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

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Abstract

Do public policies necessarily foster private-sector prosocial behaviour? Using the adoption of greenhouse gas emissions reduction targets across nine U.S. states as an empirical setting, I find that firms in adopting states exhibit weaker voluntary motives to reduce emissions, as reflected in fewer emissions-related shareholder proposals and lower voting support. Despite policy incentives, these firms do not achieve greater emissions reductions relative to their peers. The policy is least effective for firms with stronger pre-policy voluntary motives, consistent with a crowding-out mechanism. Overall, the findings suggest that when public policy crowds out private-sector voluntary motives, it may undermine policy effectiveness.

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

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1. Introduction

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

Public policies, such as regulations, subsidies, and taxes, are designed to create *extrinsic incentives* for the private sector. Their effectiveness, however, may be more complex than conventionally assumed, as private-sector prosocial behaviour is driven not only by extrinsic incentives but also by *voluntary motives*, which reflect considerations beyond policy-induced incentives (Riedl and Smeets, 2017; Hartzmark and Sussman, 2019; Bauer et al., 2021; Giglio et al., 2025). Understanding how these voluntary motives respond to public policies is essential for assessing policy effectiveness and optimising policy design, yet remains an open question.

In this paper, I explore how public policies influence private-sector voluntary motives and how these interactions shape the effectiveness of public policies in promoting private-sector prosocial behaviour. I use greenhouse gas (GHG) emissions, one of the central concerns of both public policy and corporate prosocial efforts, as the empirical setting. Because emissions are perfectly substitutable across firms in their impact on the climate, this setting is well-suited for assessing the overall policy impact.

The analysis uses 2019 as the policy treatment year, when nine U.S. states adopted economy-wide emissions reduction targets. These targets mandate reductions in aggregate state-level emissions and are accompanied by a range of policy measures that incentivise firms to reduce emissions. Owing to the separation of ownership and control, private-sector *voluntary motives* can be inferred from shareholder-management communication, such as emissions-related shareholder proposals.[1] Private-sector *prosocial behaviour* is measured by

[1] Krueger et al. (2020) show that shareholders frequently express climate-related concerns by submitting

reductions in corporate GHG emissions.

My firm- and facility-level samples comprise 2,057 firms and 4,763 facilities in the United States from 2016 to 2021. I employ a difference-in-differences (DID) framework, in which firms and facilities in the nine adopting states constitute the treatment group, while those in the twenty-five states that never adopted such targets serve as the control group. The results indicate a crowding-out of private-sector voluntary motives, as both the likelihood of firms receiving emissions-related shareholder proposals and the voting support for these proposals decline following policy adoption. Contrary to the intended policy goals, I find no evidence that the policy promotes prosocial behaviour, namely reductions in emissions. The results remain robust across alternative specifications and measures. Heterogeneity analyses further suggest that the policy is least effective for firms with stronger pre-policy voluntary motives, consistent with the crowding-out mechanism. Finally, recognising that the interaction between public policy and private-sector voluntary motives may vary across policies, I examine the Paris Agreement as an additional case for comparison.

I develop a conceptual framework to analyse how public policies affect private-sector extrinsic and voluntary motives. As a direct effect, climate policies create extrinsic incentives for firms to reduce emissions. To characterise their impact on voluntary motives, which has not been explicitly modelled in the literature, I extend existing theories of individual prosocial behaviour to the firm context. On the one hand, public policies may crowd out voluntary motives. Following the logic of Bénabou and Tirole (2006), extrinsic incentives may blur the distinction between voluntary and policy-driven actions, thereby weakening the signalling value and reputational benefits of reducing emissions. In addition, extending Andreoni (1990), if public policies are expected to induce emissions reductions, investors may perceive a reduced need for their own engagement and scale back their altruistic efforts. On the other hand, climate policies may crowd in voluntary motives by increasing public

and voting on such proposals, and Grewal et al. (2016) show that firms receiving sustainability-related proposals subsequently exhibit improvements in sustainability performance.

attention to corporate emissions and thereby strengthening reputational motives.

Accordingly, the net effect of public policies on voluntary motives, as captured by shareholder proposal activity, is theoretically ambiguous. Taking into account the extrinsic incentives created by public policies, the net policy effect on private-sector prosocial behaviour, as captured by emissions reductions, is also an empirical question.

My DID analyses suggest that, following the adoption of state-level emissions reduction targets, the probability that a firm receives an emissions-related shareholder proposal in a given year declines by 0.81 percentage points, which is economically large relative to the sample mean of 2.16%. The share of emissions-related proposals among all shareholder proposals decreases by 5.44 percentage points, relative to a sample mean of 10.77%, indicating that the policy disproportionately crowds out emissions-related proposals.

Despite firms' reduced likelihood of receiving emissions-related proposals, it remains possible that shareholders increase the intensity of their engagement through higher voting support. To assess this possibility, I examine voting outcomes for GHG emissions-related proposals. The average support rate for these proposals declines by 12.78 percentage points after treatment, which is sizable relative to the sample mean of 31.38%.

The reductions in both proposal submissions and voting support suggest that state-level emissions reduction targets crowd out private-sector voluntary motives to reduce emissions. I also find that the adopted policy measures address issues distinct from those raised in shareholder proposals, indicating that the decline in proposal activity is not driven by proposals becoming redundant due to overlap with policy measures.

These results remain robust across a range of alternative specifications, including: (1) excluding proposals submitted by pension funds, which may be directly influenced by state and local governments; (2) excluding proposals submitted by special-interest organisations and individuals, whose behaviour may differ from that of typical investors; (3) restricting the sample to firms whose high-emitting facilities are located exclusively in their headquarters states, thereby addressing concerns that firms operating in multiple states are exposed to

different policy regimes and may shift emissions across states; and (4) controlling for state-level COVID-19-related policy measures, which may be correlated with both climate policies and emissions-related proposal activity. In addition, I find suggestive evidence that the share of social proposals increases following policy adoption, indicating a potential shift in investor focus from climate-related to social issues.

Firm behaviour (e.g., reducing emissions) is determined by both extrinsic incentives and voluntary motives. Given that the policy creates extrinsic incentives while crowding out voluntary motives, its overall effectiveness in altering firm behaviour depends on whether its direct effect outweighs its crowding-out effect. Applying the DID framework to facility-level emissions, I find no evidence that the policy reduces corporate emissions relative to the control group. Firm-level and state-level analyses further corroborate this finding, likewise showing no greater reductions in corporate emissions among target-adopting states.

Cross-sectionally, given the opposing directions of the policy's direct impact and its crowding-out effect, I hypothesise that facilities with stronger pre-policy voluntary motives experience greater crowding-out, resulting in weaker or even counterproductive net policy effects.[2] Conversely, facilities with weaker ex-ante voluntary motives should be more responsive to the policy's direct impact and thus more likely to reduce emissions. To test this prediction, I partition the sample using two alternative measures of pre-policy voluntary motives: the number of emissions-related shareholder proposals received and the S&P Global climate strategy score. The regression results across subsamples support this hypothesis.

Thus far, I have used shareholder proposals to measure voluntary motives and corporate emissions to measure prosocial behaviour. However, shareholders and management may communicate through means other than proposals, and it remains possible that firms adjust their behaviour following policy adoption but that these actions take more than three years to materialise as observable emissions reductions. To address both concerns, I further ex-

[2]Because the estimates are based on difference-in-differences analyses, "counterproductive" refers to slower emissions reductions relative to the control group rather than absolute increases in emissions.

amine the frequency of green transition-related mentions during earnings calls. Specifically, I adopt measures from Li et al. (2024), who quantify the frequency of bigrams related to climate "policies and regulations, technological improvements, and evolving climate patterns" discussed during earnings calls. They classify each mention as either *non-proactive* or *proactive*, depending on whether it is accompanied by nearby proactive verbs (e.g., "change", "initiate", "reduce"). This classification aligns closely with my framework: non-proactive mentions capture voluntary motives, while proactive mentions capture prosocial behaviour. I find that, following policy adoption, both non-proactive and proactive mentions of green transitions decrease, although the decline in proactive mentions is statistically insignificant. These results further support my conclusion that state-level targets inadvertently crowd out private-sector voluntary motives and fail to effectively foster corporate prosocial behaviour.

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

and effectively alter private-sector behaviour.

Although this paper empirically focuses on climate policies, it speaks more broadly to the relationship between public policies and private-sector prosocial behaviour. The findings suggest that, in some contexts, public policies may fall short of their objectives by inadvertently crowding out private-sector voluntary motives.

This paper makes three contributions to the literature. First, despite the extensive research on the separate roles of public policy and private-sector prosocial efforts, their interaction remains relatively understudied.[3] In the context of climate, the growing literature on public–private interactions is predominantly theoretical and focuses on how firms' voluntary commitments influence climate policymaking (Biais and Landier, 2022; Acemoglu and Rafey, 2023; Heeb et al., 2023; Acharya et al., 2025; Allen et al., 2025; Carlson et al., 2025; Döttling et al., 2026). By contrast, the reverse channel—how private-sector efforts respond to public policy—remains underexplored. Piatti et al. (2025) examine one voluntary motive, altruism, and show theoretically that taxes and subsidies can crowd out the private provision of public goods. To the best of my knowledge, this paper is the first to develop a comprehensive conceptual framework that incorporates a range of private-sector motives and to provide empirical evidence on how climate policies may crowd out voluntary motives. In related work, Demerjian et al. (2025) focus on lenders and document that environmental covenants are less likely to be included in loan contracts after a borrower's state adopts an emissions target, consistent with and complementary to my findings.

Second, this paper adds to the extensive literature on policy (in)effectiveness, which documents various channels through which public interventions can fail, such as circumvention through regulatory loopholes or relocation (Bartram et al., 2022), unintended gaming of

[3]For example, in the climate context, one strand of the literature evaluates the effects of public policies (Chan and Morrow, 2019; Bartram et al., 2022; Huang and Kopytov, 2024; Inderst and Opp, 2024; Kumar and Purnanandam, 2025; Delis et al., 2026; Oehmke and Opp, 2026), while another analyses the impact of private-sector efforts (Azar et al., 2021; Flammer, 2021; Naaraayanan et al., 2021; Broccardo et al., 2022; Jagannathan et al., 2023; Akey et al., 2024; Oehmke and Opp, 2025; Atta-Darkua et al., 2025; Berk and Van Binsbergen, 2025; Duchin et al., 2025; Green and Roth, 2025; Kahn et al., 2025; Bolton and Kacperczyk, 2026).

incentives (Jacob and Levitt, 2003), offsetting supply-side responses (Diamond et al., 2019), weak legal enforcement (Bhattacharya and Daouk, 2002), and political capture (Laffont and Tirole, 1991), among others. This paper extends this body of work by identifying the crowding-out of private-sector voluntary motives as an additional mechanism through which policies may be ineffective.

Finally, this paper contributes to the vast literature on corporate social responsibility (CSR). In contrast to Friedman (1970)'s focus on shareholder value maximisation, recent research distinguishes between shareholder "value" and shareholder "welfare" or "values" (Hart and Zingales, 2017; Starks, 2023), underscoring the importance of disentangling the motives underlying CSR. This paper contributes to that effort from a different angle by distinguishing between extrinsic and voluntary motives behind prosocial behaviour and showing that public policies can affect these motives in opposing ways.

2. Background

Studying the interaction between public policies and private-sector prosocial efforts presents several challenges. First, private-sector voluntary motives and prosocial behaviours are inherently difficult to observe and quantify. For instance, ESG ratings exhibit substantial divergence across different data providers (Chatterji et al., 2016; Gibson Brandon et al., 2021; Christensen et al., 2022; Berg et al., 2022). Second, social and environmental performances are rarely interchangeable across organisations, complicating the assessment of aggregate policy effects. For example, if a policy reduces water pollution by firms in some regions but increases it in others, it is difficult to determine whether the net effect is positive. Third, many public policies are implemented at the national level, making causal inference difficult.

This paper primarily focuses on state-level GHG emissions reduction targets, a setting that offers three key advantages. First, corporate emissions reduction provides an objectively measurable dimension of private-sector prosocial behaviour. Second, emissions reductions are

perfectly substitutable: one unit of reduction by any firm has the same impact on the climate. This feature allows for assessing the aggregate policy effect even when some firms increase and others decrease emissions simultaneously. Third, these targets have been adopted by only a subset of U.S. states, creating a natural setting for applying a DID framework and facilitating causal inference.

To study private-sector voluntary motives, I examine emissions-related shareholder proposals. Shareholder proposals are formal recommendations submitted by shareholders to companies, urging specific actions that reflect their priorities and concerns. According to a survey by Krueger et al. (2020), institutional investors engage with portfolio firms on climate risks through three primary strategies: private discussions with management, submitting shareholder proposals, and voting on proposals. Because data on private discussions are proprietary, this paper focuses on the latter two channels to capture observable shareholder preferences on climate-related issues. Grewal et al. (2016) show that firms receiving sustainability-related proposals subsequently exhibit improvements in sustainability performance.

U.S. climate policy follows a multi-tiered structure comprising federal, regional, and state components. At the federal level, climate policy has undergone significant shifts across administrations. In contrast, regional and state-level policies tend to be more stable.

As of September 2023, twenty-six states have adopted economy-wide GHG reduction targets, typically specifying a percentage reduction by a certain year relative to a baseline year. These targets vary across states in terms of reduction percentage, baseline year, time frame, and legal status (established either through legislation or executive orders). The information on these targets is sourced from Korganbekova (2025) and cross-verified with original laws, mandates, and additional references (Shields, 2023; Center for Climate and Energy Solutions, 2023). A summary of these state-level targets is presented in Table A1.

Are these emissions reduction targets merely symbolic, or do they reflect substantive policy changes? To address this question, I compile state-level climate policy measures from

the State Climate Policy Dashboard (Climate XChange), focusing exclusively on measures that apply to firms and excluding those targeting households or the public sector. Panel A of Table 2 provides the names, brief descriptions, and classifications of all policy measures adopted by at least one state during the sample period, categorising each as either a regulatory requirement or a financial incentive. Panel B compares the number of policy measures enacted across target-adopting and control states before and after the 2019 treatment year. The results show that target-adopting states enacted significantly more policy measures after 2019, while control states exhibited little change. Therefore, the adoption of state-level emissions reduction targets is indeed accompanied by substantive policy actions designed to encourage firms to reduce emissions.

Despite the extrinsic incentives generated by these targets and associated policy instruments, states are generally not on track to meet their stated emissions goals. Four of the nine target-adopting states in my sample—Colorado, Illinois, Nevada, and Pennsylvania—have short-term targets for 2025. Based on the most recent available emissions data (final 2025 data are not yet available), none is projected to meet its 2025 target (Chase Woodruff, 2026; Amy Alonzo, 2026; RMI and Energy Innovation, 2026a,b).

This paper focuses on emissions reduction targets rather than specific policy measures for two reasons. First, unlike specific policy measures that typically apply to one or a few industries, these targets are economy-wide and allow for the evaluation of aggregate policy effects. Second, states set these targets prior to implementing specific measures to achieve them. Consequently, the private sector is likely to begin adjusting as soon as the targets are adopted, in anticipation of subsequent regulations and programs. Thus, the adoption of these targets represents the initial step in a broader sequence of policy actions and constitutes the appropriate intervention point for this study.

3. Conceptual Framework

I develop a simple conceptual framework to analyse how public policies affect firms' motives to reduce emissions. To my knowledge, no existing model explicitly addresses this question. I therefore draw on insights from models of individual prosocial behaviour that plausibly extend to the firm setting. Bénabou and Tirole (2006) classify individuals' motives for prosocial actions into three categories: extrinsic incentives, reputational concerns, and altruism. I apply the same classification to firms' abatement decisions. This taxonomy captures the main motives underlying corporate emissions reductions. I refer to reputational and altruistic motives jointly as "voluntary" motives, as they are not directly imposed by policy but instead arise from internal preferences or social pressure.

Consider a firm that chooses an abatement level a to maximise shareholder utility. The firm's objective function is:

$$W(a) = \underbrace{B_e(a)}_{\text{Extrinsic}} + \underbrace{B_r(a) + B_a(a)}_{\text{Voluntary}} - C(a), \tag{1}$$

where $B_e(a)$ denotes benefits from extrinsic incentives, such as subsidies earned or penalties avoided through abatement; $B_r(a)$ denotes reputational benefits, such as enhanced customer loyalty or easier access to capital; and $B_a(a)$ captures altruistic utility, such as "warm-glow" or utility derived from contributing to climate mitigation as a public good. The function $C(a)$ denotes the cost of abatement, including foregone profits and transition expenditures, as determined by the firm's production technology and available abatement technologies.

I assume that all marginal benefits are positive, $B_e'(a), B_r'(a), B_a'(a) > 0$, and that marginal abatement costs are also positive, $C'(a) > 0$. I further assume that benefit functions $B_e(a)$, $B_r(a)$, and $B_a(a)$ are all concave in a, while the cost function $C(a)$ is convex. These assumptions are standard and economically natural. Marginal reputational returns likely decline with abatement a: once a firm is already perceived as environmentally responsible, additional abatement yields only limited reputational gains. Marginal altruistic utility may

also diminish, as each additional unit of abatement generates a smaller increment in ethical satisfaction (Andreoni, 1990; Carpenter, 2021). By contrast, marginal abatement costs are likely to rise as firms exhaust low-cost opportunities first and face progressively more expensive reductions thereafter (Pindyck, 2007; Meng, 2017; Ramadorai and Zeni, 2024). The qualitative predictions of the model remain unchanged under weaker curvature assumptions.

The firm chooses a^* to maximise $W(a)$, which yields the first-order condition:

$$B'_e(a^*) \; + \; B'_r(a^*) \; + \; B'_a(a^*) \; = \; C'(a^*). \tag{2}$$

Intuitively, the firm chooses its abatement level such that the marginal benefit of an additional unit of emissions reduction, aggregated across all motive channels, equals its marginal cost. It follows that, all else equal, an increase in any component of marginal benefit (e.g., stronger extrinsic incentives induced by a policy shock) raises the optimal abatement level a^*, whereas a decrease lowers it.

I next consider how public policy affects these motives and, through them, equilibrium abatement. The direct effect of climate policy operates through extrinsic incentives. By attaching financial rewards or penalties to emissions outcomes, policy raises $B'_e(a)$, which in turn increases the optimal abatement level a^*.

The effect of policy on reputational motives follows the logic of Bénabou and Tirole (2006). In their model, extrinsic incentives may crowd out individuals' reputational motives by making voluntary actions less distinguishable from those driven by external rewards, whereas greater public attention may strengthen reputational motives. The same mechanisms are likely to operate in the context of climate policy and corporate emissions. On the one hand, extrinsic incentives may crowd out reputational motives: when cutting emissions yields monetary payoffs, it becomes less informative about a firm's underlying commitment to sustainability, weakening the reputational signal and lowering $B'_r(a)$ (the noise-to-signal channel). On the other hand, if policy heightens public scrutiny of corporate emissions, it may amplify reputational motives and raise $B'_r(a)$ (the publicity channel).

The interaction between public policy and altruistic motives can be analysed through the lens of Andreoni (1990), who models an impure altruist whose utility is quasi-concave in both private and aggregate contributions to a public good. In that framework, public provision can crowd out altruistic motives by reducing the perceived need for private contributions. Applying the same logic to the firm setting, if climate policy is expected to reduce aggregate emissions, then the marginal altruistic benefit from private corporate abatement $B'_a(a)$ declines (the replacement channel), discouraging emissions reduction.

Taken together, climate policy generates extrinsic incentives, may crowd out altruistic motives, and may either weaken or strengthen reputational motives. Figure 1 summarises these channels. The net effect on voluntary motives, $B'_r(a) + B'_a(a)$, is therefore theoretically ambiguous. As a result, the overall effect of policy on firms' optimal abatement choice a^* is also ambiguous ex ante and depends on the relative strength of the direct incentive effect and the indirect crowding-out or crowding-in effects. These interactions are ultimately empirical questions and may vary with policy design, policy stringency, and firm characteristics.

In addition, this framework extrapolates individual-level behavioural theories to firm-level decision-making. Whether this analogy holds in the data is itself an empirical question.

In this paper, I test whether the state-level emissions targets adopted in 2019 crowd out firms' voluntary motives and evaluate their effectiveness in reducing actual emissions.

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 full sample and this subsample are used in the analysis of shareholder proposals to ensure robustness.

I identify shareholder proposals that are related to GHG emissions and count their numbers. A proposal is classified as GHG emissions-related if its "resolution" (i.e., description) in the ISS Proposal database 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 emissions-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 emissions-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 with the variables *GHG Proposal Dummy*, *GHG Proposal Ratio*, and the number of emissions-related proposals. Then I match this sample with control variables constructed using data from the CCM and CRSP Monthly Stock databases.

4.2. Voting Results

The voting results on GHG emissions-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 emissions-related shareholder proposals. A proposal is classified as GHG emissions-related if its "AgendaGeneralDesc" (i.e., description) in the database 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.[4] Once a facility falls under these reporting requirements, it must continue reporting annually unless its emissions have fallen below 25,000 metric tons of CO_2e for five consecutive years or 15,000 metric tons of CO_2e for three consecutive years. These reporting requirements mitigate selection concerns commonly associated with emissions databases based on voluntary disclosures. In addition, the EPA prescribes standardised methodologies for calculating and reporting GHG emissions and verifies reported data through a multi-step process, ensuring that the data are accurate, consistent, and thus suitable for cross-facility and over-time comparisons.

This study focuses on direct emissions reported in the GHGRP database, corresponding

[4] CO_2e, or carbon dioxide equivalent, represents the amount of CO_2 that would produce the same global warming potential as one metric ton of a given greenhouse gas. A threshold of 25,000 metric tons of CO_2e corresponds approximately to the annual emissions from the energy use of 3,357 homes, 5,831 gasoline-powered passenger vehicles, or 0.7% of a typical coal-fired power plant, according to the EPA's Greenhouse Gas Equivalencies Calculator. In addition to the 25,000 metric tons of CO_2e threshold, GHGRP includes other conditions under which a facility is required to report its emissions, such as specific product categories or emissions sources. More details about the requirements can be found in U.S. Environmental Protection Agency Office of Atmospheric Protection (2024).

to Scope 1 emissions as defined by the World Resources Institute Greenhouse Gas Protocol. This scope of measurement aligns with the policy context of this paper, state-level emissions reduction targets and accompanying policy measures, which aim to reduce aggregate direct emissions within a state's boundaries. 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 emissions dataset with control variables constructed using data from the CCM and CRSP Monthly Stock databases.

For state-level analyses, I measure emissions in two ways. First, I aggregate facility-level emissions from GHGRP to the state level. While this approach leaves out most smaller facilities that emit less than 25,000 metric tons of CO_2e annually, the included facilities account for the majority of corporate emissions, making the dataset sufficiently representative for evaluating climate policies. Nonetheless, to address any concerns regarding the omission of smaller emitters, I also use an alternative data source: state-level energy-related CO_2 emissions 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.

The EIA database reports CO_2 emissions from both firms and households. Since this paper focuses on the interaction between public policy and corporate-sector prosocial behaviours, I exclude household emissions. The EIA's "sectoral specific emission tables by state" categorise 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. Other Variables

I proxy firms' ex ante climate performance using the Climate Strategy component of the S&P Global ESG Scores. The data provider annually evaluates the environmental, social, and governance practices of more than 10,000 publicly listed companies worldwide. The scores are constructed using questionnaires, companies' internal documents, and public information. To account for sectoral differences, firms are benchmarked against their sub-industry peers. For each firm, the data provider first assigns scores to more than 100 questions, then aggregates them to the criteria level, such as the *Score on Climate Strategy*, and subsequently to the ESG dimension level. In this analysis, I focus specifically on the *Score on Climate Strategy*, which captures aspects of firms' climate performance, such as climate-related targets, internal carbon pricing, low-carbon products, climate risk management, and climate risk assessment, among others. Scores range from 0 to 100, with higher values indicating stronger performance.

The stringency of state-level COVID-19 policies is proxied by the duration of lockdowns in days, sourced from Raifman et al. (2020). Specifically, I define *Lockdown1* and *Lockdown2* as the durations of general stay-at-home mandates and non-essential business closure mandates, respectively, calculated as the difference between their start and end dates. Control variables, including *Asset (ln)*, *Leverage*, *ROA*, and *MB*, are constructed using data from the 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). 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 analyse the likelihood of receiving emissions-related shareholder proposals. Voting results are examined using a proposal-level sample. Emissions are analysed at facility-year, firm-year, and state-year levels. The period during which all databases are available spans from 2010 to 2021.

The primary analyses focus on state-level emissions reduction targets, with the adoption timeline summarised in Table A1. Given the [t–3, t+2] event window and data availability from 2010 to 2021, only targets adopted between 2013 and 2019 are included. Eleven states meet this criterion: two adopted targets in 2014 and nine in 2019. Facilities in the two 2014-adopting states (Rhode Island and Delaware) account for a negligible share of the facilities in these eleven states (28 out of 1,242) and are excluded from the regressions because they are observed for only one year and would be absorbed by facility fixed effects.

I therefore focus on the 2019 adoptions, defining firms and facilities in these nine states as the treatment group and those in the twenty-five states that never adopted such targets as the control group. Observations from states that adopted targets in years other than 2019 are excluded to ensure a clean comparison. Descriptive statistics for the variables over the 2016–2021 event window are reported in Panel A of Table 1.

My samples for examining state-level targets include 2,057 firms, 4,763 facilities, and 281 emissions-related shareholder proposals, of which 100 are voted on. Table A3 categorises these 281 proposals based on their sponsors and content. On average, 2.16% of firms receive at least one emissions-related proposal each year. Although this represents a small proportion of firms, those that received at least one emissions-related proposal during the sample period are responsible for 66.4% of total emissions from all covered firms, indicating that shareholder engagement on emissions primarily targets large emitters. Therefore, shareholder proposals serve as a sufficiently representative measure of shareholder efforts to reduce corporate emissions. The average proportion of emissions-related shareholder propos-

als is 10.8%, the average voting support rate for these proposals is 31.4%, and the average facility emits 0.45 million metric tons of CO_2e per year.

The sample for the Paris Agreement analyses, reported in Table 9, spans 2013–2018, with 2016 designated as the event year. Descriptive statistics for this sample are presented in Panel B of Table 1.

5. Empirical Analyses and Results

5.1. Emissions-Related Proposals and Voting Results

In this section, I examine the impact of state-level targets on private-sector voluntary motives to reduce GHG emissions. I begin by analysing the likelihood of firms receiving emissions-related shareholder proposals and then examine the voting outcomes on these proposals.

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

$$Y_{it} = \beta_0 + \beta_1 Treated_{it} + \gamma X_{it} + d_i + d_t + \varepsilon_{it} \tag{3}$$

where Y_{it} represents *GHG Proposal Dummy*, an indicator equal to one if firm i receives at least one emissions-related proposal in year t and zero otherwise. $Treated_{it}$ is an indicator equal to one if the state in which firm i is headquartered has adopted a state-level emissions reduction target by year t, and zero otherwise. This variable is equivalent to the interaction between a treatment indicator and a post-adoption indicator, so β_1 captures the change in the likelihood of receiving emissions-related proposals for treated firms relative to control firms. X_{it} is a vector of one-year-lagged firm characteristics and performance, including abnormal returns, total assets, leverage, return on assets, and market-to-book ratio. These controls account for time-varying firm conditions that may affect shareholder proposal activity and mitigate concerns that contemporaneous changes in firms' operations or performance coincide with policy adoption. d_i denotes firm fixed effects, which absorb time-invariant firm characteristics such as political orientation or baseline emissions levels. d_t denotes year fixed

effects that capture common shocks across firms, such as federal-level climate policies or shifts in public attitudes toward environmental issues. 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 Equation (3), replacing the dependent variable Y_{it} with *GHG Proposal Ratio*, defined as the proportion of emissions-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 3. 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 emissions-related shareholder proposals after their headquarters states adopt an emissions reduction target. Considering that the average probability of receiving such a proposal across the full sample is 2.16%, the treatment effect of 0.81 percentage points (based on the specification of Column (2)) is economically significant. For robustness purposes, I also run the regressions on the subsample of firms with at least one recorded shareholder proposal in the ISS Proposal database, and the results are reported in Columns (3)–(4). Although the estimate is less statistically significant on this subsample, its magnitude is larger, corroborating that firms are less likely to receive emissions-related proposals after the adoption of state-level targets.

To assess whether the decrease is specific to emissions-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 emissions-related shareholder proposals relative to all shareholder proposals decreases by 5.44 percentage points after the treatment, which is both statistically and economically significant when compared to the sample mean of 10.77%. These findings indicate that state-level emissions-reduction targets disproportionately reduce the number of emissions-related shareholder proposals relative 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 around 2019 for each group separately. The following regression is estimated separately on the firm-year subsamples of the treatment group and the control group:

$$Y_{it} = \beta_0 + \beta_1 Post_{it} + \gamma X_{it} + d_i + \varepsilon_{it} \tag{4}$$

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 (3).

Panel B presents the results for the treatment group, revealing significant decreases in both the likelihood of receiving an emissions-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 emissions-related proposal and no significant change in the proportion of emissions-related proposals. Therefore, the pre-post comparisons suggest that the treated firms indeed become less likely to be targeted by emissions-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 present the dynamic treatment effects and corresponding 95% confidence intervals from difference-in-differences regressions of *GHG Proposal Dummy* and *GHG Proposal Ratio*. The estimates indicate significantly negative treatment effects for both measures. Moreover, the effects are dynamic and increase in magnitude over the three years post-treatment; for example, the estimated effect in year 3 exceeds those in years 1 and 2. One plausible interpretation is that emissions-reduction targets are typically followed by regulations and programs that are rolled out gradually in the years after adoption.

The figure also provides evidence consistent with the parallel trends assumption. There is no indication of differential pre-trends prior to treatment. If anything, the pre-treatment

coefficients display a slight upward pattern, in contrast to the downward trajectory observed post-treatment. These patterns suggest that the estimated effects of state-level targets are unlikely to be driven by pre-existing trends.

A potential concern is that climate policy adoption is endogenous to broader political shifts. For instance, progressive political changes could simultaneously increase the likelihood of policy adoption and influence shareholder activism. While such concerns are common in policy evaluation settings, they are less plausible here. If broader progressive shifts were driving both policy adoption and shareholder behaviour, one would expect an increase, rather than a decline, in emissions-related shareholder proposals and voting support. The opposite pattern observed in the data makes this explanation unlikely.

Another potential concern is that state GHG targets may have been proposed well before their formal adoption, allowing firms to begin preparatory actions during the policy process. To assess this possibility, I examined the earliest formal discussion points for these targets, such as bill introduction, first floor action, or committee hearings. In eight of the nine states, these signals first appeared only in 2019; the sole exception is Nevada, where the policy process can be traced back to November 2018. Given that very few firms hold shareholder annual meetings in December, 2019 can be safely treated as the treatment year without meaningful concern about anticipation bias.

I also conduct two robustness tests on the likelihood of receiving emissions-related shareholder proposals. First, some pension funds might directly implement government mandates, creating a potential link between their behaviour and climate policies. To address the concern that my results may be driven by this government-influence channel, I exclude proposals submitted by pension funds. Table IA1 shows that the findings remain robust.

Second, special-interest organisations and individuals may submit proposals reflecting idiosyncratic agendas that are not necessarily representative of broader shareholder preferences (Gantchev and Giannetti, 2021). To determine whether my results are driven by such behaviour or instead reflect the actions of more typical investors, I classify proposals into two

groups: those submitted by individual and special-interest investors, and those submitted by institutional investors. The results in Table A4 indicate that the crowding-out effect is predominantly driven by institutional investors, suggesting that the findings are not driven by "noisy" behaviour from individual or special-interest investors.

In addition to submitting proposals, shareholders can also exert influence on firms by voting on these proposals. Despite the decline in proposal frequency, it remains possible that shareholders increase engagement intensity through higher voting support. To assess this possibility, I examine voting outcomes for emissions-related shareholder proposals using the proposal-level sample, with the results reported in Table 4. Columns (1)–(2) present estimates from a DID analysis of voting outcomes, obtained by estimating Equation (3) with *Support Rate* as the outcome variable. The results show that voting support for emissions-related proposals declines significantly following policy adoption, consistent with the crowding-out of voluntary motives. According to the specification in Column (2), the average support rate for emissions-related shareholder proposals declines by 12.78 percentage points, which is economically significant relative to the sample mean of 31.38%. The estimated dynamic treatment effects and their 95% confidence intervals are presented in Panel (c) of Figure 2.

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

I conduct a series of additional tests. First, I examine the voting behaviour of different types of investors. In Table IA2, I interact the treatment dummy with *Socially Responsible Fund*, which indicates whether a mutual fund family is socially responsible based on fund names. I find that the state-level target adoption reduces voting support from both socially responsible mutual funds and other mutual funds, although the former exhibits a relatively

moderate change.

In Table IA3, I test whether my results are driven by differences in the stringency of state-level COVID-19 policies, which may be correlated with both state-level climate policies and shareholder engagement. I proxy the stringency of state-level COVID-19 policies using two alternative measures of lockdown duration, and the results remain robust after controlling for them. In addition, lockdown duration is significantly associated with neither the likelihood of receiving emissions-related shareholder proposals nor the support rate for such proposals, consistent with the notion that my findings are driven by climate policies rather than COVID-19 policies.

In Table A5, I examine whether state-level emissions reduction targets have spillover effects on shareholder proposals related to other environmental and social issues. I find no change in environmental proposals unrelated to emissions, whereas the proportion of proposals related to social issues increases significantly. This pattern is consistent with shareholders either reallocating their attention toward areas with greater perceived demand, such as social issues (if the crowded-out climate-related motives are primarily altruistic), or shifting toward social proposals as an alternative way to signal their CSR commitments (if the crowded-out motives are primarily reputational).

To address the concern that some firms have facilities located in states different from their headquarters state and are thus exposed to different local climate policies, I repeat the analyses after restricting the sample to firms whose high-emitting facilities are located exclusively in their headquarters states. A facility is classified as high-emitting if it emits more than 25,000 metric tons of CO_2e annually, namely the threshold for inclusion in the GHGRP database. The results remain qualitatively robust, as shown in Table IA4.

Finally, I examine whether the decline in proposal activity reflects some climate proposals becoming redundant due to overlap with policy measures. For example, both policies and proposals could, in principle, call for emissions disclosure. I manually review all policy measures and proposals in my sample and find no overlap in the issues they address. Panel

A of Table 2 lists all policy measures adopted by at least one state during the sample period. These measures fall into four broad categories: subsidies and fees, technical assistance, financing support, and industrial standards, all of which require government implementation rather than unilateral firm action. Panel B of Table A3 categorises the proposals in my sample by content, covering climate reporting, firm-level targets, risk management, lobbying, and governance. The absence of overlap suggests that the results are not driven by a mechanical substitution channel, but instead are consistent with a crowding-out of voluntary motives.

Overall, the results indicate that state-level emissions reduction targets crowd out private-sector voluntary motives for reducing corporate emissions.

5.2. Emissions

Given the crowding-out effect of state-level emissions reduction targets on private-sector voluntary motives, the policy's overall effectiveness in reducing corporate emissions is ex ante ambiguous. The net effect depends on the relative strength of the direct policy effect, operating through extrinsic incentives, and the offsetting reduction in voluntary motives. To assess this net effect, I conduct a difference-in-differences analysis of facility-level GHG emissions using Equation (3), replacing the dependent variable with emissions and firm fixed effects with facility fixed effects. By construction, the DID estimator captures changes in emissions at treated facilities relative to those at control facilities. Untabulated results show that emissions decline over time in both groups, implying that the estimated coefficients capture differences in the rate of emissions reduction.

Table 5 reports the regression results. Column (1) uses the full sample, while Columns (2)–(3) restrict the sample to a balanced panel of facilities observed in all years of the sample period. Column (3) additionally controls for parent-firm characteristics (available only for facilities owned by publicly listed firms). Across all specifications, the estimated coefficients on $Treated$ are statistically insignificant and positive, indicating no evidence that state-level targets lead to emissions reductions.

For robustness, I also conduct DID analyses of emissions at the state level. The two measures of state-level corporate emissions, *Emissions GHGRP* and *Emissions EIA*, are regressed on $Treated$, state-level GDP, year fixed effects, and state fixed effects. The results, reported in Table 6, again show an insignificantly positive treatment effect, further supporting the conclusion that state-level emissions reduction targets do not lead to corporate emissions reductions.

Panels (a) and (b) in Figure 3 plot the estimated dynamic treatment effects by year and their 95% confidence intervals from the DID regressions of facility-level emissions, while Panels (c) and (d) present the corresponding estimates for state-level emissions. The parallel trends assumption appears satisfied in all cases, as there is no evidence of a pre-existing trend—particularly no upward trend—before treatment. Consistent with policy ineffectiveness, none of the four panels shows evidence of greater emissions reductions after policy adoption. If anything, emissions in treated states appear to decline slightly more slowly than in control states following policy adoption, although these differences are statistically insignificant.

In the analyses above, I include facilities owned by both public and private parent firms to assess the overall policy impact on corporate emissions. To examine whether the results are robust for the subset of facilities affiliated with public parent firms that also appear in the proposal analyses, Table IA5 reports estimates from a restricted sample of facilities owned by publicly listed parent firms appearing in Columns (1)–(2) of Table 3. The estimated effects in this restricted sample are qualitatively similar to the baseline results and show no evidence of emissions reductions following policy adoption.

I primarily examine emissions at the facility rather than the firm level for two reasons. First, most operational climate policies, such as emissions regulations and transition subsidies, are applied based on the location of emitting facilities.[5] Second, facility-level GHGRP

[5]A smaller number of climate policies, such as corporate climate disclosure requirements, are based on the location of a firm's headquarters or whether the firm conducts business in a particular state.

data are free from voluntary reporting bias and methodological differences across entities, whereas firm-level emissions data are typically voluntarily disclosed. As a robustness check, I also examine the policy impact on firm-level emissions using Trucost data, defining a firm's state by the location of its headquarters. As reported in Table IA6, the results are consistent with the facility-level analysis, indicating that the policies do not lead to reductions in corporate emissions. In additional untabulated tests, I (i) redefine a firm's state as the state in which it generates the largest share of its emissions and (ii) measure firm-level emissions by aggregating facility-level emissions from the GHGRP. The results remain robust under both alternative specifications.

I next examine heterogeneity in the policy effect. As the conceptual framework suggests, the effect of state-level targets reflects two opposing forces—their direct impact and the crowding-out of voluntary motives—so the net effect is expected to vary across facilities depending on their pre-policy voluntary motives. Facilities with stronger pre-policy voluntary motives should experience greater crowding-out, resulting in weaker or even counterproductive net policy effects. The evidence in Table 7, although not uniformly statistically significant, is broadly consistent with this prediction.

In Panel A, I categorise facilities owned by public parent firms into two groups based on pre-policy shareholder pressure, measured by the number of GHG emissions-related proposals their parent firms received over the three years preceding 2019. Consistent with the notion that facilities with greater pre-policy voluntary motives experience stronger crowding-out, facilities whose parent firms received no such proposals before 2019 achieve larger emissions reductions after treatment than control facilities, whereas those whose parent firms received at least one proposal achieve smaller reductions than the control group. Notably, the average emissions level of the latter group is about three times higher than that of the former, suggesting that shareholder engagement on emissions-related issues is concentrated among high emitters.

In Panel B, I compare facilities with higher versus lower pre-policy climate performance,

measured by *Score on Climate Strategy* from the S&P Global ESG Scores. The results show that facilities whose parent firms have higher pre-policy scores achieve smaller emissions reductions than control facilities. This finding further supports the notion that firms with stronger pre-policy voluntary motives are more exposed to crowding-out. Overall, these heterogeneity tests illustrate that the net effect of climate policy reflects the balance between its direct effect and the crowding-out of voluntary motives.

Collectively, these findings suggest that although state-level targets may accelerate emissions reductions among certain facilities, there is no evidence of an overall decline in aggregate emissions, indicating that the the policy goal is not achieved. Furthermore, the policy may have adverse welfare implications. The conceptual framework implies that, in the absence of policy incentives, firms that derive higher benefits from emissions reductions or face lower underlying abatement costs optimally undertake more abatement. The introduction of climate policies may reallocate abatement away from these firms toward others with weaker voluntary motives. If such firms derive lower benefits from emissions reductions or face higher underlying abatement costs, this reallocation may be inefficient and reduce overall social welfare.

5.3. Green Transition Mentions during Earnings Calls

Thus far, I proxy private-sector voluntary motives using the likelihood that firms receive emissions-related shareholder proposals and the level of voting support for those proposals, and I proxy prosocial behaviour using firms' realised emissions reductions. Two concerns remain. First, shareholders may communicate with management through channels other than submitting and voting on proposals, and state-level climate policies may affect these alternative channels in different ways. Second, although I observe no change in corporate emissions in treated states relative to control states, it remains possible that firms in treated states have undertaken emissions-reduction actions whose effects take longer than three years (the length of my post-treatment observation window) to materialise in observable emissions.

To address both concerns, I adopt alternative measures of private-sector voluntary mo-

tives and prosocial behaviour. Li et al. (2024) construct corporate climate risk measures based on textual analyses of earnings call transcripts. One particularly relevant measure for my analysis is *transition climate risk*, defined as the frequency of mentions related to climate "policies and regulations, technological improvements, and evolving climate patterns", scaled by transcript length and then multiplied by 10,000. This measure captures any discussion related to climate policies and green transitions.[6] Moreover, the authors have extended their dataset to 2023, thereby covering the entire period of my analysis.

Li et al. (2024) further decompose this transition measure into proactive and non-proactive components based on whether proactive verbs appear within ±1 sentence.[7] These decomposed measures fit my context particularly well: non-proactive mentions correspond to private-sector motives, while proactive mentions correspond to private-sector behaviour. Because the proactive measure captures any transition-related actions or plans discussed by management or investors during earnings calls in real time, it addresses the concern that actual emissions may not capture prosocial behaviour due to delays in observable outcomes.

I conduct difference-in-differences analyses using each of the two frequency measures, *Frequency(Nonproactive)* and *Frequency(Proactive)*, as the dependent variable. The results are presented in Table 8. Columns (1)–(2) show that the frequency of non-proactive mentions significantly decreases following policy adoption, relative to the control group. The magnitude of this decline is economically substantial, representing approximately one-quarter of the sample average of 0.3366. Columns (3)–(4) show a post-adoption decline in the frequency of proactive mentions as well, though the decline is not statistically significant. Together, these findings indicate that state-level climate targets crowd out private-sector motives to reduce emissions and fail to effectively promote corporate climate-transition behaviour, re-

[6]Although the authors name this measure as transition climate "risk", it is constructed solely from mentions of transition-related bigrams, regardless of the presence of a risk synonym nearby. Examples of bigrams in their dictionary include: carbon emissions, carbon credits, carbon neutral, clean energy, climate bill, climate goals, emissions reduction, global emissions, wind energy, etc.

[7]Examples of proactive verbs include: announce, build, change, create, develop, enhance, improve, increase, initiate, reduce, replace, respond, etc.

inforcing the conclusions drawn earlier from shareholder proposals and actual emissions.

5.4. The Impacts of Paris Agreement

Public policies are not intrinsically ineffective. The conceptual framework developed above suggests that a policy's effectiveness depends on the strength of its extrinsic incentives and its interaction with private-sector voluntary motives. A policy is likely to be effective if its extrinsic incentives outweigh any crowding-out of voluntary motives, or if it instead generates a crowding-in effect. To provide complementary evidence, I examine changes in private-sector motives and corporate emissions associated with the Paris Agreement, signed in 2016. As the Agreement applies economy-wide, a DID design is infeasible for causal identification. I therefore conduct pre–post comparisons of the three years following the Agreement's signing (2016–2018) with the three years preceding it (2013–2015) for suggestive evidence.

First, I examine the changes in private-sector voluntary motives for emissions reduction. As shown in Panel A of Table 9, the likelihood of firms receiving emissions-related shareholder proposals in a given year increases by 1.08 percentage points. Furthermore, the proportion of emissions-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 is consistent with the notion that the Paris Agreement crowds in private-sector voluntary motives via the publicity channel. Given that the Paris Agreement is a landmark accord bringing together nearly all nations for the first time to combat climate change, it is unsurprising that the Paris Agreement raises firms' awareness of climate issues and their voluntary motives for reducing corporate emissions.

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

tenth of the sample mean of 0.42 million metric tons.

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

6. Conclusion

Using state-level GHG emissions-reduction targets as the empirical setting, this paper examines the relationship between public policy and private-sector voluntary motives and prosocial behaviour. I find that the policy crowds out private-sector voluntary motives. Moreover, there is no evidence that the policy achieves its intended effect of reducing corporate emissions.

Unlike much of the existing literature that evaluates the effectiveness of specific policies, this paper focuses on the interaction between public and private efforts to promote social welfare. It shows that public policies can crowd out private-sector motives in some cases and cautions that intervention may be ineffective or even counterproductive when its direct effects are weak relative to its unintended side effects. This concern is particularly relevant because public policies are often shaped by political bargaining and compromise: policies may be implemented in weaker forms than initially proposed, while their crowding-out effects remain substantial. As a result, the commonly invoked logic that "every little bit helps" does not always hold in the context of public intervention.[8] Policymakers may therefore need to account for potential crowding-out effects when determining the appropriate stringency of policy measures.

This paper opens several avenues for future research. Extending the analysis of pub-

[8]Similarly, experimental evidence on individuals suggests that the effect of monetary incentives is not monotonic and that weak incentives may lead to worse performance than no incentives at all. See, in particular, Gneezy and Rustichini (2000a,b); see also Ariely et al. (2009) and Gneezy et al. (2011) for related evidence and a review.

lic–private interactions beyond GHG emissions reduction to other common goods may yield further insights for policy design. In addition, disentangling whether the crowding-out effect of state-level climate policies operates primarily through reputational or altruistic channels remains an open question. Furthermore, the conceptual framework developed here may also prove useful for understanding related phenomena, including the recent backlash against ESG. Although these questions pose empirical challenges, they offer promising opportunities for future work.

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Figures

Figure 1

Firms' Motives for Emissions Reduction

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

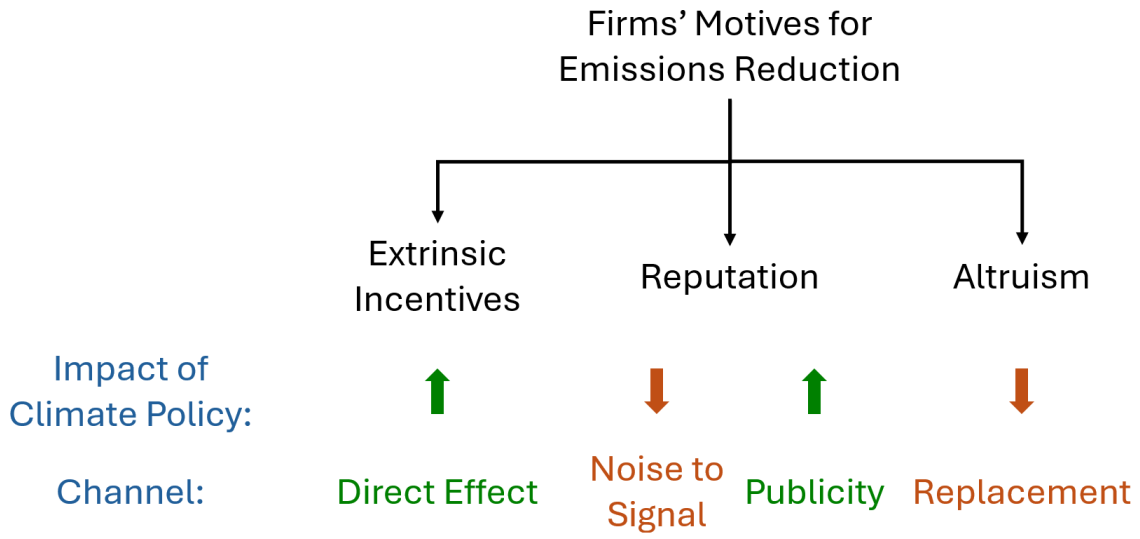
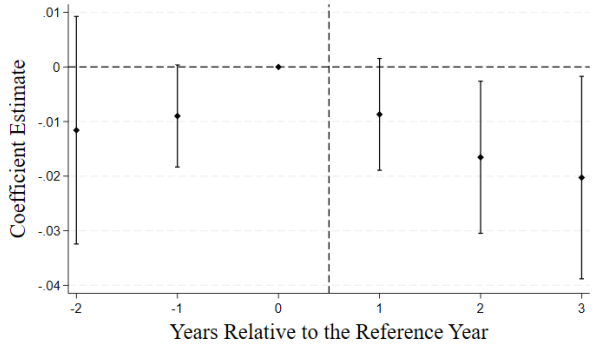


Figure 2

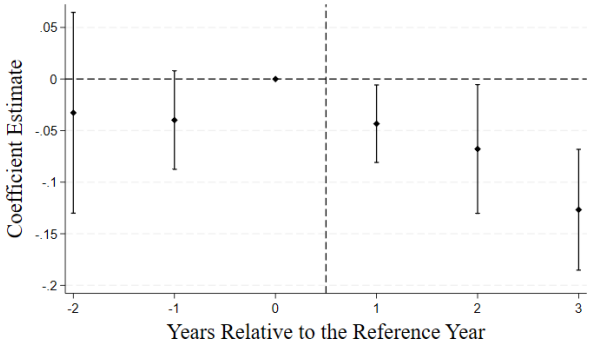
The Likelihood of Receiving Emissions-Related Proposals and Support Rates

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 emissions-related shareholder proposals in a given year, and 0 otherwise. The *GHG Proposal Ratio* represents the proportion of GHG emissions-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 emissions-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

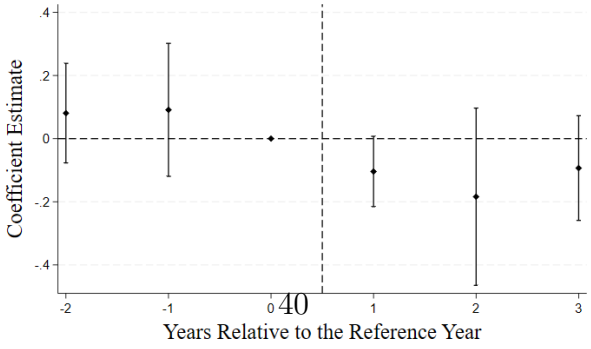


Figure 3

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.

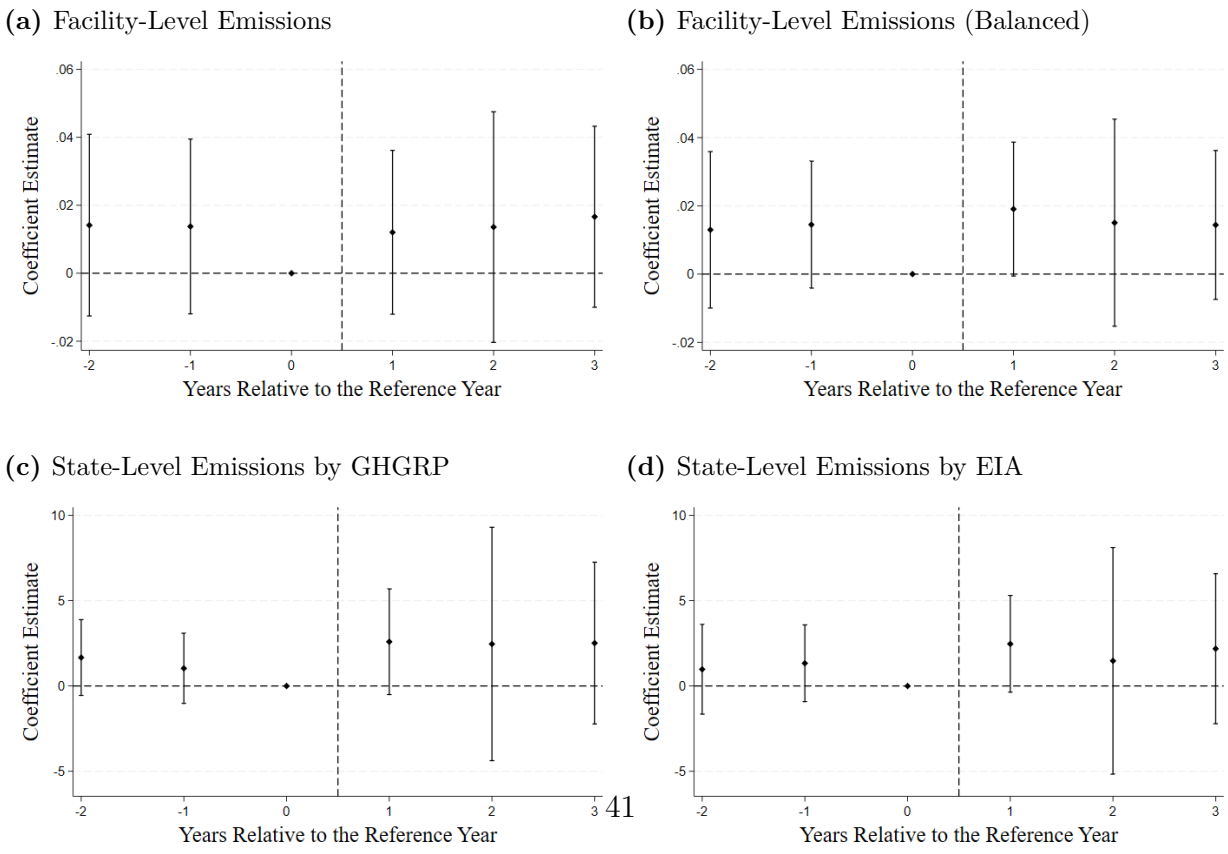
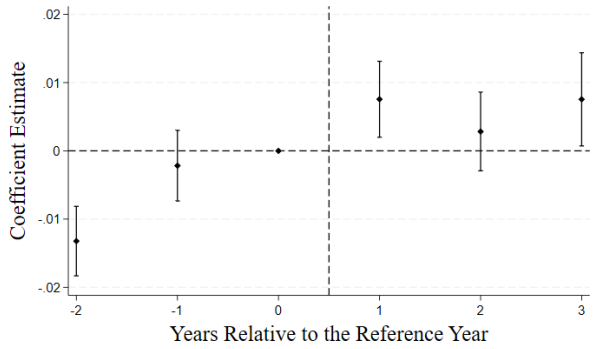


Figure 4

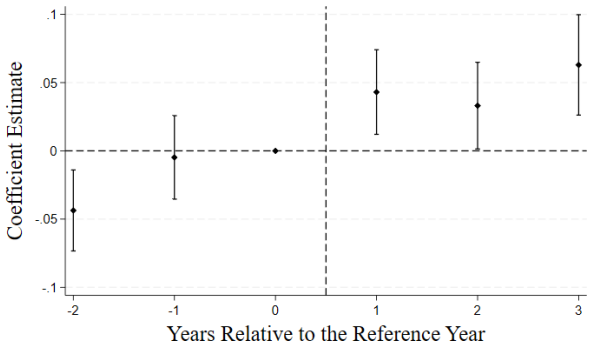
Pre-Post Comparisons Around the Paris Agreement

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 Emissions*, respectively.

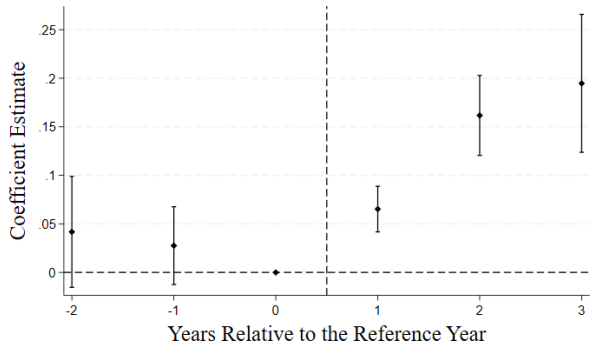
(a) GHG Proposal Dummy



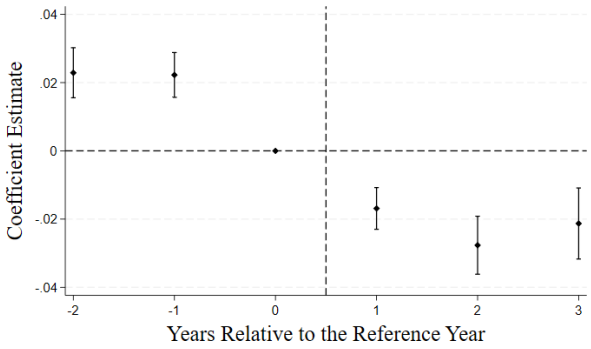
(b) GHG Proposal Ratio



(c) Support Rate



(d) Facility-Level Emissions



Tables

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 analysing the impacts of state-level emissions 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
Measures of Voluntary Motives and Behaviour											
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	0.4525	0.06846	1.3576	0.03359	0.2013	0	21	6.0894	26637	Facility-Year	Million $MTCO_2e$
Emissions GHGRP	59.1566	41.08	66.2584	26.99	75.57	0.2288	389.7	3.3567	204	State-Year	Million $MTCO_2e$
Emissions EIA	63.4879	47.26	74.5430	29.19	73.65	0.8974	450.4	3.7194	204	State-Year	Million $MTCO_2e$
Firm Emissions	1.1904	0.01976	7.2133	0.003629	0.1218	1.910e-07	296	16.3689	7754	Firm-Year	Million $MTCO_2e$
Frequency(Proactive)	0.2665	0	0.8595	0	0	0	13.9	6.2566	5308	Firm-Year	Frequency (×10,000)
Frequency(Non-Proactive)	0.3366	0	0.9460	0	0.2153	0	15.15	5.7672	5308	Firm-Year	Frequency (×10,000)
Vote for	0.4695	0	0.4991	0	1	0	1	0.1224	7726	Fund Family-Proposal	Dummy
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 $)

Continued on next page

Table 1 continued

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 $
#(Proposals)	1.6898	0	2.6158	0	3	0	11	1.4577	8268	Firm	Count
Score on Climate Strategy	64.9703	65	25.8214	41	90	0	100	-0.1450	6136	Firm	Score
Lockdown1	8.2658	0	21.7858	0	0	0	251	2.9841	10130	State	Days
Lockdown2	7.8047	0	19.4718	0	0	0	78	2.5145	10188	State	Days

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

	Mean	Median	SD	25th Perc.	75th Perc.	Min	Max	Skewness	Observations	Level	Unit
Measures of Voluntary Motives and Behaviour											
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	0.4229	0.06263	1.3490	0.03109	0.1766	0	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 Adoption of Specific Policy Measures

This table presents specific climate policy measures adopted before and after 2019, the year treatment states adopted emissions reduction targets. Panel A lists the names, brief descriptions, and classifications (regulatory requirements or financial incentives) of climate policies adopted by at least one U.S. state between 2016 and 2021. Policy details and adoption years are sourced from the State Climate Policy Dashboard. Policies targeting households and public sectors, rather than firms, are excluded. Each policy is classified as either a regulatory requirement or a financial incentive. Panel B summarises policy adoption among target-adopting and control states before and after the 2019 treatment year.

Panel A: Climate policy descriptions and classification

Policy Name	Description	Type
Agriculture Financial Incentives	Financial incentives to support healthy soils and regenerative agriculture	Financial Incentive
Agriculture Technical Assistance Programs	State assistance and mentorship to help farmers secure funding and adopt soil health practices	Financial Incentive
Carbon Pollution Pricing	Fees or cap-and-trade programs on GHG emissions	Financial Incentive
Coal Plant Securitisation	Debt refinancing to retire coal plants early without raising rates	Financial Incentive
Electric Vehicle Rebates	Rebates to make light-duty electric vehicles more affordable	Financial Incentive
Green Banks	Public or non-profit entities that invest in climate solutions and attract private investment	Financial Incentive
Just Transition Funds	Funds to help workers and communities transition away from fossil fuel industries	Financial Incentive
Property Assessed Clean Energy (PACE)	Loans tied to property that fund energy efficiency upgrades	Financial Incentive
Social Cost of Carbon	Monetary estimate of the damage of each ton of GHGs emitted	Financial Incentive
Appliance Standards	Minimum energy and water conservation requirements for appliances and equipment	Regulatory Requirement
Building Performance Standards	Targets for existing buildings to improve energy efficiency and lower emissions over time	Regulatory Requirement
Buy Clean Requirements	Mandated use of low-carbon construction materials in public projects	Regulatory Requirement
Clean Heat Standards	Requirements for heat providers to deliver an increasing share of low-emission heating services	Regulatory Requirement
Commercial Energy Codes	Codes that dictate the energy performance requirements of new commercial buildings	Regulatory Requirement
Energy Efficiency Resource Standards (EERS)	Targets and deadlines for utilities to cut electricity demand via efficiency programs	Regulatory Requirement
Energy Storage Targets	Procurement targets for energy storage capacity by set deadlines	Regulatory Requirement
Food Waste Bans and Targets	Bans on large generators from landfilling food waste and targets to reduce food waste	Regulatory Requirement

Continued on next page

Table 2 continued

Hydrofluorocarbon (HFC) Regulations	Regulations to reduce HFC usage and emissions	Regulatory Requirement
Low-Emission Vehicle (LEV) Standards	Standards to control tailpipe emissions of criteria pollutants and GHGs from light-duty vehicles	Regulatory Requirement
Medium- and Heavy-Duty (MHD) Zero-Emission Vehicle (ZEV) Mandates	Requirements for automakers to meet sales quotas for zero-emission medium- and heavy-duty vehicles	Regulatory Requirement
Oil and Gas Methane Regulations	Regulations reducing methane emissions from oil and gas production	Regulatory Requirement
Stretch Building Energy Codes	Optional stricter energy-efficiency codes for new buildings that local jurisdictions can adopt	Regulatory Requirement
Sulfur Hexafluoride (SF6) Regulations	Phasedown commitments, reporting requirements, and bans to reduce SF6 use and emissions	Regulatory Requirement
Zero-Emission Vehicle (ZEV) Mandates	Requirements for automakers to meet sales quotas for zero-emission light-duty vehicles	Regulatory Requirement

Panel B: State adoption before vs. after 2019

	Target-Adopting States		Control States	
Policy Name	Enacted 2016-2018	Enacted 2019-2021	Enacted 2016-2018	Enacted 2019-2021
Agriculture Financial Incentives		CO, ME, NM, NY, PA		NH, TX, UT
Agriculture Technical Assistance Programs		CO, ME, PA	OK	AZ, NH, TX, UT
Carbon Pollution Pricing		PA		
Coal Plant Securitization		CO, MT, NM		IN, MO
Electric Vehicle Rebates		IL		
Green Banks	CO, NV	IL, ME		
Just Transition Funds		CO, NM		
Property Assessed Clean Energy (PACE)	IL, NV, PA	ME, MT	AK, NE	TN
Social Cost of Carbon		NV, CO, NY		
Appliance Standards		CO, ME, NV		
Building Performance Standards		CO		
Buy Clean Requirements		CO, NY		
Clean Heat Standards		CO		
Commercial Energy Codes	NY, WI	ME, NV, PA	TN, TX	GA, ID, NE, WV

Continued on next page

Table 2 continued

Energy Efficiency Resource Standards (EERS)	CO, NV, NY	ME, NM	MO
Energy Storage Targets		IL, ME, NV, NY	
Food Waste Bans and Targets		NY	
Hydrofluorocarbon (HFC) Regulations		CO, ME	
Low-Emission Vehicle (LEV) Standards		NV	
Medium- and Heavy-Duty (MHD)		NY	
Zero-Emission Vehicle (ZEV) Mandates			
Oil and Gas Methane Regulations		CO, NM	
Stretch Building Energy Codes		IL, ME, NY	
Sulfur Hexafluoride (SF6) Regulations		CO	
Zero-Emission Vehicle (ZEV) Mandates		NV	

Table 3
The Likelihood of Receiving GHG Emissions-Related Proposals

This table presents regression estimates of how a firm's likelihood of receiving GHG emissions-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 Column (1) through 4, the dependent variable *GHG Proposal Dummy* equals 1 if a firm receives one or more GHG emissions-related shareholder proposals in a given year, and 0 otherwise. Columns (1)–(2) use the full sample, while Columns (3)–(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)–(6), *GHG Proposal Ratio*, represents the proportion of GHG emissions-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. The reported number of observations excludes singleton observations. 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)

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Table 3 continued

Dep. Var. =	*GHG Proposal Dummy*				*GHG Proposal Ratio*	
	(1)	(2)	(3)	(4)	(5)	(6)
Asset_{t-1}		0.0082**		0.0434**		-0.0087
		(0.00)		(0.02)		(0.04)
Leverage_{t-1}		0.0061		0.0381		-0.0284
		(0.01)		(0.06)		(0.10)
ROA_{t-1}		-0.0002**		0.0027		-0.1409
		(0.00)		(0.05)		(0.11)
MB_{t-1}		0.0000***		0.0040**		-0.0011
		(0.00)		(0.00)		(0.01)
Observations	10274	10274	3529	3529	1140	1140
Adj. R^2	0.2493	0.2493	0.2381	0.2393	0.4313	0.4298
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full Sample		$\#(\text{Proposals})_i \geq 1$		$\#(\text{Proposals})_{it} \geq 1$	
Panel B: Pre-post comparison within the treatment group around 2019						
Post	-0.0108***	-0.0110***	-0.0276***	-0.0292***	-0.0371**	-0.0318***
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	4083	4083	1464	1464	473	473
Adj. R^2	0.2345	0.2334	0.2334	0.2319	0.5172	0.5156
Controls	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full Sample		$\#(\text{Proposals})_i \geq 1$		$\#(\text{Proposals})_{it} \geq 1$	
Panel C: Pre-post comparison within the control group around 2019						
Post	-0.0026	-0.0052	-0.0070	-0.0209*	0.0160	0.0148
	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)	(0.01)

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Table 3 continued

Dep. Var. =	*GHG Proposal Dummy*				*GHG Proposal Ratio*	
	(1)	(2)	(3)	(4)	(5)	(6)
Observations	6191	6191	2065	2065	667	667
Adj. R^2	0.2558	0.2560	0.2382	0.2410	0.3793	0.3752
Controls	No	Yes	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full Sample		$\#(\text{Proposals})_i \geq 1$		$\#(\text{Proposals})_{it} \geq 1$	

Table 4
Voting Results on GHG Emissions-Related Proposals

This table presents regression estimates of how the voting results on a firm's GHG emissions-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)–(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 headquarters 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. The reported number of observations excludes singleton observations. 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)

Continued on next page

Table 4 continued

Dep. Var. =	*Support Rate*					
	(1)	(2)	(3)	(4)	(5)	(6)
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 5
Facility-level GHG Emissions

This table presents regression estimates of 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 *Emissions*. 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. Column (1) includes 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. The reported number of observations excludes singleton observations. All control variables are defined in Table A2.

	(1)	(2)	(3)
Dep. Var. =		*Emissions*	
Treated	0.0049	0.0070	0.0381
	(0.01)	(0.01)	(0.03)
AR_{t-1}			-0.0014
			(0.01)
$Asset_{t-1}$			0.0230*
			(0.01)
$Leverage_{t-1}$			0.0974
			(0.06)
ROA_{t-1}			-0.1707*
			(0.09)
MB_{t-1}			0.0000
			(0.00)
Observations	26637	23718	8300
Adj. R^2	0.9589	0.9674	0.9662

Continued on next page

Table 5 continued

	(1)	(2)	(3)
Year FE	Yes	Yes	Yes
Facility FE	Yes	Yes	Yes
Balanced	No	Yes	Yes

Table 6
State-Level GHG Emissions

This table presents difference-in-differences regression estimates of 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)–(2) is *Emissions GHGRP*, calculated by aggregating facility-level emissions from the Greenhouse Gas Reporting Program (GHGRP) of the US EPA. In Columns (3)–(4), the dependent variable is *Emissions 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 reported number of observations excludes singleton observations. The control variable is defined in Table A2.

	(1)	(2)	(3)	(4)
Dep. Var. =	*Emissions GHGRP*		*Emissions 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 reports heterogeneity tests examining how ex-ante voluntary motives influence the effect of state-level GHG emissions reduction targets on facility-level 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 in both panels is *Emissions*. 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 publicly listed parent firms are grouped based on *#(Proposals)*, the total number of GHG emissions-related shareholder proposals received by the parent firm during the three years preceding policy adoption (2016–2018). Columns (1) and (2) report results for facilities whose parent firms received zero and at least one such proposal, respectively. This panel also reports group means and standard deviations of *Emissions* and *#(Proposals)*. In Panel B, facilities are grouped according to whether their parent firm's pre-policy climate performance falls below or above the sub-industry median. Climate performance is proxied by the S&P Global ESG Score on climate strategy in 2018. This panel also reports group means and standard deviations of *Emissions* and *Score on Climate Strategy*. 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 reported number of observations excludes singleton observations.

Panel A: Facilities of publicly listed parent firms categorised by the number of GHG emissions-related proposals

Dep. Var. =	*Emissions*	
Group =	*#(Proposals)=0*	*#(Proposals)*≥ 1
	(1)	(2)
Treated	-0.0353*	0.0945
	(0.02)	(0.06)
Observations	5017	3229
Adj. R^2	0.9589	0.9636
Year FE	Yes	Yes
Facility FE	Yes	Yes
Mean(*#(Proposals)*)	0	4.3267

Continued on next page

Table 7 continued

SD(*#(Proposals)*)	(0)	(2.47)
Mean(*Emissions*)	0.2630	1.0041
SD(*Emissions*)	(0.84)	(2.41)

Panel B: Facilities of publicly listed parent firms categorised by performance on climate strategy

Dep. Var. =	*Emissions*	
Group =	*Score on Climate Strategy Below Median*	*Score on Climate Strategy Above Median*
	(1)	(2)
Treated	0.0172	0.0748*
	(0.04)	(0.04)
Observations	2386	3738
Adj. R^2	0.9773	0.9615
Year FE	Yes	Yes
Facility FE	Yes	Yes
Mean(*Score*)	41.3887	80.0674
SD(*Score*)	(14.87)	(19.19)
Mean(*Emissions*)	0.5561	0.6841
SD(*Emissions*)	(1.50)	(2.06)

Table 8
Green Transition Mentions during Earnings Calls

This table presents regression estimates of how the adoption of state-level GHG emissions reduction targets influences mentions of green transitions during firms' earnings calls. 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 frequency measures are borrowed from Li et al. (2024), who count the mentions of bigrams associated with climate "policies and regulations, technological improvements, and evolving climate patterns" during earnings calls. Each mention is classified as either *non-proactive* or *proactive*, depending on whether there are proactive verbs nearby (within ±1 sentence). This table presents the results from the difference-in-differences estimation over the 2016–2021 period. The dependent variable in Columns (1)–(2), *Frequency(Non-Proactive)*, is the frequency of non-proactive mentions. The dependent variable in Columns (3)–(4), *Frequency(Proactive)*, is the frequency of proactive mentions. 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. 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 reported number of observations excludes singleton observations. All control variables are defined in Table A2.

	(1)	(2)	(3)	(4)
Dep. Var. =	*Frequency(Non-Proactive)*		*Frequency(Proactive)*	
Treated	-0.0827**	-0.0828**	-0.0842	-0.0845
	(0.04)	(0.04)	(0.06)	(0.06)
AR_{t-1}		0.0051		0.0272
		(0.02)		(0.02)
$Asset_{t-1}$		-0.0010		-0.1044*
		(0.03)		(0.06)
$Leverage_{t-1}$		-0.0695		-0.0189
		(0.06)		(0.06)
ROA_{t-1}		-0.0184		-0.0330
		(0.02)		(0.08)

Continued on next page

Table 8 continued

	(1)	(2)	(3)	(4)
MB_{t-1}		-0.0008		-0.0004
		(0.01)		(0.01)
Observations	5308	5308	5308	5308
Adj. R^2	0.6720	0.6717	0.4610	0.4617
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes

Table 9
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)–(4), *GHG Proposal Dummy*, equals 1 if a firm receives one or more GHG emissions-related shareholder proposals in a given year and 0 otherwise. Columns (1)–(2) use the full sample, while Columns (3)–(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)–(6), *GHG Proposal Ratio*, represents the proportion of GHG emissions-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 emissions-related shareholder proposals and facility-level *Emissions*, respectively. As noted in the table, some specifications include untabulated control variables, consistent with those in Panel A of Table 3. 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 reported number of observations excludes singleton observations. All control variables are defined in Table A2.

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

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

Continued on next page

Table 9 continued

Panel B: Voting results on GHG emissions-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. =	*Emissions*		
	(1)	(2)	(3)
Post	-0.0367***	-0.0380***	-0.0640***
	(0.00)	(0.00)	(0.01)
Observations	40866	34890	9851
Adj. R^2	0.9630	0.9646	0.9704
Controls	No	No	Yes
Firm FE	Yes	Yes	Yes
Balanced	No	Yes	Yes

Appendix

Table A1
State-Level GHG Emissions Reduction Targets

This table summarizes state-level GHG emissions reduction targets, primarily sourced from Korganbekova (2025) 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

Continued on next page

Table A1 continued

8	Massachusetts	MA	2008	Global Warming Solutions Act (Chapter 298)	Statute	10-25% below 1990 levels by 2020, 80% below 1990 levels by 2050
9	Maryland	MD	2009	Greenhouse Gas Reduction Act (Chapter 171)	Statute	25% below 2006 levels by 2020, up to 90% below 2006 levels by 2050
10	Delaware	DE	2014	Preparing Delaware for Emerging Climate Impacts and Seizing Economic Opportunities from Reducing Emissions (Executive Order 41)	Executive Order	30% below 2008 levels by 2030
11	Rhode Island	RI	2014	Resilient Rhode Island Act (2014-H 7904)	Statute	10% below 1990 levels by 2020, 45% below 1990 levels by 2035, 80% below 1990 levels by 2050
12	Colorado	CO	2019	Climate Action Plan to Reduce Pollution (House Bill 19-1261)	Statute	26% below 2005 levels by 2025, 50% below 2005 levels by 2030, 90% below 2005 levels by 2050
13	Illinois	IL	2019	Joining the US Climate Alliance and Committing to the Principles of the Paris Climate Agreement (Executive Order 2019-06)	Executive Order	26-28% below 2005 levels by 2025
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

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Table A1 continued

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

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Table A1 continued

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
Measures of Voluntary Motives and Behaviour		
GHG Proposal Dummy	An indicator equal to one if a firm receives one or more GHG emissions-related shareholder proposals in a given year, and zero otherwise	ISS - Voting Analytics - Shareholder Proposals
GHG Proposal Ratio	Ratio of emissions-related shareholder proposals to the total number of shareholder proposals received by a firm in a given year	ISS - Voting Analytics - Shareholder Proposals
Support Rate	Voting support for a proposal, calculated according to a firm's own voting rule	ISS - Voting Analytics - Company Vote Results US
Emissions	Facility-level GHG emissions in million metric tonnes of CO_2e	GHGRP
Emissions GHGRP	State-level GHG emissions in million metric tonnes of CO_2e, aggregated from facility-level data	GHGRP
Emissions EIA	State-level energy-related carbon emissions in million metric tonnes of CO_2e	EIA
Firm Emissions	Firm-level Scope 1 GHG emissions, measured in million metric tonnes of CO_2e	Trucost
Frequency(Proactive)	Frequency of proactive climate-related mentions during earnings calls, multiplied by 10,000	Li et al. (2024)
Frequency(Non-Proactive)	Frequency of non-proactive climate-related mentions during earnings calls, multiplied by 10,000	Li et al. (2024)
Vote for	A dummy set to 1 if a fund family supports a proposal and 0 if it abstains or votes against the proposal	ISS - Voting Analytics - Mutual Fund Vote Records

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Table A2 continued

Variable	Description	
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
#(Proposals)	Number of emission-related shareholder proposals received by a firm in the three years prior to 2019	ISS - Voting Analytics - Shareholder Proposals
Score on Climate Strategy	Firm-level score capturing climate strategy and performance in 2018, such as climate-related targets, internal carbon pricing, low-carbon products, climate risk management, and climate risk assessment	S&P Global ESG Scores

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Table A2 continued

Variable	Description	
Lockdown1	Duration (in days) of statewide stay-at-home mandates during the COVID-19 pandemic	Raifman et al. (2020)
Lockdown2	Duration (in days) of statewide non-essential business closure mandates during the COVID-19 pandemic	Raifman et al. (2020)

Table A3
Categories of Emissions-Related Shareholder Proposals

This table presents the categories of emissions-related shareholder proposals included in the sample for Table 3. Panel A classifies these proposals into eight groups based on their sponsors. Panel B classifies these proposals into eight groups based on their contents.

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

Table A4
The Likelihood of Receiving GHG Emissions-Related Proposals by Different Types of Submitters

This table presents regression estimates of firms' likelihood of receiving GHG emissions-related shareholder proposals submitted by different types of investors. Panel A focuses on proposals submitted by individuals and special-interest investors (*Sponsor_Type* is either "special interest", "religious", or "individual" according to ISS database). The dependent variable in Columns (1)–(4), *GHG Proposal Dummy*, equals 1 if a firm receives one or more such shareholder proposals in a given year, and 0 otherwise. In Columns (5)–(6), the dependent variable, *GHG Proposal Ratio*, represents the proportion of such proposals relative to the total number of shareholder proposals a firm receives in a given year. Panel B focuses on proposals submitted by institutional investors (*Sponsor_Type* is either "SRI", "investment fund", "public pension", or "coordinated" according to ISS database) and similarly defines the dependent variables. Other aspects of the analyses, including sample selection, variable definitions, and regression specifications, are identical to those in Table 3.

Dep. Var. =	*GHG Proposal Dummy*				*GHG Proposal Ratio*	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: The likelihood of receiving emissions-related proposals submitted by individual and special-interest investors						
Treated	0.0005	0.0006	0.0017	0.0017	-0.0043	-0.0031
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
AR_{t-1}		-0.0004		-0.0030		-0.0147
		(0.00)		(0.01)		(0.01)
$Asset_{t-1}$		0.0024		0.0102		-0.0028
		(0.00)		(0.01)		(0.01)
$Leverage_{t-1}$		0.0050		0.0424		0.0620
		(0.01)		(0.04)		(0.04)
ROA_{t-1}		-0.0000		0.0307		-0.0999
		(0.00)		(0.03)		(0.07)
MB_{t-1}		0.0000		-0.0003		0.0025
		(0.00)		(0.00)		(0.00)

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Table A4 continued

Dep. Var. =	*GHG Proposal Dummy*				*GHG Proposal Ratio*	
	(1)	(2)	(3)	(4)	(5)	(6)
Observations	10274	10274	3529	3529	1140	1140
Adj. R^2	0.2051	0.2048	0.2128	0.2124	0.3603	0.3593
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full Sample		#(Proposals)$_i \geq$1		#(Proposals)$_{it} \geq$1	
Panel B: The likelihood of receiving emissions-related proposals submitted by institutional investors						
Treated	-0.0092*	-0.0090*	-0.0233	-0.0238	-0.0497***	-0.0513***
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
AR_{t-1}		-0.0009**		-0.0074*		-0.0107
		(0.00)		(0.00)		(0.02)
$Asset_{t-1}$		0.0059**		0.0333**		-0.0059
		(0.00)		(0.01)		(0.03)
$Leverage_{t-1}$		0.0017		0.0021		-0.0904
		(0.01)		(0.04)		(0.07)
ROA_{t-1}		-0.0002*		-0.0212		-0.0410
		(0.00)		(0.04)		(0.08)
MB_{t-1}		0.0000***		0.0040*		-0.0036
		(0.00)		(0.00)		(0.01)
Observations	10274	10274	3529	3529	1140	1140
Adj. R^2	0.1899	0.1897	0.1901	0.1907	0.3001	0.2977
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full Sample		#(Proposals)$_i \geq$1		#(Proposals)$_{it} \geq$1	

Table A5
The Likelihood of Receiving and Voting Outcomes of Environmental Proposals Unrelated to Emissions and Social Proposals

This table presents regression estimates of firms' likelihood of receiving environmental shareholder proposals unrelated to emissions and social shareholder proposals, as well as the voting outcomes on such proposals. In Panel A, the dependent variable in Columns (1)–(4), *GHG Proposal Dummy*, equals 1 if a firm receives one or more environmental proposals unrelated to emissions in a given year, and 0 otherwise. In Columns (5)–(6), the dependent variable, *GHG Proposal Ratio*, is the proportion of environmental proposals unrelated to emissions relative to the total number of shareholder proposals a firm receives in a given year. In Columns (7)–(8), the dependent variable, *Support Rate*, measures the voting support for environmental proposals unrelated to emissions that are brought to a vote. In Panel B, the dependent variable is the likelihood of receiving social proposals (Columns (1)–(4)), the proportion of social proposals relative to the total number of shareholder proposals a firm receives (Columns (5)–(6)), or the voting support rate for social proposals that are brought to a vote (Columns (7)–(8)). All other aspects of the analysis, including sample selection, variable definitions, and regression specifications, follow those in Table 3 and Table 4.

Dep. Var. =	*Proposal Dummy*				*Proposal Ratio*		*Support Rate*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: The likelihood of receiving and voting outcomes on environmental proposals unrelated to emissions								
Treated	-0.0007	-0.0006	-0.0012	-0.0012	-0.0177	-0.0179	-0.1159	-0.1052
	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)	(0.02)	(0.10)	(0.13)
AR_{t-1}		-0.0004		-0.0036		0.0050		0.1105
		(0.00)		(0.00)		(0.02)		(0.08)
$Asset_{t-1}$		0.0018		0.0058		0.0062		-0.3482*
		(0.00)		(0.01)		(0.03)		(0.18)

Continued on next page

Table A5 continued

Dep. Var. =	*Proposal Dummy*				*Proposal Ratio*		*Support Rate*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Leverage_{t-1}		0.0057		0.0511**		0.0106		0.4931
		(0.00)		(0.02)		(0.05)		(0.35)
ROA_{t-1}		-0.0000		0.0320		0.1399**		-0.1637
		(0.00)		(0.03)		(0.06)		(0.66)
MB_{t-1}		0.0000		-0.0007		-0.0073		-0.2251*
		(0.00)		(0.00)		(0.01)		(0.11)
Observations	10274	10274	3529	3529	1140	1140	44	44
Adj. R^2	0.3016	0.3013	0.3026	0.3021	0.3574	0.3553	0.5340	0.5901
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full Sample		$\#(\text{Proposals})_i \geq 1$		$\#(\text{Proposals})_{it} \geq 1$		Proposals voted on	
Panel B: The likelihood of receiving and the voting results on social proposals								
Treated	0.0067	0.0070	0.0163	0.0157	0.0616**	0.0626**	-0.0008	0.0223
	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)	(0.02)	(0.06)	(0.04)
AR_{t-1}		-0.0005		-0.0010		-0.0157		0.0090
		(0.00)		(0.01)		(0.03)		(0.03)

Continued on next page

Table A5 continued

Dep. Var. =	*Proposal Dummy*				*Proposal Ratio*		*Support Rate*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Asset_{t-1}		0.0096**		0.0491**		0.0359		-0.1120
		(0.00)		(0.02)		(0.05)		(0.09)
Leverage_{t-1}		0.0026		0.0053		-0.0649		-0.6360***
		(0.01)		(0.06)		(0.11)		(0.13)
ROA_{t-1}		-0.0002		-0.0176		-0.0185		0.1773
		(0.00)		(0.07)		(0.18)		(0.37)
MB_{t-1}		0.0000**		0.0055		0.0201		0.0280
		(0.00)		(0.01)		(0.01)		(0.03)
Observations	10274	10274	3529	3529	1140	1140	125	125
Adj. R^2	0.3896	0.3895	0.3635	0.3640	0.3741	0.3730	0.5892	0.6427
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full Sample		$\#(\text{Proposals})_i \geq 1$		$\#(\text{Proposals})_{it} \geq 1$		Proposals voted on	

Internet Appendix

IA.1 Additional Robustness Results

Table IA1
The Likelihood of Receiving GHG Emissions-Related Proposals by Non-Pension Fund Submitters

This table presents regression estimates of firms' likelihood of receiving GHG emissions-related shareholder proposals not submitted by pension funds. In Columns (1)–(4), the dependent variable *GHG Proposal Dummy* equals 1 if a firm receives one or more GHG emissions-related shareholder proposals from non-pension fund shareholders in a given year, and 0 otherwise. The dependent variable in Columns (5)–(6), *GHG Proposal Ratio*, represents the proportion of GHG emissions-related shareholder proposals from non-pension fund shareholders relative to the total number of shareholder proposals a firm receives in a given year. Other aspects of the analyses, including sample selection, variable definitions, and regression specifications, are identical to those in Table 3.

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

Continued on next page

Table IA1 continued

Dep. Var. =	*GHG Proposal Dummy*				*GHG Proposal Ratio*	
	(1)	(2)	(3)	(4)	(5)	(6)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full Sample		$\#(\text{Proposals})_i \geq 1$		$\#(\text{Proposals})_{it} \geq 1$	

Table IA2
Voting Decision on GHG Emissions-Related Proposals Across Fund Family Types

This table presents regression estimates of voting decisions on GHG emissions-related shareholder proposals across different types of mutual fund families. The dependent variable, *Vote for*, is a dummy set to 1 if a fund family supports a proposal and 0 if the fund family votes to abstain or against the proposal. When funds within the same family vote differently, the family's voting decision is determined by the majority vote within the family. The variable *Socially Responsible Fund* is a dummy variable set to 1 if the majority of funds within a family include keywords like "ESG", "social", and "environment" in their names, and 0 otherwise. The variable *Treated* is a dummy variable set to 1 if the firm's headquartered state has adopted a state-level target by the given year, and 0 otherwise. Year, firm, and fund family fixed effects are controlled for in both specifications. Standard errors, adjusted for clustering at the state level, are presented in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The reported number of observations excludes singleton observations. All control variables are defined in Table A2.

Dep. Var. =	*Vote for*	
	(1)	(2)
Treated	-0.2659*	-0.1989***
	(0.13)	(0.06)
Treated*Socially Responsible Fund	0.0901	0.0896
	(0.12)	(0.12)
Socially Responsible Fund	-0.0584	-0.0603
	(0.10)	(0.09)
AR_{t-1}		-0.1013
		(0.09)
$Asset_{t-1}$		-0.2263
		(0.16)
$Leverage_{t-1}$		-0.9833*
		(0.47)
ROA_{t-1}		-0.8983**

Continued on next page

Table IA2 continued

Dep. Var. =	*Vote for*	
	(1)	(2)
		(0.32)
MB_{t-1}		-0.2461**
		(0.09)
Observations	7726	7726
Adj. R^2	0.4451	0.4500
Year FE	Yes	Yes
Firm FE	Yes	Yes
Fund FE	Yes	Yes

Table IA3
GHG Emissions-Related Proposals and COVID-19 Policies

This table presents robustness test results examining whether the patterns in GHG emissions-related shareholder proposals are driven by state-level emissions reduction targets or COVID-19 policies. In Panel A, the explanatory variable *Lockdown1* represents the duration of state-level stay-at-home mandates in days. In Panel B, *Lockdown2* represents the duration of non-essential business closure mandates in days. Other aspects of the analyses, including sample selection, variable definitions, and regression specifications, are identical to those in Table 3 and Table 4.

Dep. Var. =	*GHG Proposal Dummy*				*GHG Proposal Ratio*		*Support Rate*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Patterns in emissions-related proposals and COVID-19 stay-at-home mandates								
Treated	-0.0074*	-0.0072*	-0.0184*	-0.0190*	-0.0612***	-0.0615***	-0.1360	-0.1247*
	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)	(0.02)	(0.10)	(0.06)
Lockdown1	-0.0000	-0.0000	-0.0001	-0.0001	0.0007	0.0007	-0.0007	-0.0004
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
AR_{t-1}		-0.0014		-0.0105		-0.0264*		0.1105
		(0.00)		(0.01)		(0.02)		(0.08)
$Asset_{t-1}$		0.0084**		0.0438**		-0.0081		-0.3482*
		(0.00)		(0.02)		(0.04)		(0.18)
$Leverage_{t-1}$		0.0059		0.0367		-0.0349		0.4931

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Table IA3 continued

Dep. Var. =	*GHG Proposal Dummy*				*GHG Proposal Ratio*		*Support Rate*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		(0.01)		(0.06)		(0.10)		(0.35)
ROA_{t-1}		-0.0002**		0.0065		-0.1227		-0.1637
		(0.00)		(0.05)		(0.11)		(0.66)
MB_{t-1}		0.0000***		0.0040*		-0.0015		-0.2251*
		(0.00)		(0.00)		(0.01)		(0.11)
Observations	10130	10130	3511	3511	1131	1131	100	100
Adj. R^2	0.2485	0.2485	0.2390	0.2402	0.4245	0.4228	0.5888	0.5679
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full Sample		$\#(\text{Proposals})_i \geq 1$		$\#(\text{Proposals})_{it} \geq 1$		Proposals voted on	
Panel B: Patterns in emissions-related proposals and COVID-19 non-essential business closure mandates								
Treated	-0.0071*	-0.0069*	-0.0175	-0.0182	-0.0526***	-0.0529***	-0.1716**	-0.1143***
	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)	(0.02)	(0.06)	(0.03)
Lockdown2	-0.0001	-0.0001	-0.0002	-0.0002	-0.0001	-0.0001	-0.0022	-0.0017
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
AR_{t-1}		-0.0013		-0.0103		-0.0255*		-0.0133

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Table IA3 continued

Dep. Var. =	*GHG Proposal Dummy*				*GHG Proposal Ratio*		*Support Rate*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		(0.00)		(0.01)		(0.01)		(0.03)
Asset_{t-1}		0.0083**		0.0437**		-0.0088		-0.2103
		(0.00)		(0.02)		(0.04)		(0.13)
Leverage_{t-1}		0.0062		0.0395		-0.0287		-0.4914
		(0.01)		(0.06)		(0.10)		(0.31)
ROA_{t-1}		-0.0002**		0.0027		-0.1410		-0.3612
		(0.00)		(0.05)		(0.11)		(0.25)
MB_{t-1}		0.0000***		0.0040**		-0.0010		-0.1851***
		(0.00)		(0.00)		(0.01)		(0.06)
Observations	10188	10188	3525	3525	1136	1136	100	100
Adj. R^2	0.2479	0.2478	0.2378	0.2390	0.4201	0.4185	0.5430	0.5330
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full Sample		$\#(\text{Proposals})_i \geq 1$		$\#(\text{Proposals})_{it} \geq 1$		Proposals voted on	

Table IA4
The Likelihood of Receiving GHG Emissions-Related Proposals and the Voting Results (Firms with All High-Emitting Facilities in the Same State)

This table presents a robustness check on the likelihood of firms receiving GHG emissions-related shareholder proposals and the voting outcomes, using a subsample of firms whose high-emitting facilities are all located in the same state as their headquarters. Other aspects of the analyses, including sample selection, variable definitions, and regression specifications, are identical to those in Table 3 and Table 4.

Dep. Var. =	*GHG Proposal Dummy*				*GHG Proposal Ratio*		*Support Rate*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	-0.0042	-0.0041	-0.0111	-0.0122	-0.0426**	-0.0392**	-0.2295***	-0.1327
	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)	(0.02)	(0.06)	(0.10)
AR_{t-1}		-0.0009		-0.0089		-0.0365*		0.0344
		(0.00)		(0.01)		(0.02)		(0.09)
$Asset_{t-1}$		0.0047		0.0280		-0.0331		0.0154
		(0.00)		(0.02)		(0.04)		(0.31)
$Leverage_{t-1}$		0.0070		0.0551		0.0567		-0.2752
		(0.01)		(0.05)		(0.08)		(0.93)
ROA_{t-1}		-0.0001		0.0270		-0.1434*		-0.1018
		(0.00)		(0.05)		(0.07)		(0.52)
MB_{t-1}		0.0000**		0.0058*		-0.0052		-0.2180
		(0.00)		(0.00)		(0.02)		(0.13)

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Table IA4 continued

Dep. Var. =	*GHG Proposal Dummy*				*GHG Proposal Ratio*		*Support Rate*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Observations	9588	9588	3115	3115	954	954	78	78
Adj. R^2	0.2445	0.2442	0.2363	0.2367	0.3968	0.3960	0.5092	0.4685
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full Sample		#(Proposals)$_i \geq$1		#(Proposals)$_{it} \geq$1		Proposals voted on	

Table IA5
Facility-level GHG Emissions: Subsample of Facilities Appearing in the Proposal Regressions

This table presents regression estimates of the impact of state-level GHG emissions reduction targets on facility-level GHG emissions. The specifications mirror those in Table 5, but the sample is restricted to facilities that also appear in Columns (1)–(2) of Table 3. 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 *Emissions*. 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. Column (1) includes 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. The reported number of observations excludes singleton observations. All control variables are defined in Table A2.

	(1)	(2)	(3)
Dep. Var. =		*Emissions*	
Treated	0.0251	0.0358	0.0381
	(0.03)	(0.03)	(0.03)
AR_{t-1}			-0.0014
			(0.01)
$Asset_{t-1}$			0.0230*
			(0.01)
$Leverage_{t-1}$			0.0974
			(0.06)
ROA_{t-1}			-0.1707*
			(0.09)
MB_{t-1}			0.0000
			(0.00)

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Table IA5 continued

	(1)	(2)	(3)
Observations	9426	8300	8300
Adj. R^2	0.9608	0.9662	0.9662
Year FE	Yes	Yes	Yes
Facility FE	Yes	Yes	Yes
Balanced	No	Yes	Yes

Table IA6
Firm-Level GHG Emissions

This table presents regression estimates of the impact of state-level GHG emissions reduction targets on firm-level GHG emissions. Firms headquartered in the nine states that adopted state-level targets in 2019 are designated as the treatment group, while the control group consists of firms in the twenty-five states that have never adopted such targets. The sample period spans from 2016 to 2021. The dependent variable is *Firm Emissions*, sourced from Trucost. The variable *Treated* is a dummy variable set to 1 if the state where the firm is headquartered has adopted a state-level target by the given year, and 0 otherwise. Columns 1-3 use the full sample, while Columns 4-6 restrict the sample to firms that received at least one shareholder proposal, as recorded in the ISS Proposal database. Columns (2)–(3) and Columns (5)–(6) restrict the samples in Columns (1) and (4), respectively, to balanced panels of firms observed in all years of the event window. 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 reported number of observations excludes singleton observations. All control variables are defined in Table A2.

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var. =	*Firm Emissions*					
Treated	0.1135	0.1482	0.1498	0.2524	0.2951	0.2590
	(0.09)	(0.11)	(0.10)	(0.18)	(0.21)	(0.21)
AR_{t-1}			0.0133			0.0453
			(0.04)			(0.07)
$Asset_{t-1}$			0.6371*			1.3688**
			(0.31)			(0.64)
$Leverage_{t-1}$			-0.5951*			-1.7464
			(0.30)			(1.63)
ROA_{t-1}			-0.4172*			-1.5849
			(0.23)			(1.47)
MB_{t-1}			0.0096			0.0583*
			(0.01)			(0.03)

Continued on next page

Table IA6 continued

	(1)	(2)	(3)	(4)	(5)	(6)
Observations	7754	5628	5628	3435	2964	2964
Adj. R^2	0.8734	0.8676	0.8679	0.8735	0.8643	0.8649
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Balanced	No	Yes	Yes	No	Yes	Yes
Sample		Full Sample			#(Proposals)$_i \geq$1	

IA.2 A Comparison Between Log-transformed and untransformed Outcome Variables

A contemporary paper, Korganbekova (2025), also examines the effectiveness of state-level targets and reports evidence of emissions reductions. The discrepancy between her findings and mine stems from the choice of dependent variable: she uses ln(1+facility-level emissions), while I use the untransformed facility-level emissions.

Korganbekova (2025) does not justify the transformation explicitly. In the literature, common reasons for logarithmic transformations include: (1) positively skewed outcome variables for which a log transform yields a distribution closer to normal; (2) reduction of outlier influence, especially for extreme values; and (3) an underlying multiplicative model that becomes additive and linear after logging. Relatedly, coefficients in log models are interpreted approximately as percentage changes rather than absolute changes, which can be more informative in some contexts.

However, an important distinction between using log-transformed versus untransformed outcome variables is often overlooked: they address different research questions. In linear-linear models with untransformed entity-level emissions, the average treatment effect (ATE), β_1 in Equation (3), captures the average entity-level *level* change. In log-linear models, the ATE, $e^{\beta_1} - 1$, represents the average entity-level *percentage* change. Importantly, although the *average level change* equals the *level change in the average* when the sample is balanced (no entries added or dropped), the *average percentage change* generally differs from the *percentage change in the average*. As I will show later, these two ATEs sometimes have opposite signs.

Neither specification is universally superior; instead, the form of the outcome variable depends on which treatment effect is of interest. If the research question concerns *aggregate* treatment effects (such as the effectiveness of a climate policy in reducing overall emissions), the outcome should remain untransformed, because the average individual-level level change aggregates directly to the total change. Conversely, if the focus is on the *individual*-level response, log transformation can be more appropriate because it normalises scale differences and effectively gives equal weight to each unit (for example, when assessing the impact of board independence on CEO compensation).[9]

These differences can produce opposite conclusions on the same data, especially when the outcome is highly dispersed and treatment effects are heterogeneous. For example, suppose a small number of very large facilities increase emissions while many small facilities reduce emissions. In that case, the average *level* change may be positive while the average *percentage* change is negative: the former weights entities by scale, the latter treats entities equally.

[9] As an alternative to log transformations, researchers sometimes apply log-like transformations such as $\ln(1 + Y)$ to accommodate zero-valued observations. However, Chen and Roth (2024) show that such transforms can be sensitive to the unit of Y and recommend several other approaches.

Table IA7 presents the regression results for facility-level emissions. Following Korganbekova (2025), Columns 1-3 use log-transformed facility-level emissions.[10] Consistent with her findings, these columns show a negative correlation between treatment and log-transformed emissions. It should be noted that this negative coefficient reflects an average *percentage* change rather than the aggregate effect on corporate emissions. Columns 4–6 report results using untransformed emissions (same as in Table 5 in this paper), where the estimated coefficients are insignificantly positive, suggesting no evidence that state-level targets accelerate *aggregate* corporate emissions reductions.

To explore heterogeneity by emissions scale, I split treatment and control groups into quintiles and run DID regressions for each quintile. The results are reported in Table IA8. Although most estimates are statistically insignificant, their signs and magnitudes are informative: treatment effects are negative for the 1st, 3rd, and 4th quintiles while positive for the 2nd and 5th. Additionally, as shown in the table, mean emissions by quintile exhibit extreme dispersion. The mean in the fifth quintile (1.9523) is an order of magnitude larger than in the fourth (0.1717), and the treatment effect magnitude in the fifth quintile likewise dwarfs that in the fourth. Given that most facilities reduce emissions, the average *percentage* change is negative (consistent with logged results), while the large emitters' influence on levels drives the average *level* change positive. Thus, dispersion plus heterogeneous treatment effects explain why log and level regressions can yield opposite signs.[11]

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 this context, and report robustness checks supporting the conclusion that state-level targets do not reduce aggregate 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

[10] A minor change is that I replace $\ln(1+Y)$ with $\ln(Y)$, as Chen and Roth (2024) show that the former can introduce biases. In the focal analysis, however, the two specifications yield very similar results.

[11] 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 IA10.

(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.[12] While the geometric mean usually moves in the same direction as the arithmetic mean, it is also negatively sensitive to the dispersion of values. In some cases, these two mean values can even change in opposite directions, as illustrated in the following numerical example.

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

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

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

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

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

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.

Given these considerations, an untransformed outcome is more appropriate for assessing climate policy effectiveness on aggregate emissions. Nonetheless, I examine whether standard motivations for logging apply here and provide robustness checks.

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 IA9 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 under-

standing 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 IA10). 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 IA11, which are consistent with the conclusion that state-level targets are ineffective in reducing corporate emissions.

Unlike the facility-level analyses, where the observation level is more granular than the treatment, state-level analyses have observations and treatments at the same level of granularity. Consequently, regressions of both non-log-transformed and log-transformed emissions at the state level capture the aggregate treatment effect and assess policy effectiveness. The only small difference is that regressions using non-log-transformed state-level emissions assign greater weight to states with higher emissions, while those using log-transformed state-level emissions treat all states equally. For additional robustness, I run regressions using log-transformed state-level emissions, as shown in Table IA12, 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 accelerating corporate emissions reductions.

In conclusion, regressions with log-transformed and untransformed outcome variables address different research questions. The untransformed outcome variable is more appropriate for assessing the effectiveness of

climate policies in mitigating climate change, as its estimated ATE is proportional to the aggregate treatment effect on corporate emissions. While logarithmic transformation has merits in certain contexts, they are not relevant in the context of this paper.

Table IA7
Facility-Level GHG Emissions

This table presents regression estimates of 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(Emissions)* in Columns (1)–(3) and *Emissions* 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(Emissions)*			*Emissions*		
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

Continued on next page

Table 5 continued

	(1)	(2)	(3)	(4)	(5)	(6)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Facility FE	Yes	Yes	Yes	Yes	Yes	Yes
Balanced	No	Yes	Yes	No	Yes	Yes

Table IA8
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 emissions levels in the reference year, 2018. The dependent variable is facility-level *Emissions* 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 *Emissions* for each group.

Dep. Var. =	*Emissions*									
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

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Table IA8 continued

Facility FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balanced	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Mean(*Emissions*)	0.0272		0.0425		0.0701		0.1717		1.9523	
SD(*Emissions*)	(0.19)		(0.03)		(0.03)		(0.21)		(2.52)	

Table IA9
Facility- and State-Level GHG Emissions with Bootstrapped Standard Errors

This table presents difference-in-differences regression estimates of the effects of state-level GHG emissions reduction targets on facility- and state-level GHG emissions. Unlike Tables 5 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 *Emissions*. Column (2) includes untabulated control variables, consistent with those used in Panel A of Table 3. In Panel B, the dependent variable for Columns (1)–(2) is *Emissions GHGRP*, calculated by aggregating facility-level emissions from the Greenhouse Gas Reporting Program (GHGRP) of the US EPA. For Columns (3)–(4), the dependent variable is *Emissions 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. =	*Emissions*	
	(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
Facility FE	Yes	Yes
Balanced	Yes	Yes

Panel B: State-level emissions

Dep. Var. =	*Emissions GHGRP*	*Emissions EIA*

Continued on next page

Table IA9 continued

	(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 IA10
Facility-Level GHG Emissions Excluding Largest Emitters or Winsorizing Emissions Variable

This table presents regression estimates of the impact of state-level GHG emissions reduction targets on facility-level GHG emissions. To assess whether the results in Table 5 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 *Emissions* 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. =	*Emissions*					
	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

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Table IA10 continued

	(1)	(2)	(3)	(4)	(5)	(6)
			(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 IA11
Poisson Regressions of Facility- and State-Level GHG Emissions

This table presents difference-in-differences regression estimates of the effects of state-level GHG emissions reduction targets on facility- and state-level GHG emissions. Unlike Tables 5 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 *Emissions*. Column (2) includes untabulated control variables, consistent with those used in Panel A of Table 3. In Panel B, the dependent variable for Columns (1)–(2) is *Emissions GHGRP*, calculated by aggregating facility-level emissions from the Greenhouse Gas Reporting Program (GHGRP) of the US EPA. For Columns (3)–(4), the dependent variable is *Emissions 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. =	*Emissions*	
	(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
Facility FE	Yes	Yes
Balanced	Yes	Yes

Panel B: State-level emissions

Dep. Var. =	*Emissions GHGRP*	*Emissions EIA*

Continued on next page

Table IA11 continued

	(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

Table IA12
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)–(2) is *ln(Emissions 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)–(4), the dependent variable is *ln(Emissions 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(Emissions GHGRP)*		*ln(Emissions 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