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

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Abstract

Do public policies potentially crowd out private-sector prosocial motives and behaviours aimed at similar goals? Examining the adoption of emissions reduction targets by certain U.S. states, I find that firms in these states experience reduced shareholder pressure post-treatment, as indicated by fewer emission-related shareholder proposals and lower support rates. These state-level targets appear ineffective in reducing corporate emissions. These findings suggest that public interventions may unintentionally substitute private-sector prosocial efforts, thereby undermining the intended policy effects. Methodologically, I illustrate the different economic implications of regressing log-transformed versus non-transformed emissions, an often overlooked aspect broadly relevant to policy evaluations in general.

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

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1. Introduction

Since the concept of externality was introduced over a century ago (Marshall, 1890; Pigou, 1920), economists have proposed various approaches to mitigate the negative externalities of business activities and promote the provision of public goods. Some approaches call for public interventions (Pigou, 1920; Coase, 1960), while others emphasize the role of the private sector (Freeman, 1984). In the pursuit of the common good, the interactions between public- and private-sector efforts can significantly influence both their individual and combined effectiveness in shaping firm behaviour.

This paper examines how public policy may influence private-sector prosocial motives and firm behaviours. An ideal setting for exploring this interaction requires an overlap between public- and private-sector contributions, the interchangeability of efforts by different parties, and partial adoption of the public policy within the sample. The reduction of greenhouse gas (GHG) emissions—a critical and urgent externality that drives climate change—provides such a context. Specifically, this paper investigates whether state-level climate policies crowd in or crowd out private-sector motives for reducing corporate emissions and whether these policies successfully achieve their goal of lowering corporate emissions.

The potential impact of public policy on private-sector prosocial motives is multifaceted, resulting in uncertain effects on firms' prosocial behaviors such as emissions abatement. Bénabou and Tirole (2006) categorize individuals' prosocial motives into three types: altruistic, extrinsic, and reputational, which I apply here to the private sector's motives for reducing emissions. Drawing on the theoretical frameworks of Bénabou and Tirole (2006) and Ariely et al. (2009), while climate policy directly creates extrinsic incentives, it may crowd out private-sector reputational motives by making voluntary sustainability efforts less distinguishable from policy-driven actions, or alternatively, crowd them in by increasing publicity surrounding climate issues. Following the logic of Andreoni (1988) and Andreoni (1990), climate policy could also crowd out private-sector altruistic motives by raising expectations that others will reduce emissions, thus lessening the perceived need for their own actions. Given the complex and uncertain interaction between public policy and these various motives, how specific climate policies affect private-sector motives and whether they effectively reduce corporate emissions remain empirical questions.

To address these questions, I employ a difference-in-differences (DID) framework based on the adoption of state-level GHG emission reduction targets by certain U.S. states. These targets mandate a certain percentage reduction in the total volume of GHGs emitted within a state over a defined period, typically followed by specific regulations and programs designed to meet these goals. Given the timeline of target adoption and data availability, I designate firms headquartered and facilities located in the nine states that adopted GHG emission reduction targets in 2019 as the treatment group, while those in the twenty-five states that have never adopted such targets as the control group. My firm- and facility-level samples consist of 2,057 firms and 4,763 facilities in the U.S., spanning the period from 2016 to 2021.

Among private-sector stakeholders, I focus specifically on shareholders' motives for reducing emissions, as firms have a fiduciary duty to maximize shareholder welfare (Hart and Zingales, 2017).[1] To analyze shareholder motives, I employ the DID framework to compare the likelihood of firms receiving

[1]In this paper, I disregard the motives of other private-sector actors, such as managers, which may also influence firm decisions on emissions reduction. Therefore, potential agency issues between managers and shareholders are outside the scope of this paper.

emission-related shareholder proposals and the voting outcomes on these proposals before and after the adoption of state-level targets.

I then apply the same DID framework to assess the effectiveness of these targets in reducing corporate emissions. Departing from the common practice of using log-transformed entity-level emissions as the outcome variable (Chan and Morrow, 2019; Bartram et al., 2022; Kumar and Purnanandam, 2024; Korganbekova, 2024), I propose using actual (non-transformed) emissions instead. The average treatment effects (ATEs) estimated using these two approaches have different meanings: the ATE with a log-transformed outcome variable approximates the average *percentage* change across entities but does not account for differences in emissions scale among them. In contrast, the ATE with a non-transformed outcome variable estimates the average *absolute* change, which incorporates differences in emissions scale and is proportional to the treatment effect on *aggregate* emissions. Since climate change is driven by *aggregate* GHG emissions, using actual entity-level emissions as the outcome variable is more appropriate for evaluating the overall effectiveness of climate policies.

I also apply the DID methodology to state-level emission data as a robustness check and examine the heterogeneous treatment effects across facilities subject to varying levels of ex-ante shareholder pressure. Lastly, through pre-post comparisons, I assess whether another climate policy, the Paris Agreement, influences shareholder motives and firm behaviours differently.

My results indicate that after a state adopts an emission reduction target, the average firm in that state becomes 0.81 percentage points less likely to receive a GHG emission-related shareholder proposal in a given year, a significant reduction given that the average probability across the sample is 2.16% ($2.16\% - 0.81\% = 1.35\%$). Further analysis shows that the proportion of GHG emission-related proposals relative to all shareholder proposals decreases by 5.44 percentage points, which is economically significant against a sample average of 10.77% (10.77% − 5.44% = 5.33%). Therefore, state-level emission reduction targets specifically crowd out emission-related shareholder proposals.

Beyond submitting proposals, shareholders also influence firms through their voting. My evidence shows that the average support rate on GHG emission-related proposals declines by 12.78 percentage points post-treatment, compared to the sample average of 31.38% (31.38% − 12.78% = 18.60%). Thus, state-level emission reduction targets not only decrease the submission of emission-related proposals but also reduce shareholder support for them.

Having established that state-level targets crowd out shareholder motives, the overall effectiveness of these policies depends on whether the crowding-out effect fully offsets their direct impact. Using facility-level emissions as the outcome variable, I find no evidence that state-level targets reduce emissions as intended. To reinforce this conclusion, I conduct state-level analyses, which similarly show no evidence that these targets effectively reduce corporate emissions.

For comparison, I also implement the conventional approach of using log-transformed entity-level emissions as an alternative specification. Although the results show a negative correlation between treatment and log-transformed facility emissions, this should not be interpreted as evidence of policy effectiveness in reducing total emissions. By dividing the sample into quintiles and running regressions for each, I find that while a larger number of lower-emitting facilities reduce their emissions post-treatment, a smaller number of higher-emitting facilities actually increase their emissions. Due to the highly dispersed distribution of emissions across facilities, the increases from the higher-emitting facilities outweigh the reductions from the more numerous lower-emitting ones in determining the change in total emissions, which is insignificantly positive. This explains why the ATE estimated from log-linear regressions, which overlook the variation in facility emissions scales, seemingly conflicts with the ATE from linear-linear regressions that capture the aggregate treatment effect.

Since the effectiveness of state-level targets depends on the balance between their direct impact and their crowding-out effect on shareholder motives, I hypothesize that facilities facing higher ex-ante shareholder pressure will experience a stronger crowding-out effect, making them less likely to reduce emissions. To test this, I classify facilities using three measures: the number of emission-related shareholder proposals they receive, the number of analysts covering them, and whether they are privately or publicly owned. Although most results are statistically insignificant, the direction of the estimated coefficients is consistent with the hypothesis. Moreover, the evidence shows that utilities under higher investor pressure tend to have higher average emissions, which possibly explains the heterogeneity in policy treatment effects across facilities with different emissions scales.

Climate policies are not inherently ineffective. They can be effective when their direct impact is strong, their crowding-out effect on shareholder motives is moderate, or when they crowd in shareholder motives. To explore this, I examine another public policy—the Paris Agreement—to assess whether it influences shareholder motives and corporate emissions differently. As the first universal, legally binding global climate agreement, the Paris Agreement is likely to increase public attention to climate issues and thereby crowd in private-sector motives through the "publicity channel". Additionally, its 1.5-degree target necessitates much more ambitious emissions reduction than previous accords, indicating a stronger direct impact. Pre-post analyses show that following the signing of the Paris Agreement in 2016, the average firm becomes 1.08 percentage points more likely to receive a GHG emission-related proposal each year, with the proportion of such proposals increasing by 6.04 percentage points and support for them rising by 10.77 percentage points. This increased shareholder engagement is accompanied by an average reduction of 0.04 million metric tons of carbon dioxide equivalent (CO_2e) in facility emissions. Although these pre-post comparison results should not be interpreted as causal, they provide suggestive evidence that, under certain circumstances, public policies can crowd in shareholder motives and effectively reduce corporate emissions.

This paper makes three contributions to the literature. First, despite the extensive research on the separate roles of government and the private sector in curbing GHG emissions, their interaction remains relatively understudied. Most existing research on this interaction is theoretical and focuses primarily on whether government commitments are endogenous to firms' efforts (Biais and Landier, 2022; Acharya et al., 2023; Allen et al., 2023; Carlson et al., 2023; Heeb et al., 2023). The reverse relationship, namely how public policy influences private-sector motives, has received little attention. Piatti et al. (2023) model how taxes and subsidies crowd out the private provision of public goods by consequentialist investors.[2] This paper extends the discussion to shareholder reputational motives and, to the best of my knowledge, is the first empirical study to explore the influence of climate policy on private-sector motives.

Second, this paper highlights that regressions using log-transformed versus

[2]In Piatti et al. (2023) and related studies, "consequentialist" investors derive utility from the total provision of public goods, corresponding to "pure altruism" in the terminology of Andreoni (1988, 1990). "Non-consequentialists", on the other hand, derive utility solely from their own contributions, in which sense akin to the concept of reputational motives in Bénabou and Tirole (2006).

non-transformed outcome variables address fundamentally different research questions. Through a formula-based illustration, I show that regressions with log-transformed outcomes estimate average *percentage* changes across entities, whereas those with non-transformed outcomes estimate average *level* changes. Using a numerical example and analyses of real emissions data, I demonstrate that these two approaches can even yield ATEs with opposite signs, especially when the outcome variable is highly dispersed and the treatment effect is heterogeneous. Hartzmark and Shue (2023) reveal that investors and ESG environmental ratings mistakenly focus on percentage reductions in firm emissions rather than level reductions, overlooking differences in emissions scale across firms. My findings suggest that we researchers risk making the same error by using log-transformed entity-level emissions as the outcome variable. This methodological consideration applies broadly to other regression analyses where the primary focus is on aggregate rather than individual-level outcomes.

Finally, this paper relates to the literature on the motives underlying corporate social responsibility (CSR). In contrast to Friedman (1970)'s narrow 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. Riedl and Smeets (2017), Bauer et al. (2021), and Giglio et al. (2023) demonstrate through surveys and experiments that both value-driven and welfare-driven motives coexist among investors. By identifying a crowding-out effect of extrinsic incentives on other motives for corporate prosocial behaviours—as predicted by Andreoni (1988) and Bénabou and Tirole (2006)—this paper adds further evidence to the coexistence of various motives behind CSR efforts, emphasizing that these motives are not independent but rather interact in complex ways.

2. Background

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).

This paper primarily focuses on state-level GHG emission reduction targets, a setting that presents three key advantages for studying the interaction between public policy and private-sector efforts. First, emission reduction is a shared objective to which both the government and the private sector contribute. Second, emission reduction efforts are interchangeable, meaning that one unit of reduction by any firm has the same climate impact, which allows for perfect substitution and enables the assessment of the aggregate effect of public policy. Third, these targets have only been adopted by certain states, making the setting suitable for applying the DID method. 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). 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). A summary of these state-level targets is presented in Table A1.

State-level GHG reduction targets are generally followed 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).

This paper focuses on 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 few industries. Second, since states generally set their targets first and then develop specific regulations to achieve them, the private sector is likely to begin making adjustments as soon as these targets are adopted, in anticipation of the subsequent regulations and programs.

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 signaling. 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 motives for emissions reduction.

Bénabou and Tirole (2006) develop a model that examines the interaction between different motives behind individuals' prosocial behaviours. According to their framework, prosocial behaviours can be driven by three types of motives: altruism, extrinsic incentives, and reputation. When applied to the context of corporate GHG emissions reduction, firms may be motivated by: (1) Altruism, where firms willingly sacrifice some profits to contribute to a more sustainable future; (2) Extrinsic incentives, such as subsidies, taxes, or industry standards tied to emissions, which are often mandated by climate policies; and (3) Reputation, which can hold both affective and instrumental values, such as improving employee and customer satisfaction and thereby enhancing firm value. Their model can also be used to predict how climate policies might influence various private-sector motives for emissions reduction. First, climate policies introduce extrinsic incentives to lower emissions. Second, these policies could potentially crowd out firms' or shareholders' reputational motives, as the presence of extrinsic incentives may obscure the genuine commitment to sustainability behind prosocial behaviours, thereby reducing the reputational benefits. On the other hand, if climate policies elevate public attention to firms' climate impacts, they could amplify private-sector reputational motives. In summary, while climate policies create extrinsic incentives for reducing emissions, they can either crowd in or crowd out reputational motives.

The interaction between public policy and altruism, another type of prosocial motive, can be inferred from the models by Andreoni (1988) and Andreoni (1990). In these models, altruism is represented through a utility function that depends on the aggregate provision of a public good, where private contributions are reduced in response to increased public provision in order to maintain an optimal level of total provision. Although these models do not explicitly address public policies beyond direct provision, their implications in this context are clear: if a company anticipates that state-level targets will effectively reduce aggregate emissions by other companies, its own altruistic motives to contribute are likely to be crowded out.

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 shareholders' altruistic motives while either crowding in or crowding out their reputational motives. Consequently, (2) the overall effectiveness of these policies in reducing corporate emissions is uncertain and depends on the balance between their direct impacts and the extent of any crowding-out effects. Figure 1 illustrates these different 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. I then match this sample (and the subsample) with control variables constructed using data from the CCM and CRSP Monthly Stock databases.

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.[3] Once a facility falls under these reporting requirements, it must

[3] 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

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 met-

categories or emission sources. More details about the requirements can be found in U.S. Environmental Protection Agency Office of Atmospheric Protection (2024).

ric 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 pro-social behaviours, household emissions should be excluded. 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 DID 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 DID analyses. Descriptive statistics for the variables over the event window from 2016 to 2021 are shown in Panel A of Table A1.

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 A1.

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 the conclusion 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 more 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.

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 rather than submitting 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 around 2019 within the treatment and control groups, 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.

Overall, the results presented in Table 2, Table 3, and Figure 2 indicate that state-level emissions reduction targets seem to crowd out shareholder motives for reducing corporate emissions. This aligns with the theoretical predictions by Bénabou and Tirole (2006) and Andreoni (1988), which suggest that extrinsic incentives can crowd out reputational and altruistic motives for prosocial behaviours. However, my analysis does not distinguish which specific motive is being crowded out, as this falls outside the scope of this paper.

5.2. Emissions

Given the crowding-out effect of state-level emissions reduction targets on shareholder 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 shareholders' 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[4]. Bartram et al. (2022) evaluate the effectiveness of the California cap-and-trade program using log(1+plant-level emissions) as the dependent variable. Chan and Morrow (2019) and Kumar and Purnanandam (2024) assess the RGGI cap-and-trade policy with log(plant-level emissions) as the dependent variable. Korganbekova (2024) investigates state-level emissions reduction targets using both log(1+firm-level emissions) and log(1+facility-level emissions) as dependent variables. 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 *absolute* 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

[4]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.

percentage change in the outcome variable. Notably, when the number of entities is fixed, the average *absolute* change at the entity level is proportional to the *absolute* 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 *absolute* 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 *absolute* 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 *absolute* 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 *absolute* 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.[5]

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 absolute 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 absolute state-level emissions assign greater weight to states with higher emissions, while those using log-transformed state-level emissions

[5]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 A5.

treat all states equally. For additional robustness, I run regressions using log-transformed state-level emissions, as shown in Table A3, where the estimated treatment effects are insignificant and close to zero. Thus, the evidence consistently suggests that state-level emissions reduction targets are ineffective in reducing corporate emissions.

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 on shareholder motives—so the net effect should vary among individual facilities based on their relative sensitivities to regulatory and shareholder pressures. The evidence, 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 shareholder prosocial motives. For the last set of tests in this paper, I analyse how the Paris Agreement interacts with shareholder 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 shareholder 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 shareholder 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 shareholders' awareness of climate issues and their motives for reducing corporate emissions.

Second, I assess the changes in actual emissions following the Paris Agreement. Since the Agreement appears to crowd in shareholder 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 shareholder motives and effectively reduce private-sector 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 measuring shareholder motives through 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 shareholder motives for reducing corporate emissions. Additionally, I find no evidence that these state-level policies effectively reduce corporate emissions.

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 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 on shareholder motives, or if they enhanced the visibility of firms' actions and thus bolstered shareholders' 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 a firm expects that other firms will contribute more to emissions reduction in response to the policy, its own altruistic efforts may be crowded out, as the perceived need for its actions decreases with the anticipation of greater overall emissions reduction.

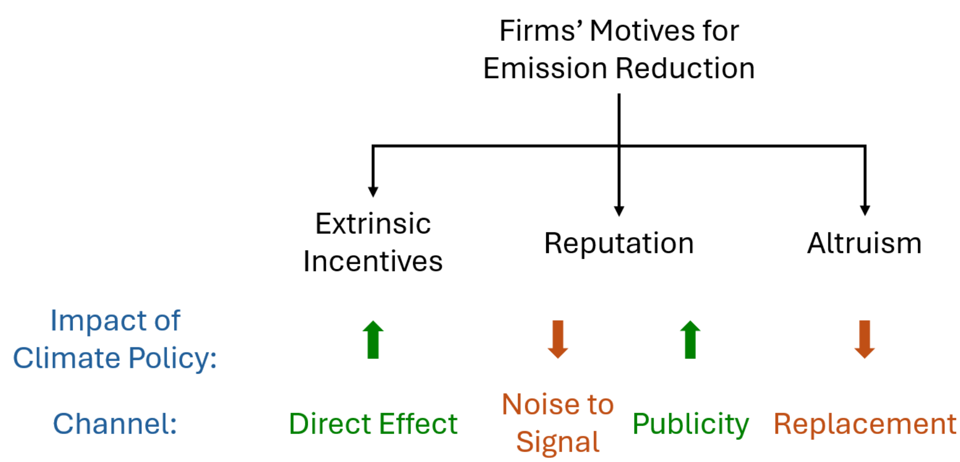
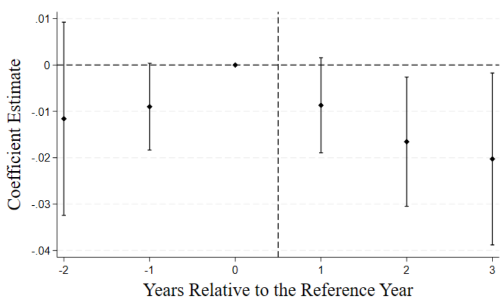


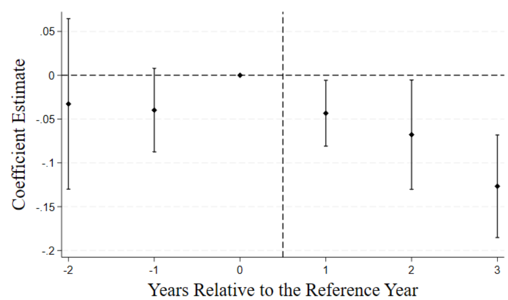
Figure 2
The Likelihood of Receiving Emission-Related Proposals and Support Rates

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

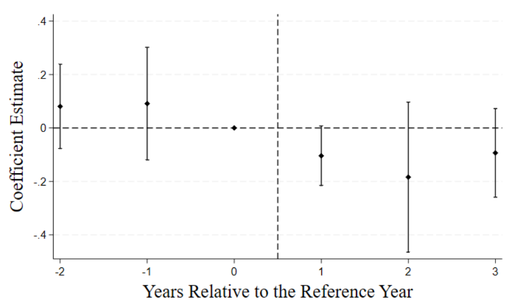
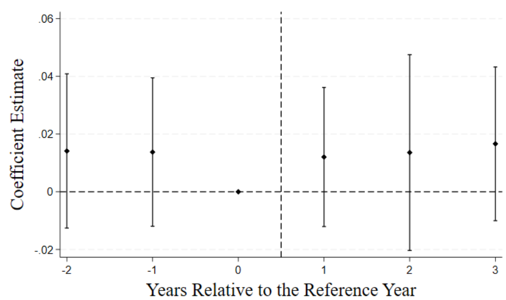


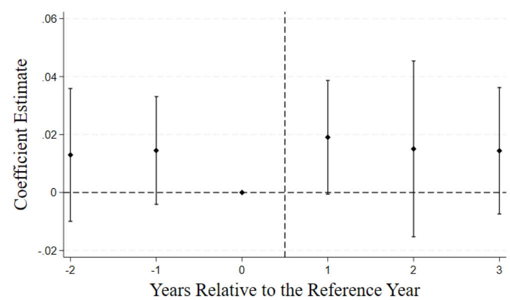
Figure 3
Facility-Level and State-Level GHG Emissions

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.

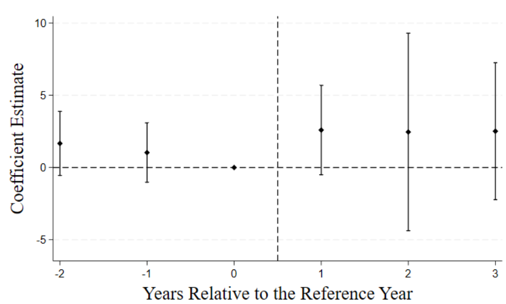
(a) Facility-Level Emission



(b) Facility-Level Emission (Balanced)



(c) State-Level Emission by GHGRP



(d) State-Level Emission by EIA

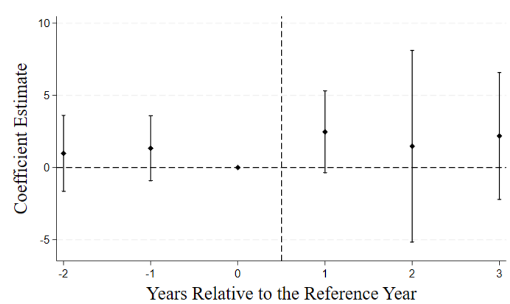
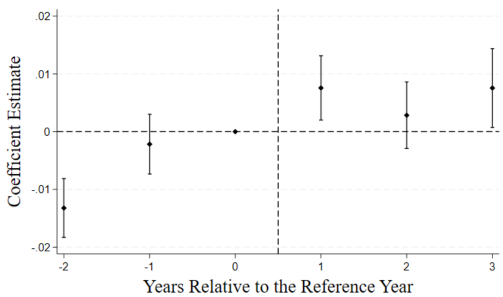


Figure 4

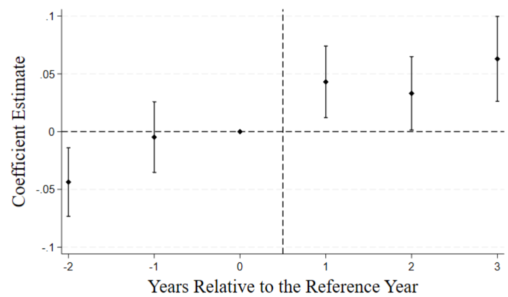
Pre-Post Comparisons Around the Paris Agreement

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.

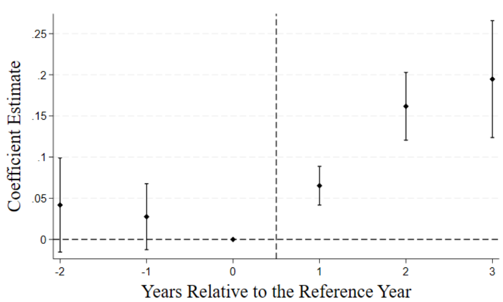
(a) GHG Proposal Dummy



(b) GHG Proposal Ratio



(c) Support Rate



(d) Facility-Level Emission

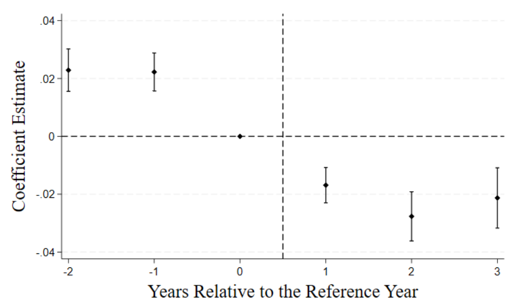


Table 1
Descriptive Statistics

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											

Continued on next page

Table 1 continued

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						

Continued on next page

Table 2 continued

Dep. Var. =	*GHG Proposal Dummy*				*GHG Proposal Ratio*	
	(1)	(2)	(3)	(4)	(5)	(6)
Post	-0.0108***	-0.0110***	-0.0276***	-0.0292***	-0.0371**	-0.0318***
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	4083	4083	1464	1464	473	473
Adj. R^2	0.2345	0.2334	0.2334	0.2319	0.5172	0.5156
Controls	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full Sample		#(Proposals)$_i$ $\geq$1		#(Proposals)$_{it}$ $\geq$1	
Panel C: Pre-post comparison within the control group around 2019						
Post	-0.0026	-0.0052	-0.0070	-0.0209*	0.0160	0.0148
	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)	(0.01)
Observations	6191	6191	2065	2065	667	667
Adj. R^2	0.2558	0.2560	0.2382	0.2410	0.3793	0.3752
Controls	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full Sample		#(Proposals)$_i$ $\geq$1		#(Proposals)$_{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	-0.0309*	0.1021
	(0.02)	(0.07)
Observations	5322	2946
Adj. R^2	0.9594	0.9630
Year FE	Yes	Yes
Facility FE	Yes	Yes
Mean(*#(Proposals)*)	0	4.4766
SD(*#(Proposals)*)	(0)	(2.52)
Mean(*Emission*)	0.2528	1.0936
SD(*Emission*)	(0.82)	(2.51)

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)

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Table 7 continued

Observations	4284	3984
Adj. R^2	0.9645	0.9636
Year FE	Yes	Yes
Facility FE	Yes	Yes
Mean(*#(Analysts)*)	48.5489	87.0731
SD(*#(Analysts)*)	(22.27)	(31.38)
Mean(*Emission*)	0.5235	0.5835
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 *Emission*s, 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***

Continued on next page

Table 8 continued

	(0.00)	(0.00)	(0.01)
Observations	40866	34890	9851
Adj. R^2	0.9630	0.9646	0.9704
Controls	No	No	Yes
Firm FE	Yes	Yes	Yes
Balanced	No	Yes	Yes

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.[6] 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

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

percentage.

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

In this example, the two largest facilities increase emissions by 10%, while the other three decrease emissions by 10%, leading to greater dispersion. The geometric mean decreases from 52.10 to 50.81, while the arithmetic mean increases from 60 to 61.2. If 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 A4 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 A5). 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 A6, 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. For assessing climate policy effectiveness on corporate emissions, the non-transformed outcome variable is more appropriate, as its estimated ATE is proportional to the aggregate treatment effect. While logarithmic transformation has its 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

Continued on next page

Table A1 continued

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

Table 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
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 A4
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 A5

Facility-Level GHG Emissions Excluding or Winsorizing Largest Emitters

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 A6

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