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

Abstract

This paper shows that the effectiveness of public policies depends on their interactions with existing private-sector prosocial motives. Examining the adoption of greenhouse gas emissions reduction targets by a subset of U.S. states, I find that firms in adopting states subsequently experience reduced shareholder pressure for reducing emissions, evidenced by fewer emission-related shareholder proposals and lower voting support rates. Accounting for emission scale variability across facilities, I find no evidence that these targets reduce corporate emissions. Overall, my findings suggest that under certain circumstances, public interventions can inadvertently crowd out private-sector prosocial efforts and thereby undermine the intended policy effects.

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

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 behaviour separately, less attention has been given to how they interact.

Public policies, such as regulations, subsidies, and taxes, create extrinsic incentives for private-sector prosocial behaviour. I propose that public policies may also have additional impacts, because private-sector prosocial behaviour is driven 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), whose response to public policies is still unknown.

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. By analysing patterns in GHG shareholder proposals following the implementation of these targets, I investigate how public policies influence private-sector prosocial motives. This empirical setting presents several advantages: GHG emissions are objectively measurable, policy 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, serve as a public provision of pressure on firms, and probably enhance publicity. Applying 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 reduces by 0.81 percentage points, a significant decline from the sample average 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 average 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 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 average 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.

My results differ from Korganbekova (2024), who finds that state-level emissions reduction targets effectively reduce emissions. This divergence stems from the choice of the outcome variable. While she uses log-transformed entity-level emissions, following the convention in the literature (Chan and Morrow, 2019; Bartram et al., 2022; Kumar and Purnanandam, 2024), I use non-transformed emissions. These two approaches have distinct economic implications and address different research questions. The average treatment effect (ATE) estimated using a log-transformed outcome variable reflects the average *percentage* change across entities. In contrast, the ATE with a non-transformed outcome variable captures the average *level* change, accounting for emissions scale and aligning with the treatment effect at the *aggregate* level. Since climate change depends on the *aggregate level* of GHG emissions, I use non-transformed entity-level emissions to evaluate the effectiveness of climate policies in mitigating climate change.

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. Consistent with this, I find that large emitters increase emissions following policy adoption, while small emitters reduce theirs, explaining the contrasting results observed when using log-transformed versus non-transformed outcome variables.

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 the empirical setting, it has broader implications for the general interaction between public policies and private-sector prosocial motives. It shows that public policies can sometimes fall short of their goals 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 contributes by examining the reverse interaction, namely how public policies influence private-sector prosocial motives. Importantly, 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.

Second, this paper highlights that using log-transformed versus non-transformed outcome variables in regressions addresses different research questions. Log-transformed outcomes estimate average *percentage* changes, whereas non-transformed outcomes capture average *level* changes. For assessing the aggregate effectiveness of public policies like emissions reduction targets, I argue that non-transformed outcomes are more appropriate. This discussion on research methodology parallels Hartzmark and Shue (2023)'s caution for real-world practice, who reveal 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. This distinction between different forms of outcome variables, though straightforward, is often overlooked and can be consequential. I

demonstrate that these two approaches can even yield ATEs with opposite signs, especially when the outcome is highly dispersed and the treatment effect is heterogeneous.

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 various motives behind corporate social responsibility.

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, Sautner, and Starks (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.

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.

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 illustrates these motives and how they may be influenced by climate policies.

4. Data and Sample

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 A3 summarises the categories of emission-related shareholder proposals, organised by sponsors and content.

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 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.

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,

[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).

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. 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.

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 A2.

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. 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.

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 A3 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 8, 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.

5. Empirical Analyses and Results

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:

$$Y_{it} = \beta_0 + \beta_1 Treated_{it} + B_2 X_{it} + d_i + d_t + \varepsilon_{it} \tag{1}$$

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 (1), 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 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 average 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:

$$Y_{it} = \beta_0 + \beta_1 Post_{it} + B_2 X_{it} + d_i + \varepsilon_{it} \tag{2}$$

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 (1).

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 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 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 A4 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 A5 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 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 (1) 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 average of 31.38%. The estimated dynamic treatment effects by year and their 95% confidence intervals are presented in Figure 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 A6, 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 A7, 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 A8, 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 envi-

ronmental issues significantly increases. This result may be explained by either shareholders reallocating 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 A9.

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.

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 (1), with the dependent variable replaced by emissions and firm fixed effects replaced by facility fixed effects.

Previous studies examining the effects of public climate policies on corporate emissions typically employ DID estimations using the natural log- (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 depen-

[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.

dent 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. However, none of these studies provide explicit reasons for taking log or log-like transformation of entity-level emissions.

I argue that applying log (or log-like) transformations to entity-level emissions might lead to misleading conclusions regarding the effectiveness of climate policies. In linear-linear models where the outcome variable is non-transformed, the average treatment effect (ATE), represented by coefficient β_1 in Equation (1), captures the average entity-level *level* change in the outcome variable. In contrast, in log-linear models where the outcome variable is log-transformed, the ATE, $e^{\beta_1} - 1$, is interpreted as the average entity-level *percentage* change in the outcome 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, whereas this property does not apply to *percentage* changes.

Neither approach is inherently superior; rather, the choice should depend on the specific goal of the analysis. When the focus is on aggregate effects, estimating the average entity-level *level* change is more appropriate. For instance, since climate change is driven by total GHG emissions, the effectiveness of climate policies should be assessed based on their impact on aggregate emissions. In such cases, a linear-linear model, which estimates the average *level* change at the entity level and thereby implies the aggregate impact, is preferable. Conversely, when the research question is more related to individual-level effects, estimating the average *percentage* change is more appropriate. For example, in evaluating how CEO compensation responds to corporate governance reforms or changes in market competition, the average *percentage* change in individual CEO's compensation provides more meaningful information than the average *level* change does.

These two approaches might even lead to opposite conclusions based on the same dataset,

especially when the outcome variable exhibits high dispersion and the treatment effects are heterogeneous. In the context of climate policies, for instance, if a smaller number of high-emitting facilities increase emissions while a larger number of low-emitting facilities reduce emissions, the average *level* change in emissions is likely to be positive, while the average *percentage* change can be negative. This discrepancy arises because the average *percentage* change treats all entities equally, regardless of scale, whereas the average *level* change accounts for the scale of each entity. A numerical example and further discussion of the differences between these two approaches are provided in the Appendix A.1.

Table 4 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 5. 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 *percentage* change is negative in Table 4. 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 4 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 4 produce results with opposite signs.[3]

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 6, show that the treatment effect is insignificantly positive, further supporting the conclusion that state-level emissions reduction targets are ineffective in reducing corporate emissions.

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 A10, where the estimated treatment

[3]Although facilities in the fifth quintile have substantially larger emissions than others, the conclusion of policy ineffectiveness is not driven by a few extreme cases. Robustness checks, by winsorizing emissions or excluding the ten largest emitters, are provided in Table A12.

effects are insignificant and close to zero. Thus, the evidence consistently suggests that state-level emissions reduction targets are ineffective in reducing corporate emissions.

Panels (a) and (b) in Figure 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 although some lower-emitting facilities might reduce emissions after the adoption of state-level targets, there is no evidence of an overall decline in aggregate emissions, 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 7, 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.

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 8, 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 average 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.

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.

Furthermore, this paper brings attention to the crucial distinction between the economic implications of using log-transformed versus non-transformed outcome variables—an issue often overlooked in the literature. When choosing between these two approaches, future research should consider not only their econometric properties, as prior studies have done, but also whether the research question centers on individual-level effects or aggregate outcomes.

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
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.

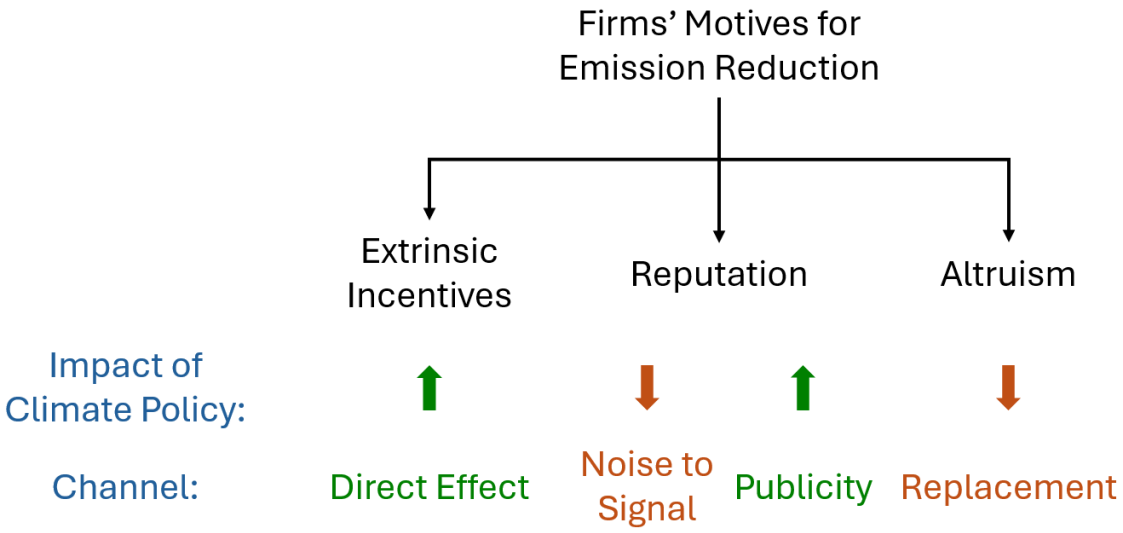
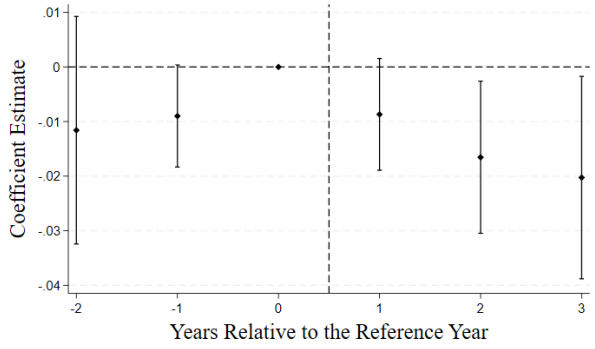


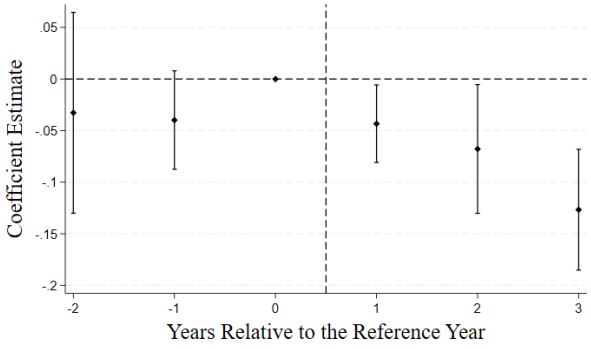
Figure 2
The Likelihood of Receiving Emission-Related Proposals and Support Rates

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

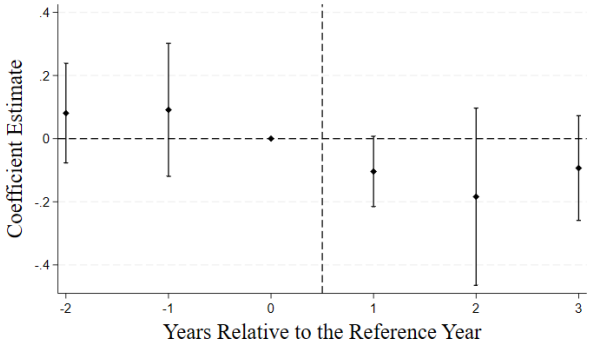
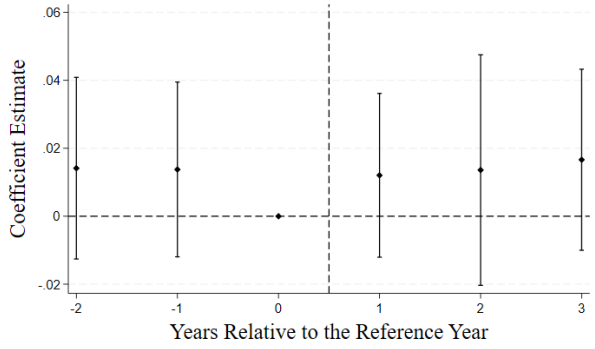


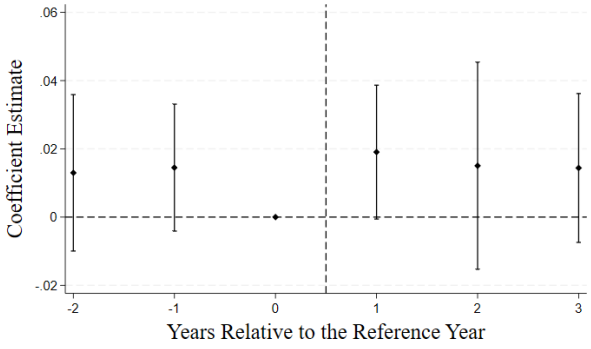
Figure 3
Facility-Level and State-Level GHG Emissions

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.

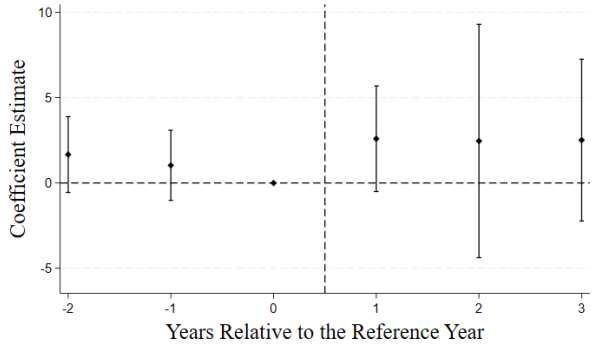
(a) Facility-Level Emission



(b) Facility-Level Emission (Balanced)



(c) State-Level Emission by GHGRP



(d) State-Level Emission by EIA

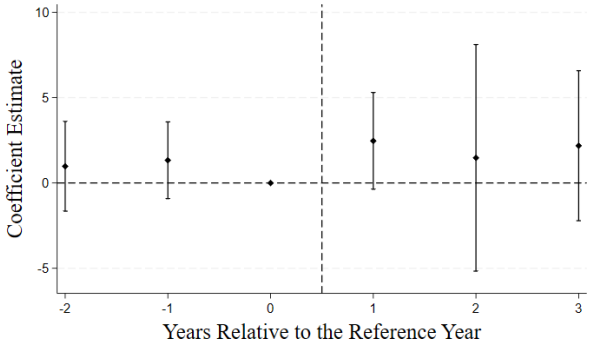
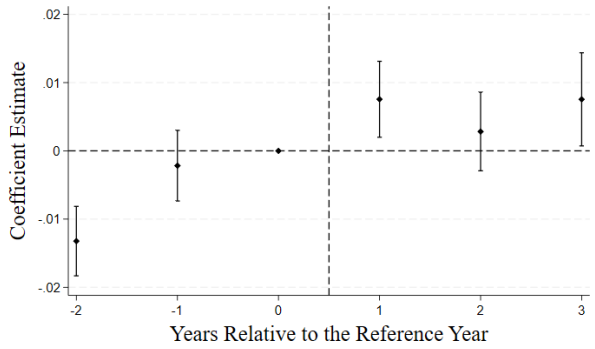


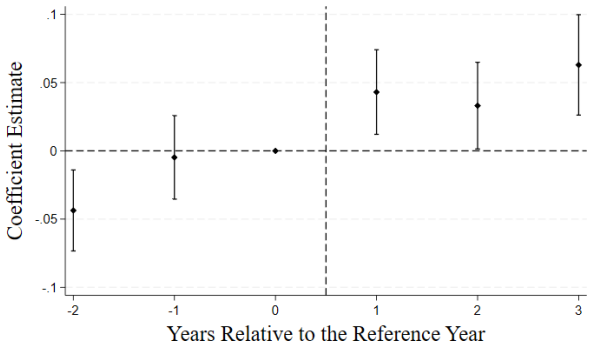
Figure 4
Pre-Post Comparisons Around the Paris Agreement

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.

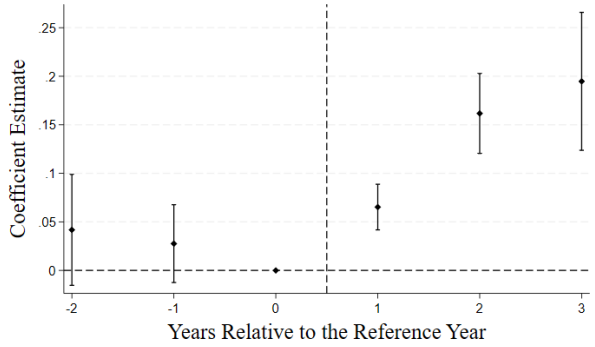
(a) GHG Proposal Dummy



(b) GHG Proposal Ratio



(c) Support Rate



(d) Facility-Level Emission

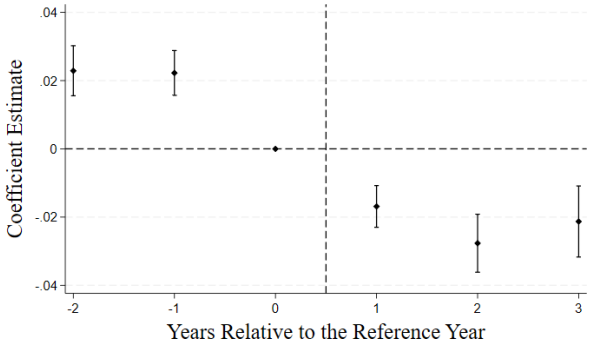


Table 1
Descriptive Statistics

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 A2.

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 $)
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 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 A2.

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

Continued on next page

Table 2 continued

Dep. Var. =	*GHG Proposal Dummy*				*GHG Proposal Ratio*	
	(1)	(2)	(3)	(4)	(5)	(6)
Adj. R^2	0.2345	0.2334	0.2334	0.2319	0.5172	0.5156
Controls	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full Sample		$\#(\text{Proposals})_i \geq 1$		$\#(\text{Proposals})_{it} \geq 1$	
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		$\#(\text{Proposals})_i \geq 1$		$\#(\text{Proposals})_{it} \geq 1$	

Table 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 A2.

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 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 A2.

	(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 5
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 6
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 A2.

	(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 7
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)*≥ 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

Continued on next page

Table 7 continued

SD(*#(Analysts)*)	(22.27)	(31.38)
Mean(*Emission*)	0.5235	0.5835
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 8
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 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 A2.

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		$\#(\text{Proposals})_i \geq 1$		$\#(\text{Proposals})_{it} \geq 1$	

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

Continued on next page

Table 8 continued

Controls	No	No	Yes
Firm FE	Yes	Yes	Yes
Balanced	No	Yes	Yes

A Appendix

A.1 A Comparison Between Log-transformed and Non-transformed Outcome Variables

Researchers sometimes apply logarithmic transformations to outcome variables in ordinary least squares (OLS) regressions. Common reasons include: (1) the distribution of the outcome variable is positively skewed, and a logarithmic transformation may approximate a normal distribution; (2) logarithmic transformation reduces the influence of outliers, especially those with extremely large values; and (3) the underlying model is multiplicative, and the transformation results in an additive and linear specification, which fits the data better. Relatedly, log transformation allows the estimated coefficient to be interpreted as a percentage change rather than an absolute change, which can be more meaningful in certain contexts.

However, an important distinction between using log-transformed versus non-transformed outcome variables is often overlooked: these two approaches address different questions. In a model with a log-transformed outcome variable, the average treatment effect (ATE) represents the average individual-level *percentage* change. In contrast, with a non-transformed outcome variable, the ATE reflects the average individual-level *level* change. These two ATEs do not always move together and can even have opposite signs, particularly when the distribution of the outcome variable is highly dispersed and the treatment effect is heterogeneous.

Therefore, the form of the outcome variable must align with the treatment effect being tested. If the focus is on *aggregate* treatment effects, the outcome variable should remain untransformed, as the average individual-level level change is proportional to the aggregate level change. If the treatment effect at the *individual* level is more relevant, log transformation may be more appropriate as it normalizes differences in scale and effectively gives equal weight to each individual.

Below, I provide a formula-based illustration and a numerical example to further explain this distinction. I also discuss whether the typical reasons for outcome variable transformation apply to the context of climate policy and corporate emissions, and provide robustness checks for my conclusion that state-level targets appear ineffective in reducing corporate

emissions.

For simplicity, consider a two-period balanced sample where each unit has two observations: pre-treatment and post-treatment. The pooled OLS regression model is specified as follows to estimate the treatment effect:

$$y_i = \alpha + \beta x_i + \varepsilon_i \tag{1}$$

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:

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

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

$$\hat{\beta} = \bar{y}_1 - \bar{y}_0 \tag{3}$$

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:

$$ln(y_i) = \alpha + \beta x_i + \varepsilon_i \tag{4}$$

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

$$\hat{\beta} = \overline{ln(y)}_1 - \overline{ln(y)}_0 \tag{5}$$

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

$$\hat{\beta} = ln(\bar{y}^{GEOM})_1 - ln(\bar{y}^{GEOM})_0 \tag{6}$$

or equivalently,

$$e^{\hat{\beta}} = \frac{\bar{y}_1^{GEOM}}{\bar{y}_0^{GEOM}} \tag{7}$$

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.[4] 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

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

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

$$\begin{aligned} \hat{\beta} &= \bar{y}_1 - \bar{y}_0 \\ &= 1.2 \end{aligned} \tag{8}$$

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

$$\begin{aligned} e^{\hat{\beta}} - 1 &= \frac{\bar{y}_1^{GEOM}}{\bar{y}_0^{GEOM}} - 1 \\ &= \frac{50.81}{52.10} - 1 \\ &= -2.48\% \end{aligned} \tag{9}$$

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 (5) as:

$$\begin{aligned} \hat{\beta} &= \overline{ln(y)}_1 - \overline{ln(y)}_0 \\ &= \frac{1}{n}\sum_{i=1}^{n}[ln(y_{i1}) - ln(y_{i0})] \\ &:= \frac{1}{n}\sum_{i=1}^{n}\hat{\beta}_i \end{aligned} \tag{10}$$

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 (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. In the final part of the appendix, I investigate the potential merits of using log-transformed outcome variables in the context of this paper. Since none of the existing studies on climate policy assessment (Bartram et al., 2022; Kumar and Purnanandam, 2024; Korganbekova, 2024) provide explicit reasons for the log-transformation of their outcome variables, i.e., entity-level emissions, I examine the three common reasons for logarithmic transformation outlined earlier. Additionally, I provide robustness checks for my conclusion.

First, skewness in the error term distribution is often cited as a reason for logarithmic transformation. However, the Gauss–Markov theorem guarantees that the OLS estimator is the best linear unbiased estimator (BLUE) under standard assumptions, even in the presence of skewness. Normality is only required for constructing confidence intervals and hypothesis testing. With a large sample size (the rule of thumb is merely 30), the central limit theorem ensures valid inference. Nevertheless, to further support my results, I provide bootstrapped standard errors, which make no parametric assumptions. The results in Table A11 show no evidence that state-level targets effectively reduce corporate-sector emissions.

Second, some researchers use logarithmic transformation to reduce the impact of outliers, but in the context of this paper, high emitters should not be treated as outliers. High emitters are integral to understanding the overall emissions and the effectiveness of climate policy. Nevertheless, I conduct two robustness checks, including winsorizing at the 1st and 99th percentiles and excluding the ten largest emitting facilities (Table A12). 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.

Third, logarithmic transformation is sometimes used to improve the regression model's fit to the data. A multiplicative relationship between the outcome and the explanatory variable may justify the transformation. However, in this case, the explanatory variable is a treatment dummy, and therefore the relationship between the outcome and the explanatory variable can 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:

$$e^{\beta} - 1 = \frac{E[y_1] - E[y_0]}{E[y_0]} \tag{11}$$

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 percentage change*, PPML estimates *the percentage 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 A13, which are consistent with the conclusion that state-level targets are ineffective in reducing corporate emissions.

In conclusion, regressions with long-transformed and non-transformed outcome variables address different research questions. The non-transformed outcome variable is more appropriate for assessing the effectiveness of climate policies in mitigating climate change, as its estimated ATE is proportional to the aggregate treatment effect on corporate emissions. While logarithmic transformation has merits in certain contexts, they are not relevant in the context of this paper.

Table A1
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
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

Continued on next page

Table A1 continued

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 A2
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 A3
Categories of Emission-Related Shareholder Proposals

This table presents the categories of emission-related shareholder proposals included in the sample for Table 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 A4
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 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 A5
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 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)

Continued on next page

Table A5 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 A6
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 A2.

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 A7
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 2 and Table 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	
Panel B: Patterns in emission-related proposals and COVID-19 non-essential business closure mandates								

Continued on next page

Table A7 continued

Dep. Var. =	*GHG Proposal Dummy*				*GHG Proposal Ratio*		*Support Rate*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
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 A8
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 2 and Table 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
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 A8 continued

Dep. Var. =	*Proposal Dummy*				*Proposal Ratio*		*Support Rate*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
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		$\#(\text{Proposals})_i \geq 1$		$\#(\text{Proposals})_{it} \geq 1$		Proposals voted on	

Table A9
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 2 and Table 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	

Table A10
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 6, 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 A2.

	(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 A11
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 4 and 6, 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 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 A2.

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 A12
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 4 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 A2.

	(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 A13
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 4 and 6, 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 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 A2.

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