Greenhouse Gas Disclosure and Emissions Benchmarking

Finance Working Paper N° 818/2022
January 2023

Sorabh Tomar
Southern Methodist University

ECGI Working Paper Series in Finance

Greenhouse Gas Disclosure and Emissions Benchmarking

Working Paper N° 818/2022
January 2023

Sorabh Tomar

This paper is based on my dissertation. I thank my committee members— Hans Christensen (co-chair), Christian Leuz (co-chair), Mark Maffett, and Abbie Smith—for their invaluable guidance. I am also grateful to Ray Ball, Phil Berger, John Barrios, Jeremy Bertomeu, Neil Bhattacharya, Matthew Bloomfield, Michael Braun, Matthias Breuer, Joey Choi (discussant), Jung Ho Choi, Carolyn Deller, Hemang Desai, Jo˜ao Granja, Michael Greenstone, Matthew Gustafson (discussant), Jody Grewal (discussant), Doug Hanna, Russ Hamilton, Yanrong Jia, Ginger Jin (discussant), Jean-Marie Meier, Dan Millimet, Liz Moyer, DJ Nanda, Jing Pan, Rachna Prakash (discussant), Robbie Sanders, Doug Skinner, Mark Templeton, Wayne Taylor, Marcel Tuijn, Hayoung Yoon, an anonymous associate editor, two anonymous reviewers, and seminar participants at CUNY Baruch College, Deakin University, LSE, Southern Methodist University, UCLA, University of Chicago, AAA FARS Conference, FMCG Conference, NBER Measuring and Reporting Corporate Carbon Footprints Conference, RSFE, and University of Delaware Weinberg Center/ECGI Corporate Governance Symposium for helpful comments and suggestions. I thank US EPA's Emissions Inventory and Analysis Group for providing much data necessary for this study and Nivedita Gupta for valuable research assistance.

Abstract

I examine the effects of the US Greenhouse Gas (GHG) Reporting Program, which requires thousands of industrial facilities to measure and report their GHG emissions. I show that facilities reduce their GHG emissions by 7.9% following the disclosure of emissions data. The evidence indicates that benchmarking—whereby facilities use the disclosures of their peers to assess their own relative GHG performance—spurs emissions reductions. Firms' concerns about future legislation appear to motivate this behavior and measurement alone (without disclosure) seems not to reduce emissions. My study highlights how mandatory GHG disclosure can create real effects for peers.

Keywords: Disclosure; ESG; Climate Change; Benchmarking; Peer Effects; Real Effects; Political Pressure

JEL Classifications: D72, M40, Q54, Q56

Sorabh Tomar
Assistant Professor of Accounting
Southern Methodist University
6214 Bishop Blvd
Dallas, TX 75275, USA
phone: +1 (214) 768-9464
e-mail: stomar@smu.edu

Greenhouse Gas Disclosure and Emissions Benchmarking

Sorabh Tomar

Southern Methodist University Cox School of Business

January 2023

Abstract

I examine the effects of the US Greenhouse Gas (GHG) Reporting Program, which requires thousands of industrial facilities to measure and report their GHG emissions. I show that facilities reduce their GHG emissions by 7.9% following the disclosure of emissions data. The evidence indicates that benchmarking—whereby facilities use the disclosures of their peers to assess their own relative GHG performance—spurs emissions reductions. Firms' concerns about future legislation appear to motivate this behavior and measurement alone (without disclosure) seems not to reduce emissions. My study highlights how mandatory GHG disclosure can create real effects for peers.

JEL Codes: *D72, M40, Q54, Q56*

Keywords: *Disclosure; ESG; Climate Change; Benchmarking; Peer Effects; Real Effects; Political Pressure*

Accepted by Rodrigo Verdi. This paper is based on my dissertation. I thank my committee members—Hans Christensen (co-chair), Christian Leuz (co-chair), Mark Maffett, and Abbie Smith—for their invaluable guidance. I am also grateful to Ray Ball, Phil Berger, John Barrios, Jeremy Bertomeu, Neil Bhattacharya, Matthew Bloomfield, Michael Braun, Matthias Breuer, Joey Choi (discussant), Jung Ho Choi, Carolyn Deller, Hemang Desai, João Granja, Michael Greenstone, Matthew Gustafson (discussant), Jody Grewal (discussant), Doug Hanna, Russ Hamilton, Yanrong Jia, Ginger Jin (discussant), Jean-Marie Meier, Dan Millimet, Liz Moyer, DJ Nanda, Jing Pan, Rachna Prakash (discussant), Robbie Sanders, Doug Skinner, Mark Templeton, Wayne Taylor, Marcel Tuijn, Hayoung Yoon, an anonymous associate editor, two anonymous reviewers, and seminar participants at CUNY Baruch College, Deakin University, LSE, Southern Methodist University, UCLA, University of Chicago, AAA FARS Conference, FMCG Conference, NBER Measuring and Reporting Corporate Carbon Footprints Conference, RSFE, and University of Delaware Weinberg Center/ECGI Corporate Governance Symposium for helpful comments and suggestions. I thank US EPA's Emissions Inventory and Analysis Group for providing much data necessary for this study and Nivedita Gupta for valuable research assistance. An Online Appendix to this paper can be downloaded at https://www.chicagobooth.edu/jar-online-supplements.

1 Introduction

This paper studies the US Greenhouse Gas Reporting Program (US Program) to understand how mandatory, granular disclosure affects the greenhouse gas (GHG) emissions of reporting entities. It highlights the role of *benchmarking* in producing real effects, whereby facilities reduce emissions after observing the GHG disclosures of their peers. Among environmental, social, and governance (ESG) topics, climate change has received substantial attention, given its potentially catastrophic risks [Eccles and Klimenko, 2019; Hoegh-Guldberg et al., 2018]. GHG disclosure mandates are thus increasingly being adopted in different regions of the world. Notably, the US Securities and Exchanges Commission (SEC) proposed an extensive climate-related disclosure rule in 2022. The impact of disclosure in areas such as toxic pollution [Chen, Hung, and Wang, 2018; Hamilton, 2005] suggests potential benefits in the GHG setting.

Administrated by the US Environmental Protection Agency (EPA), the US Program was implemented in 2010, broadly for use in guiding potential future GHG policies. It requires thousands of US facilities to report their yearly emissions by GHG and production activity. Disclosure frequently extends to the process or unit level (boiler, furnace, etc.). In 2010, the US Program covered over 6,200 facilities that together emitted 3.2 billion tons of carbon dioxide equivalent (T CO_2e), roughly half of total US emissions. Importantly, although facilities began measuring GHG emissions in 2010, the data were not publicly disclosed until 2012. The US Program therefore provides two years of pre-treatment data for this study about the effects of public disclosure. Further, because most facilities did not previously disclose GHG emissions, the US Program allows study of the impact of information provision rather than of the aggregation or dissemination of existing information.

Pro-social disclosure rules often aim to create an *action-cycle*, whereby disclosed information becomes embedded in the decisions of users, and user responses, in turn, feed back into disclosers' decisions [Hombach and Sellhorn, 2019; Weil, Fung, Graham, and Fagotto, 2006]. Although the US Program provides a significant amount of new information, there are reasons to question whether it produces a firm-level action-cycle around GHG emissions. The US Program's government website might lack salience to stakeholders, its facility-level data are difficult to aggregate and exclude emissions generated abroad, and the diffuse nature of global warming increases the coordination costs of stakeholder action around the disclosed data. Yet, US Program data are also granular and informative about operations. Facilities might therefore use their peers' disclosures to identify red flags and drive down their own emissions. Given these arguments, it bears empirical examination whether the US Program reduces emissions.

I divide my analysis into three parts. First, I conduct difference-in-differences tests to examine whether the US Program leads to GHG emissions reductions. Second, I present evidence supporting benchmarking, whereby facilities use their peers' disclosures when reducing emissions. Third, I present supplementary evidence to more completely understand the US Program's effects, including the role of external stakeholder pressure and whether facilities reduce emissions during the measurement phase of the US Program (prior to disclosure).

My difference-in-differences tests show that GHG emissions decline by 7.9% following US Program disclosure. Canadian facilities provide a plausible counterfactual—they share many commonalities with US facilities and have been disclosing their GHG emissions since 2004. The estimation accounts for industry-specific trends and time-invariant facility characteristics. Additional tests show that emissions reductions are not achieved by simply curbing or offshoring economic activity. Rather, firms are seen to increase capital expenditures, suggesting they make investments to reduce GHG emissions.

To assess benchmarking, I present four sets of supportive evidence. First, I show that measures of within-industry emissions dispersion fall by 20-31%. This is consistent with greater overlap in facilities' information following disclosure. Second, I show that a facility's carbon intensity, relative to that of its peers (and revealed through disclosure), predicts its subsequent emissions reduction. This is consistent with managers using disclosed data to assess whether their facilities are more or less carbon intensive than peers. This relation only emerges when carbon intensity is constructed using data publicly observable at that time (and not timelier data not yet publicly available at that time), which helps to rule out nondisclosure-based explanations.

Third, I classify some facilities as benchmarkers based on how much their owner-firms search for their peers' financial information, à la Bernard, Blackburne, and Thornock [2020]. Benchmarkers have significantly larger GHG emissions reductions relative to nonbenchmarkers. Fourth, I employ a novel measure of industrial process-similarity using the techniques of Fetter, Steck, Timmins, and Wrenn [2022]. GHG emissions reductions are largest when peer facilities have lower initial similarity—this is consistent with the notion that pre-existing diversity in processes provides more scope for benchmarking to reduce emissions. I also show that facilities in a peer group become more similar in terms of processes after US Program disclosure. Further, they end up sharing more (less) processes with their carbon light (carbon intense) peers.

Turning to supplementary evidence about the US Program's effects, I first explore the role of external pressure in motivating emissions reductions and benchmarking. I find a larger US Program treatment effect when facilities have climate-progressive senators, supporting the idea that facilities are attentive to the prospect of climate change-related legislation. Because constituents can seek to influence climate policy [Gelles, 2022], I also consider the role of political connections forged through campaign contributions. The US Program treatment effect is larger for facilities connected to their House representatives, consistent with these facilities being more concerned about legislation or trying to shed their representatives in a favorable environmental light. In comparison, I find no strong evidence of emissions pressure from capital markets, customers, or the general public around US Program disclosure.

My other supplementary evidence concerns the impact of emissions measurement and anticipation of external pressure prior to US Program disclosure. Using Bayesian methods, I estimate US facilities' unobservable pre-US Program CO_2 emissions (i.e., for years 2008 and 2009). Although I find a decline in CO_2 emissions after US Program disclosure, there is no significant emissions response when facilities first start measuring and reporting their emissions non-publicly to the EPA (prior to disclosure). These results underscore the importance of public disclosure in producing emissions reductions.

Although GHG disclosure mandates have becoming increasingly common (see Australia, the European Union, and the United Kingdom for policy examples), little is known about their effects. My paper demonstrates that they can produce an important social benefit by reducing GHG emissions. Though such reductions are not always the intent—the SEC states that its proposed rule primarily informs investors—this paper nonetheless highlights several points that may be informative for policy-makers interested in emissions reductions: i) benchmarking can play an important role in reducing emissions, and considering the US Program, this seems more likely when disclosed data are granular; ii) the prospect of climate-legislation is a factor that facility managers likely consider when implementing emissions reductions (Glazer and McMillan, 1992; Maxwell, Lyon, and Hackett, 2000; Suijs and Wielhouwer, 2019); and iii) measurement and reporting to the regulator alone might not affect emissions. Recent work also highlights the potential of GHG disclosure mandates to reduce emissions [Bauckloh, Klein, Pioch, and Schiemann, 2022; Downar, Ernstberger, Reichelstein, Schwenen, and Zaklan, 2021; Jouvenot and Krueger, 2021; Matisoff, 2013; Yang, Muller, and Liang, 2022]. Because these papers study emissions data that are already available in another venue prior to the disclosure rules they study, they measure an effect incremental to the one I measure.[1]

My paper also contributes to the literature on the real effects of ESG disclosure. First, it measures the initial impact of information provision on the disclosed outcome, rather than the impact of aggregating or disseminating information available elsewhere (e.g., Bennear and Olmstead, 2008; Christensen, Floyd, Liu, and Maffett, 2017). Second, it documents the effectiveness of disclosure with respect to an externality with large collective action costs

[1]Several papers also study the real effects of voluntary GHG emissions disclosures (e.g., Qian and Schaltegger, 2017; Bolton and Kacperczyk, 2021). This paper differs by estimating a treatment effect unconditional on latent factors that might also drive emissions reductions (e.g., a desire to create institutional legitimacy; Luo, 2019).

and relatively low salience and immediacy, rather than a setting where emissions are toxic and local (i.e., where effects are salient and stakeholders have low coordination costs) (e.g., Chen et al., 2018; Delmas, Montes-Sanchom, and Shimshack, 2010; Graham and Miller, 2001; Hamilton, 2005). Lastly, it shows that ESG disclosure can facilitate benchmarking. Roychowdhury, Shroff, and Verdi [2019] note the difficulty of identifying peer effects, given Manski, 1993's reflection problem. Although there is some evidence that firms take cues from rivals' financial and operational disclosures (Durnev and Mangen, 2009; Fetter et al., 2022; Grennan and Swanson, 2020; Li, 2016), firms' ESG practices could vary markedly in motivation (stakeholder orientation, alignment with financial objectives, virtue signaling, etc.), leaving the utility of peers' ESG disclosures less clear. Cao, Liang, and Zhan [2019] show that firms improve their ESG performance after their peers pass ESG-focused resolutions. They highlight a competitive concern about peers' ESG performance but do not address the role of disclosure regulation in promoting benchmarking.

2 Setting: The US GHG Reporting Program

The Consolidated Appropriations Act of 2008 provided funds for the US EPA to develop a mandatory GHG reporting rule, covering most sectors of the US economy. The US Program's main goal is to collect data for use in potential GHG rule-making, including emissions pricing, which had considerable support at that time [Richardson, 2012]. Based on its experiences with programs such as the Toxic Release Inventory (TRI), the EPA also recognized that the US Program could raise awareness of emissions among stakeholders and emitters, which could facilitate emissions reductions [US EPA, 2009]. The EPA proposed a mandatory GHG reporting rule on April 10, 2009, and, after soliciting comments, finalized a rule on October 30, 2009. Over this period and the following year, the prospects for US GHG emissions pricing dimmed significantly.[2] The US Program, however, remained intact.

[2]The posited reasons for this include high unemployment after the Great Recession, lobbying by high emitters, the Senate's refusal to vote on the Waxman-Markey Bill, the growing politicization of climate policy,

The US Program took effect on January 1, 2010, with the first reports due for submission to the EPA on September 30, 2011. The 2010 data were publicly disclosed by the EPA on January 11, 2012. Figure 1 provides a timeline of the key dates as they relate to the empirical tests. The US Program requires facilities to report their GHG emissions by specific gas, coming from any of 41 source categories. Disclosure thresholds vary by source category, but 25,000T CO_2e is the most common threshold. Depending on the source category, facilities must disclose additional information at the sub-facility level (i.e., unit or process level). Thus the US Program provides very granular data.[3]

US Program reports are self-certified by facilities (i.e., third-party verification is not required). Nonetheless, the US Program does promote data quality. Its electronic reporting platform provides real-time feedback about potential errors to reporting facilities. The EPA then subjects reports to a series of electronic checks, after which it can question facilities to understand any irregularities. Furthermore, the US Program is given force by the US Clean Air Act, which lets the EPA levy penalties of up to $37,500 per day of a violation, which includes failure to report emissions, failure to retain adequate records, and report falsification. That said, the EPA has yet to take any enforcement action regarding the US Program.

US Program data are publicly accessible in multiple formats. One is an interactive map geared toward novice users. Facility-level data are also available in spreadsheet format. Advanced users can access the totality of US Program data by querying the EPA's Envirofacts

and the Obama administration's pivot towards healthcare, financial regulation, and energy independence.

[3]Examples of GHGs are carbon dioxide (CO_2) and methane—CO_2 is the chief GHG emitted by facilities. Examples of source categories are stationary combustion and cement manufacturing. 25,000T CO_2e is equivalent to the emissions from the energy used by 2,200 homes in a year or 131 railroad cars of coal.

To assess whether a facility falls below an inclusion threshold, the EPA requires a submission of pen-and-paper calculations based on the amount of resources consumed and default conversion factors. For facilities included in the US Program, the EPA prescribes several measurement methods. For example, there are four measurement tiers for measuring CO_2 emissions from fossil fuel combustion. Larger emitting units must use higher tiers. Tier 1 is based on the mass of fuel used and default laboratory factors. Tier 4, in contrast, requires continuous emissions monitoring to directly measure CO_2 emissions at a fixed cost of $25,000-$75,000 [Singh, Bacher, Song, Sotos, and Yin, 2015]. When reporting on stationary combustion, facilities must report the following, among other things, for each combustion unit: its type (e.g., boiler, furnace), its maximum thermal input power, the types of fuel it uses, its emissions, and any relevant emissions calculations. This serves well to illustrate the granularity of US Program data.

database. From direct communications with the EPA, the GHG portion of Envirofacts received over 100,000 page-views from January 2013 to June 2020, indicating the data are frequently accessed. The EPA also provides the names of reporting facilities' highest-level US owners, though the naming conventions are often inconsistent across years.

3 Related Literature and Hypothesis Development

ESG disclosure mandates span many areas, including workplace safety, public health, and mineral extraction rights [Christensen et al., 2017; Jin and Leslie, 2003; Johnson, 2020; Dranove, Kessler, McClellan, and Satterthwaite, 2003; Rauter, 2020]. Often, their goal is to create an action-cycle, whereby disclosure affects the decisions of information users (e.g., restaurant customers), and disclosing firms anticipate these decisions and change their behavior accordingly.

As discussed in Section 2, the EPA recognizes the potential for an action-cycle to emerge around the US Program. Potential users include regulators, the public, investors, and, as will be discussed, peer facilities. In the environmental domain, mandatory disclosure has led listed Chinese firms to reduce toxic SO_2 and wastewater emissions [Chen et al., 2018] and utilities to improve their environmental performance [Bennear and Olmstead, 2008; Delmas et al., 2010]. The EPA's TRI is perhaps the most studied pollution disclosure rule and is credited for a dramatic decline in toxic emissions [Weil et al., 2006; Graham and Miller, 2001]. The TRI and US Program have similarities—both are administrated by the EPA, do not target a specific stakeholder group, provide information at the facility level or finer, and span a wide range of industries. As such, the TRI serves as a useful reference point for conjectures about the US Program's effects. The apparent success of the TRI and other disclosures in improving environmental outcomes suggests that disclosure can play a role in curbing GHG emissions too. This leads to the first hypothesis, expressed in alternate form as follows:

H1: *Facilities reduce their GHG emissions following US Program disclosure.*

A number of factors, however, militate against H1. The US Program's presence on a government website might lack the salience of financial statements or customer reports [Bennear and Olmstead, 2008; Christensen et al., 2017]. Further, aggregation of facility-level data is cumbersome and incompletely depicts a firm's global emissions. Thus the potential for external pressure at the firm level loses some force.[4]

Additionally, GHGs are quite different pollutants to those covered by the TRI and similar EPA programs. The latter are toxic, making their effects (e.g., illness, acid rain) salient and likely to trigger outrage. They also act locally, reducing the coordination costs of stakeholder action (e.g., shaming, litigation) [Coase, 1960]. In contrast, GHG emissions are largely nontoxic, and reducing them entails global coordination costs: each emitter contributes only marginally to the global temperature, and affected stakeholders are widely dispersed. Reducing emissions might also entail significant direct costs. Weighed against the cited disclosure research, these factors highlight the importance of empirically testing the relation between GHG disclosure and emissions.

Attention must also be paid to how emissions reductions could occur. Firms can mimic the ESG behavior of their competitors, and target-setting plays an important role in GHG emissions reduction strategies [Cao et al., 2019; Ioannou, Li, and Serafeim, 2016]. Connecting these ideas with the US Program's significant granularity, one hypothesis is that facilities (perhaps with the help of engineering consultants) might be better able to assess their own relative GHG performance once they have access to the US Program data of their peers. I call this *benchmarking*. My second hypothesis, expressed in alternate form, is as follows:

H2: *Facilities use their peers' GHG emissions disclosures for benchmarking (i.e., to*

[4]Online Appendix A1.1 describes the 766 letters about general stationary fuel combustion (the largest GHG emissions source) that the EPA received after proposing the US Program. The vast majority are from manufacturers arguing that reporting requirements are excessive and that reporting granularity should be no finer than the facility level, owing to financial and proprietary cost concerns. Environmental advocates submitted seven letters pushing for more comprehensive emissions reporting. A handful of state government agencies submitted letters requiring clarification of the rules, and a few GHG consulting firms made recommendations for measurement methods. No letters were from the investing community.

better assess their own GHG performance).

Benchmarking could be driven by pressure from environmentally conscious stakeholders, who have shown a concern for relative ESG performance [Clarkson, Li, Pinnuck, and Richardson, 2015; Hartzmark and Sussman, 2019]. Fung and O'Rourke [2000] describe how journalists and environmentalists fixated on the worst TRI performers. Facilities could benchmark their GHG emissions because they expect that outsiders will likewise benchmark these emissions.

Firms take cues from their peers' disclosures in financial contexts [Foster, 1989; Shroff, Verdi, and Yu, 2014]. They also occasionally forgo NPV-positive energy-efficiency improvements, possibly due to incomplete information [McKinsey Global Energy and Materials, 2009; Gerarden, Newell, and Stavins, 2017]. Therefore profit and efficiency motives could also drive benchmarking. Grennan and Swanson [2020] and Fetter et al. [2022] highlight such behavior in fracking and hospital settings. External pressure and profit/efficiency motives could coexist and interact, making it difficult to disentangle them. For example, external pressure could lead firms to discover profitable improvements. H2 therefore considers benchmarking generally.

4 GHG Emissions Reductions Following Disclosure

4.1 Control Sample

A credible estimation of the US Program's real effects must account for other factors that could lead facilities' GHG emissions to decline (e.g., changes in customer demand, input prices, and available production methods). To this end, I use Canadian facilities as a control group. Canadian facilities emitting over 100,000T (50,000T) CO_2e have been reporting their GHG emissions to Environment Canada since 2004 (2009). The scope of both reporting programs allows me to examine the US Program's effect across a wide range of industries.[5]

[5]Other potential control groups include US facilities in state-level Programs, US power plants, and facilities owned by firms reporting to the Carbon Disclosure Project. Online Appendix A2 discusses these groups

Several features of the United States and Canada support their comparability in terms of facility-level GHG output. First, both are developed countries, and the United States is by far Canada's largest trading partner; Online Appendix A2 Figure A1 shows that yearly percentage changes in both countries' GDPs move together closely. Thus many general supply and demand shocks that affect GHG emissions should affect both countries similarly. Second, the countries are culturally similar. Online Appendix A2 Figure A2 shows the co-movement of climate change interest in both countries (measured using Google Trends). This suggests that both countries' facilities face similar patterns in public pressure about GHG emissions. Third, both countries' energy markets are highly integrated through an extensive oil and gas pipeline network. Shocks to fossil fuel demand and supply should therefore propagate across both countries. Finally, the countries' environmental regulators co-operate in terms of non-climate change issues, which affects facilities' production costs and technologies used and, in turn, their GHG emissions [Weiss, 1998].

If the comparability arguments above hold, the time-series of US and Canadian facilities emissions should bear out parallel trends prior to US Program disclosure. Figure 2a supports the validity of the parallel trends assumption. The figure plots mean logged GHG emissions by country and year for this study's sample. The two countries' facilities' emissions share similar changes from 2010 to 2011, after which US facilities' emissions steadily decline. To assess parallel trends going further back, I estimate CO_2 emissions for a subsample of US facilities for the years 2008 to 2013. Section 6.3 discusses the estimation process. Figure 2b shows that the trends in both countries' facility CO_2 emissions are roughly parallel prior to disclosure and diverge noticeably following disclosure.

The availability of Canadian Program data does not necessarily negate the usefulness of US Program data for benchmarking. First, the US Program requires granular unit- and process-level disclosures, while the Canadian Program does not. Second, US facilities can combine US Program data with their knowledge about local peers (details about their output

and highlights institutional features that hinder their use as control groups.

of goods, regulatory pressures, etc.) to set more relevant benchmarks.[6] US facility managers arguably have less expertise about more distant Canadian facilities [Engelberg, Ozoguz, and Wang, 2018].

4.2 Data and Descriptive Statistics

The US and Canadian Program's facility-level GHG emissions spreadsheets provide the main data used in this study.[7] Power plants (NAICS code 2211) are excluded because they are subject to detailed reporting requirements prior to the US Program. They also face substantial regulatory incentives and scrutiny related to GHG emissions (see Online Appendix A2). Municipal waste facilities (NAICS code 5622) are excluded because they emit methane, with the emission rate driven by waste composition and by ambient conditions, including pressure, rainfall, and temperature [Héroux, Guy, and Millette, 2010; Santhosh, Lakshmikanthan, and Sivakumar Babu, 2017]. These factors potentially manifest differently across the United States and Canada. I exclude California facilities because California strictly regulates environmental issues, raising concerns about comparability with the rest of the sample (see Online Appendix A2). Lastly, noncorporate entities are excluded because benchmarking appeals to a notion of competition with peers. For noncorporate entities, it is unclear what the ultimate operating objective is (e.g., customer service, cost-minimization, profit-maximization, employment, or social welfare).

Table 1 shows data coverage across a range of industries and states/provinces. The

[6]For example, the manager of a US cement making facility might gauge how much cement a local peer produces as they compete in the same market. When US Program data become public, the manager can then assess whether his or her own facility uses more or less fuel, per unit of cement produced, than the peer.

[7]In 2011, GHGs from 10 additional source categories (e.g., electronics manufacturing, fluorinated GHG production) became applicable for US Program reporting. Emissions from these source categories are excluded to maintain comparability across years and because several of these categories were not included in the Canadian Program.

To code facility ownership type, data are obtained from the US and Canadian Programs about the highest parent owner from the respective country. I consider owners with $\geq 50\%$ facility ownership and use Bloomberg research reports to manually identify the global parent company. A global parent is coded as public if it has a GVKEY in the Compustat database or has a quoted stock price. It is coded as noncorporate if the parent's name includes words such as 'county', 'state', 'city', or 'university'. The remaining global parents are coded as private. This process took between 50 to 100 hours, highlighting the difficulty (and likely imperfections) in cleanly aggregating US Program emissions to the firm level.

five most common industries and states/provinces account for 58% and 36% of the sample. Though many industries have zero Canadian observations, 88% share an industry-year with one or more observations from the other country. Table 2a shows that US facilities compose 90% of the sample and emit roughly half the amount of GHGs per facility ($e^{11.0-11.8} - 1 = -0.55$), reflecting the larger size of the US industrial sector and the US Program's lower GHG reporting threshold. US facilities faced a similar average gas price and fewer GHG efficiency regulations during the sample period.

4.3 Research Design

To test H1, which predicts a reduction in GHG emissions following US Program disclosure, I estimate the following difference-in-differences equation using OLS:

$$GHG_{it} = \beta_1 \mathbb{1}_{\{t \geq 2012\}t} + \beta_2 US_i + \beta_3 \mathbb{1}_{\{t \geq 2012\}t} * US_i + \gamma X_{it} + \eta_i + \eta_{kt} + \varepsilon_{it}. \tag{1}$$

I also conduct cross-sectional tests by estimating the following equation:

$$\begin{aligned} GHG_{it} =& \beta_1 \mathbb{1}_{\{t \geq 2012\}t} + \beta_2 US_i + \beta_3 \mathbb{1}_{\{t \geq 2012\}t} * US_i + \\ & \beta_4 \mathbb{1}_{\{t \geq 2012\}t} * US_i * \mathbb{1}_{\{\text{CROSS-SECTION}\}it} + \gamma X_{it} + \eta_i + \eta_{kt} + \varepsilon_{it}. \end{aligned} \tag{2}$$

i, k, and t index facilities, four-digit NAICS industries, and years 2010 to 2013. *GHG* is logged tons of CO_2e emitted. US indicates US-located facilities, $\mathbb{1}_{\{t \geq 2012\}}$ indicates years 2012 and onward (2011 and onward for Massachusetts' facilities, whose emissions were disclosed a year earlier through a state-level program), and $\mathbb{1}_{\{\text{CROSS-SECTION}\}}$ indicates US facilities of particular cross-sectional interest. GHG emissions are logged to account for scale differences across facilities; as such, β_3 approximates the percentage change in emissions following the US Program's first data disclosure. X denotes the control variables described below. The use of facility and industry-year fixed effects means the estimation accounts for persistent facility characteristics and industry-level shocks. The industry definition is granular (e.g., *3311: Iron and steel mills and ferro-alloy manufacturing*). Standard errors are clustered by industry-year to account for shocks cross-sectionally correlated within industries.

Logged GDP (value-added) is included at the two-digit NAICS-country-year level to account for industry-region-specific shocks to demand and supply. The year-lagged-logged regional natural gas price is included to account for variation in incentives to use natural gas, which is more CO_2-efficient than oil. Given that US natural gas production increased after 2005 on account of hydraulic fracturing, a concern is that greater availability of natural gas might lead US facilities to reduce their GHG emissions. I use prices from Dawn Ontario and AECO Storage in Canada's east and west, and Dominion South, Henry Hub, SoCal Border, and Kern River in the United States' northeast, south, west, and center. These data come from SNL Financial and Alberta Energy Regulator. Lastly, the number of efficiency incentives and regulations that are both applicable to a facility and implemented at the federal or state levels are included. The regulations typically relate to building energy use, and the incentives typically exist as rebates for energy efficiency improvements. These data come from North Carolina State University's DSIRE and Natural Resources Canada's Directory of Energy Efficiency and Alternative Energy Programs.

4.4 Emissions Reduction Findings

4.4.1 GHG Emissions Levels

Table 3 shows the results of estimating Equation 1, which tests for a GHG emissions reduction for US facilities following US Program disclosure. Moving from Column 1 to 2 shows that facility and year fixed effects explain a large portion of the GHG emissions variation. Column 3 shows that using industry-year fixed effects reduces the estimate on β_3 appreciably, highlighting the importance of controlling for industry-level shocks. Column 4 shows that including control variables slightly increases the estimated treatment effect. Column 4 provides the baseline result: consistent with H1, US facilities reduce emissions by

7.9% ($e^{-0.082} - 1$; p = 0.014), relative to Canadian facilities, following the first disclosure of US Program data in 2012.[8] This is a 4.1% standard deviation change in US facilities' logged GHG emissions or a 21% standard deviation change when GHG emissions are residualized against the fixed effects (see DeHaan, 2021).

To explore the GHG emissions reduction dynamically, I estimate a version of Equation 1 in which the US dummy interacted with a sequence of dummies that indicate each sample year except for 2011 (the reference year). Figure 3a plots this sequence of interactions. It supports H1 by showing that the relative emissions differential stays close to zero until the US Program's first disclosure, after which US facilities display a relative decline in emissions. Figure 3a also supports the parallel trends assumption.

Online Appendix A4 Table A2 shows the GHG emissions reduction result is not driven by oil and gas facilities or the few states that have GHG emissions pricing. It is also robust to including noncorporate facilities and municipal waste facilities, extending the sample window forward to 2016, and using an entropy-balanced panel of facilities. Another concern might be that emissions reductions are unlikely to materialize in a year. Longer lead-times do exist for emissions reduction changes, such as retrofits of complex equipment for a new fuel type—this is consistent with the continued decrease in emissions in Figure 3a in 2013. However, less complex retrofits or replacements, maintenance, calibration and optimization, and other behavioral changes can be implemented within a year.[9]

[8]For comparison, related papers—Downar et al. [2021], Jouvenot and Krueger [2021], and Yang et al. [2022]—estimate 14%, 12%, and 7% emissions reductions. These studies capture the effects of dissemination and aggregation of existing information, while my paper estimates a treatment effect for facilities that largely had no emissions information available elsewhere.

Online Appendix A3 describes long-run emissions patterns following disclosure. Facilities that eventually exit the US Program see a large emissions reduction of 33.2%. Later entrants to the US Program see a significant emissions increase of 10.3%, consistent with these facilities often being growing ones.

[9]For example, Colket et al. [2012] report that a combustion control system and a sensor package retrofit for a 25 MMBtu boiler at Watervliet Arsenal in New York State resulted in a 4% reduction in CO_2 emissions. Full deployment, including commissioning, was completed in three days.

4.4.2 Offshoring, Reduced Economic Activity, and Investments

Facilities could reduce reported GHG emissions by moving emissions generation abroad, beyond the US Program's reach. This explanation would not predict emissions reductions in industries with less geographically mobile production (i.e., where the produced goods have a high weight-to-value ratio or are fragile, or the raw materials are immobile). To address this possibility, I estimate a version of Equation 2 that measures an incremental emissions reduction for facilities in industries with less mobile production (e.g., cement and concrete).[10] Table 4 Column 1 shows that emissions reductions are concentrated in facilities with relatively immobile production, which is inconsistent with the idea that reductions are driven by offshoring.

Facilities could also achieve lower GHG emissions by curbing economic activity. To address this possibility, I first produce a facility-level measure of carbon intensity (CO_2 emissions per unit of goods produced). CO_2 is largely produced by burning fuels. Online Appendix A5 details this measure. Summarized briefly, I use the emissions of pollutants that do *not* largely arise from burning fuels as a proxy for the quantity of goods produced. These data come from the EPA's National Emissions Inventory and Emissions Inventory System (US NEI/EIS), which provides information about US facilities' hazardous air emissions (not GHGs), and Environment Canada's National Pollutant Release Inventory. Importantly, the US data are available at the facility-process-pollutant level. As an example, when paper and pulp facilities convert wood chips to pulp, a chemical treatment produces volatile organic compounds (VOCs). The idea is that producing the same amount of paper will produce the same amount of VOCs, even if the fuel combustion step that heats the wood chips becomes more efficient. Thus CO_2 emissions can be scaled by VOC emissions to produce a carbon intensity proxy for paper and pulp facilities.

[10] These industries are cement and concrete product manufacturing; converted paper product manufacturing; glass and glass product manufacturing; metal ore mining; natural gas distribution; nonmetallic mineral mining and quarrying; oil and gas extraction; pipeline transportation of natural gas; pulp, paper, and paperboard mills; support activities for mining; and water, sewage, and other systems.

Carbon intensity should not respond to US Program disclosure if facilities curb economic activity to reduce GHG emissions. To test this implication, I estimate a version of Equation 1 that uses logged carbon intensity as the dependent variable. For this and all other dependent variables based on ratios, I winsorize at the first and 99$^{\text{th}}$ percentiles. I require a balanced panel because the US reporting threshold for noncombustion pollutants varies across years.[11] My preferred specification excludes *GDP* and *REGULATIONS* as control variables. *GDP* captures industry supply and demand shocks, which should not affect carbon intensity. *REGULATIONS* varies at the state level, as does the regulation of noncombustion pollutants [Chemical Watch, 2019]. If energy efficiency regulation/incentives and (non-GHG) pollution regulation are jointly determined by states, *REGULATIONS* has a potentially complicated relation with carbon intensity. Column 2 shows that US facilities' carbon intensity falls by roughly 7% in response to US Program disclosure; this is similar to the emissions *level* change of -7.9%, suggesting that US facilities do not reduce GHG emissions by simply curbing economic activity. Column 3 shows that the carbon intensity reduction remains similar in magnitude but loses statistical significance when including all control variables ($p = 0.14$).

To explore the idea that facilities instead make tangible investments to reduce GHG emissions, I examine firm-level capital expenditures (CAPEX). To aggregate my data to the firm-level, I take weighted averages of the facility-level independent variables described in Section 4.3, with weights based on each facility's 2011 GHG emissions. Because some firms have facilities in both Canada and United States, the *US* variable becomes continuous, ranging from zero to one. I then estimate the following OLS equation:

$$CAPEX_{jt} = \beta_1 \mathbb{1}_{\{t \geq 2012\}t} + \beta_2 US_j + \beta_3 \mathbb{1}_{\{t \geq 2012\}t} * US_j + \gamma X_{jt} + \eta_j + \eta_{kt} + \varepsilon_{jt}. \tag{3}$$

[11] 2008, 2011, and 2014 are comprehensive years characterized by lower emissions reporting thresholds, and thus they cover more facilities. These years' data form part of the EPA's National Emissions Inventories. Emissions data for the noncomprehensive years—2009, 2010, 2012, and 2013—are found in the US Emission Inventory System database and were provided directly by the EPA.

j indexes a firm, and $CAPEX$ is capital expenditures divided by lagged assets. X contains the firm-level aggregates of the facility-level controls and a firm's market capitalization, leverage, and market-to-book ratio (to capture potential firm-level economies of scale in managing emissions, financing constraints, and growth opportunities; Kogan and Papanikolaou, 2014; Myers, 1977).

Consistent with the idea that firms invest to reduce GHG emissions, Column 4 shows that, following US Program disclosure, US firms' CAPEX increases by 2.5% of assets relative to Canadian firms. Tangible investments and other workflow changes could affect operational performance (e.g., by changing cost structure aspects). Consistent with Downar et al. [2021], however, Column 5 shows no significant changes in gross margins for US Program firms.[12] These firm-level results require caveats. First, financial statement variables capture economic activity outside of the United States and from nonfacility aspects of a business, but US Program data do not. Additionally, many manufacturing costs are recognized when sales are made, yet the associated GHG emissions can occur in earlier years. Therefore, these tests corroborate that facilities made real changes following US Program disclosure, but they do not quantify the financial costs and benefits of these changes.

5 Benchmarking of GHG Emissions

This section presents evidence supporting H2, which states that facilities use their peers' GHG emission disclosures to benchmark their own GHG performance.

5.1 Emissions Dispersion

Grennan and Swanson [2020] show that the dispersion of negotiated prices paid by hospitals for supplies shrinks after the hospitals gain access to the purchase price history of

[12] Online Appendix A6 Table A4 shows that, in the long run, the CAPEX increase becomes statistically insignificant, consistent with investments being a one-off. The change in gross margins remains statistically insignificant.

their peers. Similarly, Berger, Choi, and Tomar [2023] document lower profitability dispersion when Korean firms provide detailed cost disclosures. If benchmarking under the US Program facilitates a convergence in practices, I expect there to be less dispersion in GHG emissions after disclosure. I test this prediction by estimating the following OLS equation:

$$GHG\_DISP_{ckt} = \beta_1 \mathbb{1}_{\{t \geq 2012\}t} + \beta_2 US_c + \beta_3 \mathbb{1}_{\{t \geq 2012\}t} * US_c + \gamma X_{ckt} + \eta_{ck} + \eta_t + \varepsilon_{ckt}. \quad (4)$$

c indexes country. GHG_DISP is the within-industry-country standard deviation, or 90^{th}-10^{th} percentile difference, of raw GHG emissions (in 1,000T CO_2e). X contains the standard deviations of GAS_PRICE and $REGULATIONS$. I include industry-country and year fixed effects. Columns 1 and 2 of Table 5 provide high-level evidence consistent with benchmarking. They show that the within-industry emissions standard deviation and 90^{th}-10^{th} percentile difference decline for US industries, relative to Canadian ones, after US Program disclosure. These reductions are 20% and 31% of the mean US industry, pre-period emissions dispersion values ($p = 0.044$; $p = 0.038$). These results are silent, however, about whether poor GHG performers reduce their emissions more than others. Specifically, high-emitting facilities might nevertheless emit a low amount of GHGs per unit of goods produced.

5.2 Relative Carbon Intensity as Revealed Through Disclosure

Under the benchmarking hypothesis, facility managers will use available US Program data—and not timelier data that is yet to be disclosed—to establish whether their facilities are more or less carbon intense than those of their peers. My next set of tests condition on emissions data in the same way. Given the previous results (i.e., emissions reductions coupled with lower emissions dispersion), I predict that facilities revealed, through US Program disclosure, to be more carbon intense than peers will have larger subsequent emissions reductions.

My starting point is the facility-level carbon intensity measure used in Section 4.4.2: CO_2

emissions scaled by emissions of a noncombustion pollutant proxy for the quantity of goods produced. For two reasons, I normalize this measure within industry-state so that the highest value becomes one (most carbon intense) and the lowest value becomes zero (most carbon light). First, it might be more useful to benchmark against industry-peers that compete in local markets. Second, many US states directly regulate VOCs (the noncombustion pollutant most commonly used by my measure) to a greater extent than federal regulations require [Chemical Watch, 2019]. I then test whether relative carbon intensity—based on 2010 values that were publicly disclosed at the start of 2012—predicts emissions reductions in 2012 by estimating the following OLS equation:

$$CH\_CO2\_2012_i = \beta_1 CARBON\_INT\_2010_i + \beta_2 CARBON\_INT\_2011_i + \gamma X_i + \eta_k + \varepsilon_i. \quad (5)$$

CH_GHG_2012 is the percentage change in 2012 CO_2 emissions, relative to 2011, $CARBON\_INT\_2010$ is peer-normalized carbon intensity based on 2010 values, X contains the year-lagged percentage change in natural gas price and the change in efficiency incentives and regulations. I include industry fixed effects. The observation level is a facility, and only US facilities are considered.

Table 5 Column 3 provides results consistent with benchmarking. Facilities that are revealed by US Program disclosure to be relatively carbon intense reduce their CO_2 emissions more in 2012. This pattern, however, could be driven by natural technological convergence or mean reversion in carbon intensity and not disclosure. Additionally, facilities might have been aware of their GHG performance without benchmarking and merely waited until disclosure in 2012 to determine the discount attached to GHG intensity by external stakeholders. Under these alternatives, 2011 carbon intensity would be a better, more relevant predictor of 2012 emissions reductions. From a disclosure perspective, however, the US Program data needed to construct 2011 carbon intensity were not public in 2012.

Column 4 presents the results when both 2010 and 2011 carbon intensities are included as regressors. Consistent with a disclosure/benchmarking effect, it shows that 2010 carbon intensity is significantly associated with 2012 CO_2 reductions but that 2011 carbon intensity is not: benchmarking requires observability of emissions data and not just their existence. Columns 5 and 6 limit the sample to facilities with above-industry-median carbon intensity. The results become more striking.

Columns 4 and 6 identify benchmarking by exploiting the staggering in time of US Program disclosure relative to the measurement date. Further, by exploiting purely within-US facility variation, they allay the concern that Table 3's results are driven purely by unobserved, time-varying differences between US and Canadian facilities.

5.3 Effect of a Benchmarking History

The next set of tests examines whether facilities with a propensity for benchmarking reduce their GHG emissions more following US Program disclosure. Bernard et al. [2020] produce a novel measure of across-firm information flows based on the extent to which firms acquire their rivals' financial information from the SEC's EDGAR database. Using their EDGAR-search data, I classify a facility as a benchmarker if its owner searches for an above-industry-median number of other firms' financial information.[13] I then estimate a version of Equation 2 that measures an incremental emissions reduction for benchmarker facilities.

The coefficient estimate on $\mathbb{1}_{\{\text{BENCHMARKER}\}}$ in Table 6 Column 1 shows that the incremental emissions reduction for benchmarkers is insignificantly negative. This test uses few observations, however, because of the low sample overlap between Bernard et al. [2020] and this study. Bernard et al. [2020] also provide a dataset that is five times larger but with a 60% firm-identifier accuracy. Column 2 provides the results when using this larger sample. The estimate of β_4 is similar in magnitude and gains statistical significance—benchmarkers reduce their emissions by 7.4% ($e^{-0.077} - 1$) percentage points more than nonbenchmarkers do. Column 3 presents the results from estimating a version of Equation 1 that measures

[13] I thank Darren Bernard, Terrence Blackburne, and Jacob Thornock for sharing their data.

a separate (non-incremental) emissions reduction for each tercile of US facilities based on their benchmarking. The emissions reduction increases monotonically by tercile ($p = 0.060$ for the difference between high and low terciles).

These results further support the benchmarking channel.[14] They also help to address the possibility that facilities did not benchmark but instead began implementing emissions reductions with long lead-times in 2010 (prior to disclosure) due to anticipated external pressure. This alternative explanation does not explain why the emissions reductions are larger for benchmarkers. A caveat for these results is that they exploit cross-sectional variation that is not randomly assigned at the time of the US Program (i.e., whether a facility's owner chooses to be a benchmarker). The results should therefore be viewed in the context of the other benchmarking results in Section 5.

5.4 Process Convergence

The last set of benchmarking tests explores whether the processes that facilities employ converge after US Program disclosure. Returning to the process-level US NEI/EIS data described in Section 4.4.2, I focus on processes that burn fossil fuels or waste.[15] In the spirit of Fetter et al. [2022], I then compute a Jaccard process-similarity index between each US facility and its (hypothetical) representative state-industry peer in a given year. Online Appendix A8 details this index's construction, but briefly, for a facility with one peer, the index divides the number of unique processes that both facilities employ by the number of

[14] In terms of information flows between connected entities, Online Appendix A7 Table A5 shows that 31% of the variation in (idiosyncratic) GHG emissions changes following US Program disclosure is common to facilities owned by the same firm. However, it is difficult to attribute this common variation purely to information flows across same-firm facilities [Manski, 1993]. Online Appendix A7 Table A6 shows that emissions changes for connected firms do not significantly predict emissions changes for focal firms, defining connection based on common investors and overlapping board members. Online Appendix A7 Table A7 shows that emissions reductions around US Program disclosure are not significantly different when considering the following two proxies for information frictions within the firm: i) the size of management guidance errors and ii) whether a facility is located far from its owner-firm's headquarters. In sum, the evidence about information flows between connected entities is less conclusive.

[15] Online Appendix A5 describes how I identified these processes. An example of a fuel combustion process is *External Combustion Boilers: Commercial/Institutional: Bituminous/Subbituminous Coal: Pulverized Coal: Wet Bottom (Subbituminous Coal).*

unique processes they together employ (i.e., the intersection of their processes used divided by the union). When a facility has multiple peers, this computation also includes weights that capture the prevalence of a process across peer facilities.

Table 7a Row 1 presents the average *percentage* change in similarity when moving from 2010 to 2013 (the start and end of the main sample). Of the distinct processes used by a facility *or* its peers, the fraction used by the facility *and* its peers increases by roughly 10% (not percentage points), on average, after US Program disclosure (p = 0.008). Meanwhile, the average similarity *level* changes insignificantly from 0.281 and 0.276 (p = 0.27). These percentage and level changes imply that facilities with low initial similarity to peers become more similar to their peers (relative to facilities that are already fairly similar to their peers).[16] To link these results to emissions reductions, I estimate a version of Equation 1 that measures a separate emissions reduction for each tercile of US facilities based on their 2010 similarities with representative peers.

Consistent with the earlier argument, Table 7b Column 1 shows that the US Program treatment effect is largest for facilities in peer groups marked by low initial similarity and fades as initial similarity grows (p = 0.035 for the difference between high and low terciles). Benchmarking is more useful when there is a pre-existing diversity in the processes used among peers. Similarly, Bernard et al. [2020] show that information flows better predict future R&D mimicking among firms with low product similarity.

To explore the benchmarking prediction that facilities shift their processes toward (away from) those of their carbon light (carbon intense) peers, I re-employ the relative carbon intensity measure used in Section 5.2. When producing a facility's 2010 representative peer, I now require the constituent peer facilities to have a 2010 carbon intensity rank ≤ 0.33 (i.e., carbon light peers). I then compute Jaccard similarities between i) a facility in 2010 and its 2010 carbon light representative peer and ii) that facility in 2013 and its 2010 carbon light

[16]The distinction between level changes and percentage changes in Jaccard similarity is important. If focusing on level changes, the results in this subsection become statistically insignificant. Some industries, however, might have more potential processes to choose from, giving them a different similarity baseline. A focus on percentage changes accounts for these baselines.

representative peer. Table 7a Row 2 presents the average percentage change when moving from the former similarity to the latter. After US Program disclosure, facilities become 6.3% proportionally more similar to their carbon light peers (as these peers were in 2010; p = 0.048). I then recompute the percentage change in Jaccard similarity, except considering a facility's carbon intense representative peer (whose constituent facilities have a 2010 carbon intensity rank > 0.67). Table 7a Row 3 shows that facilities become 3.4% proportionally less similar to their carbon intense peers after US Program disclosure (p = 0.037).

Table 7b presents the results from estimating a version of Equation 2 that connects these last process-similarity results to CO_2 emissions changes. Column 2 shows that, for facilities that shift their processes toward their carbon light peers, the incremental emissions reduction is a statistically insignificant 6.8% ($e^{-0.070} - 1$; p = 0.142). Column 3 provides a similar inference when focusing on facilities that also become less like their carbon intense peers (10.6% incremental reduction; p = 0.212).

My process convergence results echo those of Fetter et al. [2022], who show that mandatory disclosure induces fracking firms to shift their fracking chemical compositions toward those of their more productive peers. There are some caveats. First, there is likely to be variation within a process as defined by the US NEI/EIS. Thus, some process changes will not appear in the data, nor will behavioral changes (e.g., maintenance and calibration). This might explain why an emissions reduction is observed for the broader sample of facilities whose processes did not change in the US NEI/EIS data. Second, the process convergence tests do not use a Canadian control sample (for which process-level data are not available). Thus, they are not provided as conclusive evidence of benchmarking but rather to give context to the other benchmarking results. Third, these results do not disentangle whether process convergence results from facilities embarking on a technological search after benchmarking or whether peers' GHG disclosures themselves provide technological cues. By whichever pathway, benchmarking prompts facilities to make real changes.

6 Supplementary Analyses

I now provide supplementary evidence about the US Program's effects. I first explore sources of external pressure that might motivate emissions reductions and benchmarking.

6.1 External Pressure: Concern About Future Legislation

A major US Program aim is to aid future potential GHG-related rule-making (see Section 2). Thus, I study whether concern about future GHG legislation motivates emissions reductions. Once US facilities learn about their own GHG performance, they might self-regulate to avoid facing stringent legislation (e.g, Maxwell et al., 2000; Suijs and Wielhouwer, 2019). Sanchez, Matthews, and Fischbeck [2012], however, argue that the US Program would be unlikely to yield benefits because US climate policy lacked momentum at the time.

I proxy for GHG legislation pressure by exploiting geographic variation in political support for climate-progressive legislation. For a given legislator-year, I use League of Conservation Voters' Scorecards to compute the fraction of climate-progressive bills that federal legislator supported from 2008 to that year. For senators, I then take the within-state average. Highlighting the partisan nature of climate politics (e.g., McCright and Dunlap, 2011), Democratic Party membership explains 86% of senators' and House representatives' climate-progressiveness scores.

I then estimate a version of Equation 2 that measures an incremental emissions reduction for facilities whose legislators have above-industry-median climate-progressiveness. Table 8a Column 1 shows a 5.1% ($e^{-0.052} - 1$) incremental emissions reduction for facilities with climate-progressive senators, following US Program disclosure. Column 2 reveals no statistically significant impact of representatives' progressiveness. Column 3 considers both groups of legislators simultaneously and yields the same inferences as Columns 1 and 2. Concern about GHG legislation outcomes at the Senate appears to motivate emissions reductions.[17]

[17] Online Appendix A7 Table A5 shows that emissions reductions associated with senatorial progressiveness are not common to facilities in other states owned by the same firm. That is, when reacting to concern about

Senators frequently turn over and can adjust their stances on issues. To assess the impact of this variation on emissions, I estimate the following OLS equation:

$$\begin{aligned} GHG_{it} = &\beta_1 \mathbb{1}_{\{t \geq 2012\}t} + \beta_2 US_i + \beta_3 \mathbb{1}_{\{t \geq 2012\}t} * US_i + \beta_4 SENATE\_CC\_SCORE_{it} + \\ &\beta_5 \mathbb{1}_{\{t \geq 2012\}t} * SENATE\_CC\_SCORE_{it} + \gamma X_{it} + \eta_i + \eta_{kt} + \varepsilon_{it}. \end{aligned} \tag{6}$$

SENATE_CC_SCORE is the raw senatorial climate-progressiveness score described above (its value is set to zero for Canadian facilities). The use of facility fixed effects, which absorb average location effects, makes Equation 6 akin to a changes specification. The estimate of β_4 in Column 4 shows that changes in senatorial progressiveness are insignificantly negatively associated with GHG emissions before US Program disclosure. The estimate on β_5, however, shows that this association becomes larger and statistically significant after disclosure. A one standard-deviation change in *SENATE_CC_SCORE* (33.2%) implies a 3.9% GHG emissions reduction under disclosure—facilities respond more to concern about legislation once they can benchmark using peer disclosures.

Facilities with political connections could face different emissions reduction incentives. Facilities might use their connections to stifle future GHG legislation [Gelles, 2022; Weiss, Lefton, and Lyon, 2010]. Connected facilities could then exhibit a muted emissions response to disclosure. Alternatively, in exchange for preferential treatment in other domains (e.g., Cooper, Gulen, and Ovtchinnikov, 2010; Tahoun, 2014), facilities might reduce GHG emissions to cast their local legislators in a favorable environmental light. Connected facilities might also be more concerned about legislation generally [Hassan, Hollander, Van Lent, and Tahoun, 2019]. In these latter cases, politically connected facilities might reduce GHG emissions more once they can benchmark using US Program data. To explore these possibilities,

GHG legislation, firms consider their individual facilities' visibility to legislators.

I estimate the following OLS equation:

$$\begin{aligned} GHG_{it} = &\beta_1 \mathbb{1}_{\{t \geq 2012\}t} + \beta_2 US_i + \beta_3 \mathbb{1}_{\{t \geq 2012\}t} * US_i + \beta_4 \mathbb{1}_{\{LEGIS\_CONN\}it} + \\ &\beta_5 \mathbb{1}_{\{t \geq 2012\}t} * \mathbb{1}_{\{LEGIS\_CONN\}it} + \gamma X_{it} + \eta_i + \eta_{kt} + \varepsilon_{it}. \end{aligned} \tag{7}$$

$\mathbb{1}_{\{LEGIS\_CONN\}}$ indicates US facilities owned by firms that made campaign contributions to the senators or representatives of those facilities—I consider these facilities politically connected. To identify contributing firms, I use data provided Hassan et al. [2019], which is based on data from OpenSecrets.org.[18] Only public firms in my sample can be matched to these data, and thus I exclude US facilities not owned by public firms.

Table 8b Column 1 shows that a senator connection does not significantly affect facilities' GHG emissions, either pre- or post-US Program disclosure (considering $\hat{\beta}_4$ and $\hat{\beta}_5$). Column 2 shows that a representative connection has a significantly greater negative effect on GHG emissions following disclosure (although $\hat{\beta}_4 + \hat{\beta}_5$ is insignificantly negative). Column 3 considers connections to senators and representatives simultaneously and yields the same inferences as Columns 1 and 2. After being able to benchmark using US Program disclosures, representative connected facilities worry more about GHG legislation, or they try to cast their representative in a favorable environmental light. Although the idea of political connections granting preferential treatment seems socially harmful, the contribution facilities provide in this latter case (lower GHG emissions) seems socially beneficial.[19]

A caveat for this subsection's results is that they are based on cross-sectional variation

[18] I thank Tarek Hassan, Stephan Hollander, Markus Schwedeler, Ahmed Tahoun, and Laurence Van Lent for sharing their data. Regarding timing, if Firm A contributes to Senator B's successful November 2010 election campaign, $\mathbb{1}_{\{LEGIS.\_CONN.\}}$ indicates Firm A's facilities in Senator B's state in 2011 and the remaining years of Senator B's term.

[19] That facilities appear more responsive to the Senate regarding legislation pressure (Table 8a) and more responsive to the House regarding connections (Table 8b) is consistent with the literature. Diermeier, Keane, and Merlo [2005] suggest that the nonpecuniary, policy-shaping rewards to being in Congress are large, and especially so for senators. Meanwhile, Tahoun and Van Lent [2019] find that voting in the House is associated with representatives' personal wealth interests, but the equivalent association is weaker for senators. Relatedly, Cooper et al. [2010] show that firms contributing to legislators have positive future abnormal returns, and that there is an incremental House effect after controlling for the Senate effect.

that is not randomly assigned at the time of the US Program (i.e., a facility's location and its owner's choice to contribute to a legislator's campaign). Thus they should be considered alongside the US Program's broad purpose to provide guidance for future rule-making.

6.2 External Pressure: Investor Scrutiny

Although Section 3 describes why the US Program might not create a GHG emissions action-cycle at the firm level, given the recent growth in ESG-investing, the willingness of capital market regulators to require emissions disclosures, and the tendency of capital markets to make across-firm comparisons [De Franco, Hope, and Larocque, 2015], I explore whether investor pressure moderates the GHG emissions response to US Program disclosure.

I begin by measuring firm-level GHG intensity for 2010 (constructible by the public in 2012) by aggregating facility emissions to the firm level and then dividing by cost of goods sold (a proxy for economic activity). I then split US-facility-only firms into groups with above and below industry-median 2010 GHG intensity. Figure 4 plots the average buy-and-hold industry-adjusted returns for both groups of firms around US Program disclosure. With the exception of a return runup for GHG intense firms from days -50 to -30 in Figure 4a, the two return time-series track each other and do not suggest a differential stock market reaction to disclosure. Online Appendix A9.1 Table A8 presents the related regression-based evidence. Echoing Figure 4a, the incremental buy-and-hold returns for GHG intense firms around disclosure are statistically insignificant.[20]

[20] In Online Appendix A9.1 Table A9, I study the holdings of ESG-focused mutual funds and shareholder resolutions; I find no strong evidence of investor pressure. Online Appendix A9.1 Table A10 shows that the GHG emissions reduction around US Program disclosure is larger for facilities owned by firms that are nonpublic or that report higher environmental political risk and climate change exposure to investors. Because these cross-sectional results can be interpreted in multiple ways, I place less weight on them.

Online Appendix A9.2 Table A11 shows no significant evidence of pressure from the public or customers. The emissions reduction is not significantly different for facilities in i) states with higher intensities of belief in human-caused climate change and ii) industries with relatively more business-to-customer economic activity.

Regarding profit/efficiency motives, Online Appendix A10 Table A12 shows that emissions reductions for US facilities near the Canadian border are muted when they have a nearby Canadian peer. These US facilities possibly benchmarked their GHG emissions against their Canadian peers prior to the US Program; given that their emissions were not subject to external pressure via disclosure at that time, this suggests benchmarking for profit/efficiency motives. However, the muted response is statistically insignificant—few US facilities

6.3 GHG Emissions Responses Prior to US Program Disclosure

To understand the US Program's effects more completely, I explore emissions responses prior to disclosure. Facilities knew as early as April 2009 that their emissions would be publicly disclosed, and thus they might have anticipated stakeholder pressure (e.g., Fiechter, Hitz, and Lehmann, 2022). The US Program's measurement requirements might also have improved managers' own-firm GHG information sets (e.g., Shroff, 2017). These forces could have led facilities to curb emissions prior to disclosure. Grennan and Swanson [2020] and Shroff [2020] also disentangle pre- and post-disclosure responses. In terms of information creation, my setting less resembles that of Grennan and Swanson [2020], where hospitals were arguably aware of the prices they paid to suppliers, and more resembles that of Shroff [2020], where firms possibly learned about their auditors' quality following PCAOB inspections.

To address a key empirical challenge—the absence of GHG emissions data for years prior to US Program implementation (e.g., for 2008 and 2009)—I estimate US facilities' CO_2 emissions by leveraging the physical relations underlying fossil fuel combustion. The key idea is that the same fuel burned under the same conditions should produce carbon dioxide (CO_2; a GHG) and carbon monoxide (CO; toxic, not a GHG, and disclosed since 2008 in the US NEI/EIS) in constant proportions [Gurney et al., 2009]. The estimation is detailed in Online Appendix A11.1, and is briefly as follows. I employ a Bayesian linear model to relate US facilities' logged CO_2 emissions to their process-level CO emissions. The model inputs are 1) facility-level CO_2 emissions from the 2014 US Program, 2) process-level CO emissions from the 2014 NEI/EIS, and 3) a set of priors about the process-level CO_2-CO relations, provided by Gurney et al. [2009]. I then combine the estimated CO_2-CO relations with US facilities' process-level CO emissions from 2008 to 2013 to produce estimates of these facilities' logged CO_2 emissions.[21] To measure the US Program's effect prior to disclosure, I

provide the needed variation by having nearby Canadian peers—leaving the result only suggestive.

[21]The estimation relies heavily on US Program data and hence could not be conducted prior to the US Program. Online Appendix A11.1 Figure A4 shows that the Bayesian estimates explain 42% of the variation in out-of-sample actual values. Variation in factors such as equipment specification, CO abatement technology, and fuel carbon content reduces the goodness of fit. For reference, naive OLS estimates explain

extend the sample window back to 2008 and estimate the following OLS equation:

$$
\begin{aligned}
CO2_{it} = &\beta_1 \mathbb{1}_{\{t \geq 2010\}t} + \beta_2 \mathbb{1}_{\{t \geq 2012\}t} + \beta_3 US_i + \beta_4 \mathbb{1}_{\{t \geq 2010\}t} * US_i + \beta_5 \mathbb{1}_{\{t \geq 2012\}t} * US_i + \\
&\gamma X_{it} + \eta_i + \eta_{jt} + \varepsilon_{it}.
\end{aligned} \tag{8}
$$

CO2 is logged T CO_2 emissions, estimated as above for US facilities and as reported by Canadian facilities. β_4 captures facilities' emissions responses prior to US Program disclosure. A balanced panel is required because the Canadian Program's reporting threshold fell from 100,000T to 50,000T CO_2e in 2009 and because the CO reporting thresholds are lower in 2008, 2011, and 2013 (see Footnote 11).

Table 9 Column 1 presents the results when facility and year fixed effects are included, and Column 2 includes control variables and industry-year fixed effects. Both columns show no significant emissions response in 2010 and 2011, when facilities begin measuring and reporting emissions (absent disclosure); however, both show a significant emissions reduction after disclosure in 2012, consistent with Table 3. The Column 2 estimate of the emissions reduction following disclosure is 11.2% ($e^{-0.119} - 1$; p = 0.019). Figure 3b, the expanded sample window analog of Figure 3a, mirrors this pattern of US emissions relative to Canadian emissions. Figure 3b also provides further support for the parallel trends assumption.[22]

To assess pre-disclosure emissions responses without using estimated data, I study firm-level emissions data voluntarily submitted to the Carbon Disclosure Project (CDP), a non-profit organization that surveys firms annually about their GHG performance. During my sample period, CDP surveys are sent to S&P 500 firms and the 200 largest Canadian firms

35% of the variation in actual values (or 8% if treating negative, infeasible OLS fitted value as zeroes). The Bayesian approach fares better because it assigns sensible distributions to the parameters (e.g., no negative support) and incorporation of priors that shrink noisy parameter estimates toward plausible values.

[22]Because estimated CO_2 emissions are based on carbon monoxide data that come from the EPA's NEI/EIS, these tests also support the credibility of US Program data. For this paper's results to be explained by misreporting, facilities would have to be shown to misreport not only their GHG emissions but also their toxic pollutant emissions and the processes they employ (which are also studied in Section 5.4). Combined with the enforceability of the US Program and US NEI/EIS under the US Clean Air Act, these tests make the misreporting explanation unlikely, given the extent of sustained legal and reputation risk it would entail.

by market capitalization. I collect CDP data from years 2008 to 2013 for firms within the industries in my US Program sample. I then estimate the following OLS equation:

$$\begin{aligned} GHG_{jt} = & \beta_1 \mathbb{1}_{\{t \geq 2010\}t} + \beta_2 \mathbb{1}_{\{t \geq 2012\}t} + \beta_3 US_j + \beta_4 \mathbb{1}_{\{t \geq 2010\}t} * US_j + \beta_5 \mathbb{1}_{\{t \geq 2012\}t} * US_j + \\ & \gamma X_{jt} + \eta_j + \eta_{kt} + \varepsilon_{jt}. \end{aligned} \tag{9}$$

GHG is logged tons of Scope 1 (on-site, non-vehicular) GHG emissions reported to the CDP, and X contains a firm's market capitalization, leverage, and market-to-book ratio as controls (as in Section 4.4.2). I use a balanced panel to mitigate concerns about selection effects related the CDP participation decision.

Table 9 Column 3 provides the results obtained when using firm and year fixed effects, and Column 4 adds industry-year fixed effects and control variables. Although the Column 4 estimate on β_5 is statistically insignificant, the results corroborate those in Columns 1 and 2. Collectively, Table 9 highlights uncertainty around whether measurement and private reporting of GHG emissions leads to emissions reductions.[23] Yet, these same tests suggest disclosure-driven effects. I leave it to future research to make a stronger claim about pre-disclosure emissions responses.

7 Conclusion

I explore whether the mandatory, granular disclosures of the US Greenhouse Gas Reporting Program (US Program) lead to GHG emissions reductions. I find that US facilities reduce emissions by 7.9% following US Program disclosure. In contrast to much of the related

[23]For instance, the estimate of $\mathbb{1}_{\{t \geq 2010\}} * US$ in Table 9 Column 2—one of the more precise estimates—gives a 95% confidence interval of (-3.1%, 11%). In Online Appendix A11.2, I assess pre-disclosure emissions responses by exploiting only within-US facility variation. Using the same techniques as in Section 5.2, I show that 2010 carbon intensity rank does not significantly predict CO_2 emissions reductions in 2011, as might be expected if US Program measurement and private reporting to the EPA drives emissions reductions.

Columns 3 and 4 also raise the question of whether highly aggregated CDP data substitute for granular US Program data in terms of driving emissions reductions. Online Appendix A12 explores facilities' emissions reductions by CDP participation and disclosure status, and by whether facilities had peers participating in the CDP. The results support the nonsubstitutability of US Program information with CDP information.

research, I estimate a treatment effect for facilities that mostly had no other GHG emissions information in the public domain. My emissions and process-based tests are consistent with facilities using the US Program data of their peers for benchmarking. Supplementary analyses suggest that concern about GHG legislation partly motivates emissions reductions and that measurement alone (absent disclosure) does not significantly reduce emissions.

My paper contributes to the literature on the real effects of environmental, social, and governance (ESG) disclosures. It also has policy relevance. The main implication is that an ESG disclosure mandate can produce real effects through benchmarking. Considering the nature of US Program disclosures, data granularity can facilitate this process while also improving the usefulness of disclosed data for stakeholders interested in a subset of a firm's activities (e.g., regional legislators interested in GHGs produced locally). However, aggregation of ESG information can also produce real effects (e.g., Christensen et al., 2017), suggesting a trade-off around the level of granularity.

My paper leaves important avenues for future research. First, it considers the US Program from a benefits perspective (i.e., emissions reductions). A potentially significant cost of the program is the potential stifling of innovation through disclosure of proprietary information (e.g., Breuer, Leuz, and Vanhaverbeke, 2022). Further work assessing this cost would allow for a more rounded view of the US Program's impact. Second, this paper examines the effect of a single reporting/disclosure event. Although recent work in this area is very beneficial, further research on other mandatory GHG disclosure settings will be valuable. Climate change affects many stakeholders, and variation in the institutional features studied in the future can highlight different economic channels that affect emissions patterns.

Appendix: Variable Description

Facility-level Analyses

Variable Name	Description
CARBON_INT	Logarithm of: CO_2 emissions in metric tons divided by non-combustion pollutant in metric tons
CH_CO2_2012	The percentage change in CO_2 emissions in 2012 relative to 2011
CO2	CO_2 emissions in logged metric tons. In Table 9, these are estimated for US facilities.
GHG	Greenhouse gas (GHG) emissions in logged metric tons CO_2 equivalent
GHG_SD	Standard deviation of raw GHG emissions, in thousands of metric tons CO_2 equivalent, within country-industry-year
GHG_P90_P10	90^{th} minus 10^{th} percentile of raw GHG emissions, in thousands of metric tons CO_2 equivalent, within country-industry-year
$\mathbb{1}_{\{t \geq k\}}$	Indicates year k ($k-1$ for Massachusetts obs.) and beyond
$\mathbb{1}_{\{\text{CROSS-SECTION}\}}$	Indicates US facilities in a cross-section as described in the relevant section and table description
CARBON_INT_201X	CO_2 emissions over industry-specific noncombustion pollutant emissions in 201X, normalized within industry-state
GAS_PRICE	Yearly, regional, lagged natural gas price (logged)
GDP	Gross domestic product at the country-year-2-digit NAICS industry level (logged value-added)
REGULATIONS	Number of energy reduction incentives/regulations applicable to a facility, implemented at the federal or state/province level
SENATE_CC_SCORE	For a US state, the average of the following over each of its active senators: the percentage of climate-change progressive bills, since 2008, that senator has supported
US	Indicates a US facility

Firm-level Analyses

Variable Name	Description
CAPEX	Compustat item CAPX, over beginning total assets
GROSS_MARGIN	Gross profit over revenue
LEVERAGE	Total liabilities over total assets
MV	Market value of equity
MTB	Market value of equity over book value of equity

Firm-level analogs of *US*, *GDP*, *GAS_PRICE*, and *REGULATIONS* are formed by taking weighted-averages of these variables across facilities within firm-year. Facilities' GHG emissions in 2011 form the weights.

Regression Equation Indices

Index	Feature
i	Facility
j	Firm
k	Four-digit NAICS industry
c	Country
t	Year

References

Baucklloh, T., C. Klein, T. Pioch, and F. Schiemann (2022). Under Pressure? The Link Between Mandatory Climate Reporting and Firms' Carbon Performance. *Organization and Environment*.

Bennear, L. S. and S. M. Olmstead (2008). The impacts of the "right to know": Information disclosure and the violation of drinking water standards. *Journal of Environmental Economics and Management 56*(2), 117–130.

Berger, P. G., J. H. Choi, and S. Tomar (2023). Breaking it Down: Economic Consequences of Disaggregated Cost Disclosures. *Management Science*.

Bernard, D., C. Blackburne, and J. Thornock (2020). Information flows among rivals and corporate investment. *Journal of Financial Economics 136*(3), 760–779.

Bolton, P. and M. Kacperczyk (2021). Carbon Disclosure and the Cost of Capital. *SSRN Working Paper*.

Breuer, M., C. Leuz, and S. Vanhaverbeke (2022). Reporting Regulation and Corporate Innovation. *NBER Working Paper*.

Cao, J., H. Liang, and X. Zhan (2019). Peer effects of corporate social responsibility. *Management Science 65*(12), 5487–5503.

Chemical Watch (2019). Feature: US states adopt tough line on volatile organic compounds. https://chemicalwatch.com/77489/feature-us-states-adopt-tough-line-on-volatile-organic-compounds.

Chen, Y. C., M. Hung, and Y. Wang (2018). The effect of mandatory CSR disclosure on firm profitability and social externalities: Evidence from China. *Journal of Accounting and Economics 65*(1), 169–190.

Christensen, H. B., E. Floyd, L. Y. Liu, and M. Maffett (2017). The real effects of mandated information on social responsibility in financial reports: Evidence from mine-safety records. *Journal of Accounting and Economics 64*(2-3), 284–304.

Clarkson, P. M., Y. Li, M. Pinnuck, and G. Richardson (2015). The Value Relevance of Greenhouse Gas Emissions under the European Union Carbon Emissions Trading Scheme. *European Accounting Review 24*(3), 551–580.

Coase, R. H. (1960). The Problem of Social Cost. *Journal of Law and Economics 3*.

Colket, M., R. Garvey, H. Hollick, D. Liscinsky, J. Mantese, G. Poncia, and K. Swanson (2012). High Efficiency - Reduced Emissions Boiler Systems for Steam, Heat, and Processing. Technical Report July.

Cooper, M. J., H. Gulen, and A. V. Ovtchinnikov (2010). Corporate political contributions and stock returns. *Journal of Finance 65*(2), 687–724.

De Franco, G., O. K. Hope, and S. Larocque (2015). Analysts' choice of peer companies. *Review of Accounting Studies 20*(1), 82–109.

DeHaan, E. (2021). Using and Interpreting Fixed Effects Models. *SSRN Working Paper*.

Delmas, M., M. Montes-Sanchom, and J. Shimshack (2010). Mandatory Information Disclosure Policies: Evidence from the Electric Industry. *Economic Inquiry 48*(2), 483–498.

Diermeier, D., M. Keane, and A. Merlo (2005). A political economy model of congressional careers. *American Economic Review 95*(1), 347–373.

Downar, B., J. Ernstberger, S. Reichelstein, S. Schwenen, and A. Zaklan (2021). The impact of carbon disclosure mandates on emissions and financial operating performance. *Review of Accounting Studies 26*(3), 1137–1175.

Dranove, D., D. Kessler, M. McClellan, and M. Satterthwaite (2003). Is More Information Better? The Effects of "Report Cards" on Health Care Providers. In *Journal of Political Economy*, Volume 111.

Durnev, A. and C. Mangen (2009). Corporate investments: Learning from restatements. *Journal of Accounting Research 47*(3), 679–720.

Eccles, R. G. and S. Klimenko (2019). The investor revolution. *Harvard Business Review 2019*(May-June), 106–116.

Engelberg, J., A. Ozoguz, and S. Wang (2018). Know Thy Neighbor: Industry Clusters, Information Spillovers, and Market Efficiency. *Journal of Financial and Quantitative Analysis 53*(5), 1937–1961.

Fetter, T. R., A. Steck, C. Timmins, and D. Wrenn (2022). Learning By Viewing? Social Learning, Regulatory Disclosure, and Firm Productivity in Shale Gas. *NBER Working Paper*.

Fiechter, P., J. M. Hitz, and N. Lehmann (2022). Real Effects of a Widespread CSR Reporting Mandate: Evidence from the European Union's CSR Directive. *Journal of Accounting Research* (March).

Foster, G. (1989). Externalities and Financial Reporting. *Journal of Finance 35*(2), 521–533.

Fung, A. and D. O'Rourke (2000). Reinventing environmental regulation from the grassroots up: Explaining and expanding the success of the toxics release inventory. *Environmental Management 25*(2), 115–127.

Gelles, D. (2022, aug). How Republicans Are 'Weaponizing' Public Office Against Climate Action.

Gerarden, T. D., R. G. Newell, and R. N. Stavins (2017). Assessing the Energy-Efficiency Gap. *Journal of Economic Literature 55*, 1486–1525.

Glazer, A. and H. McMillan (1992). Pricing by the Firm Under Regulatory Threat. *Quarterly Journal of Economics 107*(3), 1089–1099.

Graham, M. and C. Miller (2001). Disclosure of Toxic Releases in the United States. *Environment: Science and Policy for Sustainable Development 43*(8), 8–20.

Grennan, M. and A. Swanson (2020). Transparency and negotiated prices: The value of information in hospital-supplier bargaining. *Journal of Political Economy 128*(4), 1234–1268.

Gurney, K. R., D. L. Mendoza, S. Geethakumar, Y. Zhou, V. Chandrasekaran, C. C. Miller, A. Godbole, B. Seib, W. Ansley, S. Peraino, X. Chen, U. Maloo, J. Kam, J. Binioon, M. Fisher, and S. de la Rue du Can (2009). Vulcan Science Methods Documentation. http://vulcan.rc.nau.edu/assets/files/Vulcan.documentation.v2.0.online.pdf.

Gurney, K. R., D. L. Mendoza, Y. Zhou, M. L. Fischer, C. C. Miller, S. Geethakumar, and S. de la Rue du Can (2009). High Resolution Fossil Fuel Combustion CO 2 Emission Fluxes for the United States. *Environmental Science & Technology 43*(14), 5535–5541.

Hamilton, J. (2005). *Regulation Through Revelation: The Origin, Politics, And Impacts Of The Toxics Release*. Cambridge University Press.

Hartzmark, S. M. and A. B. Sussman (2019). Do Investors Value Sustainability? A Natural Experiment Examining Ranking and Fund Flows. *Journal of Finance 74*(6), 2789–2837.

Hassan, T. A., S. Hollander, L. Van Lent, and A. Tahoun (2019). Firm-level political risk: Measurement and effects. *Quarterly Journal of Economics 134*(4), 2135–2202.

Héroux, M., C. Guy, and D. Millette (2010). A statistical model for landfill surface emissions. *Journal of the Air and Waste Management Association 60*(2), 219–228.

Hoegh-Guldberg, O., D. Jacob, M. Taylor, M. Bindo, S. Brown, I. Camilloni, A. Diedhiou, R. Djalante, K. L. Ebi, F. Engelbrecht, J. Guiot, Y. Hijioka, S. Mehrotra, A. Payne, S. I. Seneviratne, A. Thomas, R. Warren, and G. Zhou (2018). 2018: Impacts of 1.5ºC Global Warming on Natural and Human Systems. pp. 175–311.

Hombach, K. and T. Sellhorn (2019). Shaping Corporate Actions Through Targeted Transparency Regulation: A Framework and Review of Extant Evidence. *Schmalenbach Business Review 71*(2), 137–168.

Ioannou, I., S. X. Li, and G. Serafeim (2016). The effect of target difficulty on target completion: The case of reducing carbon emissions. *The Accounting Review 91*(5), 1467–1492.

Jin, G. and P. Leslie (2003). The Effect of Information Product Quality: Evidence From Restaurant Hygiene Grade Cards. *Quarterly Journal of Economics* (May), 409–451.

Johnson, M. S. (2020). Regulation by Shaming: Deterrence Effects of Publicizing Violations of Workplace Safety and Health Laws. *American Economic Review 110*(6), 1866–1904.

Jouvenot, V. and P. Krueger (2021). Mandatory Corporate Carbon Disclosure: Evidence from a Natural Experiment. *SSRN Electronic Journal*.

Kogan, L. and D. Papanikolaou (2014). Growth opportunities, technology shocks, and asset prices. *Journal of Finance 69*(2), 675–718.

Li, V. (2016). Do false financial statements distort peer firms' decisions? *Accounting Review 91*(1), 251–278.

Luo, L. (2019). The influence of institutional contexts on the relationship between voluntary carbon disclosure and carbon emission performance. *Accounting and Finance 59*(2), 1235–1264.

Manski, C. F. (1993). Identification of Endogenous Social Effects: The Reflection Problem. *Review of Economic Studies 60*(3), 531.

Matisoff, D. C. (2013). Different rays of sunlight: Understanding information disclosure and carbon transparency. *Energy Policy 55*, 579–592.

Maxwell, J. W., T. P. Lyon, and S. C. Hackett (2000). Self-Regulation and Social Welfare: The Political Economy of Corporate Environmentalism. *The Journal of Law and Economics 43*(2), 583–618.

McCright, A. M. and R. E. Dunlap (2011). The Politicization of Climate Change and Polarization in the American Public ' S Views of Global Warming , 2001 – 2010. *Sociological Quarterly 52*(2), 155–194.

McKinsey Global Energy and Materials (2009). Unlocking energy efficiency opportunities. Technical report.

Myers, S. C. (1977). Determinants of Corporate Borrowing. *Journal of Financial Economics 5*, 147–175.

Qian, W. and S. Schaltegger (2017). Revisiting carbon disclosure and performance: Legitimacy and management views. *British Accounting Review 49*(4), 365–379.

Rauter, T. (2020). The Effect of Mandatory Extraction Payment Disclosures on Corporate Payment and Investment Policies Abroad. *Journal of Accounting Research 58*(5), 1075–1116.

Richardson, N. (2012). Policy Significance of EPA's Greenhouse Gas Reporting Program. *Resources for the Future* (November).

Roychowdhury, S., N. Shroff, and R. S. Verdi (2019). The effects of financial reporting and disclosure on corporate investment: A review. *Journal of Accounting and Economics 68*(2-3), 101246.

Sanchez, M., S. Matthews, and P. Fischbeck (2012). How much is United States greenhouse gas emissions certainty worth? *Energy Policy 51*, 259–263.

Santhosh, L. G., P. Lakshmikanthan, and G. L. Sivakumar Babu (2017). Laboratory Investigation of Large Scale MSW Reactor Under Anaerobic Conditions. *Indian Geotechnical Journal 47*(4), 395–409.

Shroff, N. (2017). Corporate investment and changes in GAAP. *Review of Accounting Studies 22*(1).

Shroff, N. (2020). Real effects of PCAOB international inspections. *Accounting Review 95*(5), 399–433.

Shroff, N., R. S. Verdi, and G. Yu (2014). Information environment and the investment decisions of multinational corporations. *Accounting Review 89*(2), 759–790.

Singh, N., K. Bacher, R. Song, M. E. Sotos, and L. Yin (2015). Guide for Designing Mandatory Greenhouse Gas Reporting Programs. Technical report.

Suijs, J. and J. L. Wielhouwer (2019). Disclosure policy choices under regulatory threat. *RAND Journal of Economics 50*(1), 3–28.

Tahoun, A. (2014). The role of stock ownership by US members of Congress on the market for political favors. *Journal of Financial Economics 111*(1), 86–110.

Tahoun, A. and L. Van Lent (2019). The personal wealth interests of politicians and government intervention in the economy. *Review of Finance 23*(1), 37–74.

US EPA (2009). Mandatory Reporting of Greenhouse Gases. *Federal Register (40 CFR Parts 86, 87, 89 et al.) 74*(209).

Weil, D., A. Fung, M. Graham, and E. Fagotto (2006). The Effectiveness of Regulatory Disclosure Policies. *Journal of Policy Analysis and Management 25*(1), 155–181.

Weiss, A. L. (1998). An Analysis of the North American Agreement On Environmental Cooperation. *ILSA Journal of International & Comparative Law 5*(1), 185.

Weiss, D. J., R. Lefton, and S. Lyon (2010). Dirty Money. https://www.americanprogressaction.org/article/dirty-money/.

Yang, L., N. Z. Muller, and P. J. Liang (2022). The Real Effects of Mandatory CSR Disclosure on Emissions: Evidence from the Greenhouse Gas Reporting Program. *NBER Working Paper*.

Figures and Tables

Figure 1: Timeline of Important US Greenhouse Gas Reporting Program Events

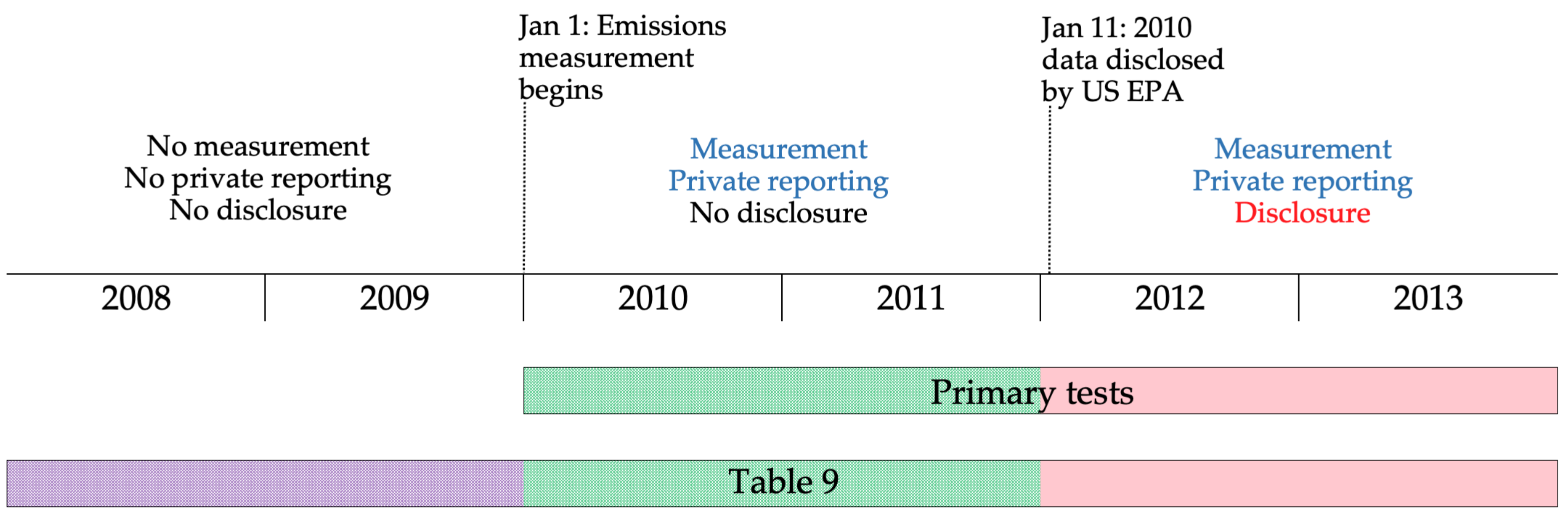
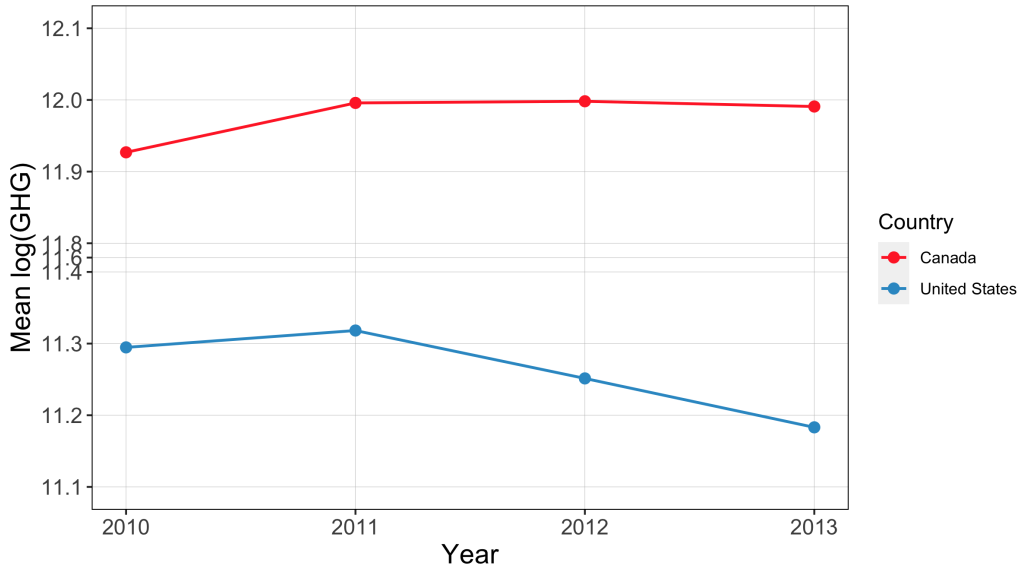


Figure 2: Average Logged Emissions for US and Canadian Facilities

(a) GHG Emissions



(b) CO_2 Emissions

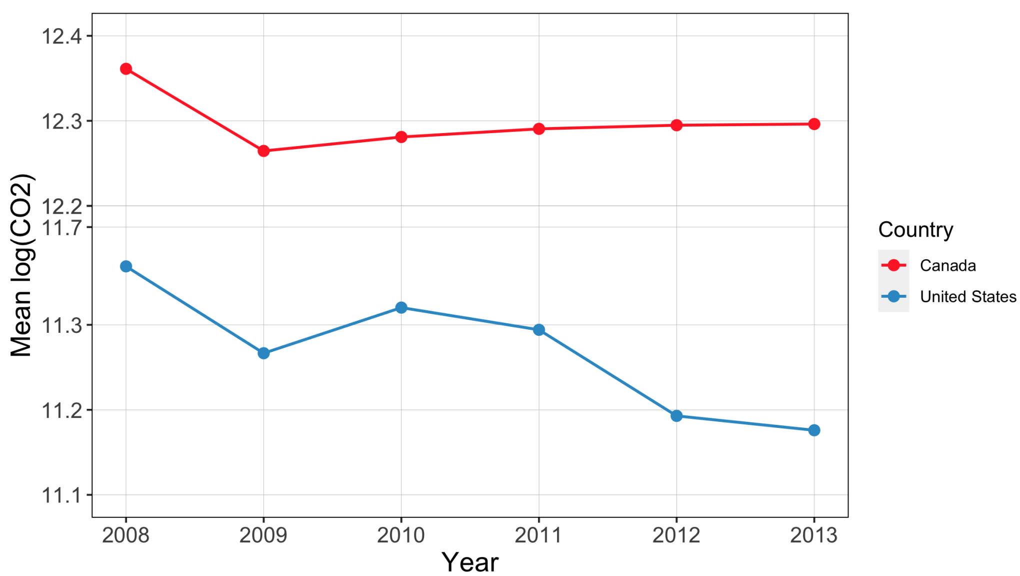
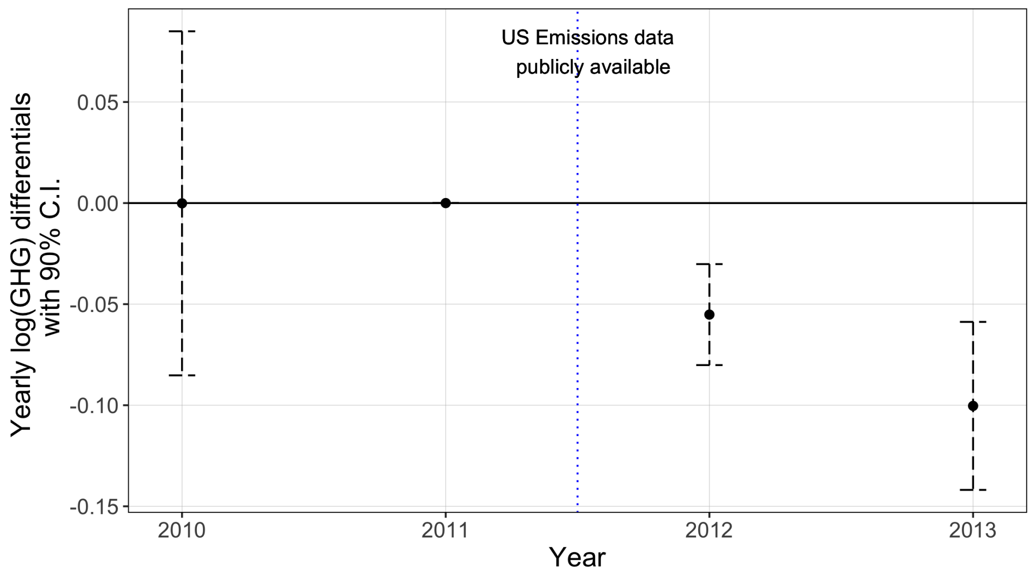


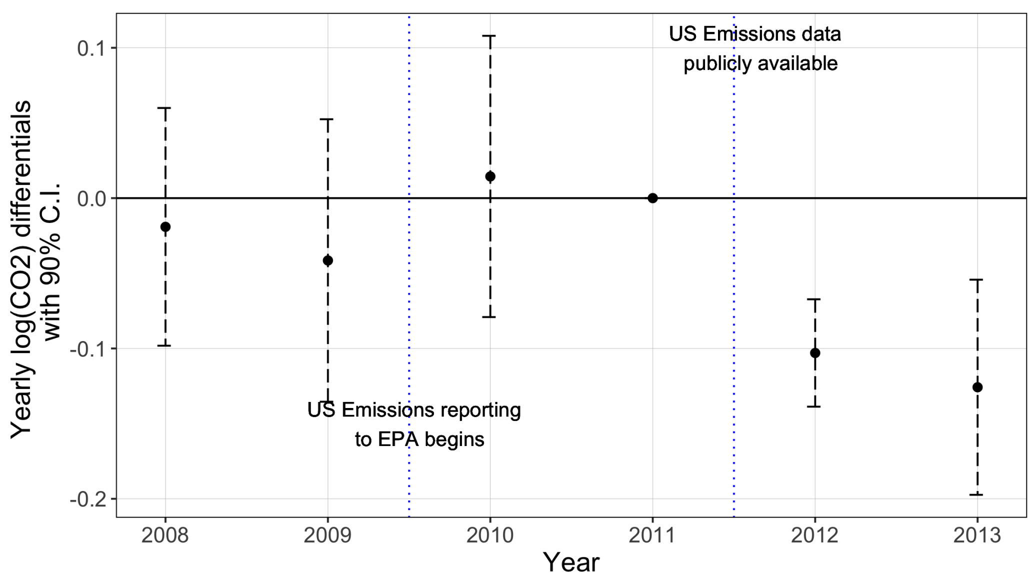
Figure 2a plots average annual GHG emissions (in logged T CO_2e) for US and Canadian facilities that reported emissions to their national GHG reporting programs. Figure 2b plots average annual reported CO_2 emissions for Canadian facilities and estimated CO_2 for a subsample of US facilities—the estimation is described in Section 6.3. For visual consistency, both figures use a balanced panel and exclude Massachusetts' facilities, which began emissions reporting and disclosure a year earlier than other US facilities as part of a state-level Program. Additional data filters are described in Sections 4.2 and 6.3. The Appendix provides variable definitions.

Figure 3: Emissions Differences (US - Canada) by Year Relative to the 2011 Difference

(a) GHG Emissions



(b) CO_2 Emissions

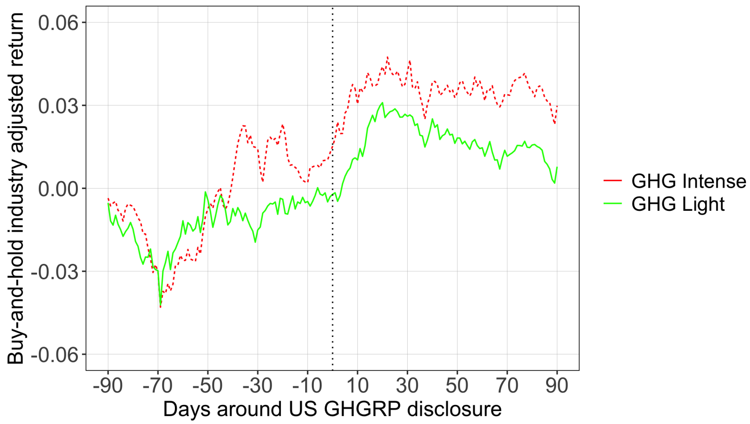


As described in Section 4.4.1, these figures plot the $\beta_{3,k}$s obtained from estimating the following OLS equation:

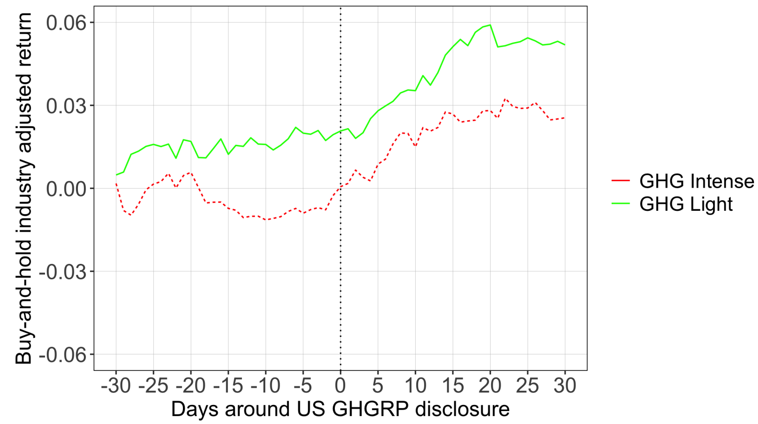
$$GHG_{it} \text{ or } CO2_{it} = \beta_1 US_i + \sum_{k\in 2008 \text{ or } 2010 \text{ to } 2013}^{\text{excluding } 2011} \left(\beta_{2,k}\mathbb{1}_{\{t=k\}} + \beta_{3,k}\mathbb{1}_{\{t=k\}} * US_i\right) + \gamma X_{it} + \eta_i + \eta_{jt} + \varepsilon_{it}.$$

The $\beta_{3,k}$s track how US and Canadian facilities' emissions differ relative to their 2011 difference. US CO_2 emissions are estimated as described in Section 6.3. The sample comprises US and Canadian facilities that reported GHG emissions to their national GHG reporting programs. Additional data filters are described in Sections 4.2 and 6.3. The Appendix provides variable definitions.

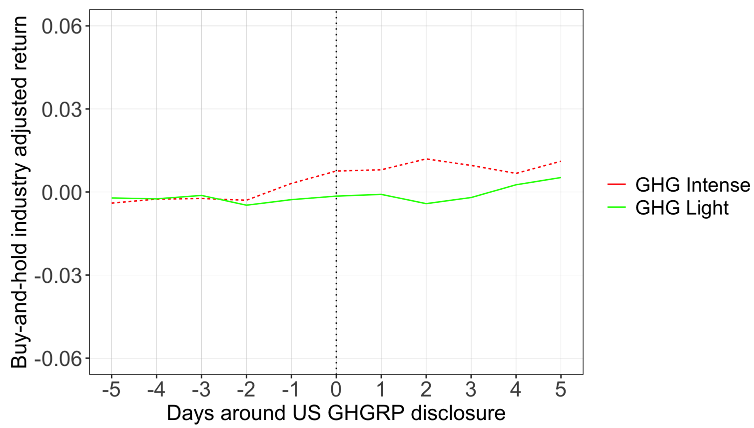
Figure 4: External Pressures—Buy-and-Hold Industry-Adjusted Returns Around GHG Disclosure



(a) $t \in$ [-90, 90]



(b) $t \in$ [-30, 30]



(c) $t \in$ [-5, 5]

These figures are described in Section 6.2. They depict the buy-and-hold excess returns for two sets of firms over different windows relative to the date of the US GHG Reporting Program's first data release. The sample comprises the owner-firms of US facilities that reported GHG emissions. "GHG Intense" ("GHG Light") firms have above (below) industry-median GHG emissions divided by COGS (as computed for 2010). Data filters are described in Sections 4.2 and 4.4.2.

Table 1a: Facility-Year Frequency by four-Digit NAICS Industry and Country

Industry	Canada N	US N	*GHG* Mean (10^3T CO_2e)
Aerospace Product and Parts Manuf.	0	90	40
Agriculture, Construction, and Mining Machinery Manuf.	0	28	45
Alumina and Aluminum Production	43	193	384
Animal Food Manuf.	0	14	54
Animal Slaughtering and Processing	0	299	36
Architectural, Engineering, and Related Services	0	6	36
Basic Chemical Manuf.	114	1,667	272
Beverage Manuf.	4	88	67
Cement and Concrete Product Manuf.	60	326	611
Clay Product and Refractory Manuf.	3	43	73
Coal Mining	59	93	82
Coating, Engraving, Heat Treating, and Allied Activities	0	16	73
Colleges, Universities, and Professional Schools	0	27	94
Computer and Peripheral Equipment Manuf.	0	8	6
Converted Paper Product Manuf.	0	19	65
Dairy Product Manuf.	0	52	31
Electrical Equipment Manuf.	0	12	2
Engine, Turbine, and Transmission Equipment Manuf.	0	21	33
Fabric Mills	0	8	97
Facilities Support Services	0	8	1,050
Forging and Stamping	0	36	45
Foundries	12	153	62
Fruit and Vegetable Preserving and Specialty Food Manuf.	9	134	45
General Medical and Surgical Hospitals	0	32	35
Glass and Glass Product Manuf.	8	345	79
Grain and Oilseed Milling	16	338	234
Household Appliance Manuf.	0	8	25
Iron and Steel Mills and Ferroalloy Manuf.	44	494	679
Lessors of Real Estate	0	8	69
Lime and Gypsum Product Manuf.	48	346	292
Metal Ore Mining	50	64	341
Metalworking Machinery Manuf.	0	8	53
Motor Vehicle and Parts and Supplies Wholesalers	0	8	448
Motor Vehicle Manuf.	18	138	62
Motor Vehicle Parts Manuf.	0	58	33
Natural Gas Distribution	24	73	214
Nonferrous Metal (except Aluminum) Production	32	136	110
Nonmetallic Mineral Mining and Quarrying	40	170	182
Office Furniture (including Fixtures) Manuf.	20	4	8
Oil and Gas Extraction	469	2,095	156
Other Chemical Product and Preparation Manuf.	6	60	119

Other Electrical Equipment and Component Manuf.	0	12	77
Other Fabricated Metal Product Manuf.	0	15	23
Other Food Manuf.	0	86	58
Other Miscellaneous Manuf.	24	16	31
Other Nonmetallic Mineral Product Manuf.	7	145	81
Other Pipeline Transportation	0	6	22
Paint, Coating, and Adhesive Manuf.	0	12	1
Pesticide/Fertilizer/Other Agricultural Chemical Manuf.	35	197	640
Petroleum and Coal Products Manuf.	74	606	1,089
Petroleum and Petroleum Products Merchant Wholesalers	0	9	29
Pharmaceutical and Medicine Manuf.	0	113	62
Pipeline Transportation of Crude Oil	0	20	71
Pipeline Transportation of Natural Gas	49	1,835	55
Plastics Product Manuf.	11	52	75
Printing and Related Support Activities	0	12	22
Pulp, Paper, and Paperboard Mills	195	880	190
Railroad Rolling Stock Manuf.	0	8	46
Resin, Synthetic Rubber, and Synthetic Fiber Manuf.	12	342	301
Rubber Product Manuf.	3	78	40
Sawmills and Wood Preservation	0	21	10
Scheduled Air Transportation	0	8	47
Scientific Research and Development Services	0	30	64
Seafood Product Preparation and Packaging	0	8	36
Semiconductor and Other Electronic Component Manuf.	0	146	31
Soap, Cleaning Compound, and Toilet Preparation Manuf.	0	25	75
Steel Product Manuf. from Purchased Steel	8	50	79
Sugar and Confectionery Product Manuf.	8	108	188
Support Activities for Mining	0	297	47
Textile and Fabric Finishing and Fabric Coating Mills	0	24	39
Textile Furnishings Mills	0	31	45
Tobacco Manuf.	0	12	84
Traveler Accommodation	0	12	67
Utility System Construction	0	4	70
Veneer, Plywood, and Engineered Wood Product Manuf.	16	60	32
Water, Sewage and Other Systems	19	167	151
All	1,540	13,173	234

Table 1b: Facility-Year Frequency by US State

State	US N
Alabama	430
Alaska	122
Arizona	112
Arkansas	370
Colorado	294
Connecticut	69
Delaware	32
Florida	184
Georgia	291
Hawaii	16
Idaho	106
Illinois	511
Indiana	421
Iowa	364
Kansas	278
Kentucky	285
Louisiana	1,129

State	US N
Maine	57
Maryland	53
Massachusetts	90
Michigan	429
Minnesota	299
Mississippi	233
Missouri	184
Montana	73
Nebraska	154
Nevada	62
New Hampshire	16
New Jersey	130
New Mexico	198
New York	249
North Carolina	224
North Dakota	111
Ohio	560

State	US N
Oklahoma	386
Oregon	143
Pennsylvania	586
Rhode Island	12
South Carolina	220
South Dakota	85
Tennessee	260
Texas	1,921
Utah	175
Vermont	16
Virginia	173
Washington	198
West Virginia	211
Wisconsin	280
Wyoming	371
All	13,173

Table 1c: Facility-Year Frequency by Canadian Province

Province	Canada N
Alberta	524
British Columbia	253
Manitoba	34
New Brunswick	36
Newfoundland and Labrador	28
Northwest Territories	16
Nova Scotia	27
Ontario	325
Prince Edward Island	4
Quebec	196
Saskatchewan	97
All	1,540

These tables count the observations used in the primary regression analysis (Table 3 Column 4) by four-digit NAICS industry-country and by Canadian province/US state. The sample spans 2010 to 2013 and comprises US and Canadian facilities that reported GHG emissions to their national GHG reporting programs. Data filters are described in Section 4.2.

Table 2: Descriptive Statistics

(a) Facility-level

	Canada							
	N	Mean	SD	q1	q25	q50	q75	q99
GHG	1,540	11.8	1.52	6.67	11.1	11.7	12.8	15.3
GDP	1,540	11.9	0.316	10.6	11.7	12.1	12.1	12.1
GAS_PRICE	1,540	1.29	0.204	0.891	1.12	1.31	1.45	1.56
REGULATIONS	1,540	6.39	2.37	2	6	7	8	10

	US							
	N	Mean	SD	q1	q25	q50	q75	q99
GHG	13,173	11.0	1.52	6.03	10.3	10.9	11.8	14.9
GDP	13,173	13.90	0.793	12.5	13.0	14.5	14.5	14.5
GAS_PRICE	13,173	1.30	0.189	0.891	1.02	1.38	1.45	1.52
REGULATIONS	13,173	2.40	1.53	1	1	2	3	8

(b) Firm-level

	N	Mean	SD	q1	q25	q50	q75	q99
CAPEX	1,189	0.081	0.080	0.003	0.032	0.055	0.101	0.448
GROSS_MARGIN	1,189	0.330	0.201	0.025	0.181	0.277	0.441	0.838
US	1,189	0.887	0.290	0.000	1.00	1.00	1.00	1.00
GDP	1,189	13.8	0.948	11.7	12.9	14.5	14.5	14.5
GAS_PRICE	1,189	1.30	0.189	0.891	1.08	1.38	1.43	1.52
REGULATIONS	1,189	2.56	1.82	1	1	2	3	8
LEVERAGE	1,189	0.597	0.217	0.146	0.477	0.587	0.698	1.272
MV	1,189	22.6	43.8	0.060	1.60	5.48	23.4	211
MTB	1,189	3.25	24.2	− 5.06	1.23	1.83	2.72	16.5

Table 2a describes the observations used in primary regression analysis (Table 3 Column 4). Its sample spans 2010 to 2013 and comprises US and Canadian facilities that reported GHG emissions to their national GHG reporting programs. Table 2b describes the highest-level parent-firm of these facilities. Data filters are described in Sections 4.2 and 4.4.2. The Appendix provides variable definitions.

Table 3: Facility GHG Emissions Reductions Following US Program Disclosure

VARIABLES	(1) GHG	(2) GHG	(3) GHG	(4) GHG
$\mathbb{1}_{\{t\geq 2012\}} * US$	-0.302	-0.121***	-0.065**	-0.082**
	(0.255)	(0.039)	(0.027)	(0.033)
$\mathbb{1}_{\{t\geq 2012\}}$	0.067	0.136**	0.035	0.051
	(0.175)	(0.060)	(0.066)	(0.066)
US	-0.605***			
	(0.172)			
GDP				0.220
				(0.420)
GAS_PRICE				-0.021
				(0.342)
$REGULATIONS$				-0.007
				(0.008)
Observations	15,041	14,791	14,713	14,713
Adjusted R^2	0.028	0.897	0.902	0.902
Facility & Year fixed effects		Y		
Facility & Ind-Year fixed effects			Y	Y

Standard errors clustered by industry-year in parentheses; *** p<0.01, ** p<0.05, * p<0.1

This table is discussed in Section 4.4.1 and shows how US facilities' logged GHG emissions change following the US GHG Reporting Program's first disclosure of emissions data in January 2012. Canadian facilities provide the control. The sample spans 2010 to 2013 and comprises US and Canadian facilities that reported emissions to their national GHG reporting programs. Data filters are described in Section 4.2. The Appendix provides variable definitions.

Table 4: Offshoring, Reduced Economic Activity, and Investments

	Facility-Year Level			Firm-Year Level	
	(1)	(2)	(3)	(4)	(5)
VARIABLES	*GHG*	*CARBON_INT*	*CARBON_INT*	*CAPEX*	*GROSS_MARGIN*
$\mathbb{1}_{\{t\geq 2012\}} * US * \mathbb{1}_{\{\text{LOW MOBILITY}\}}$	-0.147**				
	(0.073)				
$\mathbb{1}_{\{t\geq 2012\}} * US$	-0.018	-0.072**	-0.075	0.025**	0.018
	(0.024)	(0.034)	(0.050)	(0.012)	(0.013)
GAS_PRICE	-0.004	0.354	0.356	0.036	-0.227*
	(0.343)	(0.320)	(0.329)	(0.120)	(0.129)
REGULATIONS	-0.011		-0.002	-0.002	-0.006*
	(0.008)		(0.011)	(0.002)	(0.003)
GDP	0.807		-0.045	-0.023**	0.060
	(0.593)		(0.534)	(0.010)	(0.037)
MV				0.065	0.186
				(0.101)	(0.220)
LEVERAGE				-0.033	-0.088***
				(0.031)	(0.030)
MTB				-0.000	0.000
				(0.000)	(0.000)
Observations	14,713	6,149	6,149	1,187	1,189
Adjusted R^2	0.902	0.968	0.968	0.726	0.921
Facility & Ind-Year fixed effects	Y	Y	Y		
Firm & Ind-Year fixed effects				Y	Y

Standard errors clustered by industry-year in parentheses; *** p<0.01, ** p<0.05, * p<0.1

This table is discussed in Section 4.4.2. Column 1 shows how US facilities' logged GHG emissions change following the US GHG Reporting Program's first disclosure of emissions data in January 2012, with an emphasis on industries in which production is difficult to relocate. Columns 2 and 3 examine changes in US facilities' carbon intensity. Columns 4 and 5 examine changes in US firms' capital expenditures and gross margins. Canadian facilities/firms provide the control. The sample spans 2010 to 2013 and comprises US and Canadian facilities (or their owners) that reported emissions to their national GHG reporting programs. Data filters are described in Sections 4.3 and 4.4.2. The Appendix provides variable definitions.

Table 5: Benchmarking—GHG Emissions Convergence and the Effect of Relative Carbon Intensity on Emissions Reductions

	Industry-Year Level		Facility Level			
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	*GHG_SD*	*GHG_P90_P10*	*CH_CO2_2012*	*CH_CO2_2012*	*CH_CO2_2012*	*CH_CO2_2012*
$\mathbb{1}_{\{t \geq 2012\}} * US$	-45.962**	-63.004**				
	(21.937)	(28.775)				
CARBON_INT_2010			-0.030**	-0.070*	-0.091***	-0.121***
			(0.012)	(0.040)	(0.018)	(0.041)
CARBON_INT_2011				0.053		0.041
				(0.045)		(0.042)
GAS_PRICE	-494.360	-671.100	1.652***	1.647***	1.647***	1.643***
	(338.720)	(444.313)	(0.125)	(0.123)	(0.159)	(0.158)
REGULATIONS	8.219	6.217	-0.058	-0.057	-0.062	-0.061
	(19.589)	(25.696)	(0.042)	(0.041)	(0.053)	(0.053)
Observations	392	392	1,111	1,111	545	545
Adjusted R^2	0.961	0.960	0.263	0.264	0.200	0.201
Ind-Country & Year fixed effects	Y	Y				
Industry fixed effects			Y	Y	Y	Y
Carbon intense facilities					Y	Y

Standard errors in parentheses (clustered by industry in Columns 3 to 6); *** p<0.01, ** p<0.05, * p<0.1

This table is discussed in Sections 5.1 and 5.2. Columns 1 and 2 examine how the within-industry dispersion of US facility GHG emissions changes following the US GHG Reporting Program's first disclosure of emissions data in 2012. They examine the standard deviation and 90th−10th percentile difference. The sample spans 2010 to 2013 and comprises US and Canadian facilities that reported emissions to their national GHG Reporting Programs. Canadian facilities' emissions provide the control. Columns 3 to 6 show how the percentage change in US facilities' carbon dioxide (CO_2) emissions from 2011 to 2012 responds to their 2010 within-industry-state rankings of carbon intensity (CO_2 emissions scaled by a proxy for goods produced). This ranking becomes publicly known in 2012. The sample comprises US facilities only. Data filters are described in Sections 4.2, 5.1, and 5.2. The Appendix provides variable definitions.

Table 6: Benchmarking—Effect of Being a *Benchmarker* on Facility GHG Emissions Reductions

VARIABLES	(1) *GHG*	(2) *GHG*	(3) *GHG*
$\mathbb{1}_{\{t \geq 2012\}} * US * \mathbb{1}_{\{\text{BENCHMARKER}\}}$	-0.077	-0.077*	
	(0.062)	(0.040)	
$\mathbb{1}_{\{t \geq 2012\}} * US$	-0.034	-0.072*	
	(0.032)	(0.041)	
$\mathbb{1}_{\{t \geq 2012\}} * US * \mathbb{1}_{\{\text{67th-100th pctile BENCHMARKER}\}}$			-0.171***
			(0.064)
$\mathbb{1}_{\{t \geq 2012\}} * US * \mathbb{1}_{\{\text{33rd-67th pctile BENCHMARKER}\}}$			-0.093*
			(0.055)
$\mathbb{1}_{\{t \geq 2012\}} * US * \mathbb{1}_{\{\text{0th-33rd pctile BENCHMARKER}\}}$			-0.074*
			(0.040)
GDP	-0.063	-0.036	-0.042
	(0.270)	(0.563)	(0.568)
GAS_PRICE	-0.595	-0.951***	-0.914***
	(0.385)	(0.300)	(0.279)
$REGULATIONS$	0.001	-0.007	-0.007
	(0.007)	(0.009)	(0.009)
Observations	2,988	5,289	5,289
Adjusted R^2	0.954	0.930	0.930
Ind-Year & Facility fixed effects	Y	Y	Y

Standard errors clustered by industry-year in parentheses; *** p<0.01, ** p<0.05, * p<0.1

This table is discussed in Section 5.3. It explores whether the extent to which a facility's owner accesses the information of its peers affects the reduction in that facility's logged GHG emissions following the US GHG Reporting Program's first disclosure of emissions data in 2012. A facility is classified as a "benchmarker" if its owner accesses an above-median amount of its peer firms' financial information from the US SEC's EDGAR website (see Bernard et al., 2020). The sample spans 2010 to 2013 and comprises US and Canadian facilities that reported emissions to their national GHG Reporting Programs. Canadian facilities' emissions provide the control. Data filters are described in Sections 4.2 and 5.3. The Appendix provides variable definitions.

Table 7a: Benchmarking—Facility Process Convergence Statistics

Facilities and their...	% Chg. Similarity	p.val
...contempraneous peers	10.5	0.008
...prior carbon-light peers	6.3	0.048
...prior carbon-intense peers	-3.4	0.037

Table 7b: Benchmarking—Process Changes and Facility GHG Emissions Reductions

VARIABLES	(1) $CO2$	(2) $CO2$	(3) $CO2$
$\mathbb{1}_{\{t\geq 2012\}} * US * \mathbb{1}_{\{\text{LOW SIM.}\}}$	-0.192**		
	(0.079)		
$\mathbb{1}_{\{t\geq 2012\}} * US * \mathbb{1}_{\{\text{MED. SIM.}\}}$	-0.119**		
	(0.053)		
$\mathbb{1}_{\{t\geq 2012\}} * US * \mathbb{1}_{\{\text{HIGH SIM.}\}}$	-0.018		
	(0.043)		
$\mathbb{1}_{\{t\geq 2012\}} * US$		-0.102**	-0.101**
		(0.044)	(0.044)
$\mathbb{1}_{\{t\geq 2012\}} * US * \mathbb{1}_{\{\text{IMPROVE}\}}$		-0.070	-0.112
		(0.047)	(0.090)
Observations	7,096	3,528	3,497
Adjusted R^2	0.936	0.950	0.947
Ind-Year & Facility fixed effects	Y	Y	Y

Standard errors clustered by industry-year in parentheses; *** p<0.01, ** p<0.05, * p<0.1

This table is discussed in Section 5.4. Table 7a examines whether facilities become more (less) similar to their carbon light (carbon intense) peers following the US GHG Reporting Program's first disclosure of emissions data in 2012. Column 1 of Table 7b examines how the emissions reductions of a facility varies with the degree of process-similarity across the facilities in its industry-state. Column 2 examines how the emissions reduction of a facility is affected when its processes become more similar to its more carbon light peers; Column 3 does the same, but further conditions on the facility becoming less similar to its carbon intense peers. The sample spans 2010 to 2013 and comprises US and Canadian facilities that reported emissions to their national GHG reporting programs. Data filters are described in Sections 4.2 and 5.4. The Appendix provides variable definitions.

Table 8a: External Pressure—Climate Change Progressiveness of Legislators and Facility GHG Emissions Reductions

VARIABLES	(1) *GHG*	(2) *GHG*	(3) *GHG*	(4) *GHG*
$\mathbb{1}_{\{t\geq 2012\}} * US$	-0.057*	-0.081**	-0.059*	-0.050*
	(0.029)	(0.035)	(0.031)	(0.030)
$\mathbb{1}_{\{t\geq 2012\}} * US * \mathbb{1}_{\{\text{CC HOUSE}\}}$		-0.004	0.006	
		(0.019)	(0.022)	
$\mathbb{1}_{\{t\geq 2012\}} * US * \mathbb{1}_{\{\text{CC SENATE}\}}$	-0.052*		-0.053*	
	(0.028)		(0.031)	
$\mathbb{1}_{\{t\geq 2012\}} * SEN\_CC\_SCR$				-0.084**
				(0.035)
SEN_CC_SCR				-0.033
				(0.077)
GDP	0.215	0.220	0.214	0.147
	(0.423)	(0.419)	(0.423)	(0.397)
GAS_PRICE	-0.054	-0.023	-0.050	-0.034
	(0.338)	(0.344)	(0.336)	(0.337)
$REGULATIONS$	-0.007	-0.007	-0.007	-0.007
	(0.007)	(0.008)	(0.007)	(0.007)
Observations	14,713	14,713	14,713	14,713
Adjusted R^2	0.902	0.902	0.902	0.902
Ind-Year & Facility fixed effects	Y	Y	Y	Y

Standard errors clustered by industry-year in parentheses; *** p<0.01, ** p<0.05, * p<0.1

This table is discussed in Section 6.1. It examines how the climate change progressiveness of a facility's state senators and US House representative affect that facility's reduction in logged GHG emissions following the US GHG Reporting Program's first disclosure of emissions data in 2012. Legislators' climate change progressiveness is measured using those legislators' voting records on climate change progressive bills. The sample spans 2010 to 2013 and comprises US and Canadian facilities that reported emissions to their national GHG reporting programs. Data filters are described in Section 4.2. The Appendix provides variable definitions.

Table 8b: External Pressure—Political Connections and Facility GHG Emissions Reductions

VARIABLES	(1) *GHG*	(2) *GHG*	(3) *GHG*
$\mathbb{1}_{\{t\geq 2012\}} * US$	-0.067*	-0.068*	-0.064*
	(0.037)	(0.035)	(0.036)
$\mathbb{1}_{\{t\geq 2012\}} * \mathbb{1}_{\{\text{SENATE CONN.}\}}$	-0.060		-0.030
	(0.039)		(0.034)
$\mathbb{1}_{\{\text{SENATE CONN.}\}}$	0.029		0.017
	(0.036)		(0.036)
$\mathbb{1}_{\{t\geq 2012\}} * \mathbb{1}_{\{\text{HOUSE CONN.}\}}$		-0.084**	-0.063**
		(0.040)	(0.031)
$\mathbb{1}_{\{\text{HOUSE CONN.}\}}$		0.048	0.037
		(0.033)	(0.029)
Observations	11,286	11,286	11,286
Adjusted R^2	0.906	0.906	0.906
Ind-Year & Facility fixed effects	Y	Y	Y

Standard errors clustered by industry-year in parentheses; *** p<0.01, ** p<0.05, * p<0.1

This table is discussed in Section 6.1. It examines how the presence of US GHG Reporting Program disclosure affects the relation between a facility's GHG emissions and its political connections. Political connections are measured using the existence of known election campaign contributions from facilities' owners to incumbent legislators. The sample spans 2010 to 2013 and comprises US and Canadian facilities that reported emissions to their national GHG reporting programs. Data filters are described in Section 4.2. The Appendix provides variable definitions.

Table 9: Emissions Responses Prior to US Program Disclosure

VARIABLES	(1) *CO2*	(2) *CO2*	(3) *CO2*	(4) *CO2*
$\mathbb{1}_{\{t\geq 2012\}} * US$	-0.133^{***}	-0.119^{***}	-0.175^{**}	-0.072
	(0.038)	(0.045)	(0.078)	(0.118)
$\mathbb{1}_{\{t\geq 2010\}} * US$	0.017	0.037	-0.055	0.042
	(0.034)	(0.042)	(0.115)	(0.122)
GDP		-0.152		
		(0.429)		
GAS_PRICE		0.059		
		(0.204)		
REGULATIONS		-0.002		
		(0.009)		
MV				-0.225
				(1.082)
LEVERAGE				0.668^{**}
				(0.313)
MTB				-0.001
				(0.001)
Observations	6,078	6,078	550	550
Adjusted R^2	0.864	0.864	0.986	0.984
Year and Facility fixed effects	Y			
Ind-Yr and Facility fixed effects		Y		
Year and Firm fixed effects			Y	
Ind-Yr and Firm fixed effects				Y

Standard errors clustered by industry-year in parentheses; *** p<0.01, ** p<0.05, * p<0.1

This table is discussed in Section 6.3. It examines how US facilities' GHG emissions change following the implementation of the US GHG Reporting Program. Columns 1 and 2 examine logged CO_2 emissions (estimated for US facilities as described in Section 6.3). Columns 3 and 4 examine logged Scope 1 GHG emissions voluntarily disclosed by large firms under the Carbon Disclosure Project. The samples span 2008 to 2013, with Canadian observations forming the control. Data filters are described in Sections 4.2 and 6.3. The Appendix provides variable definitions.

about ECGI

The European Corporate Governance Institute has been established to improve *corporate governance through fostering independent scientific research and related activities.*

The ECGI will produce and disseminate high quality research while remaining close to the concerns and interests of corporate, financial and public policy makers. It will draw on the expertise of scholars from numerous countries and bring together a critical mass of expertise and interest to bear on this important subject.

The views expressed in this working paper are those of the authors, not those of the ECGI or its members.

Electronic Access to the Working Paper Series

The full set of ECGI working papers can be accessed through the Institute's Web-site (www.ecgi.global/content/working-papers) or SSRN:

Finance Paper Series	http://www.ssrn.com/link/ECGI-Fin.html
Law Paper Series	http://www.ssrn.com/link/ECGI-Law.html