009	Greenhouse Gas Reduction Act (Chapter 171)	Statute	25% below 2006 levels by 2020, up to 90% below 2006 levels by 2050
10	Delaware	DE	2014	Preparing Delaware for Emerging Climate Impacts and Seizing Economic Opportunities from Reducing Emissions (Executive Order 41)	Executive Order	30% below 2008 levels by 2030
11	Rhode Island	RI	2014	Resilient Rhode Island Act (2014-H 7904)	Statute	10% below 1990 levels by 2020, 45% below 1990 levels by 2035, 80% below 1990 levels by 2050
12	Colorado	CO	2019	Climate Action Plan to Reduce Pollution (House Bill 19-1261)	Statute	26% below 2005 levels by 2025, 50% below 2005 levels by 2030, 90% below 2005 levels by 2050

Continued on next page

Table A1.1 continued

13	Illinois	IL	2019	Joining the US Climate Alliance and Committing to the Principles of the Paris Climate Agreement (Executive Order 2019-06)	Executive Order	26-28% below 2005 levels by 2025
14	Maine	ME	2019	An Act To Achieve Carbon Neutrality in Maine by the Year 2045 (38 MRSA §576-A)	Statute	45% below 1990 levels by 2030, 80% below 1990 levels by 2050
15	Montana	MT	2019	Creating the Montana Climate Solutions Council and Joining the State of Montana to the U.S. Climate Alliance (Executive Order 8-2019)	Executive Order	net zero by 2035
16	New Mexico	NM	2019	Addressing Climate Change and Energy Waste Prevention (Executive Order 2019-003)	Executive Order	45% below 2005 levels by 2030
17	Nevada	NV	2019	Next Generation Energy Act (SB358)	Statute	28% below 2005 levels by 2025, 45% below 2005 levels by 2030, near zero by 2050
18	New York	NY	2019	Climate Leadership and Community Protection Act (A.8429)	Statute	40% below 1990 levels by 2030, net zero by 2050
19	Pennsylvania	PA	2019	Commonwealth Leadership in Addressing Climate Change and Promoting Energy Conservation and Sustainable Governance (Executive Order 2019-01)	Executive Order	26% below 2005 levels by 2025, 80% below 2005 levels by 2050
20	Wisconsin	WI	2019	Relating to Clean Energy in Wisconsin (Executive Order 38)	Executive Order	net zero by 2050
21	Louisiana	LA	2020	Climate Initiatives Task Force (Executive Order JBE 2020-18)	Executive Order	26-28% below 2005 levels by 2025, 40-50% below 2005 levels by 2030, net zero by 2050
22	Michigan	MI	2020	Building a Carbon-Neutral Michigan (Executive Order 2020-10)	Executive Order	28% below 2005 levels by 2025
23	Vermont	VT	2020	Global Warming Solutions Act (H.688 Act 153)	Statute	26% below 2005 levels by 2025, 40% below 1990 levels by 2030, 80% below 1990 levels by 2050
24	Virginia	VA	2020	Virginia Energy Plan (Senate Bill 94)	Statute	net zero by 2045
25	Washington	WA	2020	Climate Pollution Limits bill (HB 2311)	Statute	45% below 1990 levels by 2030, 70% below 1990 levels by 2040, 95% below 1990 levels by 2050
26	North Carolina	NC	2022	North Carolina's Transformation to a Clean, Equitable Economy (Executive Order 246)	Executive Order	50% below levels by 2030, net zero by 2050

Table A1.2: Definition of Variables

Variable	**Description**	**Sources**
Governance-Related Variables		
GHG Proposal Dummy	An indicator equal to one if a firm receives one or more GHG emission-related shareholder proposals in a given year, and zero otherwise	ISS - Voting Analytics - Shareholder Proposals
GHG Proposal Ratio	The ratio of the number of GHG emission-related shareholder proposals to the total number of shareholder proposals a firm receives in a given year	ISS - Voting Analytics - Shareholder Proposals
Support Rate	The support rate for a proposal, calculated based on a firm's own voting rule	ISS - Voting Analytics - Company Vote Results US
Emissions		
Emission	Facility-level GHG emissions in million metric tonnes of CO_2e	GHGRP
Emission GHGRP	State-level GHG emissions in million metric tonnes of CO_2e, aggregated from facility-level data	GHGRP
Emission EIA	State-level energy-related carbon emissions in million metric tonnes of CO_2e	EIA
Control Variables		
AR	Yearly abnormal return, calculated as the difference between compounded monthly returns and compounded fitted monthly returns, where the fitted returns are based on the four-factor model, over one calendar year	CRSP; Kenneth French's Website
Asset (ln)	Natural logarithm of the total asset	CRSP/Compustat Merged
Leverage	Book leverage, calculated as the sum of short-term and long-term debts divided by total assets	CRSP/Compustat Merged
ROA	Return on assets, calculated as net income divided by the average total assets at the beginning and end of the period	CRSP/Compustat Merged
MB	Market-to-book ratio (i.e, Tobin's Q), calculated as the market value of a firm divided by total assets	CRSP/Compustat Merged
GDP	State-level real GDP in trillions of chained 2012 dollars	Bureau of Economic Analysis
N(Analysts)	The number of analysts following a given firm, averaged over the three years before the treatment, i.e., 2016-2018	IBES - Detail History - Detail File with Actuals

Table A1.3: Categories of Emission-Related Shareholder Proposals

This table presents the categories of emission-related shareholder proposals included in the sample for Table 1.2. Panel A classifies these proposals into eight groups based on their sponsors. Panel B classifies these proposals into eight groups based on their contents.

	(1)	(2)
Panel A: Categories of Emission-Related Shareholder Proposals by Sponsors		
	Frequency	*Percent*
Socially Responsible Investors	57	20.28
Special Interest Organizations	55	19.57
Religious Investors	49	17.44
Public Pension Funds	34	12.10
Coordinated Investor Groups	26	9.25
Individual Investors	26	9.25
Investment Funds	19	6.76
Non-Financial Firms	15	5.34
Total	281	100
Panel B: Categories of Emission-Related Shareholder Proposals by Contents		
	Frequency	*Percent*
General Sustainability And Climate Reporting	74	26.33
GHG/Carbon Emissions Goals	67	23.84
Climate Change Risk Management And Scenario Analysis	54	19.22
Renewable Energy Goals	34	12.10
Climate Lobbying	20	7.12
Corporate Governance On Climate Issues	17	6.05
Climate-Related Environmental Risk Management	10	3.56
Climate Performance-Based Executive Compensation	5	1.78
Total	281	100

Table A1.4: The Likelihood of Receiving GHG Emission-Related Proposals by Non-pension Fund Submitters

This table presents regression estimates of firms' likelihood of receiving GHG emission-related shareholder proposals not submitted by pension funds. In Columns 1 through 4, the dependent variable *GHG Proposal Dummy* equals 1 if a firm receives one or more GHG emission-related shareholder proposals from non-pension fund shareholders in a given year, and 0 otherwise. The dependent variable in Columns 5 and 6, *GHG Proposal Ratio*, represents the proportion of GHG emission-related shareholder proposals from non-pension fund shareholders relative to the total number of shareholder proposals a firm receives in a given year. Other aspects of the analyses, including sample selection, variable definitions, and regression specifications, are identical to those in Table 1.2.

Dep. Var. =	*GHG Proposal Dummy*				*GHG Proposal Ratio*	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.0076*	-0.0074*	-0.0188*	-0.0187*	-0.0463**	-0.0459**
	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)	(0.02)
AR_{t-1}		-0.0007		-0.0062		-0.0216
		(0.00)		(0.01)		(0.01)
Asset_{t-1}		0.0049**		0.0260**		-0.0259
		(0.00)		(0.01)		(0.02)
Leverage_{t-1}		0.0108*		0.0739		0.0317
		(0.01)		(0.05)		(0.06)
ROA_{t-1}		-0.0001*		-0.0196		-0.2929***
		(0.00)		(0.05)		(0.08)
MB_{t-1}		0.0000***		0.0027		0.0021
		(0.00)		(0.00)		(0.01)
Observations	10274	10274	3529	3529	1140	1140
Adj. R^2	0.2575	0.2574	0.2504	0.2510	0.4267	0.4287
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full Sample		$\#(\text{Proposals})_i \geq 1$		$\#(\text{Proposals})_{it} \geq 1$	

Table A1.5: The Likelihood of Receiving GHG Emission-Related Proposals by Different Types of Submitters

This table presents regression estimates of firms' likelihood of receiving GHG emission-related shareholder proposals submitted by different types of investors. Panel A focuses on proposals submitted by individuals and special-interest investors (*Sponsor_Type* is either "special interest", "religious", or "individual" according to ISS database). The dependent variable in Columns 1 through 4, *GHG Proposal Dummy*, equals 1 if a firm receives one or more such shareholder proposals in a given year, and 0 otherwise. In Columns 5 and 6, the dependent variable, *GHG Proposal Ratio*, represents the proportion of such proposals relative to the total number of shareholder proposals a firm receives in a given year. Panel B focuses on proposals submitted by institutional investors (*Sponsor_Type* is either "SRI", "investment fund", "public pension", or "coordinated" according to ISS database) and similarly defines the dependent variables. Other aspects of the analyses, including sample selection, variable definitions, and regression specifications, are identical to those in Table 1.2.

Dep. Var. =	*GHG Proposal Dummy*				*GHG Proposal Ratio*	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: The likelihood of receiving emission-related proposals submitted by individual and special-interest investors						
Treated	0.0005	0.0006	0.0017	0.0017	-0.0043	-0.0031
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
AR_{t-1}		-0.0004		-0.0030		-0.0147
		(0.00)		(0.01)		(0.01)
$Asset_{t-1}$		0.0024		0.0102		-0.0028
		(0.00)		(0.01)		(0.01)
$Leverage_{t-1}$		0.0050		0.0424		0.0620
		(0.01)		(0.04)		(0.04)
ROA_{t-1}		-0.0000		0.0307		-0.0999
		(0.00)		(0.03)		(0.07)
MB_{t-1}		0.0000		-0.0003		0.0025
		(0.00)		(0.00)		(0.00)
Observations	10274	10274	3529	3529	1140	1140
Adj. R^2	0.2051	0.2048	0.2128	0.2124	0.3603	0.3593
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full Sample		$\#(\text{Proposals})_i \geq 1$		$\#(\text{Proposals})_{it} \geq 1$	
Panel B: The likelihood of receiving emission-related proposals submitted by institutional investors						
Treated	-0.0092*	-0.0090*	-0.0233	-0.0238	-0.0497***	-0.0513***
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
AR_{t-1}		-0.0009**		-0.0074*		-0.0107
		(0.00)		(0.00)		(0.02)
$Asset_{t-1}$		0.0059**		0.0333**		-0.0059
		(0.00)		(0.01)		(0.03)
$Leverage_{t-1}$		0.0017		0.0021		-0.0904
		(0.01)		(0.04)		(0.07)
ROA_{t-1}		-0.0002*		-0.0212		-0.0410
		(0.00)		(0.04)		(0.08)

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Table A1.5 continued

Dep. Var. =	*GHG Proposal Dummy*				*GHG Proposal Ratio*	
	(1)	(2)	(3)	(4)	(5)	(6)
MB_{t-1}		0.0000***		0.0040*		-0.0036
		(0.00)		(0.00)		(0.01)
Observations	10274	10274	3529	3529	1140	1140
Adj. R^2	0.1899	0.1897	0.1901	0.1907	0.3001	0.2977
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full Sample		$\#(\text{Proposals})_i \geq 1$		$\#(\text{Proposals})_{it} \geq 1$	

Table A1.6: Voting Decision on GHG Emission-Related Proposals Across Fund Family Types

This table presents regression estimates of voting decisions on GHG emission-related shareholder proposals across different types of mutual fund families. The dependent variable, *Vote for*, is a dummy set to 1 if a fund family supports a proposal and 0 if the fund family votes to abstain or against the proposal. When funds within the same family vote differently, the family's voting decision is determined by the majority vote within the family. The variable *Socially Responsible Fund* is a dummy variable set to 1 if the majority of funds within a family include keywords like "ESG", "social", and "environment" in their names, and 0 otherwise. The variable *Treated* is a dummy variable set to 1 if the firm's headquartered state has adopted a state-level target by the given year, and 0 otherwise. Year, firm, and fund family fixed effects are controlled for in both specifications. Standard errors, adjusted for clustering at the state level, are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. All control variables are defined in Table A1.2.

Dep. Var. =	*Vote for*	
	(1)	(2)
Treated	-0.2659*	-0.1989***
	(0.13)	(0.06)
Treated*Socially Responsible Fund	0.0901	0.0896
	(0.12)	(0.12)
Socially Responsible Fund	-0.0584	-0.0603
	(0.10)	(0.09)
AR_{t-1}		-0.1013
		(0.09)
$Asset_{t-1}$		-0.2263
		(0.16)
$Leverage_{t-1}$		-0.9833*
		(0.47)
ROA_{t-1}		-0.8983**
		(0.32)
MB_{t-1}		-0.2461**
		(0.09)
Observations	7726	7726
Adj. R^2	0.4451	0.4500
Year FE	Yes	Yes
Firm FE	Yes	Yes
Fund FE	Yes	Yes

Table A1.7: GHG Emission-Related Proposals and COVID-19 Policies

This table presents robustness test results examining whether the patterns in GHG emission-related shareholder proposals are driven by state-level emissions reduction targets or COVID-19 policies. In Panel A, the explanatory variable *Lockdown1* represents the duration of state-level stay-at-home mandates in days. In Panel B, *Lockdown2* represents the duration of non-essential business closure mandates in days. Other aspects of the analyses, including sample selection, variable definitions, and regression specifications, are identical to those in Table 1.2 and Table 1.3.

Dep. Var. =	*GHG Proposal Dummy*				*GHG Proposal Ratio*		*Support Rate*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Patterns in emission-related proposals and COVID-19 stay-at-home mandates								
Treated	-0.0082**	-0.0080**	-0.0205*	-0.0212*	-0.0595***	-0.0591***	-0.1360	-0.1247*
	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)	(0.02)	(0.10)	(0.06)
Lockdown1	-0.0001	-0.0001	-0.0002	-0.0002	0.0011	0.0010	-0.0007	-0.0004
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
AR_{t-1}		-0.0016		-0.0134		-0.0309		0.1105
		(0.00)		(0.01)		(0.02)		(0.08)
$Asset_{t-1}$		0.0081**		0.0438**		0.0185		-0.3482*
		(0.00)		(0.02)		(0.04)		(0.18)
$Leverage_{t-1}$		0.0054		0.0364		-0.0231		0.4931
		(0.01)		(0.07)		(0.10)		(0.35)
ROA_{t-1}		-0.0002*		0.0117		-0.0166		-0.1637
		(0.00)		(0.06)		(0.13)		(0.66)
MB_{t-1}		0.0000***		0.0049**		-0.0017		-0.2251*
		(0.00)		(0.00)		(0.01)		(0.11)
Observations	10274	10274	3529	3529	1140	1140	100	100
Adj. R^2	0.2522	0.2521	0.2432	0.2445	0.4153	0.4134	0.5888	0.5679
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full Sample		$\#(\text{Proposals})_i \geq 1$		$\#(\text{Proposals})_{it} \geq 1$		Proposals voted on	

Continued on next page

Table A1.7 continued

Dep. Var. =	*GHG Proposal Dummy*				*GHG Proposal Ratio*		*Support Rate*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel B: Patterns in emission-related proposals and COVID-19 non-essential business closure mandates								
Treated	-0.0071*	-0.0069*	-0.0175	-0.0182	-0.0526***	-0.0529***	-0.1716**	-0.1143***
	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)	(0.02)	(0.06)	(0.03)
Lockdown2	-0.0001	-0.0001	-0.0002	-0.0002	-0.0001	-0.0001	-0.0022	-0.0017
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
AR_{t-1}		-0.0013		-0.0103		-0.0255*		-0.0133
		(0.00)		(0.01)		(0.01)		(0.03)
$Asset_{t-1}$		0.0083**		0.0437**		-0.0088		-0.2103
		(0.00)		(0.02)		(0.04)		(0.13)
$Leverage_{t-1}$		0.0062		0.0395		-0.0287		-0.4914
		(0.01)		(0.06)		(0.10)		(0.31)
ROA_{t-1}		-0.0002**		0.0027		-0.1410		-0.3612
		(0.00)		(0.05)		(0.11)		(0.25)
MB_{t-1}		0.0000***		0.0040**		-0.0010		-0.1851***
		(0.00)		(0.00)		(0.01)		(0.06)
Observations	10274	10274	3529	3529	1140	1140	100	100
Adj. R^2	0.2479	0.2478	0.2378	0.2390	0.4201	0.4185	0.5430	0.5330
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full Sample		$\#(\text{Proposals})_i \geq 1$		$\#(\text{Proposals})_{it} \geq 1$		Proposals voted on	

Table A1.8: The Likelihood of Receiving and the Voting Results on Non-Emission-Related Environmental and Social Proposals

This table presents regression estimates of firms' likelihood of receiving non-emission-related environmental and social shareholder proposals, as well as the voting results on such proposals. In Panel A, the dependent variable in Columns 1 through 4, *GHG Proposal Dummy*, equals 1 if a firm receives one or more non-emission-related environmental proposals in a given year, and 0 otherwise. In Columns 5 and 6, the dependent variable, *GHG Proposal Ratio*, represents the proportion of non-emission-related environmental proposals relative to the total number of shareholder proposals a firm receives in a given year. In Columns 7 and 8, the dependent variable, *Support Rate*, is the support rate for non-emission-related environmental proposals that are voted on. In Panel B, the dependent variable is either the likelihood of receiving social proposals (Columns 1 through 4), the proportion of social proposals relative to the total number of shareholder proposals a firm receives (Columns 5 and 6), or the support rate for social proposals that are voted on. Other aspects of the analyses, including sample selection, variable definitions, and regression specifications, are identical to those in Table 1.2 and Table 1.3.

Dep. Var. =	*Proposal Dummy*				*Proposal Ratio*		*Support Rate*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: The likelihood of receiving and the voting results on non-emission-related environmental proposals								
Treated	0.0067	0.0070	0.0163	0.0157	0.0616**	0.0626**	-0.1159	-0.1052
	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)	(0.02)	(0.10)	(0.13)
AR_{t-1}		-0.0005		-0.0010		-0.0157		0.1105
		(0.00)		(0.01)		(0.03)		(0.08)
$Asset_{t-1}$		0.0096**		0.0491**		0.0359		-0.3482*
		(0.00)		(0.02)		(0.05)		(0.18)
$Leverage_{t-1}$		0.0026		0.0053		-0.0649		0.4931
		(0.01)		(0.06)		(0.11)		(0.35)
ROA_{t-1}		-0.0002		-0.0176		-0.0185		-0.1637
		(0.00)		(0.07)		(0.18)		(0.66)
MB_{t-1}		0.0000**		0.0055		0.0201		-0.2251*
		(0.00)		(0.01)		(0.01)		(0.11)
Observations	10274	10274	3529	3529	1140	1140	44	44
Adj. R^2	0.3896	0.3895	0.3635	0.3640	0.3741	0.3730	0.5340	0.5901
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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Table A1.8 continued

Dep. Var. =	*Proposal Dummy*				*Proposal Ratio*		*Support Rate*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full Sample		#(Proposals)$_i$ ≥1		#(Proposals)$_{it}$ ≥1		Proposals voted on	
Panel B: The likelihood of receiving and the voting results on social proposals								
Treated	-0.0007	-0.0006	-0.0012	-0.0012	-0.0177	-0.0179	-0.0008	0.0223
	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)	(0.02)	(0.06)	(0.04)
AR$_{t-1}$		-0.0004		-0.0036		0.0050		0.0090
		(0.00)		(0.00)		(0.02)		(0.03)
Asset$_{t-1}$		0.0018		0.0058		0.0062		-0.1120
		(0.00)		(0.01)		(0.03)		(0.09)
Leverage$_{t-1}$		0.0057		0.0511**		0.0106		-0.6360***
		(0.00)		(0.02)		(0.05)		(0.13)
ROA$_{t-1}$		-0.0000		0.0320		0.1399**		0.1773
		(0.00)		(0.03)		(0.06)		(0.37)
MB$_{t-1}$		0.0000		-0.0007		-0.0073		0.0280
		(0.00)		(0.00)		(0.01)		(0.03)
Observations	10274	10274	3529	3529	1140	1140	125	125
Adj. R^2	0.3016	0.3013	0.3026	0.3021	0.3574	0.3553	0.5892	0.6427
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full Sample		#(Proposals)$_i$ ≥1		#(Proposals)$_{it}$ ≥1		Proposals voted on	

Table A1.9: The Likelihood of Receiving GHG Emission-Related Proposals and the Voting Results (Firms with All Facilities in the Same State)

This table presents a robustness check regarding the likelihood of firms receiving GHG emission-related shareholder proposals and the corresponding voting outcomes, using a subsample of firms whose facilities are all in the same state as its headquarter. Other aspects of the analyses, including sample selection, variable definitions, and regression specifications, are identical to those in Table 1.2 and Table 1.3.

Dep. Var. =	*GHG Proposal Dummy*				*GHG Proposal Ratio*		*Support Rate*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	-0.0042	-0.0041	-0.0111	-0.0122	-0.0426**	-0.0392**	-0.2295***	-0.1327
	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)	(0.02)	(0.06)	(0.10)
AR_{t-1}		-0.0009		-0.0089		-0.0365*		0.0344
		(0.00)		(0.01)		(0.02)		(0.09)
$Asset_{t-1}$		0.0047		0.0280		-0.0331		0.0154
		(0.00)		(0.02)		(0.04)		(0.31)
$Leverage_{t-1}$		0.0070		0.0551		0.0567		-0.2752
		(0.01)		(0.05)		(0.08)		(0.93)
ROA_{t-1}		-0.0001		0.0270		-0.1434*		-0.1018
		(0.00)		(0.05)		(0.07)		(0.52)
MB_{t-1}		0.0000**		0.0058*		-0.0052		-0.2180
		(0.00)		(0.00)		(0.02)		(0.13)
Observations	9588	9588	3115	3115	954	954	78	78
Adj. R^2	0.2445	0.2442	0.2363	0.2367	0.3968	0.3960	0.5092	0.4685
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full Sample		$\#(\text{Proposals})_i \geq 1$		$\#(\text{Proposals})_{it} \geq 1$		Proposals voted on	

Chapter 2

To Log or Not to Log? It Depends on the Question

To Log or Not to Log?
It Depends on the Question*

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Abstract

In reduced-form regressions, should the outcome variable be log-transformed? Researchers conventionally base this decision on the outcome variable's statistical properties or the assumed functional form of the economic model. This paper highlights a frequently overlooked but critical distinction: regressing the raw outcome variable estimates average level sensitivities, whereas regressing its logarithmic transformation captures average proportional sensitivities. Notably, the average proportional change differs from the proportional change of the average, and these two specifications can sometimes even yield estimates of opposite signs. My findings underscore the importance of matching the outcome variable's form to the research question being addressed.

Keywords: Average treatment effect, Regression model, Functional form

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2.1 Introduction

Researchers frequently face the decision of whether to apply logarithmic transformations to outcome variables in ordinary least squares (OLS) regressions. Surprisingly, there remains no clear consensus on when and why this transformation is appropriate. Typical justifications include: (1) reducing the skewness of the outcome variable to improve its normality; (2) diminishing the influence of outliers, especially those with extremely large values; and (3) aligning regression models with underlying economic models that assume multiplicative relationships.

However, the validity of these justifications is debatable. First, skewness alone does not compromise the unbiasedness of OLS estimators. Furthermore, the central limit theorem guarantees asymptotic normality of estimates, ensuring valid inference. Second, whether outliers should be mitigated depends on the research context and the importance of aggregate versus typical unit outcomes. Third, while aligning the regression specification with an underlying economic model enables structural interpretation, such structural interpretation is not always essential, and the correct functional form of the underlying economic model is often uncertain.

More fundamentally, a distinction between using log-transformed and non-transformed outcome variables is often overlooked in empirical studies: these two approaches address different questions. Using a log-transformed outcome variable estimates average proportional sensitivities, whereas the non-transformed approach estimates average level sensitivities. Crucially, these two approaches can diverge substantially; under conditions of highly dispersed baseline outcomes and heterogeneous treatment effects, they may even yield estimates of opposite signs.

In this paper, I revisit conventional arguments for log transformations, clarifying their applicability. I then analytically and numerically illustrate how log versus non-log specifications capture fundamentally distinct economic relationships. Finally, to concretely demonstrate these distinctions, I evaluate the effectiveness of state-level emissions reduction targets on corporate emissions as an empirical example.

I argue that the functional form of the outcome variable should reflect the research question being asked. If the focus is on the average level (proportional) sensitivity, the outcome variable should be non-transformed (log-transformed). The former incorporates the differences in scale across units, while the latter normalises differences and effectively gives equal weight to each unit. Furthermore, if the focus is on the aggregate level, the outcome variable should remain untransformed, as the average level change is proportional to the aggregate level change, but the average proportional change and the proportional change in the aggregate do not always move in the same direction.

I use the impacts of state-level greenhouse gas (GHG) emissions reduction targets on corporate emissions as an empirical example. The convention in the literature on evaluating climate policies is log-transforming the entity-level emissions as the outcome variable (Bartram et al., 2022; Kumar and Purnanandam, 2024; Korganbekova, 2024). This regression specification answers the question of how climate policies affect a typical firm's behaviour regarding emissions. However, given the fact that climate change is driven by the aggregate level of GHG in the atmosphere, a climate policy's effectiveness in mitigating climate change can only be answered by a regression of non-transformed entity-level emissions. I illustrate that the state-level emissions reduction targets adopted in nine U.S. states in 2019 reduce entity-level emissions but have no effect in reducing aggregate emissions.

This paper contributes to the literature by highlighting that using log-transformed versus non-transformed outcome variables in regressions addresses different research questions. When assessing the unit-level behaviour in response to the explanatory factors (e.g., how a typical firm reacts to a climate policy), log-transformed outcome variables are more appropriate. In contrast, when assessing the aggregate effectiveness of public policies (e.g., whether a climate policy effectively reduces emissions from the corporate sector), non-transformed outcomes are more suitable. This recommendation for academic research parallels the caution to practitioners in Hartzmark and Shue (2023), who show that investors and ESG environmental ratings focus on percentage reductions in firm emissions rather than level reductions, neglecting differences in emissions scale and thus exerting a trivial impact on total emissions. Although the distinction in economic meanings between different outcome variables is straightforward, it is surprisingly often overlooked in regression-based studies.

2.2 Conventional Practices

In this section, I investigate the merits of logarithmic transformation in regression analyses. In many studies, the reasoning behind log-transformations remains implicit. For example, in the climate policy evaluation literature (Bartram et al., 2022; Kumar and Purnanandam, 2024; Korganbekova, 2024), researchers routinely log-transform entity-level emissions without explicitly justifying this choice. Below, I discuss three commonly cited rationales.

First, researchers frequently cite skewness in the error term distribution as justification for log transformation. However, the Gauss–Markov theorem ensures that the OLS estimator is the best linear unbiased estimator (BLUE) under standard assumptions, regardless of skewness. Normality assumptions are needed only for constructing confidence intervals and hypothesis tests. Given that most contemporary studies use samples larger than the threshold (the rule of thumb is merely 30), the central limit theorem ensures approximate normality and valid inference. Even with smaller samples or robustness concerns, bootstrapping offers a non-parametric approach to valid inference. Thus, skewness alone does not justify logarithmic transformation.

Second, some studies log-transform outcomes to reduce the influence of outliers. Reducing outlier influence may be appropriate when outliers reflect measurement errors or atypical cases, or when the research objective is to identify common behavioural patterns rather than aggregate outcomes. For instance, in analysing CEO compensation, researchers might be interested in typical responses rather than how exceptionally large pay packages change (Guthrie et al., 2012). However, discounting outliers can bias analyses when aggregate impacts matter, such as evaluating how tax reforms affect total investment or how climate policies affect aggregate emissions. Moreover, log transformation applies an arbitrary scaling that may inadequately or overly address influential outliers, as demonstrated by Guthrie et al. (2012), who show that the conclusion by Chhaochharia and Grinstein (2009) remains driven by two extreme observations despite log transformation.

Third, log transformation is sometimes employed to align regression models with economic models that assume multiplicative relationships, thus linearising the specification. However, the suitability of this reason depends on context. The correct functional form of the underlying economic model may be uncertain, and structural interpretations are not always central to empirical analyses. Hence, this justification alone does not mandate logarithmic transformations.

2.3 Difference in Economic Meanings

A fundamental difference between regressions using log-transformed versus non-transformed outcome variables is the economic meanings. Below, I provide both an analytical illustration and a numerical example to explain this distinction.

Without loss of generality, consider a simple two-group setting with equal-sized groups differing only by treatment status. The pooled OLS regression model is specified as follows to estimate the treatment effect:

$$y_i = \alpha + \beta x_i + \varepsilon_i \tag{2.1}$$

where x_i is a dummy variable equal to 1 if the observation is post-treatment (treated) and 0 if pre-treatment (untreated). α is the intercept, and ε_i is the error term. The OLS estimator of β is given by:

$$\hat{\beta} = \frac{\sum_{i=1}^{n} (x_i - \bar{x})(y_i - \bar{y})}{\sum_{i=1}^{n} (x_i - \bar{x})^2} \tag{2.2}$$

Given that x_i is binary (0 or 1), this simplifies to:

$$\hat{\beta} = \bar{y}_1 - \bar{y}_0 \tag{2.3}$$

where $\bar{y}_1$ and $\bar{y}_0$ are the arithmetic means of y_i for treated and untreated observations, respectively. Therefore, the ATE is the difference in the arithmetic means of the outcome variable between treated and untreated groups.

Now, consider the case where the outcome variable is log-transformed. The regression becomes:

$$ln(y_i) = \alpha + \beta x_i + \varepsilon_i \tag{2.4}$$

The estimator now captures the difference in the arithmetic means of $\ln(y_i)$ between treated and untreated groups:

$$\hat{\beta} = \overline{ln(y)}_1 - \overline{ln(y)}_0 \tag{2.5}$$

Since the arithmetic mean of logarithms equals the logarithm of the geometric mean, this implies:

$$\hat{\beta} = ln(\bar{y}^{GEOM})_1 - ln(\bar{y}^{GEOM})_0 \tag{2.6}$$

or equivalently,

$$e^{\hat{\beta}} = \frac{\bar{y}_1^{GEOM}}{\bar{y}_0^{GEOM}} \tag{2.7}$$

Thus, $e^{\hat{\beta}}$ represents the ratio of the geometric means of the outcome variable between treated and untreated groups, and $(e^{\hat{\beta}} - 1) * 100\%$ represents the percentage change in the geometric mean after the treatment.[1] While the geometric mean usually moves in the same direction as the arithmetic mean, it is also negatively sensitive to the dispersion of values. In some cases, these two mean values can even change in opposite directions, as illustrated in the following numerical example.

Consider a simple example with five facilities, where Columns (1) and (2) report pre-treatment and post-treatment emissions, respectively. Columns (3) and (4) show the absolute and percentage changes for each facility. The last two rows report the geometric and arithmetic means of each of the two groups. The last row also reports the arithmetic means of their changes in level and in percentage.

	(1)	(2)	(3)	(4)	(5)
Emissions	Pre-Treatment	Post-Treatment	d(Level)	d(Percentage)	$\hat{\beta}_i$
Facility 1	100	110	+10	+10%	ln(1.1)
Facility 2	80	88	+8	+10%	ln(1.1)
Facility 3	60	54	-6	-10%	ln(0.9)
Facility 4	40	36	-4	-10%	ln(0.9)
Facility 5	20	18	-2	-10%	ln(0.9)
Geometric Mean	52.10	50.81			
Arithmetic Mean	60	61.2	1.2	-2%	-0.0251

In this example, the two largest facilities increase emissions by 10%, while the other three decrease emissions by 10%, leading to greater dispersion. The geometric mean decreases from 52.10 to 50.81, while the arithmetic mean increases from 60 to 61.2. If

[1]For small values of $\hat{\beta}$, $\hat{\beta} \approx e^{\hat{\beta}} - 1$.

we run a regression with the non-transformed outcome variable, the estimated ATE is:

$$\begin{aligned} \hat{\beta} &= \bar{y}_1 - \bar{y}_0 \\ &= 1.2 \end{aligned} \tag{2.8}$$

However, if we use log-transformed emissions as the outcome variable, the estimated ATE becomes:

$$\begin{aligned} e^{\hat{\beta}} - 1 &= \frac{\bar{y}_1^{GEOM}}{\bar{y}_0^{GEOM}} - 1 \\ &= \frac{50.81}{52.10} - 1 \\ &= -2.48\% \end{aligned} \tag{2.9}$$

This demonstrates that, while the arithmetic mean increases, the ATE based on the geometric mean suggests a decrease due to the increased dispersion. In other words, after the treatment, the average emissions *level* across facilities increases, although the average *percentage* change in facility emissions is negative.

At the beginning of this appendix, I stated that the ATE with a log-transformed outcome variable represents the average individual-level percentage change. More precisely, it *approximates* the change. As demonstrated in this example, there is a small difference between the ATE of -2.48% and the average individual-level percentage change of -2%. The reason can be understood by rewriting Equation (2.5) as:

$$\begin{aligned} \hat{\beta} &= \overline{ln(y)}_1 - \overline{ln(y)}_0 \\ &= \frac{1}{n}\sum_{i=1}^{n}[ln(y_{i1}) - ln(y_{i0})] \\ \frac{1}{n}\sum_{i=1}^{n}\hat{\beta}_i \end{aligned} \tag{2.10}$$

where n is the number of facilities, and $\hat{\beta}_i$ is defined as the estimation for β solely based on the two observations of facility i, as shown in Column (5) in the table. Equation (2.10) suggests that, in regressions with log-transformed outcome variables, it is the $\hat{\beta}_i$ rather than the percentage change $(e^{\hat{\beta}_i} - 1) * 100\%$ that is averaged across individuals. The ATE is thus $(e^{\frac{1}{n}\sum_{i=1}^{n}\hat{\beta}_i} - 1) * 100\%$, rather than the average percentage change, $\frac{1}{n}\sum_{i=1}^{n}(e^{\hat{\beta}_i} - 1) * 100\%$. However, since $e^{\beta} - 1$ is approximately linear in β around $\beta = 0$, these two expressions have roughly the same value in the vicinity of zero. Therefore, it is safe to say that the ATE with a log-transformed outcome variable approximates the average percentage change, as long as the magnitudes of the changes are relatively small.

The discussion above demonstrates that, to match the research question in this paper, a non-transformed outcome variable is more appropriate for climate policy assessment.

2.4 Empirical Example

In Chapter 2, I explore the impact of state-level emissions reduction targets on corporate emissions. Departing from the common practice of using log-transformed entity-level emissions as the outcome variable (Chan and Morrow, 2019; Bartram et al., 2022; Kumar and Purnanandam, 2024; Korganbekova, 2024), I primarily employ non-transformed emissions. These two approaches have different economic implications and address different research questions. The average treatment effect (ATE) estimated using a log-transformed outcome variable approximates the average *percentage* change across entities without accounting for their differences in emissions scale. In contrast, the ATE with a non-transformed outcome variable estimates the average *level* change, which incorporates differences in emissions scale and is proportional to the treatment effect on *aggregate* level. Since climate change is driven by the *aggregate level* of GHG emissions, I choose non-transformed entity-level emissions as the outcome variable to evaluate the effectiveness of climate policies in mitigating climate change. Nonetheless, for comparison, I also report results using the conventional log-transformed approach.

Following the empirical framework in Chapter 2, I estimate the regression:

$$Y_{it} = \beta_0 + \beta_1 Treated_{it} + B_2 X_{it} + d_i + d_t + \varepsilon_{it} \tag{2.11}$$

where Y_{it} represents the GHG emissions by facility i in year t, either in logged or non-logged form. $Treated_{it}$ is a dummy variable set to 1 if the state where facility i is headquartered has adopted a state-level target by year t, and 0 otherwise. X_{it} is a vector of facility characteristics, d_i is the facility fixed effect, d_t is the year fixed effect, and ϵ_{it} is the error term adjusted for heteroskedasticity and clustered at the state level.

Previous studies examining the effects of public climate policies on corporate emissions typically employ DID estimations using logged (or log-like transformed) entity-level emissions as the dependent variable[2]. Bartram et al. (2022) evaluate the effectiveness of the California cap-and-trade program using log(1+plant-level emissions) as the dependent variable. Chan and Morrow (2019) and Kumar and Purnanandam (2024) assess the RGGI cap-and-trade policy with log(plant-level emissions) as the dependent variable. Korganbekova (2024) investigates state-level emissions reduction targets using both log(1+firm-level emissions) and log(1+facility-level emissions) as dependent variables. None of these studies explicitly justify their choice of log or log-like transformations.

Neither the log nor non-log specification is universally superior; the optimal choice depends on the research goal. If the question concerns how the typical entity responds to climate policy, a log-linear specification is more appropriate as it weighs entities equally, independent of baseline emission levels. Conversely, assessing climate policy effectiveness in mitigating overall emissions calls for a linear-linear specification with non-transformed outcomes. In this case, the average treatment effect (ATE), captured by β_1 in Equation (2.11), represents the average entity-level *level* change in the outcome

[2]Researchers often apply log-like transformations, such as log(1+Y), when Y can take on a value of zero. However, Chen and Roth (2024) argue that the estimated treatment effect in such cases is sensitive to the unit of Y, which is arbitrarily chosen. As a result, they recommend avoiding log-like transformations and suggest several alternative approaches.

variable. Notably, when the number of entities is fixed, the average *level* change at the entity level is proportional to the *level* change on aggregate, thus indicating the effect on climate change.

The two approaches can yield different conclusions from the same dataset, particularly when outcome distributions are highly dispersed and treatment effects heterogeneous. For example, if fewer high-emitting facilities increase their emissions while numerous lower-emitting ones reduce theirs, the average *level* change might be positive, whereas the average *percentage* change is negative. This occurs because the average percentage change weights entities equally regardless of scale, while the average level change accounts for entity-specific emission levels.

Table 2.1 presents the regression results of facility-level GHG emissions. Following the convention in related studies, the dependent variable in Columns 1-3 is log-transformed facility-level emissions. The results show a negative correlation between treatment variable and log-transformed emissions, but this correlation loses significance once control variables are introduced. Furthermore, as discussed earlier, this negative correlation could only be interpreted as the average percentage change in facility-level emissions rather than policy effectiveness in reducing corporate emissions at the aggregate level. To evaluate the effectiveness of the policy in reducing aggregate corporate emissions, the outcome variable should be non-transformed emissions. The results are presented in Columns 4-6, where the estimated coefficient of $Treated$ is insignificantly positive, suggesting that state-level climate policies are ineffective in reducing corporate emissions.

To explore the heterogeneity in the treatment effect across facilities of different emissions scales, I divide the control and treatment groups into quintiles separately. For each quintile, I perform a DID analysis of facility-level emissions, with the results presented in Table 2.2. While the estimated treatment effects are statistically insignificant for most quintiles and specifications, their magnitudes provide useful information.

The estimated treatment effects are positive for the first, third, and fourth quintiles of facilities, but negative for the second and fifth quintiles. Additionally, the table reports the average emissions for each quintile, revealing the highly dispersed nature of the emissions across facilities. Notably, the mean emissions in the fifth quintile (1.9523) are over ten times higher than those in the fourth quintile (0.1717), and similarly, the magnitude of the treatment effect for facilities in the fifth quintile is more than ten times greater than that of the fourth quintile.

Given that the majority of facilities appear to reduce emissions, it is not surprising that the estimated average *proportional* change is negative in Table 2.1. However, because facilities in the fifth quintile have significantly higher emissions than those in the lower quintiles, they disproportionately influence the average *level* change in emissions. As a result, it is also unsurprising that the average *level* change estimated in Table 2.1 is positive. In sum, the combination of the high dispersion in emissions across facilities and the heterogeneous treatment effects explains why regressions using log-transformed and non-transformed outcome variables in Table 2.1 produce results with opposite signs.

I also conduct DID analyses of emissions at the state level. Unlike the facility-level analyses, where the observation level is more granular than the treatment, state-level analyses have observations and treatments at the same level of granularity. Consequently, regressions of both non-log-transformed and log-transformed emissions at the state level capture the aggregate treatment effect and assess policy effectiveness. The only small difference is that regressions using non-log-transformed state-level emissions assign greater weight to states with higher emissions, while those using log-transformed state-level emissions treat all states equally. For additional robustness, I run regressions using log-transformed state-level emissions, as shown in Table A2.1, where the estimated treatment effects are insignificant and close to zero. Thus, the evidence consistently suggests that state-level emissions reduction targets are ineffective in reducing corporate emissions.

Finally, I revisit the conventional rationales for log transformation—skewness, outliers, and functional form matching—in the context of climate policy analysis, providing robustness checks. Since prior climate-policy studies do not explicitly justify log transformations, I briefly examine each rationale here.

Regarding skewness, I reiterate that it does not undermine unbiasedness nor inference validity as long as the sample size is not too small. Nevertheless, to further support my results, I provide bootstrapped standard errors, which make no parametric assumptions. The results in Table A2.2 confirm the ineffectiveness of state-level targets in reducing corporate-sector emissions.

Regarding outliers, high-emitting entities are integral to understanding the overall emissions and the effectiveness of climate policy, so they should not be treated as outliers. Nevertheless, I conduct two robustness checks, including winsorizing at the 1st and 99th percentiles and excluding the ten largest emitting facilities (Table A2.3). Both tests show consistent results, indicating that state-level targets are ineffective in reducing corporate emissions. Therefore, my results are not driven by a few exceptional facilities.

Lastly, concerning matching the economic model's functional form, given that the explanatory variable is binary (treatment), the relationship with emissions could be seen as either multiplicative or additive.

An alternative approach to estimate the aggregate treatment effect is Poisson regression, which assumes the errors are positively skewed. For an outcome variable that is continuous or overdispersed, one can apply Poisson regression with robust standard errors, i.e., Poisson pseudo-maximum likelihood regressions (PPML). Gourieroux et al. (1984), Silva and Tenreyro (2006), and Chen and Roth (2024) suggest that PPML consistently estimates the population coefficient β that satisfies:

$$e^{\beta} - 1 = \frac{E[y_1] - E[y_0]}{E[y_0]} \tag{2.12}$$

where $E[y_1]$ and $E[y_0]$ are the expectations of y_i for treated and untreated observations, respectively. Compared with the log-transformed OLS that estimates *the average proportional change*, PPML estimates *the proportional change in average*. In the context of climate policy assessment, the latter approach aligns with the research question. Therefore, I conduct robustness checks using PPML and report the results in Table A2.4, which are consistent with the conclusion that state-level targets are ineffective in reducing corporate emissions.

In summary, lower-emitting facilities reduce emissions post-policy, while higher-emitting facilities increase theirs. Due to the highly dispersed distribution of emissions across facilities, the increases from the fewer higher-emitting facilities outweigh the reductions from the more numerous lower-emitting ones, resulting in an (insignificant) net increase in total emissions. This explains why the ATE estimated from log-linear regressions, which ignore variations in facility emissions scales, seemingly (but actually does not) conflict with the ATE from linear-linear regressions, which account for such differences. This empirical example illustrates that regressions with log-transformed and non-transformed outcome variables address different research questions.

2.5 Conclusion

This paper brings attention to the crucial yet frequently overlooked distinction in economic meanings between regressions of log-transformed versus non-transformed outcome variables. When choosing between these two functional forms, future research should consider not only their econometric properties but also whether the research question centres on the average proportional change or level change.

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Table 2.1: Facility-level GHG Emissions

This table presents regression estimates on the impact of state-level GHG emissions reduction targets on facility-level GHG emissions. Facilities located in the nine states that adopted state-level targets in 2019 are designated as the treatment group, while the control group consists of facilities in the twenty-five states that have never adopted such targets. The sample period spans from 2016 to 2021. The dependent variable is *ln(Emission)* in Columns 1-3 and *Emission* in Columns 4-6. The variable *Treated* is a dummy variable set to 1 if the state where the facility is located has adopted a state-level target by the given year, and 0 otherwise. Columns 1 and 4 include all facilities with available data, while the remaining columns use a balanced subsample of facilities that are consistently present throughout the period. Standard errors, adjusted for clustering at the state level, are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. All control variables are defined in Table A1.2.

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var. =		*ln(Emission)*			*Emission*	
Treated	-0.0780***	-0.0797***	-0.0512	0.0049	0.0070	0.0381
	(0.02)	(0.02)	(0.03)	(0.01)	(0.01)	(0.03)
AR_{t-1}			-0.0058			-0.0014
			(0.01)			(0.01)
Asset_{t-1}			0.0476***			0.0230*
			(0.02)			(0.01)
Leverage_{t-1}			0.1673			0.0974
			(0.15)			(0.06)
ROA_{t-1}			0.0069			-0.1707*
			(0.09)			(0.09)
MB_{t-1}			0.0002***			0.0000
			(0.00)			(0.00)
Observations	26558	23683	8289	26637	23718	8300
Adj. R^2	0.9052	0.9183	0.9383	0.9589	0.9674	0.9662
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Facility FE	Yes	Yes	Yes	Yes	Yes	Yes
Balanced	No	Yes	Yes	No	Yes	Yes

Table 2.2: Facility-level GHG Emissions by Quintiles

This table presents difference-in-differences regression estimates of facility-level GHG emissions by dividing facilities into quintiles. Facilities located in the nine states that adopted state-level targets in 2019 are designated as the treatment group, while the control group consists of facilities in the twenty-five states that have never adopted such targets. The sample period spans from 2016 to 2021. Facilities in the treatment and control groups are divided into quintiles separately based on their emission levels in the reference year, 2018. The dependent variable is facility-level *Emission* in all columns. The variable *Treated* is a dummy set to 1 if the state where the facility is located has adopted a state-level target by the given year, and 0 otherwise. Columns 1, 3, 5, 7, and 9 include all facilities with available data, while the remaining columns use a balanced subsample of facilities that are consistently present throughout the period. Standard errors, adjusted for clustering at the state level, are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. In addition to regression results, this table also reports the mean and standard deviation of *Emission* for each group.

Dep. Var. =	*Emission*									
Sample =	*Quintile 1*		*Quintile 2*		*Quintile 3*		*Quintile 4*		*Quintile 5*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treated	-0.0031	-0.0036	0.0006	0.0012	-0.0041**	-0.0033	-0.0030	-0.0082*	0.0309	0.0409
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.06)	(0.06)
Observations	4871	3750	5188	4734	5270	5058	5300	5058	5296	5118
Adj. R^2	0.5701	0.2831	0.3187	0.3120	0.4513	0.4402	0.6026	0.6395	0.9479	0.9556
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Facility FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balanced	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Mean(*Emission*)	0.0272		0.0425		0.0701		0.1717		1.9523	
SD(*Emission*)	(0.19)		(0.03)		(0.03)		(0.21)		(2.52)	

Table A2.1: State-Level GHG Emissions with Logarithmic Transformation

This table presents difference-in-differences regression estimates of the effect of state-level GHG emissions reduction targets on aggregate state-level GHG emissions. Unlike Table 1.5, the dependent variables in this table are the natural logarithms of state-level emissions. The treatment group consists of the nine states that adopted state-level targets in 2019, while the control group comprises the twenty-five states that have never adopted such targets. The sample period spans from 2016 to 2021. The dependent variable in Columns 1 and 2 is *ln(Emission GHGRP)*, which represents the log-transformed sum of facility-level emissions obtained from the Greenhouse Gas Reporting Program (GHGRP) of the US EPA. In Columns 3 and 4, the dependent variable is *ln(Emission EIA)*, the log-transformed state-level energy-related carbon emissions reported by the US EIA. The variable *Treated* is a dummy variable set to 1 if the state has adopted a state-level target by the given year, and 0 otherwise. Standard errors, adjusted for clustering at the state level, are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The control variable is defined in Table A1.2.

	(1)	(2)	(3)	(4)
Dep. Var. =	*ln(Emission GHGRP)*		*ln(Emission EIA)*	
Treated	-0.0100	-0.0108	-0.0003	-0.0007
	(0.03)	(0.03)	(0.02)	(0.02)
lnGDP		0.3624		0.1551
		(0.32)		(0.38)
Observations	204	204	204	204
Adj. R^2	0.9979	0.9979	0.9977	0.9977
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes

Table A2.2: Facility- and State-Level GHG Emissions with Bootstrapped Standard Errors

This table presents difference-in-differences regression estimates on the effects of state-level GHG emissions reduction targets on facility- and state-level GHG emissions. Unlike Tables 2.1 and 1.5, the standard errors in this table are estimated through bootstrapping. The treatment group consists of (facilities in) the nine states that adopted state-level targets in 2019, while the control group comprises (facilities in) the twenty-five states that have never adopted such targets. The sample period spans from 2016 to 2021. In Panel A, the dependent variable is facility-level *Emission*. Column 2 includes untabulated control variables, consistent with those used in Panel A of Table 1.2. In Panel B, the dependent variable for Columns 1 and 2 is *Emission GHGRP*, calculated by aggregating facility-level emissions from the Greenhouse Gas Reporting Program (GHGRP) of the US EPA. For Columns 3 and 4, the dependent variable is *Emission EIA*, which measures state-level energy-related carbon emissions reported by the US EIA. In both panels, the variable *Treated* is a dummy variable set to 1 if the state (where the facility is located) has adopted a state-level target by the given year, and 0 otherwise. Bootstrapped standard errors are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The control variable is defined in Table A1.2.

Panel A: Facility-level emissions

Dep. Var. =	*Emission*	
	(1)	(2)
Treated	0.0070	0.0381
	(0.01)	(0.03)
Observations	23718	8496
Adj. R^2	0.9674	0.9658
Controls	No	Yes
Year FE	Yes	Yes
State FE	Yes	Yes
Balanced	Yes	Yes

Panel B: State-level emissions

Dep. Var. =	*Emission GHGRP*		*Emission EIA*	
	(1)	(2)	(3)	(4)
Treated	1.6217	1.5658	1.2719	1.2575
	(2.23)	(2.27)	(1.85)	(1.88)
GDP		-41.3423		-10.6589
		(29.90)		(30.12)
Observations	204	204	204	204
Adj. R^2	0.9964	0.9965	0.9974	0.9974
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes

Table A2.3: Facility-Level GHG Emissions Excluding Largest Emitters or Winsorizing Emissions Variable

This table presents regression estimates on the impact of state-level GHG emissions reduction targets on facility-level GHG emissions. To assess whether the results in Table 2.1 are driven by a few large emitters, this table either excludes the ten largest emitters from the sample (based on cumulative emissions from 2016 to 2021) or winsorizes the variable *Emission* at the 1st and 99th percentiles. Facilities located in the nine states that adopted state-level targets in 2019 are designated as the treatment group, while the control group consists of facilities in the twenty-five states that have never adopted such targets. The sample period spans from 2016 to 2021. The variable *Treated* is a dummy variable set to 1 if the state where the facility is located has adopted a state-level target by the given year, and 0 otherwise. Columns 1 and 4 include all facilities with available data, while the remaining columns use a balanced subsample of facilities that are consistently present throughout the period. Standard errors, adjusted for clustering at the state level, are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. All control variables are defined in Table A1.2.

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var. =	*Emission*					
	Ten Largest Emitters Excluded			*Winsorized*		
Treated	0.0015	0.0033	0.0286	0.0044	0.0061	0.0249
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)
AR_{t-1}			0.0041			-0.0010
			(0.01)			(0.01)
$Asset_{t-1}$			0.0185			0.0202
			(0.01)			(0.01)
$Leverage_{t-1}$			0.0581			0.0603
			(0.05)			(0.05)
ROA_{t-1}			-0.1340			-0.1268
			(0.10)			(0.10)
MB_{t-1}			0.0000			0.0000
			(0.00)			(0.00)
Observations	26577	23658	8258	26637	23718	8300
Adj. R^2	0.9545	0.9654	0.9673	0.9595	0.9697	0.9709
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Facility FE	Yes	Yes	Yes	Yes	Yes	Yes
Balanced	No	Yes	Yes	No	Yes	Yes

Table A2.4: Poisson Regressions of Facility- and State-Level GHG Emissions

This table presents difference-in-differences regression estimates on the effects of state-level GHG emissions reduction targets on facility- and state-level GHG emissions. Unlike Tables 2.1 and 1.5, which employ linear regression models, this table uses Poisson regressions as a robustness check. The treatment group consists of (facilities in) the nine states that adopted state-level targets in 2019, while the control group comprises (facilities in) the twenty-five states that have never adopted such targets. The sample period spans from 2016 to 2021. In Panel A, the dependent variable is facility-level *Emission*. Column 2 includes untabulated control variables, consistent with those used in Panel A of Table 1.2. In Panel B, the dependent variable for Columns 1 and 2 is *Emission GHGRP*, calculated by aggregating facility-level emissions from the Greenhouse Gas Reporting Program (GHGRP) of the US EPA. For Columns 3 and 4, the dependent variable is *Emission EIA*, which measures state-level energy-related carbon emissions reported by the US EIA. In both panels, the variable *Treated* is a dummy variable set to 1 if the state (where the facility is located) has adopted a state-level target by the given year, and 0 otherwise. Standard errors, adjusted for clustering at the state level, are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. All control variables are defined in Table A1.2.

Panel A: Facility-level emissions

Dep. Var. =	*Emission*	
	(1)	(2)
Treated	-0.0160	0.0017
	(0.02)	(0.04)
Observations	23718	8297
Pseudo R^2	0.6372	0.6870
Controls	No	Yes
Year FE	Yes	Yes
State FE	Yes	Yes
Balanced	Yes	Yes

Panel B: State-level emissions

Dep. Var. =	*Emission GHGRP*		*Emission EIA*	
	(1)	(2)	(3)	(4)
Treated	-0.0080	0.0039	0.0022	0.0141
	(0.03)	(0.03)	(0.03)	(0.02)
GDP		0.4646***		0.4514***
		(0.13)		(0.15)
Observations	204	204	204	204
Pseudo R^2	0.9030	0.9031	0.9083	0.9084
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes

Chapter 3

Powerful CEOs in Uncertain Times: Survival of the Fittest

Powerful CEOs in Uncertain Times: Survival of the Fittest*

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Abstract

Contrary to the conventional focus on the costs of excessive CEO power, this study investigates whether powerful CEOs are beneficial and desirable under uncertainty. The evidence shows that powerful CEOs have a lower dismissal rate in uncertain times. As they exhibit better performance but no increased compensation, powerful CEOs are likely retained optimally for their effectiveness rather than by entrenched power. To mitigate endogeneity concerns surrounding CEO power, this paper utilizes the onset of COVID-19 pandemic as an unanticipated sudden spike in uncertainty, during which CEO power is unlikely to adjust swiftly to external conditions due to stickiness. The study proposes two potential mechanisms explaining why powerful CEOs are more effective under uncertainty: their willingness to share information with the board and their capability to take swift action. Overall, this study challenges the view that CEO power is always manipulative and detrimental.

Keywords: Corporate governance, CEO power, Uncertainty, CEO turnover

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"Power tends to corrupt and absolute power corrupts absolutely." - Dalberg-Acton (1887).

*"The term dictatorship comes from the Latin title dictator, which in the Roman Republic designated a temporary magistrate who was granted **extraordinary powers** in order to deal with state **crises**."* - Britannica, The Editors of Encyclopaedia (2020).

3.1 Introduction

The role and extent of a CEO's power have long been debated in corporate governance. Conventional wisdom suggests excessive CEO power can be detrimental, given the extensive literature on managerial entrenchment and the regulations curbing CEO authority. Thus, the common practice of many CEOs also serving as board chairs raises frequent concerns. On the other hand, history reminds us of anecdotes where strong leadership, marked by decisive action, was pivotal for a community or an organization in tumultuous times. During the recent COVID-19 pandemic, numerous firms centralized power: some long-tenured CEOs postponed their planned retirements, some firms welcomed the comeback of their once-distanced influential leaders, and some firms transformed from co-CEO to the sole CEO model (Appendix A.1 provides examples in more detail).

This dichotomy raises the question: Are powerful CEOs particularly valuable during uncertain times? Two prevailing theories offer contrasting perspectives. Entrenchment theory posits that powerful CEOs can maneuver board decisions on turnover and compensation in their own favour. It should be especially the case during uncertain times when the cost of replacing the entrenched CEO can be especially high, leading to not only reduced dismissals but also increased compensation and suboptimal firm performance. Conversely, optimal dismissal theory contends that boards optimally make turnover decisions based on CEOs' and their potential substitutes' perceived match qualities. Accordingly, if more powerful CEOs are retained in uncertain times, it is because they become more advantageous rather than entrenched. Therefore, the lower dismissal rate should be accompanied by neither exorbitant compensation nor diminished performance.

To test both theories, I examine three sequential questions: (1) Do powerful CEOs face fewer dismissals during uncertain times? (2) If so, is it due to their entrenchment or their value to the firm? (3) If the latter, what mechanisms make powerful CEOs particularly valuable in uncertain conditions?

Using a sample of 2,732 US public firms between 1999 and 2020, I document that powerful CEOs (measured by metrics like *CEO duality*) indeed experience significantly fewer forced turnovers during increased uncertainty.[1] Notably, powerful CEOs neither underperform nor receive exorbitant compensation in such periods. To mitigate the endogeneity concern of CEO power, this paper leverages the onset of the COVID-19 pandemic as an unanticipated sudden surge in uncertainty, when CEO power is unlikely to adjust swiftly to external conditions in such a short period due to its inherent stickiness. I find that powerful CEOs are associated with significantly higher stock returns than their less powerful counterparts over the first month of the pandemic. Those results contradict entrenchment theory while supporting an optimal decision process where uncertainty increases firms' preferences for powerful CEOs. Finally, this paper suggests two potential mechanisms for the efficacy of powerful CEOs during uncertain times. First, powerful CEOs appear more willing to share information with their boards, enhancing decision-making. Second, they seem capable of making faster decisions, a critical trait during volatile times.

I start my analyses by charting the dismissal rates of powerful versus weak CEOs from 1999 to 2020 against the annual market uncertainties. In most years, the dismissal rate of powerful CEOs is lower than that of weak ones. Intriguingly, as uncertainty rises, the dismissal rate of powerful CEOs decreases, while that of less powerful CEOs

[1] I use the terms "dismissal" and "forced turnover" interchangeably.

increases.

Then, I formally test how the CEO dismissal rate relates to CEO power and uncertainty (measured by industry-year-level *Stock volatility* and *Delisting rate*). Adopting a firm-year-level panel regression, and accounting for potential endogeneity through year- and industry-fixed effects, I observe that powerful CEOs typically have a lower risk of involuntary departure. Notably, once introducing the interaction term between CEO power and uncertainty, the significance of the CEO power diminishes while the coefficient of the interaction term is significantly negative. That means, compared with other CEOs, powerful CEOs become less likely to be dismissed when uncertainty increases. My finding is robust to alternative measures of CEO power and uncertainty.

Next, to discern between the entrenchment theory and optimal dismissal theory, I conducted three tests. If the former theory holds true, powerful CEOs retained during uncertain periods should underperform and capitalize on their entrenchment for higher compensation. The latter theory predicts no such anomalies. To test both pairs of predictions, I regress firm performance and CEO compensation, respectively, on CEO power, uncertainty, and their interaction term. Furthermore, since adjusting CEO power is complex and time-consuming for the firm (for instance, transitioning from an independent chair to a combined chair-CEO role necessitates either the chair's or the CEO's replacement or both), the level of CEO power is sticky within a relatively short period of time. Therefore, if powerful CEOs are more effective in uncertain times, they should outperform other CEOs at times of sudden uncertainty shocks before uncertainty-incurred CEO power adjustment can take place. Hence, for the third test, I exploit the Coronavirus Stock Market Crash between February 20th and March 20th in 2020 as an unexpected uncertainty spike, and compare stock returns associated with powerful CEOs versus their less powerful peers during that one-month period.

My findings lend no support to the entrenchment theory, while consistently aligning with the notion of optimal dismissal. In uncertain times, firms with powerful CEOs perform at least as well as other firms. Specifically, Q ratio and sales growth are indistinguishable between those two groups of firms, while ROA is even higher among firms with powerful CEOs. Also inconsistent with the notion of entrenchment, although powerful CEOs do earn more on average, this differential doesn't expand in uncertain times. Furthermore, between February 20th and March 20th in 2020, when the stock market collapsed as the COVID-19 pandemic spurred extreme uncertainty and anxiety, powerful CEOs were associated with 2.8% higher stock returns in that one-month period. This result does not exist in a placebo test during the same period in the previous year, 2019, suggesting that powerful CEOs are particularly desirable in uncertain periods. In sum, the fewer dismissals of powerful CEOs during uncertain times are unlikely to result from managerial entrenchment. Instead, the evidence is consistent with an efficient dismissal decision process where uncertainty increases firms' preferences for powerful CEOs.

Finally, I explore the mechanisms of why powerful CEOs are more desirable and effective in uncertain times. The first potential mechanism is information sharing. As predicted in the model by Adams and Ferreira (2007), CEOs under stringent oversight may hesitate in sharing firm-specific with boards, which in turn compromises the quality of advising. Such hesitance can be especially detrimental in uncertain periods when information influx is rapid, making the relationship between CEOs and boards crucial. Powerful CEOs are less checked by boards and thus more willing to disclose information to boards, which might explain why they are more desirable in uncertain times. Testing this hypothesis using the sample bifurcated by information asymmetry, I find that powerful CEOs face fewer uncertain-time dismissals only if they are in more opaque firms. Therefore, better information sharing seems to be one of the explanations for powerful CEOs' desirability during uncertain periods.

The second potential mechanism concerns the speed of taking action. Unlike executives who run the firm on a daily basis, directors often have other obligations elsewhere. If a CEO is less powerful, then more decisions need approval from the board, which unavoidably leads to slower decision-making. In line with this notion, if a board consists of busier directors, having a CEO with more decision-making power should be optimal in uncertain times. Dividing firms based on director busyness, my results validate this hypothesis: among firms with busier directors, powerful CEOs are significantly less likely to be fired when uncertainty increases, while no such result is found among firms with less busy directors. Therefore, swift action-taking seems to be another potential explanation for why powerful CEOs are more favored during uncertain periods.

To the best of my knowledge, this is the first paper to explore the desirability and efficacy of powerful CEOs in uncertain periods. It contributes to the literature discussing the link between CEO power and firm performance, underscoring how a CEO's effectiveness can be contingent on market conditions. According to the canonical agency theory (Fama and Jensen, 1983; Jensen, 1993), the misalignment between CEOs' interests and shareholder value determines that excessive CEO power (or insufficient CEO monitoring) undermines firm value. However, there is a well-known lack of empirical evidence to support this prediction (Bhagat and Black, 2001; Hermalin and Weisbach, 2003; Adams et al., 2005).[2] Recent studies find that the effectiveness of CEO monitoring varies with firm characteristics, like the cost of acquiring information (Duchin et al., 2010) and the relative importance of board advice and monitoring (Schmidt, 2015). Unlike those papers examining firms' *intrinsic characteristics*, I investigate firms' *external conditions* and provide evidence that powerful CEOs are more desirable and effective under uncertain market conditions.

This paper also adds new evidence to the debate between optimal dismissal theory and entrenchment theory. According to optimal dismissal theory, the board optimally makes CEO turnover decisions (Gibbons and Murphy, 1990; Bushman et al., 2010) in

[2]Other related studies include: Villalonga and Amit (2006), Palia et al. (2008) Adams et al. (2009), and Fahlenbrach (2009) all find that firms with founder-CEOs, a subset of powerful CEOs, actually have better performance. Graham et al. (2020) find that the announcement of the sudden death of a powerful CEO (relatively long job tenure, dual board chair, or founder of the firm) is associated with higher abnormal market return, compared with that of a less powerful CEO. Bennedsen et al. (2020) document that firms' accounting performance declines after relatively long-tenured CEOs are hospitalized, while the hospitalizations of relatively new CEOs have insignificant effects.

the best interests of shareholders. By contrast, a much larger literature argues that entrenched CEOs influence their own retentions (Weisbach, 1988; Shleifer and Vishny, 1989; Hermalin and Weisbach, 1998; Denis et al., 1997; Almazan and Suarez, 2003; Taylor, 2010; Fisman et al., 2014). I find that the lower dismissal rate of powerful CEOs in uncertain times is accompanied by neither worse performance nor increased compensation, which supports optimal dismissal theory. In addition, both lines of literature above assume that a CEO's effectiveness is constant. Contrarily, my results highlight that a CEO's effectiveness varies with market conditions, similar to the modeling assumptions in Jovanovic (1979) and Garrett and Pavan (2012).

The literature also divides on whether CEO compensation is optimally determined (Gabaix and Landier, 2008; Peters and Wagner, 2014; Cheng et al., 2015) or influenced by entrenched CEOs (Bebchuk and Fried, 2004; Morse et al., 2011). My results are consistent with optimal contracting. Furthermore, this paper extends the literature on product market competition and corporate governance, which finds that powerful CEOs are more effective when the product market is more competitive (Giroud and Mueller, 2010, 2011; Yang and Zhao, 2014; Li et al., 2019). I complement this literature by turning the focus from product market competition to a different facet of the business environment, i.e., external uncertainty.

3.2 Data, variables, and sample construction

I obtain data on US public firms from various sources and build a sample in their intersection. I start with Execucomp, which contains CEOs' compensation and characteristics, and then merge it with board information from Boardex, firm characteristics from Compustat, forced CEO turnovers from Gentry et al. (2021), stock market return and delisting events from CRSP, and analyst forecasts from IBES. The details of the variables and the sample are described below.

3.2.1 Forced CEO turnover

With rare exceptions (like CEO sudden deaths), CEO turnovers can be roughly categorized as either forced (i.e. dismissal) or voluntary. Forced CEO turnovers mostly occur when firms think their incumbent CEOs are less qualified than potential successors, while voluntary turnovers are mainly due to CEOs' personal choices, like outside opportunities or retirement. Consistent with the vast literature on CEO turnover, I focus on the first type, forced turnovers, which are initiated by firms.

As well-documented in the literature, it is challenging for researchers to distinguish dismissals from voluntary turnovers. It is because firms have no obligation to disclose the reasons for CEO turnovers (Weisbach, 1988; Kaplan and Minton, 2012; Jenter and Lewellen, 2021). Even when firms voluntarily disclose, they sometimes disguise forced CEO turnovers as voluntary retirements. Researchers design various algorithms to identify forced turnovers based on CEO age, press coverage and whether the CEO remains on the board after the turnover (Parrino, 1997; Bushman et al., 2010; Peters and Wagner, 2014; Jenter and Kanaan, 2015). Unavoidably, this identifying process involves researchers' subjective assessment, and hence the sets of forced turnovers in different studies do not fully overlap (Gentry et al., 2021).

Gentry et al. provide an open-source dataset of CEO departures in S&P 1500 firms from 1987 through 2020. They code each CEO departure for one of eight voluntary and involuntary reasons. Furthermore, they provide web references (SEC filings and/or press releases) on which each coding is based. This level of transparency makes their dataset easily verifiable and helps minimize subjective bias.

I identify forced CEO turnovers based on the dataset from Gentry et al.. There are 1490 forced CEO turnovers (i.e. ceo_dismissal=1) in their dataset from 1999 to 2020. After merging with other datasets and deleting observations with missing values in the baseline regression model, my final sample contains 900 forced CEO turnovers.

3.2.2 Proxies for uncertainty

Milliken (1987) defines the uncertainty of the business environment as individuals' inability to forecast the direction of environmental changes, the impacts on organizations, and their optimal responses. Guided by this definition and related literature, I adopt two measures of uncertainty in this paper: *Stock volatility* and *Delisting rate*. Since my purpose is to capture exogenous uncertainty rather than the riskiness of endogenously-chosen firm policies, both measures are on the industry-year level. Firms with the identical first two digits of SIC codes are regarded as in the same industry.

These two measures are correlated but emphasize different aspects of external uncertainty. The first measure, *Stock volatility*, is defined as the industry equal-weighted average standard deviation of individual firms' monthly returns in one year, similar to Peters and Wagner (2014). *Stock volatility* signals the uncertainty over the values of firms in an industry. The second measure, *Delisting rate*, is the fraction of firms in an industry that are delisted within a year (Gillan et al., 2009). CRSP Stock Events - Delisting Information records delisting events of public US firms for various reasons. In this paper, I calculate *delisting rate* as the fraction of firms that are delisted because of mergers (first digit of delisting code=2), liquidations (first digit of delisting code=4), or dropped (first digit of delisting code=5). Industry-years with more of those events are generally considered more uncertain.

To visualize the fluctuations of those two uncertainty measures over the sample period, I pick ten representative industries with relatively large numbers of observations and plot their uncertainty measures from 1999 to 2020, as shown in Figure 3.1.

3.2.3 CEO power

Although all CEOs have legitimate authority as the highest-ranking executive in their companies, their power varies with many factors. In the literature, CEO power is often measured by CEOs' additional titles, status, or board composition. For example, Grinstein and Hribar (2004) measure CEO power by CEO-chair duality, membership of the nominating committee, and the ratio of insider directors; Adams et al. (2005) measure CEO power by founder status, being the only insider on the board, and concentration of titles; Custódio and Metzger (2013) use CEO duality; Song and Wan (2019) use duality, founder status and concentration of titles.

In addition, CEO power is also affected by the relationship between the CEO and other top corporate leaders, like the fraction of executives and directors who have been appointed during the current CEO' tenure (Morse et al., 2011; Khanna et al., 2015). The rationale here is, as shown in the literature (Coles et al., 2014), that the CEO has substantial influence in shaping the board composition, and therefore directors and executives appointed during a CEO's tenure might feel beholden to the CEO.

Furthermore, the literature shows that CEO power strengthens over a CEO's tenure. Hermalin and Weisbach (1998) build a framework where retained CEOs bargain for less independent boards; consistent with the prediction of this model, Boone et al. (2007) find a negative relation between board independence and CEO tenure.

Therefore, I adopt multiple measures for CEO power. In the baseline regression, I use *CEO duality*, which equals one if the CEO is also the board chair and zero otherwise. For robustness purposes, CEO power is alternatively proxied by *CEO's concentration of titles*, the length of *CEO tenure*, whether the CEO became a director earlier than (if there exists) the independent chair (*Longer directorship*), and if the CEO is a *Founder CEO*. The intuitions are: additional titles like chairperson and president give the CEO a bigger say among other directors or executives, respectively; long-tenured CEOs and founder CEOs possess a bigger influence via their achievements, expertise, and long-term relationship with company constituents; a more senior independent chairperson provides a check on the CEO's power. The detailed definition of those measures can be found in Table A3.1.

3.2.4 Sample and summary statistics

The sources and construction of other variables not discussed above are listed in Table A3.1. Since Boardex collects board characteristics on a yearly basis at the end of each fiscal year, I merge variables from Boardex to Execucomp with special caution for turnover years in order to make sure departing CEOs are matched with the right board characteristics. Specially, if a CEO leaves before the annual shareholder meeting (AGM), the values of director-related variables are taken from the previous fiscal year rather than the current one. It is because directors are appointed during AGMs, which are usually in the middle of fiscal years. Therefore, for pre-AGM CEO turnovers, the board composition at the time of turnover remains the same as at the end of the previous fiscal year. In addition, for variables that might change with the CEO turnover, like *CEO duality*, their values in turnover years are taken from the previous fiscal year rather than the current one in order to avoid capturing the characteristics of the incoming CEO rather than the departing CEO.

Conditioning on none of the variables in the baseline regression missing, my final sample contains 32,033 firm-years between 1999 and 2020, with 900 forced CEO turnovers. The sample size shrinks for some other analyses in this paper due to partially missing values of certain variables.

Table 3.1 reports the descriptive statistics. The incidence of forced turnover is 2.8% among all firm-years. Among 53.8% of firm-years, the CEO is also the board chair. Both measures of uncertainty are within $[0, 0.5]$, and their standard deviations are 0.050 and 0.037, respectively. All variables are winsorized at the 1^{st} and 99^{th} percentiles to minimize the impact of outliers.

3.3 Empirical Results

3.3.1 Baseline results

In this section, I examine the relations between CEO forced turnover, uncertainty, and CEO power. I first visualize the relations between yearly market-level uncertainty and forced CEO turnover rates and then conduct formal statistical analyses.

Figure 3.2 shows the relation between market-average uncertainty and the fraction of dismissed CEOs in each year from 1999 to 2020 among dual CEOs (Subfigures (a) and (c)) and non-dual CEOs (Subfigures (b) and (d)), respectively. Two alternative measures are used for uncertainty: *Market average stock volatility* (Subfigures (a) and (b)) and *Market average delisting rate* (Subfigures (c) and (d)). Comparing Subfigures (a) and (c) with Subfigures (b) and (d) indicates that forced turnovers are in general rarer among dual CEOs than among non-dual ones. Specifically, the yearly fraction of dismissed dual CEOs ranges from 0% (in 1999) to 3.51% (in 2017), while that number for non-dual CEOs ranges from 2.28% (in 2017) to 6.25% (in 1999). More interestingly, the relation between forced turnover percentage and uncertainty is exactly the opposite for those two groups of CEOs. As uncertainty increases (moving towards the right-hand side along the horizontal axis), the forced turnover percentage decreases among dual CEOs but increases among non-dual ones. In other words, the dismissal risk for dual (non-dual) CEOs is negatively (positively) associated with uncertainty.

To formally test whether uncertainty affects the forced turnover probabilities of powerful CEOs and other CEOs differently, I estimate the following firm-year panel regression:

$$
\begin{aligned}
Forced\ turnover_{it} = \beta_0 + \beta_1 Uncertainty_{It} + \beta_2 CEO\ power_{it} \\
+ \beta_3 CEO\ power_{it} \times Uncertainty_{It} + B_4 X_{it} \\
+ B_5 X_{it} \times Uncertainty_{It} + d_I + d_t + \varepsilon_{it}
\end{aligned}
\tag{3.1}
$$

where $Forced\ Turnover_{it}$ is a dummy indicating whether firm i experiences a forced CEO turnover in year t, $Uncertainty_{It}$ is the average uncertainty of firm i's industry I in year t, $CEO\ power_{it}$ measures the power of the CEO of firm i in year t, X_{it} is a vector of CEO and firm characteristics, d_I is a dummy for firm i's industry I, d_t is a dummy for year t, and ϵ_{it} is the error term adjusted for heteroskedasticity and industry-level clustering. Both $CEO\ power_{it}$ and X_{it} are interacted with $Uncertainty_{It}$, in order to examine how the relations between forced CEO turnover and CEO/firm characteristics are moderated by uncertainty. The industry fixed effects and year fixed effects absorb time-invariant industry heterogeneities and common time trends, respectively. Therefore, the estimation builds on the cross-firm and over-time variations of variables within the same industry. In an alternative specification, I control for industry-year fixed effects d_{It}, rather than industry fixed effects d_I and year fixed effects d_t. In that case, the estimation builds on the cross-firm variations of variables within the same industry and year, and thus $Uncertainty_{It}$ is excluded from the controls.

The interpretation of the estimands is as follows: the impact of uncertainty on forced turnovers ($\beta_1 + \beta_3 CEO\ power_{it}$) consists of two parts: β_1 is the common effect of uncertainty on all CEOs, while $\beta_3 CEO\ power_{it}$ gauges the differential effect of uncertainty that is in proportion to CEO power. If β_3 is non-zero, then it implies that the impact of uncertainty on dismissal probability varies with the level of CEO power. Similarly, the impact of CEO power on forced turnovers ($\beta_2 + \beta_3 Uncertainty_{It}$) also has two components: β_2 is the common effect of CEO power regardless of the uncertainty level, while $\beta_3 Uncertainty_{It}$ varies with uncertainty. If β_3 is non-zero, then it also implies that the impact of CEO power on dismissal probability is moderated by uncertainty.

Uncertainty is measured by two alternative proxies: *Stock volatility* and *Delisting rate*. Both measures emphasize different aspects of external uncertainty: *Stock volatility* measures the uncertainty manifested in the equity market, while *Delisting rate* gauges the prevalence of extreme cases where firms are delisted from stock exchanges. Both measures are averaged across all firms in the same industry and year and thus exogenous to individual firms' policies and characteristics.

CEO Power is measured by five alternative proxies: *CEO duality*, *CEO's concentration of titles*, *CEO tenure*, *Longer directorship*, and *Founder CEO*. The detailed definition of those measures can be found in Table A3.1. Table 3.2 reports the regression results measuring CEO power with *CEO duality*. In the interest of space, the results using the other four measures are reported in Table A3.2.

In Columns (1) and (5) of Table 3.2, I regress CEO forced turnover on uncertainty and CEO power without their interaction term, in order to evaluate how forced CEO turnover is associated with uncertainty and CEO power, respectively. The correlation between forced CEO turnover and uncertainty is significantly positive when uncertainty is measured by *Stock volatility*, which indicates that the dismissal risk increases with stock value uncertainty. This result is consistent with Peters and Wagner (2014). When uncertainty is measured by *Delisting rate*, the correlation between forced CEO turnover and uncertainty becomes insignificant. Forced CEO turnover is negatively associated with CEO power, implying that the more powerful a CEO is, the less likely she is to be fired.

On the basis of Columns (1) and (5), Columns (2) and (6) add the interaction term between CEO power and uncertainty to the control list. For both measures of uncertainty, the coefficient of CEO power becomes insignificant and positive after adding in the interaction term, while the estimate of the interaction term itself is significantly negative. Therefore, the dismissal probabilities gap between powerful CEOs and other CEOs in Columns (1) and (5) is entirely correlated with uncertainty. Columns (3) and (7) add in more controls and estimate the specification in Equation (3.1). Columns (4) and (8) estimate the same specification except for controlling for alternative fixed effects (industry-year fixed effects rather than industry fixed effects and year fixed effects). Both specifications confirm the first main finding in this paper, i.e., the more uncertain the environment is, the less likely a powerful CEO is to be fired relative to other CEOs. The dismissal probability of powerful CEOs decreases with uncertainty, which is not the case for other CEOs. The results also show that, among other explanatory variables, *CEO power* is the only one whose relation with forced CEO turnover is consistently moderated by both measures of uncertainty. Those results of statistical tests confirm the graphical patterns in Figure 3.2.

The results are also economically significant: when uncertainty increases by one standard deviation, dual CEOs become 0.47% or 0.57% less likely to be forced out, depending on whether uncertainty is measured by *Stock volatility* or *Delisting rate*. These magnitudes are substantial, given that the average ratio of forced turnover is merely 2.31% among dual CEOs. For robustness purposes, I run the same regressions with alternative measures of CEO power. The results are reported in Table A3.2. For all of the four alternative measures of CEO power, the estimates of the interaction term between CEO power and uncertainty are negative, which confirms my first main finding: compared with other CEOs, powerful CEOs become less likely to be fired as uncertainty increases.

To more directly examine the relation between turnover risk and CEO power under various uncertainty levels, I run separate regressions in stable times and uncertain times, respectively. Specifically, I split my sample of firm-years into two halves based on whether the industry-level uncertainty is below or above the median of that industry across all sample years. Then I regress the dummy variable *Forced turnover* on CEO power and other control variables.

Table 3.3 shows the results. During relatively stable periods, as shown in Panel A, being a dual CEO is associated with either similar or slightly lower dismissal risk, depending on the specification. At maximum, a dual CEO is 0.06% (uncertainty measured by *Stock volatility*) or 0.03% (uncertainty measured by *Delisting rate*) less likely to be fired than a non-dual CEO. In contrast, in relatively uncertain times, the difference in dismissal risk between dual CEOs and non-dual CEOs is much more significant and substantial, as shown in Panel B. Being a dual CEO is associated with 0.23% (uncertainty measured by *Stock volatility*) or 0.15% (uncertainty measured by *Delisting rate*) less probability to be fired in relatively uncertain times. In addition, I test whether CEOs' power affects their turnover-performance sensitivities by adding *Abnormal return* $\times$ *CEO power* to the controls in Columns (4) and (8). Panel B implies that dual CEOs are associated with lower turnover-performance sensitivities in uncertain times, but no such result exists in stable times, as shown in Panel A.

I also do the same analyses using four alternative measures of CEO power and find generally similar results, as shown in Table OA1. The only exception is *Founder CEO*s, who are significantly less likely to be fired either in uncertain times or stable times. The reason might be that *Founder CEO*s are so powerful that they are rarely fired regardless of the external uncertainty. Table 3.3 and Table OA1 confirm my first main finding from the perspective of cross-CEO-group comparison: during uncertain times, powerful CEOs are significantly less likely to be fired compared with other CEOs, while this difference is much smaller and less significant during stable times.

3.3.2 Optimal dismissal decision or CEO entrenchment?

The second question approached in this paper is whether the fact that powerful CEOs are less likely to be fired during times of uncertainty is efficient. The fact could be possibly explained by either firms' changing preferences on CEO power or CEOs' entrenchment, depending on which CEO turnover theory is employed. Optimal dismissal theory assumes that firms make retention or dismissal decisions efficiently (Gibbons and Murphy, 1990; Bushman et al., 2010). According to that theory, firms assess the suitability of both their incumbent CEOs and potential replacements. Firms retain the incumbent CEOs if and only if they are assessed as better than their potential replacements. Applying this theory to the context of uncertainty, if firms' preferences for powerful CEOs increase with uncertainty, then the dismissal probabilities of powerful

CEOs optimally decrease with uncertainty. As opposed to optimal dismissal theory, entrenchment theory assumes that incumbent CEOs are capable of taking various measures to reduce their possibilities of being fired (Shleifer and Vishny, 1989; Hermalin and Weisbach, 1998; Denis et al., 1997; Almazan and Suarez, 2003; Bebchuk and Fried, 2004; Taylor, 2010; Fisman et al., 2014). Following the idea of entrenchment theory, powerful CEOs may take advantage of uncertain periods to exploit their entrenchment, given that replacing them in uncertain times could be extraordinarily costly for firms. This notion of entrenchment provides an alternative explanation for why powerful CEOs are less likely to be dismissed when uncertainty is high. To distinguish those two potential explanations, I examine firm performance, CEO compensation, and stock return during the 2020 Coronavirus Stock Market Crash.

Firm performance

If powerful CEOs are less likely to be fired in uncertain times because of entrenchment, then their average ability should be lower than that of other CEOs. Therefore, in times of uncertainty, firms led by powerful CEOs should perform worse than other firms. On the contrary, if the turnover decisions are efficiently made, firms led by powerful CEOs should not perform worse in uncertain times. I estimate the following firm-year panel regression:

$$\begin{aligned} Firm\ performance_{it} = {} & \beta_0 + \beta_1 Uncertainty_{It} + \beta_2 CEO\ power_{it} \\ & + \beta_3 CEO\ power_{it} \times Uncertainty_{It} + B_4 X_{it} \\ & + B_5 X_{it} \times Uncertainty_{It} + d_i + d_t + \varepsilon_{it} \end{aligned} \tag{3.2}$$

where $Firm\ performance_{it}$ is measured by either Q, ROA, or *Sales growth* of firm i in year t, $CEO\ power_{it}$ is measured by *CEO duality* of firm i in year t, d_i is a dummy for firm i. Other control variables are defined in the identical way as in Equation (3.1). In alternative specifications, I control for either d_{ij} (a dummy for the pair of firm i and CEO j) and d_t or d_{It} (industry-year fixed effects), as a substitute for d_i and d_t

in Equation (3.2). For cross-CEO-group comparisons, $\beta_2 + \beta_3 Uncertainty_{It}$ measures the performance gap between firms run by powerful CEOs versus other firms when the uncertainty level is $Uncertainty_{It}$. β_3 measures how the relation between firm performance and CEO power is moderated by uncertainty. Eentrenchment theory predicts that β_3 is negative. By contrast, if optimal dismissal theory is valid, β_3 should be nonnegative.

The results are reported in Table 3.4. The estimated β_3 is either insignificant or significantly positive, no matter the performance is measured by either Q (Panel A), *ROA* (Panel B), or *Sales growth* (Panel C). That means powerful CEOs' relative performances compared with other CEOs do not worsen with uncertainty, which is predicted by optimal dismissal theory.

Therefore, the evidence on firm performance is consistent with optimal dismissal theory as opposed to CEO entrenchment theory.

CEO compensation

Another test I employ to distinguish between optimal dismissal theory and CEO entrenchment theory is on CEO compensation. Studies find that entrenched CEOs receive higher compensation relative to other CEOs (Bebchuk and Fried, 2004; Masulis et al., 2009; Morse et al., 2011). CEO entrenchment theory interprets this compensation premium as (partly) due to CEO entrenchment and predicts that, if powerful CEOs exploit their entrenchment in uncertain times, they are likely to enjoy an even higher payment during uncertain periods relative to other CEOs' compensation.

I regress both the total and the components of CEO yearly compensation on *Uncertainty*, *CEO power* and other controls. The sum and the components of CEO compensation are both in log terms, not only because those variables are skewed but also to be consistent with a model where CEO pay scales linearly with firm size (Edmans et al., 2012). Following Guthrie et al. (2012), I exclude two firms with outlier CEO compensations, Apple and Fossil, from my sample. Uncertainty is measured by either *Stock volatility* (Panel A) or *Delisting rate* (Panel B). CEO power is measured by *CEO duality*. The firm fixed effects and year fixed effects are controlled for in Columns (1) - (3); the year-industry fixed effects are controlled for in Column (4); the firm-CEO fixed effects and year fixed effects are controlled for in Columns (5) - (8). The coefficient of *CEO power* $\times$ *Uncertainty* indicates how powerful CEOs' higher compensation changes with uncertainty. If the positive wage gap between powerful CEOs and other CEOs widens with uncertainty, then the coefficient of *CEO power* $\times$ *Uncertainty* should be positive.

The results are reported in Table 3.5. In Column (1), the total compensation is regressed on *Uncertainty* and *CEO power*. The result in Column (1) shows that on average powerful CEOs do receive higher compensation, which is consistent with the literature. However, this result itself is insufficient to attest to CEO entrenchment because the higher compensation may be simply a reward for dual CEOs who undertake additional workloads as chairmen. In Columns (2) - (5), the estimated coefficients of *CEO power* $\times$ *Uncertainty* are negative, indicating that the compensation gap between powerful CEOs and other CEOs does not enlarge in times of uncertainty; if anything, the compensation gap decreases with uncertainty. In Columns (6) - (8), I inspect the salary, bonus, and equity-based compensation separately and find that neither of those compensation components witnesses a widened gap between powerful CEOs and other CEOs in times of uncertainty.

Although powerful CEOs are not exceptionally higher paid in uncertain years, they might alternatively secure larger compensation afterward when the firm's operation returns to normal. I examine this possibility by regressing future CEO compensation (in the next year or the year after next) on current uncertainty and other explanatory variables. The results are reported in Appendix A3.3, showing that powerful CEOs are not exceptionally higher paid in the years subsequent to uncertain periods either.

In conclusion, the evidence on CEO compensation is also consistent with optimal dismissal theory as opposed to CEO entrenchment theory.

Stock return during the 2020 Coronavirus Stock Market Crash

The results in previous sections confirm the predictions of optimal dismissal theory for the *equilibrium state*: powerful CEOs are more advantageous in uncertain times, firms accordingly raise the evaluation of powerful CEOs, and consequently, they become less likely to be fired when uncertainty increases. In equilibrium, powerful CEOs and other CEOs have similar performances because they are assessed and retained based on the same criteria.

However, since finding a suitable new CEO is generally an arduous and time-consuming process, the firm-CEO match is sticky to some extent, and there is often a lag between a board's plan to replace the CEO and the actual occurrence. Therefore, optimal dismissal theory also predicts the *out-of-equilibrium state*: when a sudden uncertainty shock hits, the value of incumbent powerful CEOs increases compared with that of other CEOs. Before that change in market conditions materializes in the CEO retention decisions, powerful CEOs should have better performances than other CEOs.

I test this prediction by examining the stock market performances at the beginning of the COVID-19 pandemic, an unforeseen and hugely influential uncertainty shock. Between February 20th and March 20th in 2020, the stock market collapsed as the COVID-19 pandemic spurred lots of uncertainty and anxiety. In addition, firms are unlikely to adjust their management team in reaction to the pandemic outbreak during such a short period. Therefore, the beginning of the pandemic provides an ideal setting to test the out-of-equilibrium prediction.

I compare the stock returns during this period of firms with powerful CEOs versus other firms. If optimal dismissal theory is valid, which implies that powerful CEOs are more advantageous and favored during uncertain times, then powerful CEOs should be associated with higher stock returns than other CEOs at the onset of the pandemic. Otherwise, if the CEO entrenchment theory is valid, then the stock returns during this period should be either uncorrelated with CEO power or negatively correlated due to the concern over entrenchment.

Table 3.6 shows the results. In Panel A, the dependent variable *Return pandemic*, the accumulated return between February 20th and March 20th in 2020, is regressed on *CEO power* and other control variables. *CEO power* is measured by five alternative proxies: *CEO duality*, *CEO's concentration of titles*, *CEO tenure*, *Longer directorship*, and *Founder CEO*. A tiny fraction (13 out of 1546) of firms that experienced a CEO turnover during the one-month period are excluded from the regression sample. Consistent with the prediction of optimal dismissal theory, the correlation between *Return pandemic* and *CEO power* is positive for all the five measures and is significant for four measures except *Longer directorship*. Quantitatively, firms with a dual CEO have 2.8% higher stock returns on average in the one-month period than the other firms.

To alleviate the concern that the observed correlation between higher stock returns and CEO power is not unique to uncertain periods, I repeat the analyses above on the same period (i.e. from February 20th to March 20th) in the previous year, 2019. The results of this placebo test are presented in Panel B, showing no differences in the returns between powerful CEOs and other CEOs.

In summary, based on the evidence on firm performance and CEO compensation from 1999 to 2020 and stock return during the 2020 Coronavirus Stock Market Crash, powerful CEOs' lowered forced turnover probabilities during uncertain times should result from optimal dismissal decisions rather than CEO entrenchment. In other words, firms reveal increased preferences for powerful CEOs in times of uncertainty.

3.3.3 Mechanisms

In the previous section, I show that powerful CEOs are more beneficial and favored by firms in uncertain times. This section examines two mechanisms that potentially explain this increased preference for powerful CEOs in times of uncertainty.

Information sharing

The first potential mechanism I examine is information sharing. During uncertain times, the business environment and firms' internal conditions change fast, with old information becoming obsolete and new information being generated quickly. Therefore, up-to-date information becomes even more crucial for directors to advise and monitor CEOs.

Boards meet periodically to monitor and advice the executives. Independent directors, who are not involved in daily operations, rely on executive directors for firm-specific information. Therefore, during uncertain times, information sharing between the CEO and the board becomes especially important. Adams and Ferreira (2007) model that a CEO faces a trade-off in sharing information, which helps the CEO receive better advice from the board but at the same time possibly causes stricter monitoring. Adams and Ferreira thus argue that a friendly board could be optimal. Following the logic of their theory, friendly boards and powerful CEOs become more beneficial in uncertain times, when efficient information sharing is especially crucial. Therefore, better information sharing might make powerful CEOs more popular in times of uncertainty.

I examine this potential mechanism by dividing the sample of firm-year observations into two parts based on their relative information asymmetry compared with their peer firms in the same industry and year. I measure information asymmetry following Duchin et al. (2010). For each firm-year observation, I calculate the number of following analysts, the standard deviation of analysts' quarterly earnings forecasts scaled by book value, and the average bias between analysts' forecasts and actual earnings scaled by book value. Then I convert those three numbers into percentiles (*reversed* percentile for the number of analysts) within the same industry and year. Next, I take the average of those three (reversed) percentiles as the information asymmetry index. The half of the firm-year observations with above-median asymmetry indices constitute the opaque group. The other half of the firm-year observations constitute the transparent group. Since opaque firms face a more severe information asymmetry in times of uncertainty, I hypothesize that the decrease in powerful CEOs' turnover rate is more evident among opaque firms than in transparent firms.

I estimate Equation (3.1) on each of the two groups, respectively. The results are reported in Table 3.7. Consistent with my hypothesis, Panel A shows that powerful CEOs of opaque firms become less likely to be fired when uncertainty increases, which is consistent with the baseline results on the entire sample. In contrast, Panel B shows that such a result is not found among transparent firms. The results are robust to whether uncertainty is measured by *Stock volatility* or *Delisting rate*.

Reaction speed

The second possible mechanism I examine is reaction speed. Some firm decisions, especially the major ones, need to be consulted with the board and approved. Unlike executives who run the firm on a daily basis, directors often have other commitments elsewhere. That means more board power and less CEO power unavoidably cause delays in firms' decision-making. In times of uncertainty, the business environment is fast-changing and delayed actions might incur costly consequences.

As busier directors are less likely to respond to requests promptly, having a CEO with more decision-making power might be optimal in uncertain times if the board consists of busier directors. Therefore, I hypothesize that firms with busier directors have especially higher preferences for powerful CEOs in times of uncertainty.

I divide firms into two groups based on the average busyness of their directors, which is measured by the number of directorships they hold in publicly listed firms. A firm is assigned to the busier group if its directors' average number of directorships is above the median among other firms in the same industry and year; otherwise, it is assigned to the less busy group. I reestimate Equation (3.1) on each of those two groups and report the results in Table 3.8. The evidence supports my hypothesis: among firms with busier directors, powerful CEOs are significantly less likely to be fired when uncertainty increases, which is consistent with the baseline result on the entire sample; such a result is not found among firms with less busy directors. The evidence is robust to whether uncertainty is measured by *Stock volatility* or *Delisting rate*.

Besides the two mechanisms discussed above, inspired by the risk-shifting literature, I test one more potential mechanism that powerful CEOs are increasingly preferred in times of uncertainty because they transfer wealth from creditors to shareholders by choosing riskier firm policies. For this mechanism to be valid, powerful CEOs need to be associated with subsequent higher default rates and/or lower survival rates. As shown in Appendix A3.4, when uncertainty is higher, firms with powerful CEOs are neither less likely to survive nor more likely to default in the subsequent two years. Besides, in untabulated results, I examine risk-related firm policies like leverage and find no significant distinctions between powerful CEOs and other CEOs in times of uncertainty. Therefore, the riskiness of firm policies is unlikely to be a mechanism for my baseline results.

In conclusion, two mechanisms potentially explain why powerful CEOs are more beneficial and increasingly preferred in times of uncertainty: they are more willing to share information with the board, and more capable of making swift responses to changing market conditions.

3.4 Conclusion

This paper examines whether powerful CEOs are more beneficial in uncertain times. Documenting less powerful CEOs dismissed in uncertain times, I find that the evidence supports optimal dismissal theory, which implies powerful CEOs are more desirable in turbulent times. Two potential mechanisms explain why powerful CEOs are more effective during uncertain times: they are more willing to share information with the board, and more capable of taking quick action.

By showing powerful CEOs benefit firms' performance under uncertainty, this paper complements the existing literature on the consequences of CEO power, which predominantly focuses on the costs of managerial entrenchment. This paper also compares two rival theories for CEO turnover and provides supporting evidence for optimal dismissal theory. In addition, this paper has important policy implications. Much of the existing and proposed regulations focus on limiting CEOs' power.[3] This paper serves as a caution that having a powerful CEO is sometimes a firm's optimal choice, and thus externally imposed constraints on CEO power might create rather than fix distortions. Alternative to restricting CEO power, it might be advisable for policymakers to assess other tools for protecting shareholder value, like regulating long-term incentive plans to align the CEOs' and shareholders' interests.

Furthermore, my results suggest that business environment interacts with corporate governance, which has been under-discussed in the literature. Future research on corporate governance might benefit from exploring other factors of business environ-

[3]Following the Sarbanes-Oxley Act of 2002, the NYSE and NASDAQ updated their listing rules, which require a majority of independent directors on the corporate board and fully independent nominating, compensation, and audit committees (Guo and Masulis, 2015). The Dodd-Frank Wall Street Reform and Consumer Protection Act of 2010 (Dodd-Frank Act) contains provisions that require firms to allow stockholders to nominate directors, make more comprehensive proxy disclosures, and hold shareholder advisory votes on certain corporate governance issues like executive compensation ("Say on Pay"). In 2009 SEC adopted amendments to Regulation S-K, which require companies to disclose why they have chosen to combine or separate the CEO and chairperson roles. There is increasing pressure from investors and experts to split the roles of CEO and board chair. Although SEC has not adopted such a rule, it did force Elon Musk to step aside as chairperson of the Tesla board for three years as part of their settlement in 2018.

ment than uncertainty that also influence the effectiveness of CEO power. In addition, this paper provides an explanation for why CEO duality exists in many firms, while simultaneously bringing up further questions: what are the causes and consequences of the declining prevalence of CEO duality over the past decades? Is that trend efficient? More research needs to be done to answer those questions.

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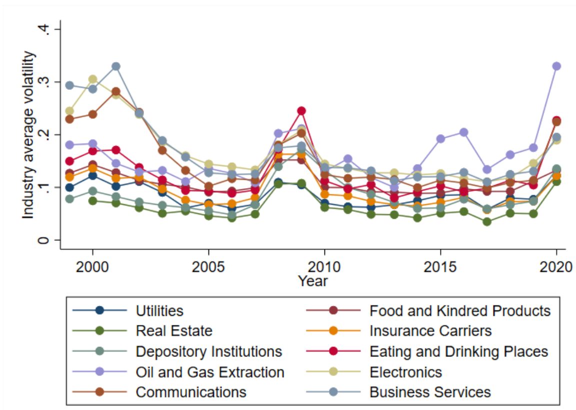
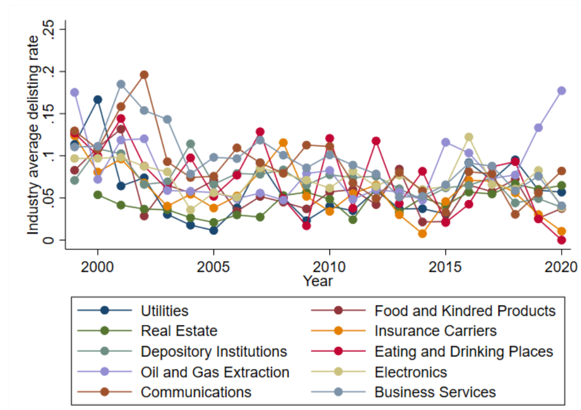
Figure 3.1: Industry uncertainty over the years

This figure illustrates the trends in industry uncertainty between 1999 and 2020. I pick ten representative industries with relatively large numbers of observations. Uncertainty is measured by two alternative proxies on the industry-year level: *Stock volatility* in Subfigures (a) and (c), and *Delisting rate* in Subfigures (b) and (d). In Subfigures (a) and (b), the vertical axis is the original value of *Stock volatility* and *Delisting rate*, respectively. In Subfigures (c) and (d), the vertical axis is the demeaned-and-detrended value of *Stock volatility* and *Delisting rate*, respectively. Specifically, I subtract the industry average of uncertainty measures from the original value to obtain demeaned value; then, I subtract the year average of demeaned value from the demeaned value to obtain the demeaned-and-detrended value. The demeaning process corresponds to controlling for industry fixed effects in regressions, while detrending corresponds to controlling for year fixed effects.

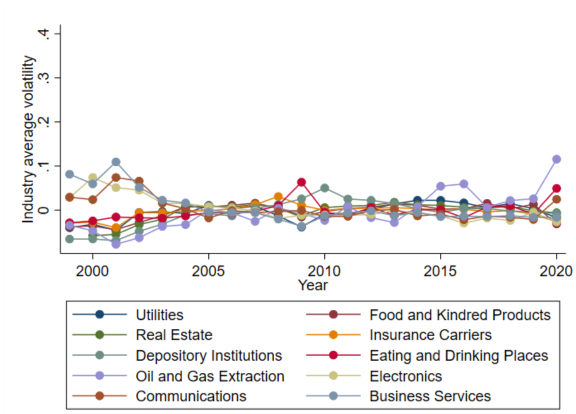
(a) Stock volatility



(b) Delisting rate



(c) Stock volatility, demeaned-and-detrended



(d) Delisting rate, demeaned-and-detrended

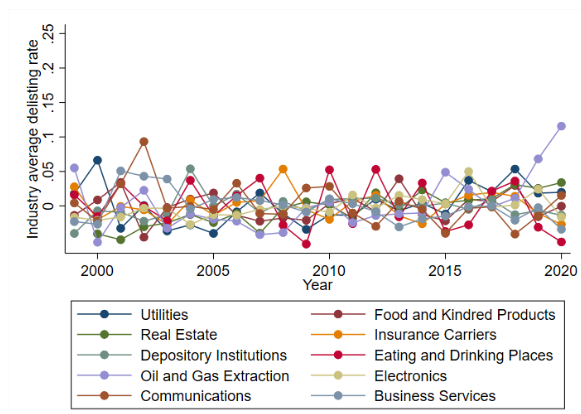
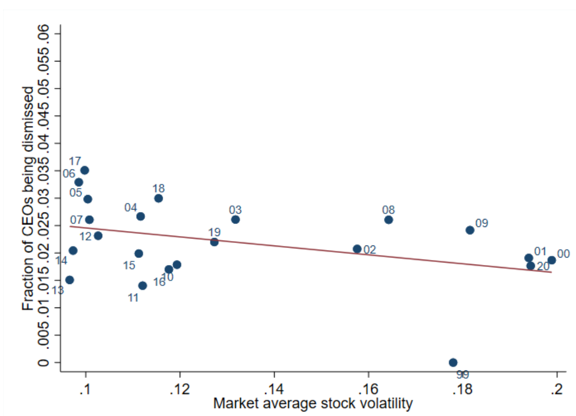


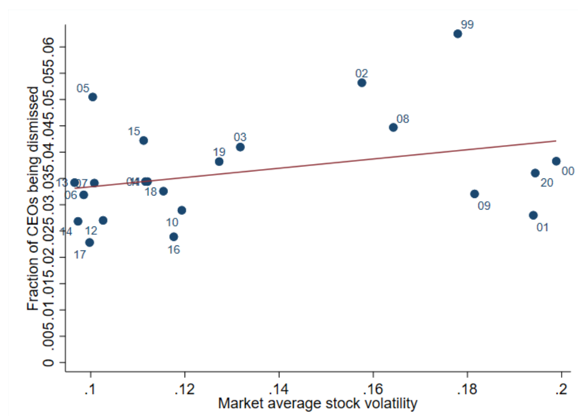
Figure 3.2: Market uncertainty and CEO forced turnover

This figure illustrates the relations between market-level uncertainty and the fractions of powerful CEOs and other CEOs being dismissed in each year, repectively. I split the entire sample into two subgroups based on whether the CEO is also the board chair, and then for each subgroup plot the relation between uncertainty and dismissal rate. In all subfigures, each dot represents a year between 1999 and 2020 (the last two digits of the corresponding year are tagged next to each dot). The vertical coordinate of each dot is the fraction of dual CEOs (Subfigures (a) and (c)) or non-dual CEOs (Subfigures (b) and (d)) being dismissed in the corresponding year. The horizontal coordinate of each dot is the market-level uncertainty in the corresponding year, proxied by one of two alternative measures: *Market average stock volatility* (Subfigures (a) and (b)) and *Market average delisting rate* (Subfigures (c) and (d)). Both measures of uncertainty are averaged across all firms in each specific year.

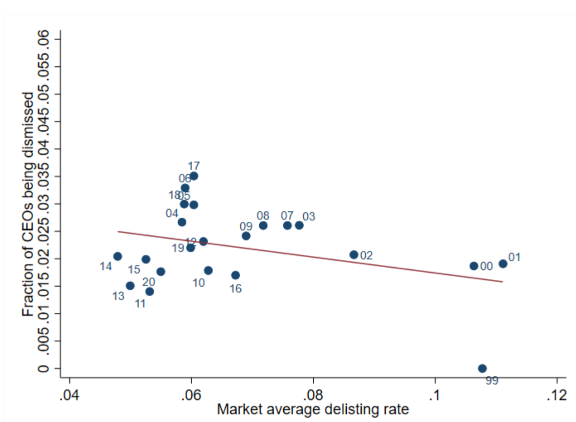
(a) Dual CEOs; uncertainty = *Market average stock volatility*



(b) Non-dual CEOs; uncertainty = *Market average stock volatility*



(c) Dual CEOs; uncertainty = *Market average delisting rate*



(d) Non-dual CEOs; uncertainty = *Market average delisting rate*

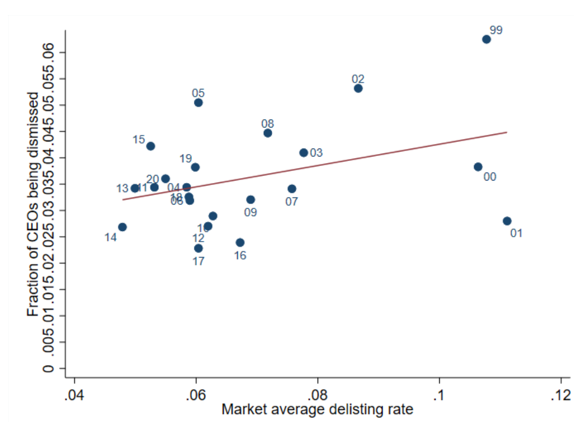


Table 3.1: Descriptive statistics

This table reports the descriptive statistics of variables. The sample consists of 30,129 firm-years from 1999 to 2020. The description and sources of these variables can be found in Table A3.1.

	N	Mean	Median	SD	Min	Max
Forced Turnover of CEO						
Forced turnover	32033	0.028	0.000	0.165	0.000	1.000
Environmental Uncertainty						
Stock volatility	32033	0.129	0.123	0.050	0.035	0.334
Delisting rate	32033	0.066	0.063	0.037	0.000	0.405
Market average stock volatility	32033	0.127	0.112	0.033	0.097	0.199
Market average delisting rate	32033	0.065	0.060	0.015	0.048	0.111
CEO Characteristics						
CEO duality	32033	0.538	1.000	0.499	0.000	1.000
CEO's concentration of titles	32033	2.424	2.000	0.522	1.000	3.000
CEO tenure	32033	8.445	6.247	7.150	0.584	39.025
Longer directorship	32029	0.641	1.000	0.480	0.000	1.000
Founder CEO	32033	0.044	0.000	0.205	0.000	1.000
CEO age $\geq$ 60	32033	0.333	0.000	0.471	0.000	1.000
CEO is female	32033	0.031	0.000	0.172	0.000	1.000
Ln(compensation)	31932	8.151	8.225	1.039	4.405	11.348
Ln(salary)	31946	6.450	6.620	1.250	-6.908	7.824
Ln(bonus)	10611	6.147	6.319	1.521	-3.442	9.402
Ln(equity-based)	6069	7.470	7.544	1.354	-1.609	11.052

Continued on next page

Table 3.1 continued

Firm Performance and Characteristics						
Q	32028	1.934	1.490	1.349	0.588	37.772
ROA	30594	0.118	0.115	0.100	-0.521	0.471
Sales growth	32011	0.090	0.063	0.235	-0.697	2.790
Surviving the next two years	32033	0.948	1.000	0.222	0.000	1.000
Defaulting in the next two years	14809	0.002	0.000	0.042	0.000	1.000
Abnormal return	32033	0.049	-0.002	0.430	-0.902	6.620
Independent board	32033	0.779	0.818	0.136	0.000	1.000
Firm Size	32033	7.388	7.296	1.605	2.926	11.999
Board size	32033	9.454	9.000	2.403	5.000	25.000
Female director	32033	0.741	1.000	0.438	0.000	1.000
CEO successor	32033	0.196	0.000	0.397	0.000	1.000
Information asymmetry	31133	50.150	49.333	19.506	7.333	96.667
# Directorships	32033	1.877	1.800	0.607	1.000	4.727

Table 3.2: Regression of forced CEO turnover

This table presents the regression estimation of the impacts of uncertainty, CEO characteristics, and firm characteristics on forced CEO turnovers. The dependent variable *Forced turnover* is a dummy, equal to 1 if a CEO is dismissed in that year and 0 otherwise. Uncertainty is measured by two alternative proxies on the industry-year level: *Stock volatility* in Columns (1) - (4), and *Delisting rate* in Columns (5) - (8). All variables except uncertainty measures are on the firm-year level. CEO power is measured by *Dual CEO*, an indicator equal to one if the CEO is also the board chair and zero otherwise. In Columns (2) - (4) and (6) - (8), CEO power and other CEO and firm characteristics are interacted with uncertainty, in order to show how their impacts on forced CEO turnover are moderated by uncertainty. The year fixed effects and industry fixed effects are controlled for in Columns (1) - (3) and (5) - (7), while the year-industry fixed effects are controlled for in Columns (4) and (8). Standard errors, adjusted for clustering at the industry level, are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. All variables are defined in Table A3.1.

Dependent variable =	*Forced turnover*							
Uncertainty =	*Stock volatility*				*Delisting rate*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Uncertainty	0.078**	0.127***	0.060		0.044	0.121***	0.105	
	(0.04)	(0.05)	(0.14)		(0.03)	(0.05)	(0.18)	
CEO power	-0.006**	0.005	0.005	-0.000	-0.006**	0.003	0.001	-0.002
	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
CEO power × Uncertainty		-0.081**	-0.104***	-0.095***		-0.134***	-0.141***	-0.156***
		(0.04)	(0.04)	(0.03)		(0.04)	(0.04)	(0.05)
Abnormal return			-0.058***	-0.070***			-0.040***	-0.045***
			(0.01)	(0.01)			(0.01)	(0.01)
Abnormal return × Uncertainty			0.122**	0.161***			0.018	0.020
			(0.05)	(0.06)			(0.07)	(0.09)
Independent board			-0.018	0.028			-0.021	0.006
			(0.03)	(0.03)			(0.02)	(0.02)
Independent board × Uncertainty			0.063	-0.166			0.154	-0.032
			(0.14)	(0.16)			(0.19)	(0.22)
Firm Size			0.004	0.003			0.002	0.000

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Table 3.2 continued

Uncertainty =	*Stock volatility*				*Delisting rate*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			(0.00)	(0.00)			(0.00)	(0.00)
Firm Size × Uncertainty			-0.015	-0.013			-0.003	0.017
			(0.02)	(0.02)			(0.02)	(0.02)
CEO age ≥ 60			0.002	0.000			-0.002	-0.006
			(0.00)	(0.00)			(0.00)	(0.00)
CEO age ≥ 60 × Uncertainty			0.026	0.013			0.104**	0.130**
			(0.03)	(0.03)			(0.05)	(0.06)
CEO is female			0.005	0.009			-0.002	0.006
			(0.02)	(0.01)			(0.02)	(0.02)
CEO is female × Uncertainty			-0.073	-0.033			-0.027	-0.017
			(0.12)	(0.12)			(0.25)	(0.26)
Board size			-0.003**	-0.002			-0.001	0.001
			(0.00)	(0.00)			(0.00)	(0.00)
Board size × Uncertainty			0.010	0.006			-0.008	-0.030*
			(0.01)	(0.01)			(0.02)	(0.02)
Female director			-0.001	-0.001			0.003	0.004
			(0.01)	(0.01)			(0.01)	(0.01)
Female director × Uncertainty			0.011	0.013			-0.031	-0.039
			(0.05)	(0.05)			(0.06)	(0.07)
CEO successor			0.002	0.004			0.010*	0.005
			(0.01)	(0.01)			(0.01)	(0.01)
CEO successor × Uncertainty			0.037	0.018			-0.032	0.015
			(0.05)	(0.05)			(0.06)	(0.06)
Observations	32033	32033	32033	32033	32033	32033	32033	32033
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No

Continued on next page

Table 3.2 continued

Uncertainty =	Stock volatility				Delisting rate			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Industry FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
YearIndustry FE	No	No	No	Yes	No	No	No	Yes

Table 3.3: Separate regressions of forced CEO turnover during stable times and uncertain times

This table presents the regression estimation of the impacts of uncertainty, CEO characteristics, and firm characteristics on forced CEO turnovers in more stable times and more uncertain times, respectively. The sample of firm-years is split into two halves based on whether the industry-level uncertainty, measured by either *Stock volatility* or *Delisting rate*, is lower or higher than the median uncertainty of that industry across all sample years. The dependent variable *Forced turnover* is a dummy, equal to 1 if a CEO is dismissed in that year and 0 otherwise. Uncertainty is measured by two alternative proxies on the industry-year level: *Stock volatility* in Columns (1) - (4), and *Delisting rate* in Columns (5) - (8). All variables except uncertainty measures are on the firm-year level. CEO power is measured by *Dual CEO*, an indicator equal to one if the CEO is also the board chair and zero otherwise. In Columns (4) and (8), CEO power is interacted with *Abnormal return*, in order to show how CEO power affects their turnover-performance sensitivity. The year fixed effects and industry fixed effects are controlled for in Columns (1) - (2) and (5) - (6), while the year-industry fixed effects are controlled for in Columns (3) - (4) and (7) - (8). Standard errors, adjusted for clustering at the industry level, are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. All variables are defined in Table A3.1.

Dependent variable =	*Forced turnover*							
Uncertainty =	*Stock volatility*				*Delisting rate*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Relatively stable times								
Uncertainty	0.215	0.145			0.057	0.050		
	(0.16)	(0.18)			(0.09)	(0.08)		
CEO power	-0.002	-0.004	-0.006*	-0.006*	-0.004	-0.005*	-0.006**	-0.006**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Abnormal return		-0.051***	-0.056***	-0.060***		-0.039***	-0.041***	-0.045***
		(0.01)	(0.01)	(0.01)		(0.00)	(0.00)	(0.01)
Independent board		0.003	0.012	0.012		0.007	0.010	0.011
		(0.02)	(0.02)	(0.02)		(0.02)	(0.02)	(0.02)
Firm Size		0.002	0.001	0.001		0.000	-0.000	-0.000
		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)
CEO age $\geq$ 60		0.003	0.002	0.002		0.001	0.001	0.000
		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)
CEO is female		-0.001	0.002	0.002		-0.003	0.000	0.001

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Table 3.3 continued

Uncertainty =	*Stock volatility*				*Delisting rate*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.01)
Board size		-0.002***	-0.002***	-0.002***		-0.001*	-0.001	-0.001
		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)
Female director		0.001	0.002	0.002		0.002	0.001	0.001
		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)
CEO successor		0.005	0.006	0.006		0.007	0.006	0.006
		(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.01)
Abnormal return × CEO power				0.009				0.008
				(0.01)				(0.01)
Observations	16216	16216	16216	16216	17043	17043	17043	17043
Year FE	Yes	Yes	No	No	Yes	Yes	No	No
Industry FE	Yes	Yes	No	No	Yes	Yes	No	No
YearIndustry FE	No	No	Yes	Yes	No	No	Yes	Yes
Panel B: Relatively uncertain times								
Uncertainty	0.139***	0.126***			-0.024	-0.017		
	(0.04)	(0.04)			(0.06)	(0.06)		
CEO power	-0.010***	-0.013***	-0.015***	-0.016***	-0.010***	-0.014***	-0.016***	-0.017***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Abnormal return		-0.032***	-0.035***	-0.042***		-0.040***	-0.043***	-0.052***
		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.01)
Independent board		-0.013	-0.010	-0.011		-0.012	-0.008	-0.009
		(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.01)
Firm Size		0.001	0.001	0.002		0.003**	0.003**	0.003**
		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)

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Table 3.3 continued

Uncertainty =	*Stock volatility*				*Delisting rate*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CEO age $\geq$ 60		0.007**	0.005	0.005		0.009***	0.007**	0.007**
		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)
CEO is female		-0.004	0.000	0.000		-0.004	0.001	0.001
		(0.01)	(0.01)	(0.01)		(0.01)	(0.01)	(0.01)
Board size		-0.001	-0.001	-0.001		-0.002*	-0.002*	-0.002*
		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)
Female director		0.001	-0.000	-0.000		0.001	0.000	0.000
		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)
CEO successor		0.008**	0.006*	0.006*		0.006*	0.006*	0.006*
		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)
Abnormal return $\times$ CEO power				0.014**				0.016***
				(0.01)				(0.01)
Observations	15817	15817	15817	15817	14990	14990	14990	14990
Year FE	Yes	Yes	No	No	Yes	Yes	No	No
Industry FE	Yes	Yes	No	No	Yes	Yes	No	No
YearIndustry FE	No	No	Yes	Yes	No	No	Yes	Yes

Table 3.4: Regression of firm performance

This table presents the regression estimation of the impacts of CEO power and uncertainty on firm performance. The dependent variable is *Q* in Panel A, *ROA* in Panel B, and *Sales growth* in Panel C. Uncertainty is measured by two alternative proxies on the industry-year level: *Stock volatility* in Columns (1) - (5), and *Delisting rate* in Columns (6) - (10). All variables except uncertainty measures are on the firm-year level. CEO power is measured by *Dual CEO*, an indicator equal to one if the CEO is also the board chair and zero otherwise. In Columns (2) - (5) and (7) - (10), CEO power and other controls are interacted with uncertainty, in order to show how their impacts on firm performance are moderated by uncertainty. The untabulated control variables in Panels B - C are the same as in Panel A. The year fixed effects are controlled for throughout all specifications except for Columns (4) and (9). The firm fixed effects are controlled for in Columns (1) - (3) and (6) - (8), while the firm-CEO fixed effects are controlled for in Columns (5) and (10). The year-industry fixed effects are controlled for in Columns (4) and (9). Standard errors, adjusted for clustering at the industry level, are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. All variables are defined in Table A3.1.

Uncertainty =	*Stock volatility*					*Delisting rate*				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Dependent variable = *Q*										
Uncertainty	2.170**	1.930*	3.641**		1.277	-0.900**	-0.921	-0.539		-1.641
	(0.92)	(1.12)	(1.46)		(1.39)	(0.37)	(0.58)	(2.00)		(2.03)
CEO power	0.063**	0.010	-0.037	0.056	-0.080	0.064**	0.062	0.066	0.067	0.008
	(0.03)	(0.07)	(0.06)	(0.06)	(0.07)	(0.03)	(0.05)	(0.04)	(0.04)	(0.06)
CEO power × Uncertainty		0.412	0.767	0.021	0.699*		0.036	0.027	-0.113	0.040
		(0.55)	(0.49)	(0.44)	(0.38)		(0.58)	(0.56)	(0.51)	(0.47)
Abnormal return			0.487***	0.622***	0.502***			0.561***	0.654***	0.509***
			(0.15)	(0.14)	(0.15)			(0.14)	(0.14)	(0.15)
Abnormal return × Uncertainty			1.346	0.587	0.996			1.923	0.835	2.060
			(1.20)	(1.05)	(1.26)			(2.36)	(2.05)	(2.57)
Independent board			0.188	-1.008***	0.185			-0.246	-0.429**	-0.105
			(0.29)	(0.23)	(0.18)			(0.19)	(0.18)	(0.16)
Independent board × Uncertainty			-1.495	6.428***	-1.081			2.413	4.941**	1.589
			(1.77)	(1.70)	(1.43)			(2.08)	(2.35)	(2.24)
Firm Size			-0.022	-0.081**	-0.001			-0.039	-0.061**	-0.007

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Table 3.4 continued

Uncertainty =	Stock volatility					Delisting rate				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
			(0.06)	(0.04)	(0.05)			(0.04)	(0.03)	(0.03)
Firm Size × Uncertainty			-0.063	0.046	0.070			0.011	-0.196	0.176
			(0.26)	(0.16)	(0.27)			(0.17)	(0.14)	(0.14)
CEO age ≥ 60			0.093*	0.042	0.127**			0.048	0.044	0.046
			(0.05)	(0.04)	(0.06)			(0.03)	(0.03)	(0.03)
CEO age ≥ 60 × Uncertainty			-0.587	-0.178	-0.780*			-0.515	-0.394	-0.249
			(0.45)	(0.37)	(0.46)			(0.38)	(0.36)	(0.29)
CEO is female			-0.032	-0.059				0.019	0.114	
			(0.19)	(0.17)				(0.11)	(0.10)	
CEO is female × Uncertainty			0.973	1.274				1.163	-0.199	
			(1.60)	(1.34)				(1.12)	(1.10)	
Board size			-0.011	0.005	-0.020			-0.008	-0.015	-0.009
			(0.02)	(0.02)	(0.02)			(0.01)	(0.01)	(0.01)
Board size × Uncertainty			-0.141	-0.228*	-0.036			-0.328*	-0.147	-0.243*
			(0.19)	(0.14)	(0.18)			(0.18)	(0.15)	(0.14)
Female director			-0.075	-0.102	-0.045			-0.053	-0.043	-0.025
			(0.07)	(0.06)	(0.07)			(0.05)	(0.05)	(0.05)
Female director × Uncertainty			0.393	0.786	0.373			0.531	0.661	0.477
			(0.70)	(0.61)	(0.66)			(0.52)	(0.50)	(0.57)
CEO successor			-0.147*	-0.093	-0.118**			-0.034	-0.044	-0.035
			(0.08)	(0.06)	(0.05)			(0.03)	(0.03)	(0.04)
CEO successor × Uncertainty			1.346**	0.838	0.845**			1.096***	0.996***	0.548
			(0.62)	(0.57)	(0.40)			(0.34)	(0.30)	(0.48)
Observations	28569	28569	28569	28569	28569	28569	28569	28569	28569	28569
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	No	Yes

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Table 3.4 continued

Uncertainty =	*Stock volatility*					*Delisting rate*				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Firm FE	Yes	Yes	Yes	No	No	Yes	Yes	Yes	No	No
FirmCEO FE	No	No	No	No	Yes	No	No	No	No	Yes
YearIndustry FE	No	No	No	Yes	No	No	No	No	Yes	No
Panel B: Dependent variable = *ROA*										
Uncertainty	-0.318**	-0.392***	-0.408***		-0.453***	-0.078	-0.128	-0.114		-0.133
	(0.13)	(0.14)	(0.14)		(0.13)	(0.06)	(0.09)	(0.18)		(0.14)
CEO power	0.006**	-0.011**	-0.016***	-0.006**	-0.010**	0.006*	0.000	-0.004	-0.002	0.001
	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
CEO power × Uncertainty		0.126***	0.124***	0.052**	0.099***		0.087	0.066	0.038	0.044
		(0.05)	(0.04)	(0.02)	(0.03)		(0.06)	(0.04)	(0.04)	(0.03)
Observations	27231	27231	27231	27231	27231	27231	27231	27231	27231	27231
Controls	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	No	Yes
Firm FE	Yes	Yes	Yes	No	No	Yes	Yes	Yes	No	No
FirmCEO FE	No	No	No	No	Yes	No	No	No	No	Yes
YearIndustry FE	No	No	No	Yes	No	No	No	No	Yes	No
Panel C: Dependent variable = *Sales growth*										
Uncertainty	-0.661*	-0.681*	-0.461		-0.976*	-0.278	-0.281	-0.733		-1.104
	(0.34)	(0.38)	(0.75)		(0.57)	(0.19)	(0.21)	(0.66)		(0.69)
CEO power	0.011***	0.007	-0.001	0.021*	0.016	0.011***	0.011	0.013*	0.009	0.022**
	(0.00)	(0.01)	(0.01)	(0.01)	(0.02)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
CEO power × Uncertainty		0.034	0.056	-0.118	0.009		0.005	-0.093	-0.050	-0.045
		(0.11)	(0.10)	(0.08)	(0.11)		(0.10)	(0.09)	(0.11)	(0.09)
Observations	28551	28551	28551	28551	28551	28551	28551	28551	28551	28551
Controls	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes

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Table 3.4 continued

Uncertainty =	*Stock volatility*					*Delisting rate*				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	No	Yes
Firm FE	Yes	Yes	Yes	No	No	Yes	Yes	Yes	No	No
FirmCEO FE	No	No	No	No	Yes	No	No	No	No	Yes
YearIndustry FE	No	No	No	Yes	No	No	No	No	Yes	No

Table 3.5: Regression of CEO compensation

This table presents the regression estimation of the impacts of CEO power and uncertainty on CEO compensation. In Columns (1) - (5), the dependent variable is the natural logarithm of CEO total compensation. In Columns (6) - (8), the dependent variable is the natural logarithm of one of the components of CEO compensation: *Salary*, *Bonus*, or *Equity-based* component. Uncertainty is measured by two alternative proxies on the industry-year level: *Stock volatility* in Panel A, and *Delisting rate* in Panel B. All variables except uncertainty measures are on the firm-year level. CEO power is measured by *Dual CEO*, an indicator equal to one if the CEO is also the board chair and zero otherwise. In Columns (2) - (8), CEO power and other controls are interacted with uncertainty, in order to show how their impacts on CEO compensation are moderated by uncertainty. The year fixed effects are controlled for throughout all specifications except for Column (4). The firm fixed effects are controlled for in Columns (1) - (3), while the firm-CEO fixed effects are controlled for in Columns (5) - (8). The year-industry fixed effects are controlled for in Column (4). Standard errors, adjusted for clustering at the industry level, are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. All variables are defined in Table A3.1.

Dependent variable =	Ln(*Compensation*)					Ln(*Salary*)	Ln(*Bonus*)	Ln(*Equity-based*)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Uncertainty = *Stock volatility*								
Uncertainty	-0.370	-0.118	0.778		1.333	0.682	3.119	3.858
	(0.51)	(0.58)	(1.01)		(1.03)	(1.87)	(2.01)	(2.38)
CEO power	0.095***	0.148***	0.118***	0.138***	0.084*	0.040	0.245	0.231
	(0.02)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.17)	(0.14)
CEO power × Uncertainty		-0.406	-0.453*	-0.651**	-0.319	0.243	-0.734	-0.851
		(0.25)	(0.23)	(0.26)	(0.29)	(0.28)	(0.69)	(0.78)
Abnormal return			0.187***	0.207***	0.180***	-0.004	0.319***	0.028
			(0.03)	(0.04)	(0.04)	(0.03)	(0.08)	(0.09)
Abnormal return × Uncertainty			-0.389*	-0.494**	-0.427	0.117	-0.830**	-0.150
			(0.20)	(0.23)	(0.30)	(0.14)	(0.38)	(0.46)
Independent board			0.337***	0.219	0.360**	0.308	0.051	0.468
			(0.12)	(0.17)	(0.15)	(0.23)	(0.37)	(0.34)
Independent board × Uncertainty			-0.125	1.581	-1.020	-1.371	-0.843	-1.029
			(0.77)	(1.08)	(0.80)	(1.76)	(1.72)	(1.71)
Firm Size			0.375***	0.434***	0.361***	0.141***	0.446***	0.583***

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Table 3.5 continued

Dependent variable =	Ln(*Compensation*)					Ln(*Salary*)	Ln(*Bonus*)	Ln(*Equity-based*)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			(0.04)	(0.02)	(0.04)	(0.03)	(0.07)	(0.09)
Firm Size × Uncertainty			0.030	-0.174	0.051	-0.126	-0.177	-0.156
			(0.14)	(0.12)	(0.14)	(0.12)	(0.20)	(0.30)
CEO age $\geq$ 60			0.046	0.031	0.057*	0.066	-0.082	-0.111
			(0.04)	(0.04)	(0.03)	(0.06)	(0.07)	(0.10)
CEO age $\geq$ 60 × Uncertainty			-0.521**	-0.431	-0.483**	-0.252	0.284	0.122
			(0.26)	(0.28)	(0.23)	(0.33)	(0.48)	(0.68)
CEO is female			0.133	0.116				
			(0.09)	(0.08)				
CEO is female × Uncertainty			-1.035*	-0.961*				
			(0.53)	(0.50)				
Board size			0.008	-0.007	-0.002	-0.009	-0.015	-0.047*
			(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Board size × Uncertainty			0.003	0.098	-0.006	0.131**	-0.079	0.288**
			(0.09)	(0.08)	(0.08)	(0.06)	(0.14)	(0.14)
Female director			0.090***	0.057*	0.102***	0.063	-0.063	0.185
			(0.03)	(0.03)	(0.03)	(0.08)	(0.13)	(0.16)
Female director × Uncertainty			-0.381*	-0.129	-0.600***	-0.198	-0.059	-1.170
			(0.20)	(0.24)	(0.16)	(0.49)	(0.80)	(1.02)
CEO successor			-0.074*	-0.047	-0.037	-0.008	-0.155	-0.087
			(0.04)	(0.04)	(0.04)	(0.03)	(0.09)	(0.11)
CEO successor × Uncertainty			0.526*	0.346	0.286	0.305	0.602	0.682
			(0.30)	(0.30)	(0.23)	(0.39)	(0.46)	(0.51)
Observations	25432	25432	25432	25432	25432	25439	8698	4979
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes

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Table 3.5 continued

Dependent variable =	Ln(*Compensation*)					Ln(*Salary*)	Ln(*Bonus*)	Ln(*Equity-based*)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Firm FE	Yes	Yes	Yes	No	No	No	No	No
FirmCEO FE	No	No	No	No	Yes	Yes	Yes	Yes
YearIndustry FE	No	No	No	Yes	No	No	No	No
Panel B: Uncertainty = *Delisting rate*								
Uncertainty	0.083	0.076	-1.831*		-0.676	1.842	-1.802	-2.689
	(0.18)	(0.23)	(0.96)		(1.13)	(1.88)	(1.88)	(3.24)
CEO power	0.095***	0.094***	0.089***	0.081***	0.065**	0.066**	0.188*	0.143
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.10)	(0.09)
CEO power × Uncertainty		0.011	-0.431*	-0.397*	-0.302	0.098	-0.546	-0.573
		(0.25)	(0.22)	(0.22)	(0.22)	(0.25)	(0.63)	(0.74)
Abnormal return			0.117***	0.128***	0.101***	0.004	0.237***	0.024
			(0.02)	(0.02)	(0.02)	(0.03)	(0.06)	(0.07)
Abnormal return × Uncertainty			0.109	0.002	0.147	0.129	-0.667	-0.188
			(0.22)	(0.25)	(0.23)	(0.33)	(0.58)	(0.67)
Independent board			0.334***	0.506***	0.224**	0.202	-0.230	0.268
			(0.10)	(0.11)	(0.10)	(0.13)	(0.24)	(0.21)
Independent board × Uncertainty			-0.160	-0.751	-0.160	-1.318	1.892	-0.121
			(0.91)	(1.18)	(0.96)	(1.63)	(1.75)	(2.20)
Firm Size			0.366***	0.402***	0.359***	0.130***	0.435***	0.532***
			(0.03)	(0.02)	(0.04)	(0.02)	(0.08)	(0.09)
Firm Size × Uncertainty			0.172	0.140	0.112	-0.068	-0.206	0.379
			(0.13)	(0.14)	(0.11)	(0.11)	(0.35)	(0.32)
CEO age $\geq$ 60			-0.023	-0.025	-0.022	0.026	-0.051	-0.033
			(0.02)	(0.02)	(0.02)	(0.04)	(0.05)	(0.07)

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Table 3.5 continued

Dependent variable =	Ln(*Compensation*)					Ln(*Salary*)	Ln(*Bonus*)	Ln(*Equity-based*)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CEO age $\geq$ 60 $\times$ Uncertainty			0.043	0.032	0.342*	0.149	0.099	-0.499
			(0.23)	(0.27)	(0.20)	(0.33)	(0.59)	(0.74)
CEO is female			0.047	0.042				
			(0.06)	(0.06)				
CEO is female $\times$ Uncertainty			-0.677	-0.672				
			(0.64)	(0.58)				
Board size			-0.001	-0.006	-0.002	0.012	-0.040**	-0.014
			(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Board size $\times$ Uncertainty			0.120	0.155	-0.009	-0.064	0.181	0.050
			(0.07)	(0.10)	(0.06)	(0.06)	(0.17)	(0.17)
Female director			0.042*	0.046*	0.017	0.029	-0.038	0.118
			(0.02)	(0.02)	(0.02)	(0.04)	(0.10)	(0.08)
Female director $\times$ Uncertainty			-0.079	-0.129	0.102	0.137	-0.435	-1.197
			(0.25)	(0.34)	(0.25)	(0.32)	(0.62)	(0.91)
CEO successor			0.001	-0.006	-0.005	0.015	-0.167***	-0.055
			(0.02)	(0.02)	(0.02)	(0.03)	(0.06)	(0.08)
CEO successor $\times$ Uncertainty			-0.052	0.060	0.078	0.281	1.259**	0.812
			(0.24)	(0.30)	(0.26)	(0.40)	(0.55)	(0.72)
Observations	25432	25432	25432	25432	25432	25439	8698	4979
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	No	No	No	No	No
FirmCEO FE	No	No	No	No	Yes	Yes	Yes	Yes
YearIndustry FE	No	No	No	Yes	No	No	No	No

Table 3.6: Regression of the returns during the 2020 Coronavirus Stock Market Crash

This table presents the regression estimation of the impacts of CEO power on the stock returns during the 2020 Coronavirus Stock Market Crash. In panel A, the dependent variable is *Return pandemic*, the accumulated return between February 20th and March 20th in 2020, during which period the stock market in the U.S. collapsed sharply due to fear of uncertainty. CEO power is measured by five alternative proxies: *Dual CEO*, *CEO's concentration of titles*, *CEO tenure*, *Longer directorship*, and *Founder CEO*. All explanatory variables are on the firm level. Panel B shows the results of a placebo test, which replicates the regressions in Panel A on the same period but in the previous year (i.e. between Febrary 20th and March 20th in 2019). Standard errors are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. All variables are defined in Table A3.1.

CEO power =	*Dual CEO*		*Concentration of titles*		*CEO tenure*		*Longer directorship*		*Founder CEO*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Dependent variable = *Return pandemic*										
CEO power	0.030***	0.028***	0.027***	0.027***	0.001*	0.001**	0.007	0.005	0.045**	0.052***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)	(0.02)
Independent board		-0.062		-0.074		-0.045		-0.055		-0.041
		(0.05)		(0.05)		(0.05)		(0.05)		(0.05)
Firm Size		0.009**		0.009**		0.010***		0.010***		0.010***
		(0.00)		(0.00)		(0.00)		(0.00)		(0.00)
CEO age $\geq$ 60		-0.006		-0.005		-0.010		-0.000		-0.002
		(0.01)		(0.01)		(0.01)		(0.01)		(0.01)
CEO is female		0.007		0.003		0.001		0.003		0.003
		(0.02)		(0.02)		(0.02)		(0.02)		(0.02)
Board size		0.001		0.001		0.001		0.000		0.001
		(0.00)		(0.00)		(0.00)		(0.00)		(0.00)
Female director		-0.014		-0.035		-0.008		-0.039		-0.011
		(0.03)		(0.04)		(0.03)		(0.04)		(0.03)
CEO successor		0.012		0.013		0.008		0.013		0.011
		(0.02)		(0.02)		(0.02)		(0.02)		(0.02)
Constant	-0.411***	-0.417***	-0.459***	-0.440***	-0.407***	-0.446***	-0.403***	-0.401***	-0.401***	-0.441***

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Table 3.6 continued

CEO power =	*Dual CEO*		*Concentration of titles*		*CEO tenure*		*Longer directorship*		*Founder CEO*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	(0.01)	(0.05)	(0.02)	(0.05)	(0.01)	(0.05)	(0.01)	(0.05)	(0.00)	(0.05)
Observations	1427	1424	1449	1447	1468	1465	1400	1398	1478	1475
Panel B: Dependent variable = *Return placebo*										
CEO power	-0.007	-0.006	-0.004	-0.005	-0.000	-0.000	0.002	0.004	0.011	0.011
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
Independent board		0.014		0.016		0.008		0.010		0.009
		(0.02)		(0.03)		(0.02)		(0.03)		(0.02)
Firm Size		0.001		0.001		0.001		0.001		0.001
		(0.00)		(0.00)		(0.00)		(0.00)		(0.00)
CEO age $\geq$ 60		-0.007		-0.006		-0.006		-0.009*		-0.007
		(0.00)		(0.00)		(0.01)		(0.00)		(0.00)
CEO is female		0.004		0.001		0.002		0.005		0.002
		(0.01)		(0.01)		(0.01)		(0.01)		(0.01)
Board size		-0.001		-0.001		-0.001		-0.001		-0.001
		(0.00)		(0.00)		(0.00)		(0.00)		(0.00)
Female director		-0.009		-0.017		-0.017		-0.008		-0.014
		(0.01)		(0.01)		(0.01)		(0.01)		(0.01)
CEO successor		-0.023***		-0.024***		-0.023***		-0.023***		-0.023***
		(0.01)		(0.01)		(0.01)		(0.01)		(0.01)
Constant	-0.015***	-0.016	-0.008	0.003	-0.015***	-0.002	-0.019***	-0.015	-0.019***	-0.007
	(0.00)	(0.02)	(0.01)	(0.02)	(0.00)	(0.02)	(0.00)	(0.02)	(0.00)	(0.02)
Observations	1539	1537	1562	1561	1585	1583	1512	1511	1591	1589

Table 3.7: Forced CEO turnover in opaque versus transparent firms

This table presents the regression estimation of the impacts of CEO power and uncertainty on forced CEO turnovers among opaque firms and transparent firms, respectively. Firms are split into two groups based on their relative information asymmetry compared with their peer firms in the same industry and year. The half of firms that have fewer analysts following and less homogeneous or accurate analysts' quarterly earnings forecasts constitute the opaque group (Panel A). The other half of firms constitute the transparent group (Panel B). The dependent variable *Forced turnover* is a dummy, equal to 1 if a CEO is dismissed in that year and 0 otherwise. Uncertainty is measured by two alternative proxies on the industry-year level: *Stock volatility* in Columns (1) - (4), and *Delisting rate* in Columns (5) - (8). All variables except uncertainty measures are on the firm-year level. CEO power is measured by *Dual CEO*, an indicator equal to one if the CEO is also the board chair and zero otherwise. The untabulated control variables in Columns (3) - (4) and (7) - (8) are the same as in Table 3.2. The year fixed effects and industry fixed effects are controlled for in Columns (1) - (3) and (5) - (7), while the year-industry fixed effects are controlled for in Columns (4) and (8). Standard errors, adjusted for clustering at the industry level, are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. All variables are defined in Table A3.1.

Dependent variable =	*Forced turnover*							
Uncertainty =	*Stock volatility*				*Delisting rate*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Opaque firms								
Uncertainty	0.255***	0.302***	-0.058		0.114*	0.211***	0.063	
	(0.06)	(0.07)	(0.29)		(0.06)	(0.08)	(0.37)	
CEO power	-0.008**	0.003	0.004	0.004	-0.008**	0.004	0.003	0.002
	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
CEO power × Uncertainty		-0.083	-0.120**	-0.140**		-0.184***	-0.216***	-0.241***
		(0.05)	(0.06)	(0.06)		(0.07)	(0.07)	(0.09)
Observations	15105	15105	15105	15105	15105	15105	15105	15105
Controls	No	No	Yes	Yes	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Industry FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
YearIndustry FE	No	No	No	Yes	No	No	No	Yes
Panel B: Transparent firms								

Continued on next page

Table 3.7 continued

Uncertainty =	*Stock volatility*				*Delisting rate*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Uncertainty	-0.060	-0.038	0.124		-0.016	-0.002	0.094	
	(0.06)	(0.07)	(0.19)		(0.03)	(0.06)	(0.25)	
CEO power	-0.005	-0.000	-0.001	-0.006	-0.005	-0.003	-0.005	-0.006
	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
CEO power × Uncertainty		-0.035	-0.046	-0.026		-0.023	-0.024	-0.045
		(0.05)	(0.05)	(0.05)		(0.06)	(0.06)	(0.08)
Observations	16028	16028	16028	16028	16028	16028	16028	16028
Controls	No	No	Yes	Yes	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Industry FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
YearIndustry FE	No	No	No	Yes	No	No	No	Yes

Table 3.8: Forced CEO turnover in firms with a busier versus less busy board

This table presents the regression estimation of the impacts of CEO power and uncertainty on forced CEO turnovers among firms with a busier board and those with a less busy board, respectively. Firms are split into two groups based on the average number of directorships in listed firms held by each of their directors, compared with those of their peer firms in the same industry and year. The half of firms that have more directorships per director constitute the busier group (Panel A). The other half of firms constitute the less busy group (Panel B). The dependent variable *Forced turnover* is a dummy, equal to 1 if a CEO is dismissed in that year and 0 otherwise. Uncertainty is measured by two alternative proxies on the industry-year level: *Stock volatility* in Columns (1) - (4), and *Delisting rate* in Columns (5) - (8). All variables except uncertainty measures are on the firm-year level. CEO power is measured by *Dual CEO*, an indicator equal to one if the CEO is also the board chair and zero otherwise. The untabulated control variables in Columns (3) - (4) and (7) - (8) are the same as in Table 3.2. The year fixed effects and industry fixed effects are controlled for in Columns (1) - (3) and (5) - (7), while the year-industry fixed effects are controlled for in Columns (4) and (8). Standard errors, adjusted for clustering at the industry level, are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. All variables are defined in Table A3.1.

Dependent variable =	*Forced turnover*							
Uncertainty =	*Stock volatility*				*Delisting rate*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Firms with a busier board								
Uncertainty	0.126**	0.208***	0.045		0.067	0.196***	0.087	
	(0.06)	(0.06)	(0.24)		(0.04)	(0.07)	(0.30)	
CEO power	-0.003	0.015**	0.014*	0.013	-0.003	0.012**	0.009	0.013*
	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
CEO power × Uncertainty		-0.134***	-0.141**	-0.154**		-0.218***	-0.209**	-0.305***
		(0.05)	(0.06)	(0.06)		(0.08)	(0.09)	(0.11)
Observations	15697	15697	15697	15697	15697	15697	15697	15697
Controls	No	No	Yes	Yes	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Industry FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
YearIndustry FE	No	No	No	Yes	No	No	No	Yes

Continued on next page

Table 3.8 continued

Uncertainty =	*Stock volatility*				*Delisting rate*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel B: Firms with a less busy board								
Uncertainty	0.001	0.007	0.045		0.021	0.051	0.178	
	(0.05)	(0.07)	(0.18)		(0.04)	(0.06)	(0.20)	
CEO power	-0.005*	-0.004	-0.001	-0.002	-0.005*	-0.002	-0.003	-0.003
	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
CEO power × Uncertainty		-0.012	-0.053	-0.068		-0.053	-0.073	-0.116
		(0.07)	(0.07)	(0.08)		(0.07)	(0.07)	(0.09)
Observations	16336	16336	16336	16336	16336	16336	16336	16336
Controls	No	No	Yes	Yes	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Industry FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
YearIndustry FE	No	No	No	Yes	No	No	No	Yes

A Appendix

A.1 Examples of power rearrangement during the pandemic

Anecdotal evidence tends to show that a larger number of firms were managed by a powerful CEO during the extraordinarily uncertain period at the beginning of the COVID-19 pandemic. Some firms extended the tenures of their experienced CEOs: IAG postponed the planned retirement of its CEO Willie Walsh on June 30 in 2020, because "As we respond to COVID-19, . . . , management stability across the Group should be a priority" (Garcia, 2020); in July 2020 Greenbrier announced that its CEO Bill Furman postponed his retirement for two years because "the current COVID-19 crisis and accompanying environment of economic uncertainty requires an experienced industry and management team to lead Greenbrier through extraordinary times" (Rattner, 2020).

At the same time, some firms witness the comeback of their once-distanced powerful CEOs: Amazon's founder and then-CEO Jeff Bezos, who had distanced himself from day-to-day management since years ago, took back charge of its daily operation soon after the pandemic started spreading across US in March 2020 (Weise, 2020); Bob Iger, who passed the baton of CEO of Disney to Bob Chapek in February 2020 and became executive chairman himself, effectively returned to running the company merely a few weeks later, explaining that "a crisis of this magnitude, and its impact on Disney, would necessarily result in my actively helping Bob [Chapek] and the company contend with it, particularly since I ran the company for 15 years!" (Smith, 2020).

For some other firms, this crisis leads to further concentration of power: SAP SE transformed from co-CEO to sole CEO model amid the coronavirus pandemic "to ensure strong, unambiguous steering in times of an unprecedented crisis" (Armental, 2020).

Table A3.1: Definition of Variables

Variable	Description	Sources
Forced Turnover of CEO		
Forced turnover	A dummy variable with value “1" indicating a firm's CEO is dismissed in that year and value “0" otherwise.	Gentry et al. (2021)
Environmental Uncertainty		
Stock volatility	Industry equally-weighted average of individual stocks' yearly volatilities, computed from their monthly returns. Firms with the same two-digit SIC code are viewed as in the same industry. (same below)	CRSP-monthly
Delisting rate	The fraction of delisted firms in each industry and year, due to either merger (the first digit of the delisting code=2), liquidation (the first digit of the delisting code=4), or delisting by NYSE, NYSE MKT, NASDAQ or Arca (the first digit of the delisting code=5).	CRSP-Delisting
Market average stock volatility	The equally-weighted average of the yearly volatilities of individual stocks of the entire sample firms, computed from their monthly returns.	CRSP-monthly
Market average delisting rate	The fraction of delisted firms each year among the entire sample firms, due to either merger (the first digit of the delisting code=2), liquidation (the first digit of the delisting code=4), or delisting by NYSE, NYSE MKT, NASDAQ or Arca (the first digit of the delisting code=5).	CRSP-Delisting
CEO Characteristics		
CEO duality	A dummy variable with value “1" indicating in that year a firm's CEO is also its board chair and value “0" indicating that there exists a separate board chair.	BoardEx
CEO's concentration of titles	The number of titles (CEO, president, COO and board chair) a CEO have in that year. If no president or COO title exists, add one to the actual number of titles.	BoardEx
CEO tenure	The number of years since the CEO started their tenure	Execucomp
Longer directorship	A dummy variable with value “1" indicating in that year a firm's CEO has sitting on its board for longer or equal time than its separate board chair and “0" otherwise. If the CEO is also the chair, assign value “1" to this variable.	BoardEx
Founder CEO	A dummy variable with value “1" indicating in that year a firm's CEO has a founder status in that firm and value “0" otherwise.	Execucomp
CEO age ≥ 60	A dummy equal to one if the age of CEO is larger or equal to 60 and zero otherwise.	Execucomp
CEO is female	A dummy equal to one if the CEO is female and zero otherwise.	Execucomp
Ln(compensation)	The natural log of total compensation in thousands of US$ for the CEO in that year	Execucomp
Ln(salary)	The natural log of salary in thousands of US$ for the CEO in that year	Execucomp
Ln(bonus)	The natural log of bonus in thousands of US$ for the CEO in that year	Execucomp
Ln(equity-based)	The natural log of equity-based part of compensation in thousands of US$ for the CEO in that year	Execucomp

continued on next page

Table A3.1 continued

Variable	Description	
Firm Performance and Characteristics		
Q	Market capitalisation (at - ceq + prcc_f*csho) / book value (at)	Compustat-Fundamentals
ROA	Operating income before depreciation (oibdp) / book value (at)	Compustat-Fundamentals
Sales growth	The growth rate of yearly sales, $(Sales_t - Sales_{t-1})/Sales_{t-1}$	Compustat-Fundamentals
Surviving the next two years	A dummy equal to one if a firm is not delisted in the next two years, and equal to zero otherwise.	CRSP-Delisting
Defaulting in the next two years	A dummy equal to one if a firm defaults (splticrm="D") in the next two years, while equal to zero otherwise.	Compustat-Ratings
Abnormal return	The difference between the return of a firm's stock and the value-weighted market average return in one year. For fiscal years when a CEO turnover happened, this variable is calculated on the 12 months before the turnover. Otherwise, this variable is calculated on the entire fiscal year.	CRSP-Monthly
Independent board	A dummy equal to one if more than half of the directors are independent, and equal to zero otherwise.	Boardex
Firm Size	The natural log of sales.	Compustat-Fundamentals
Board size	The number of directors.	Boardex
Female director	A dummy equal to one if there is at least one female director, and zero otherwise.	Boardex
CEO successor	A dummy equal to one if a firm has a COO or president who ranks among the top five executives in terms of compensation, and equal to zero otherwise, following Kini and Williams (2012).	Execucomp
Information asymmetry	The average of a firm's three percentile rankings according to the number of following analysts, the dispersion of earnings forecasts across analysts and the forecast error of the mean analyst earnings forecast.	IBES
#Directorships	The number of directorships in listed firms a director has in a specific year.	Boardex

Table A3.2: Regression of forced CEO turnover with alternative measures of CEO power

This table presents the regression estimation of the impacts of uncertainty and CEO power on forced CEO turnovers. The dependent variable *Forced turnover* is a dummy, equal to 1 if a CEO is dismissed in that year and 0 otherwise. Uncertainty is measured by two alternative proxies on the industry-year level: *Stock volatility* in Columns (1) - (4), and *Delisting rate* in Columns (5) - (8). All variables except uncertainty measures are on the firm-year level. CEO power is measured in four alternative ways: *CEO's concentration of titles* in Panel A, *CEO tenure* in Panel B, *Longer directorship* in Panel C, and *Founder CEO* in Panel D. In Columns (2) - (4) and (6) - (8), CEO power and other CEO and firm characteristics are interacted with uncertainty, in order to show how their impacts on forced CEO turnover are moderated by uncertainty. The untabulated control variables in Columns (3) - (4) and (7) - (8) are the same as in Table 3.2. The year fixed effects and industry fixed effects are controlled for in Columns (1) - (3) and (5) - (7), while the year-industry fixed effects are controlled for in Columns (4) and (8). Standard errors, adjusted for clustering at the industry level, are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. All variables are defined in Table A3.1.

Dependent variable =	*Forced turnover*							
Uncertainty =	*Stock volatility*				*Delisting rate*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: CEO power = *CEO's concentration of titles*								
Uncertainty	0.079**	0.217**	0.173		0.044	0.302***	0.291	
	(0.04)	(0.09)	(0.16)		(0.03)	(0.11)	(0.19)	
CEO power	-0.006**	0.002	0.002	-0.002	-0.006**	0.002	0.001	-0.003
	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
CEO power × Uncertainty		-0.057	-0.062*	-0.053*		-0.106***	-0.108***	-0.098**
		(0.03)	(0.04)	(0.03)		(0.04)	(0.04)	(0.05)
Observations	32033	32033	32033	32033	32033	32033	32033	32033
Controls	No	No	Yes	Yes	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Industry FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
YearIndustry FE	No	No	No	Yes	No	No	No	Yes
Panel B: CEO power = *CEO tenure*								
Uncertainty	0.078**	0.150***	0.148		0.044	0.065	0.159	

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Table A3.2 continued

Uncertainty =	*Stock volatility*				*Delisting rate*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	(0.04)	(0.05)	(0.16)		(0.03)	(0.05)	(0.18)	
CEO power	-0.000	0.001***	0.001**	0.000	-0.000	0.000	-0.000	-0.001*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
CEO power × Uncertainty		-0.009***	-0.010***	-0.010***		-0.003	-0.006*	-0.008**
		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)
Observations	32033	32033	32033	32033	32033	32033	32033	32033
Controls	No	No	Yes	Yes	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Industry FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
YearIndustry FE	No	No	No	Yes	No	No	No	Yes
Panel C: CEO power = *Longer directorship*								
Uncertainty	0.073*	0.113**	0.074		0.045	0.121**	0.132	
	(0.04)	(0.05)	(0.14)		(0.03)	(0.05)	(0.18)	
CEO power	-0.003	0.004	0.004	-0.000	-0.003	0.004	0.003	-0.001
	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
CEO power × Uncertainty		-0.061	-0.073*	-0.074*		-0.116**	-0.122**	-0.130**
		(0.04)	(0.04)	(0.04)		(0.05)	(0.05)	(0.06)
Observations	32029	32029	32029	32029	32029	32029	32029	32029
Controls	No	No	Yes	Yes	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Industry FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
YearIndustry FE	No	No	No	Yes	No	No	No	Yes
Panel D: CEO power = *Founder CEO*								
Uncertainty	0.074*	0.076**	0.029		0.044	0.041	0.073	
	(0.04)	(0.04)	(0.15)		(0.03)	(0.03)	(0.18)	

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Table A3.2 continued

Uncertainty =	Stock volatility				Delisting rate			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CEO power	-0.013***	-0.002	-0.004	-0.002	-0.013***	-0.018*	-0.014	-0.011
	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
CEO power × Uncertainty		-0.080	-0.079	-0.121		0.082	-0.010	-0.095
		(0.07)	(0.07)	(0.09)		(0.14)	(0.14)	(0.15)
Observations	32033	32033	32033	32033	32033	32033	32033	32033
Controls	No	No	Yes	Yes	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Industry FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
YearIndustry FE	No	No	No	Yes	No	No	No	Yes

Table A3.3: Regression of the CEO's future compensation

This table presents the regression estimation of the impacts of CEO power and uncertainty on the CEO's future compensation. The dependent variable is the natural logarithm of total CEO compensation either in the next year (Columns (1) - (4)) or in the year after next year (Columns (5) - (8)). Uncertainty is measured by two alternative proxies on the industry-year level: *Stock volatility* in Panel A, and *Delisting rate* in Panel B. All variables except uncertainty measures are on the firm-year level. CEO power is measured by *Dual CEO*, an indicator equal to one if the CEO is also the board chair and zero otherwise. In Columns (2) - (4) and (6) - (8), CEO power and other controls are interacted with uncertainty, in order to show how their impacts on CEO's future compensation are moderated by uncertainty. Only the key regressors are tabulated here, while the complete list of controls is identical as in Table 3.5. The year fixed effects are controlled for throughout all specifications except for Columns (3) and (7). The firm fixed effects are controlled for in Columns (1) - (2) and (5) - (6), while the firm-CEO fixed effects are controlled for in Columns (4) and (8). The industry-year fixed effects are controlled for in Column (3) and (7). Standard errors, adjusted for clustering at the industry level, are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. All variables are defined in Table A3.1.

Dependent variable =	Ln(*Compensation* in one year)				Ln(*Compensation* in two years)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Uncertainty = *Stock volatility*								
Uncertainty	-0.651	-0.837		0.547	-0.698	-1.425		-0.211
	(0.55)	(1.65)		(1.20)	(0.45)	(1.33)		(1.08)
CEO power	0.084***	0.086*	0.094**	0.045	0.056*	0.072*	0.091**	0.065
	(0.03)	(0.04)	(0.05)	(0.06)	(0.03)	(0.04)	(0.04)	(0.06)
CEO power × Uncertainty		-0.208	-0.345	-0.161		-0.219	-0.421**	-0.210
		(0.26)	(0.27)	(0.32)		(0.18)	(0.16)	(0.27)
Observations	22593	22593	22593	22593	18961	18961	18961	18716
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year FE	Yes	Yes	No	Yes	Yes	Yes	No	Yes
Firm FE	Yes	Yes	No	No	Yes	Yes	No	No
FirmCEO FE	No	No	No	Yes	No	No	No	Yes
YearIndustry FE	No	No	Yes	No	No	No	Yes	No
Panel B: Uncertainty = *Delisting rate*								
Uncertainty	0.085	-3.383**		-0.912	0.270	-2.383*		0.703
	(0.19)	(1.41)		(0.99)	(0.18)	(1.29)		(0.99)
CEO power	0.084***	0.068**	0.061**	0.027	0.056*	0.033	0.036	0.029
	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.04)
CEO power × Uncertainty		-0.116	-0.178	-0.015		0.161	0.010	0.122
		(0.24)	(0.25)	(0.24)		(0.24)	(0.27)	(0.24)
Observations	22593	22593	22593	22593	18961	18961	18961	18716
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year FE	Yes	Yes	No	Yes	Yes	Yes	No	Yes
Firm FE	Yes	Yes	No	No	Yes	Yes	No	No
FirmCEO FE	No	No	No	Yes	No	No	No	Yes
YearIndustry FE	No	No	Yes	No	No	No	Yes	No

Table A3.4: Regression of survivals and defaults

This table presents the regression estimation of the impacts of CEO power and uncertainty on firms' survivals and defaults. The dependent variable in Panel A is *Survives the next two years*, a dummy equal to one if a firm survives through the next two years and zero otherwise. In Panel B, the dependent variable is *Defaults in the next two years*, a dummy equal to one if a firm defaults in the next two years and zero otherwise. Uncertainty is measured by two alternative proxies on the industry-year level: *Stock volatility* in Columns (1) - (4), and *Delisting rate* in Columns (5) - (8). All variables except uncertainty measures are on the firm-year level. CEO power is measured by *Dual CEO*, an indicator equal to one if the CEO is also the board chair and zero otherwise. In Columns (2) - (4) and (6) - (8), CEO power and other CEO and firm characteristics are interacted with uncertainty, in order to show how their impacts on survivals and defaults are moderated by uncertainty. The untabulated control variables in Panel B are the same as in Panel A. The year fixed effects and industry fixed effects are controlled for in Columns (1) - (3) and (5) - (7), while the year-industry fixed effects are controlled for in Columns (4) and (8). Standard errors, adjusted for clustering at the industry level, are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. All variables are defined in Table A3.1.

Uncertainty =	*Stock volatility*				*Delisting rate*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Dependent variable = *Surviving the next two years*								
Uncertainty	0.069	0.069	0.771***		0.024	-0.033	0.795***	
	(0.07)	(0.07)	(0.25)		(0.04)	(0.06)	(0.23)	
CEO power	0.001	0.001	-0.004	0.002	0.001	-0.005	-0.011*	-0.010*
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
CEO power × Uncertainty		-0.000	0.026	-0.023		0.100	0.178**	0.153
		(0.08)	(0.09)	(0.10)		(0.09)	(0.09)	(0.09)
Abnormal return			0.047***	0.052***			0.021***	0.027***
			(0.01)	(0.01)			(0.01)	(0.01)
Abnormal return × Uncertainty			-0.179***	-0.204***			-0.053	-0.094
			(0.05)	(0.06)			(0.08)	(0.10)
Independent board			0.045	0.004			0.060	0.030
			(0.07)	(0.10)			(0.04)	(0.05)

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Table A3.4 continued

Uncertainty =	Stock volatility				Delisting rate			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Independent board × Uncertainty			0.037	0.293			-0.137	0.207
			(0.30)	(0.51)			(0.26)	(0.37)
Firm Size			0.022***	0.020**			0.016***	0.014***
			(0.01)	(0.01)			(0.00)	(0.00)
Firm Size × Uncertainty			-0.083**	-0.073			-0.069***	-0.064**
			(0.04)	(0.05)			(0.02)	(0.03)
CEO age ≥ 60			0.002	-0.007			0.005	0.004
			(0.01)	(0.01)			(0.01)	(0.01)
CEO age ≥ 60 × Uncertainty			-0.073	0.006			-0.189**	-0.171***
			(0.07)	(0.07)			(0.07)	(0.06)
CEO is female			-0.018	-0.022			-0.037	-0.034
			(0.03)	(0.03)			(0.02)	(0.02)
CEO is female × Uncertainty			0.007	0.022			0.327	0.236
			(0.20)	(0.20)			(0.32)	(0.30)
Board size			0.000	0.001			-0.000	0.000
			(0.00)	(0.00)			(0.00)	(0.00)
Board size × Uncertainty			-0.004	-0.007			-0.000	0.000
			(0.02)	(0.02)			(0.02)	(0.02)
Female director			0.005	0.006			0.008	0.009
			(0.01)	(0.01)			(0.01)	(0.01)
Female director × Uncertainty			-0.093	-0.075			-0.218***	-0.183*
			(0.07)	(0.08)			(0.08)	(0.09)
CEO successor			-0.015	-0.014			0.003	0.003
			(0.01)	(0.02)			(0.01)	(0.01)

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Table A3.4 continued

Uncertainty =	Stock volatility				Delisting rate			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CEO successor × Uncertainty			0.103	0.072			-0.061	-0.087
			(0.08)	(0.09)			(0.08)	(0.09)
Observations	28572	28572	28572	28572	28572	28572	28572	28572
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Industry FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
YearIndustry FE	No	No	No	Yes	No	No	No	Yes
Panel B: Dependent variable = *Defaulting in the next two years*								
Uncertainty	0.015	0.034	0.021		0.001	0.019	-0.082	
	(0.03)	(0.03)	(0.06)		(0.02)	(0.02)	(0.20)	
CEO power	0.000	0.004	0.004	0.002	0.000	0.002	0.002	0.001
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
CEO power × Uncertainty		-0.029	-0.024	-0.014		-0.027	-0.016	-0.007
		(0.02)	(0.02)	(0.02)		(0.02)	(0.02)	(0.03)
Observations	13153	13153	13153	13153	13153	13153	13153	13153
Controls	No	No	Yes	Yes	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Industry FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
YearIndustry FE	No	No	No	Yes	No	No	No	Yes

OA Online Appendix

Table OA1: Separate regressions of forced CEO turnover during stable times and uncertain times, with alternative measures of CEO power

This table presents the regression estimation of the impacts of uncertainty and CEO power on forced CEO turnovers in more stable times and more uncertain times, respectively. The sample of firm-years is split into two halves based on whether the industry-level uncertainty, measured by either *Stock volatility* or *Delisting rate*, is lower or higher than the median uncertainty of that industry across all years. The dependent variable *Forced turnover* is a dummy, equal to 1 if a CEO is dismissed in that year and 0 otherwise. Uncertainty is measured by two alternative proxies on the industry-year level: *Stock volatility* in Columns (1) - (4), and *Delisting rate* in Columns (5) - (8). All variables except uncertainty measures are on the firm-year level. CEO power measures the concentration of the CEO's power in four alternative ways: *CEO's concentration of titles* in Panels A and B, *CEO tenure* in Panels C and D, *Longer directorship* in Panels E and F, and *Founder CEO* in Panels G and H. In Columns (4) and (8), CEO power is interacted with *Abnormal return*, in order to show how CEO power affects their turnover-performance sensitivity. The untabulated control variables in Columns (2) - (4) and (6) - (8) are the same as in Table 3.3. The year fixed effects and industry fixed effects are controlled for in Columns (1) - (2) and (5) - (6), while the year-industry fixed effects are controlled for in Columns (3) - (4) and (7) - (8). Standard errors, adjusted for clustering at the industry level, are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. All variables are defined in Table A3.1.

Dependent variable =	*Forced turnover*							
Uncertainty =	*Stock volatility*				*Delisting rate*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Relatively stable times, CEO power = *CEO's concentration of titles*								
Uncertainty	0.216	0.147			0.057	0.050		
	(0.16)	(0.18)			(0.09)	(0.08)		
CEO power	-0.003	-0.004	-0.005*	-0.005*	-0.003	-0.005	-0.006**	-0.006*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Abnormal return		-0.051***	-0.056***	-0.074***		-0.039***	-0.041***	-0.041**
		(0.01)	(0.01)	(0.03)		(0.00)	(0.01)	(0.02)
Abnormal return × CEO power				0.007				0.000
				(0.01)				(0.01)
Observations	16216	16216	16216	16216	17043	17043	17043	17043
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes

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Table OA1 continued

Uncertainty =	Stock volatility				Delisting rate			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Year FE	Yes	Yes	No	No	Yes	Yes	No	No
Industry FE	Yes	Yes	No	No	Yes	Yes	No	No
YearIndustry FE	No	No	Yes	Yes	No	No	Yes	Yes
Panel B: Relatively uncertain times, CEO power = *CEO's concentration of titles*								
Uncertainty	0.141***	0.127***			-0.023	-0.017		
	(0.04)	(0.04)			(0.06)	(0.06)		
CEO power	-0.008***	-0.009***	-0.011***	-0.011***	-0.007**	-0.009***	-0.010***	-0.011***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Abnormal return		-0.032***	-0.035***	-0.048***		-0.040***	-0.043***	-0.069***
		(0.00)	(0.00)	(0.01)		(0.00)	(0.00)	(0.02)
Abnormal return × CEO power				0.005				0.011
				(0.01)				(0.01)
Observations	15817	15817	15817	15817	14990	14990	14990	14990
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	Yes	Yes	No	No
Industry FE	Yes	Yes	No	No	Yes	Yes	No	No
YearIndustry FE	No	No	Yes	Yes	No	No	Yes	Yes
Panel C: Relatively stable times, CEO power = *CEO tenure*								
Uncertainty	0.217	0.146			0.056	0.050		
	(0.16)	(0.18)			(0.09)	(0.08)		
CEO power	0.000	-0.000	-0.001*	-0.001**	-0.000	-0.000	-0.001**	-0.001**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Abnormal return		-0.052***	-0.057***	-0.079***		-0.039***	-0.041***	-0.055***
		(0.01)	(0.01)	(0.01)		(0.00)	(0.01)	(0.01)
Abnormal return × CEO power				0.003***				0.002***

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Table OA1 continued

Uncertainty =	*Stock volatility*				*Delisting rate*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				(0.00)				(0.00)
Observations	16216	16216	16216	16216	17043	17043	17043	17043
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	Yes	Yes	No	No
Industry FE	Yes	Yes	No	No	Yes	Yes	No	No
YearIndustry FE	No	No	Yes	Yes	No	No	Yes	Yes
Panel D: Relatively uncertain times, CEO power = *CEO tenure*								
Uncertainty	0.138***	0.123***			-0.024	-0.020		
	(0.04)	(0.04)			(0.06)	(0.06)		
CEO power	-0.000***	-0.001***	-0.001***	-0.001***	-0.000**	-0.001***	-0.001***	-0.001***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Abnormal return		-0.032***	-0.035***	-0.042***		-0.040***	-0.044***	-0.053***
		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.01)
Abnormal return × CEO power				0.001***				0.001***
				(0.00)				(0.00)
Observations	15817	15817	15817	15817	14990	14990	14990	14990
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	Yes	Yes	No	No
Industry FE	Yes	Yes	No	No	Yes	Yes	No	No
YearIndustry FE	No	No	Yes	Yes	No	No	Yes	Yes
Panel E: Relatively stable times, CEO power = *Longer directorship*								
Uncertainty	0.215	0.146			0.057	0.050		
	(0.16)	(0.18)			(0.09)	(0.08)		
CEO power	0.000	-0.001	-0.003	-0.003	-0.002	-0.003	-0.004	-0.004
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)

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Table OA1 continued

Uncertainty =	Stock volatility				Delisting rate			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Abnormal return		-0.051***	-0.056***	-0.064***		-0.039***	-0.041***	-0.050***
		(0.01)	(0.01)	(0.01)		(0.00)	(0.01)	(0.01)
Abnormal return × CEO power				0.013*				0.015**
				(0.01)				(0.01)
Observations	16216	16216	16216	16216	17043	17043	17043	17043
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	Yes	Yes	No	No
Industry FE	Yes	Yes	No	No	Yes	Yes	No	No
YearIndustry FE	No	No	Yes	Yes	No	No	Yes	Yes
Panel F: Relatively uncertain times, CEO power = *Longer directorship*								
Uncertainty	0.131***	0.117**			-0.023	-0.017		
	(0.05)	(0.05)			(0.06)	(0.06)		
CEO power	-0.008**	-0.010***	-0.012***	-0.013***	-0.007**	-0.010***	-0.012***	-0.013***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Abnormal return		-0.032***	-0.034***	-0.045***		-0.039***	-0.043***	-0.053***
		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.01)
Abnormal return × CEO power				0.016***				0.015**
				(0.01)				(0.01)
Observations	15813	15813	15813	15813	14986	14986	14986	14986
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	Yes	Yes	No	No
Industry FE	Yes	Yes	No	No	Yes	Yes	No	No
YearIndustry FE	No	No	Yes	Yes	No	No	Yes	Yes
Panel G: Relatively stable times, CEO power = *Founder CEO*								
Uncertainty	0.209	0.138			0.056	0.049		

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Table OA1 continued

Uncertainty =	*Stock volatility*				*Delisting rate*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	(0.16)	(0.18)			(0.09)	(0.08)		
CEO power	-0.016**	-0.016**	-0.019**	-0.022***	-0.015***	-0.014***	-0.015***	-0.018***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)
Abnormal return		-0.051***	-0.056***	-0.060***		-0.039***	-0.041***	-0.043***
		(0.01)	(0.01)	(0.01)		(0.00)	(0.01)	(0.01)
Abnormal return × CEO power				0.042***				0.028**
				(0.01)				(0.01)
Observations	16216	16216	16216	16216	17043	17043	17043	17043
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	Yes	Yes	No	No
Industry FE	Yes	Yes	No	No	Yes	Yes	No	No
YearIndustry FE	No	No	Yes	Yes	No	No	Yes	Yes
Panel H: Relatively uncertain times, CEO power = *Founder CEO*								
Uncertainty	0.138***	0.124***			-0.022	-0.016		
	(0.04)	(0.05)			(0.06)	(0.06)		
CEO power	-0.009**	-0.011**	-0.013**	-0.014***	-0.014*	-0.016**	-0.018**	-0.019**
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Abnormal return		-0.032***	-0.034***	-0.035***		-0.039***	-0.043***	-0.044***
		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)
Abnormal return × CEO power				0.016*				0.019
				(0.01)				(0.02)
Observations	15817	15817	15817	15817	14990	14990	14990	14990
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	Yes	Yes	No	No
Industry FE	Yes	Yes	No	No	Yes	Yes	No	No

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Table OA1 continued

Uncertainty =	*Stock volatility*				*Delisting rate*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
YearIndustry FE	No	No	Yes	Yes	No	No	Yes	Yes