Greenhouse Gas Disclosure and Emissions Benchmarking

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This paper is based on my dissertation titled "Firm Responses to Mandatory Greenhouse Gas Disclosure," completed at the University of Chicago. 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, Matthias Breuer, Jung Ho Choi, Hemang Desai, Jo˜ao Granja, Michael Greenstone, Jody Grewal, Doug Hanna, Russ Hamilton, Ruihao Ke, Jean-Marie Meier, Dan Millimet, Liz Moyer, DJ Nanda, Jing Pan, Rachna Prakash, Robbie Sanders, Doug Skinner, Mark Templeton, Wayne Taylor, Marcel Tuijn, Rodrigo Verdi, and seminar participants at the University of Chicago, Southern Methodist University, UCLA, London School of Economics, and CUNY Baruch College 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. Lastly, I gratefully acknowledge financial support from the Southern Methodist University Cox School of Business, The University of Chicago Booth School of Business, and the Ernie Wish Fellowship. Any errors and omissions are solely mine.

Abstract

In 2010, the United States mandated the reporting of greenhouse gas (GHG) emis- sions for thousands of manufacturing facilities. Studying this rule, and focusing on facilities for which emissions information was largely not available elsewhere, I find a 7.9% emissions reduction following disclosure. I highlight the role of ‘benchmarking’. Specifically, facilities are able to assess their own, relative GHG performance once they can observe their peers’ disclosures. This benchmarking facilitates emissions reduc- tions. In contrast, I highlight uncer- tainty around whether measurement and reporting to the regulator alone, prior to disclosure, leads to emissions reductions. Lastly, I show that concern about future legislation partly motivates the observed responses. The main takeaway is that mandatory, granular disclosure can curb GHG emissions, and that benchmarking plays an important role in this process.

Keywords: Corporate Social Responsibility; Disclosure Regulation; Climate Change; Benchmarking; Peer Effects; Real Effects

JEL Classifications: D22; D83; K32; L11; L21; L51; M41; M48; Q54; Q58

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Abstract

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1 Introduction

The scientific consensus is that humans have significantly contributed to recent global warming, which if unabated, will lead to a host of dire consequences (Anderegg, Prall, Harold, and Schneider, 2010; Cook et al., 2016; Hoegh-Guldberg et al., 2018). Still, the bulk of greenhouse gas (GHG) emissions invite no legal remedies, and with some notable exceptions, the widespread adoption of carbon taxes and emissions trading schemes has met formidable resistance [Nordhaus, 2011]. Disclosure—Tietenberg [1998]'s third phase of pollution control after 'command-and-control' standards and market-mechanisms—is arguably less interventionist than its regulatory counterparts, and thus faces fewer political barriers to implementation. A case in point is the United States, which has no economy-wide GHG emissions pricing scheme, but which has an economy-wide emissions reporting scheme: US Environmental Protection Agency's GHG Reporting Program ('US Program'). I study how the US Program affects the emissions of disclosing facilities, many of which previously had no emissions information in the public domain. Given the granularity of US Program data, I also highlight the role of across-facility comparisons in emissions reductions. Despite the US Program's scope and scale, to date, there has been no comprehensive study of its impact on GHG emissions.

Regulators have used disclosure to target a range of social ills, such as corporate tax avoidance, mine-safety violations, and restaurant hygiene [Christensen, Floyd, Liu, and Maffett, 2017; Dyreng, Hoopes, and Wilde, 2016; Jin and Leslie, 2003]. The goal is to create an 'action-cycle' between information users and disclosers, which leads disclosers to internalize the costs of their actions [Weil, Fung, Graham, and Fagotto, 2006]. These disclosures have shown promise in mitigating environmental damage in non-GHG contexts (e.g., Bennear and Olmstead [2008]; Chen, Hung, and Wang [2018]; Hamilton [2005]), which points to their potential benefits in the GHG setting.

The initially 711-page US Program was implemented in 2010, and was created broadly for 'use in analyzing, developing, and implementing current and potential future...GHG policies and programs.' It requires the bulk of US facilities emitting over 25,000T of carbon dioxide equivalent (CO_2e) to report their yearly emissions by GHG and production activity. Depending on the production activity, more granular disclosures are frequently required at the level of the process or unit (boiler, furnace, etc.). In its first year, the US Program covered over 6,200 facilities that together emitted 3.2 billion T CO_2e, roughly half of total US emissions.[1]

Although the US Program provides a significant infusion of GHG emission information into the public domain, there are reasons to question whether it leads to emissions reductions. US Program data are not easily aggregated to the firm-level, potentially dampening firm-level stakeholder pressure. Additionally, the climate change puzzle entails an enormous collective action problem, muting the incentives for stakeholders to mobilize and for facilities to curb emissions. Further, connecting a facility's GHG emissions to the social damages they inflict is fraught with uncertainty. Given these impediments to contracting between stakeholders and emitters, it bears empirical examination whether or not GHG disclosure produces an 'action-cycle.'

Before proceeding with the analysis, I note two points. First, 2010 US Program data became public only in January 2012; in effect, the researcher can observe US facilities' emissions prior to disclosure, without the need to rely on disclosures made in another venue. Importantly, this allows for an assessment of the impact of disclosure on facilities that did not previously disclose any emissions information, rather than of the impact of aggregation and dissemination of existing information disclosed elsewhere. Second, Canadian facilities, which were disclosing their GHG emissions to Environment and Climate Change Canada since 2008, provide a reasonable counterfactual for US facilities. This is predicated on economic commonalities between the US and Canada, for example, their integrated energy network.

[1] Vehicular emissions is a large excluded category. The denominator comes from US EPA's top down emissions inventory, which measures emissions using aggregate fossil fuel production and purchases. 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.

Using a difference-in-differences framework, I find that GHG emissions fall by 7.9% following disclosure. The use of facility and four-digit-industry-year fixed effects provide a tight comparison between treatment and control facilities, while accounting for all time-invariant facility characteristics. To show that facilities did not simply reduce economic activity to lower GHG emissions, I move to the firm-level and demonstrate that GHG-intensity (GHG emissions scaled by Cost of Goods Sold) also falls following disclosure. Consistent with costly investments by facilities to reduce GHG-intensity, I find significant increases in capital expenditure and gross margins following disclosure.

Target-setting and relative performance appear important in the corporate GHG setting [Clarkson, Li, Pinnuck, and Richardson, 2015; Grewal, 2021; Ioannou, Li, and Serafeim, 2016]. US Program disclosures are highly granular, and GHG emissions are closely linked to the industrial production of goods. US Program data are therefore informative about operations in ways that aggregated, firm-level CSR disclosures are not. Thus, facilities can meaningfully assess their own, relative GHG performance once they can observe their peers' disclosures. This assessment can provide a 'red flag' that then spurs emissions reductions. I call this 'benchmarking'.

I first provide high-level evidence consistent with benchmarking, by showing that within-industry emissions dispersion falls. This suggests increased overlap in facilities' information sets following disclosure. To explore further, I rank facilities against their peers, based on their carbon-intensities. 2010 carbon-intensity-rank predicts a larger emissions reduction in 2012 (when the 2010 data became public). 2011 carbon-intensity rank (first visible in 2013), however, does *not* incrementally predict emissions reductions in 2012, suggesting that the observability of data matters beyond its existence. This alleviates the concern that the observed 'catching-up' is driven by mean-reversion or natural technological convergence, and not by disclosure.

Next, I classify some facilities as 'benchmarkers', based on how much their owner-firms search for their peers' financial information on the US SEC's EDGAR platform [Bernard, Blackburne, and Thornock, 2020]. Benchmarker facilities have significantly larger GHG emissions reductions relative to non-benchmarker facilities. This result reduces the concern that, rather than the occurrence of benchmarking, we are instead seeing that facilities had an anticipatory response in 2010 to future disclosure, with emissions reduction plans having lengthy lead-times. It also alleviates the concern that facilities already knew their relative GHG-performance, and were simply waiting to assess external stakeholders' preferences.

Lastly, I produce a novel measure of industrial process-similarity across facilities, à la Fetter, Steck, Timmins, and Wrenn [2020]. GHG emissions reductions are largest when peer facilities have low and intermediate degrees of process-similarity—if there is pre-existing diversity in processes used, there is more scope for benchmarking to result in process changes. I then show that facilities in a peer group become more similar post-US Program disclosure. Further, they become more (less) like their carbon-light (carbon-intense) peers. I do not, however, make a claim about whether benchmarking spurs facilities to conduct their own technological search, or whether facilities take specific technological cues from the US Program disclosures of their peers. In either case, benchmarking leads to subsequent emissions reductions.

Having demonstrated GHG emissions reductions and benchmarking following US Program disclosure, I conduct additional analyses that explore the US Program's impacts prior to disclosure. Improved measurement practices can help facilities to better learn about their own fundamentals [Barrios, Lisowsky, and Minnis, 2019; Shroff, 2017]. Additionally, facilities might take action before a disclosure rule is implemented if they anticipate external pressure when disclosure eventually occurs (e.g., Fiechter, Hitz, and Lehmann, 2018). Thus, there are reasons to expect a GHG emissions reduction following measurement/reporting. I first estimate US facilities' unobservable pre-reporting period (2008 and 2009) CO_2 emissions using a Bayesian linear model. The estimation uses data about facilities' locally toxic emissions and embeds priors based on physical relations underlying fossil fuel combustion. I then extend the baseline difference-in-differences analysis backwards by two years. A significant US facility CO_2 reduction is observed after disclosure, but no significant response is observed prior to disclosure, when facilities first start measuring/reporting their emissions. I corroborate this result using two additional tests, one of which uses voluntary GHG disclosures made to the Carbon Disclosure Project. I conclude that there is considerable uncertainty around whether facilities have emissions responses prior to disclosure, as compared to their responses after disclosure.

Facilities might benchmark and reduce GHG emissions because of pressure from stakeholders with environmentally friendly preferences, or because of a desire to implement profitable efficiency improvements (absent environmental concerns). The final additional analyses explore these motives. I find that disclosure leads to relatively larger emissions reductions when facilities' state senators have progressive voting records with respect to climate change. In addition, *changes* in senatorial progressiveness are related to reductions, but only after US Program disclosure. Consistent with the US Program's broad goal of aiding policy formation, this suggests that concern about legislation is one motive for facilities to reduce their emissions; this concern becomes more salient following disclosure. No strong conclusions emerge when I examine pressure from other external stakeholders—capital markets, customers, and the general public—across-facility information linkages, or proxies for within-firm information frictions.

This paper firstly contributes to the literature on the real effects of mandatory, non-financial/CSR disclosure. Importantly, it does so in a setting where the disclosed information was largely not available elsewhere. Therefore, it examines the initial impact of information provision on the disclosed outcome itself. In contrast, related work examines the impact of information aggregation, disaggregation, or dissemination (e.g., Bennear and Olmstead, 2008; Christensen et al., 2017). Thus, the effects they study are incremental to the effect I measure.[2] In addition, the paper shows the effectiveness of disclosure with respect to a

[2]Some studies also consider the effect of information provision on indirect outcomes, for example, the impact of restaurant hygiene disclosures on food-borne illnesses (e.g., Dranove, Kessler, McClellan, and Satterthwaite, 2003; Jin and Leslie, 2003). In these studies, it tends to be difficult to pin down whether the disclosed outcome itself improves (e.g., restaurant hygiene), as sorting mechanisms can also affect the

global-externality characterized by an immense collective action problem, and whose impacts, despite being structural and pervasive, have relatively low salience/immediacy. Prior work has established that disclosure can be effective when, in contrast, emissions are toxic and act locally—that is, their effects are salient and coordination costs of stakeholder action are low (e.g., Bennear and Olmstead, 2008; Chen et al., 2018; Delmas, Montes-Sanchom, and Shimshack, 2010; Fetter, 2018; Graham and Miller, 2001; Hamilton, 2005). Whether these findings extend to the economically-distinct and important GHG setting, however, is an open question. I provide one of the first sets of economy-wide evidence on this matter.

Three concurrent papers—Downar, Ernstberger, Reichelstein, Schwenen, and Zaklan [2020], Grewal [2021], and Jouvenot and Krueger [2020]—study UK firms that began mandatory GHG reporting in 2013. My study differs in multiple important respects. First, it considers the effects of disclosure in an environment largely free of emissions pricing. Second, US Program disclosures are highly granular, and thus likely actionable at an operational-level, whereas the UK rule mandated highly-aggregated, firm-level disclosures. Third, the bulk of US Program data were not available elsewhere. The firms studied in the concurrent works, as well as the power plants in Matisoff [2013], already disclosed emissions through another medium. Hence, these studies examine the effects of dissemination and aggregation.[3]

Second, this paper contributes to the real and peer effects literatures by showing that mandatory disclosure can effect CSR-related improvements through 'benchmarking'. Roychowdhury, Shroff, and Verdi [2019] note the difficulty of identifying peer effects given Manski

indirect outcome (e.g., customers start patronizing cleaner restaurants, leading to reduced illness).

[3]In the voluntary disclosure space, Qian and Schaltegger [2017] find that firms reduce GHG emissions subsequent to initiating voluntary CDP disclosure. Conversely, Bolton and Kacperczyk [2020] find no such effect. A potential explanation for these contrasting results is a divergence in the factors dictating selection into voluntary disclosure. By examining mandatory disclosure, my study estimates a treatment effect unconditional on latent disclosure choice factors that might also drive emissions reductions through non-disclosure channels (e.g. a desire to create institutional legitimacy as Luo, 2019 suggests).

An adjacent literature examines how firms' CSR *activities* affect their other economic outcomes. See, for example: Deng, koo Kang, and Low [2013]; Flammer [2013]; Gillan, Koch, and Starks [2021]; Hart and Zingales [2017]; Lins, Servaes, and Tamayo [2017]; Luo and Bhattacharya [2006]; Manchiraju and Rajgopal [2017], and Servaes and Tamayo [2013]. A branch of this literature demonstrates the valuation consequences of voluntary GHG disclosures (both the disclosure decision and the disclosed content) [Bolton and Kacperczyk, 2020; Griffin, Lont, and Sun, 2017; Matsumura, Prakash, and Vera-Muñoz, 2014, 2020]. In contrast, I find ambiguous evidence of a return response to US Program disclosure.

[1993]'s reflection problem. Despite this, we have some evidence that firms take cues from their rivals' financial statements [Beatty, Liao, and Yu, 2013; Bernard et al., 2020; Durnev and Mangen, 2009; Li, 2016]. Using operational-level data, Fetter et al. [2020] and Grennan and Swanson [2020] show benchmarking following disclosure in fracking and hospital settings. In the CSR context, however, Christensen, Hail, and Leuz [2019] reveal a paucity of empirical evidence about benchmarking. Here, Cao, Liang, and Zhan [2019] show that firms improve their CSR performance after their peers pass CSR-focused resolutions. They highlight a competitive concern about peers' CSR performance, but do not speak to the role of disclosure regulation in promoting benchmarking. In concurrent work, Grewal [2021] follows the empirical approach of this paper and highlights benchmarking following the UK's GHG disclosure mandate. Given our papers' settings differ markedly in terms of disclosure granularity, disclosure platform, and agents' prior disclosure status, our papers' findings are complementary.

In constructing a novel measure of process-similarity to highlight benchmarking, this paper also adds to the literature studying competition. Specifically, I combine the granular, process-level data of the US National Emissions Inventory System/Emissions Inventory System with the methods of Fetter et al. [2020]. My measure complements existing similarity measures, such as industrial classification codes, and those based on product descriptions and internet co-search [Hoberg and Phillips, 2016; Lee, Ma, and Wang, 2015; Li, 2017].

Finally, this paper provides additional insights about the design and context of a granular, mandatory GHG reporting rule that might affect emissions. First, it highlights additional uncertainty, relative to the case of disclosure, around whether measurement and reporting to the regulator alone leads to emissions reductions. Disentangling measurement/reporting and disclosure also enriches the real effects of disclosure literature, which typically studies their joint effects, or one component only. Further, the paper suggests that disclosure promotes larger emissions reductions in regions where representatives in national legislative affairs (e.g., senators) are climate change progressives. That is, the prospect of credible climate change legislation is a factor that facilities consider when implementing emissions reduction strategies [Glazer and McMillan, 1992; Maxwell, Lyon, and Hackett, 2000; Suijs and Wielhouwer, 2019].

2 The US Greenhouse Gas Reporting Program

2.1 Background of the US Program

On December 26, 2007, US President, George W. Bush, signed into law the Consolidated Appropriations Act of 2008. This provided funds for the US Environmental Protection Agency (US EPA) to develop a mandatory GHG reporting rule, covering most sectors of the US economy. Although the Appropriations Act did not explicitly link the US Program to future emissions pricing, such motivations were evident [Richardson, 2012]. Shortly before the measure was passed, Senator Diane Feinstein said, "It's so critical that we have the data to understand the scope of the emissions problem. Solid data is essential to the establishment of an effective cap-and-trade system. . . this funding is an important first step towards helping to understand and reduce our nation's carbon footprint." In short, the main goal of the US Program is to canvas information for use in future potential GHG-related legislation and rule-making. Based on its experiences with programs such as the Toxic Release Inventory (TRI), however, US EPA recognizes that a GHG reporting rule can also lead to increased awareness of emissions among stakeholders and emitters, which in turn can lead to emissions reductions.[4] The TRI was created under the Emergency Planning and Community Right-to-

[4]More completely, US Environmental Protection Agency [2009a] states in the Federal Register that "EPA is promulgating this rule to gather GHG information to assist EPA in assessing how to address GHG emissions and climate change under the Clean Air Act. However, we expect that the information will prove useful for other purposes as well. For example, using the rich data set provided by this rule-making, EPA, States and the public will be able to track emission trends from industries and facilities within industries over time, particularly in response to policies and potential regulations. The data collected by this rule will also improve the U.S. government's ability to formulate climate policies, and to assess which industries might be affected, and how these industries might be affected by potential policies. Finally, EPA's experience with other reporting programs is that such programs raise awareness of emissions among reporters and other stakeholders, and thus contribute to efforts to identify and implement emission reduction opportunities. These data can also be coupled with efforts at the local, State and Federal levels to assist corporations

Know Act in the wake of the Bhopal gas-leak disaster in India. As such, it places relatively more emphasis on empowering the community with data and increasing the accountability of polluters [Hamilton, 2005].

US EPA proposed a mandatory GHG reporting rule on April 10, 2009, and after soliciting comments, published a final rule on October 30, 2009. Over this period and into the first year of the US Program, however, the prospects for a national cap-and-trade system dimmed significantly for a number of reasons. First, high unemployment after the Great Recession, coupled with vigorous lobbying by the electric utility, oil, and gas industries, cast the burden to be borne by facilities in a far less favorable light.[5] Second, the Waxman-Markey Bill, approved by the House of Representatives in June, 2009, which would have established a national GHG emissions trading scheme, never made it to the Senate for discussion or vote. Its Senate proponents and some environmental groups had also turned on the bill by January 2010, arguing it had lost ambition and made too many concessions to large emitters. Third, Republicans took control of the House of Representatives in November 2010, displaying a resistance to climate change policy. Lastly, US President Barack Obama's administration gave greater prominence to healthcare and financial regulation, while his focus on climate policy shifted to energy independence. Despite these changes in attitude towards cap-and-trade, the US Program remained intact, though it was no longer the harbinger of emissions pricing it had been conceived to be.

2.2 Details of the US Program

The US Program took effect on January 1, 2010, with first reports due for submission (after an extension) to US EPA on September 30, 2011. These 2010 data were publicly disclosed by US EPA on January 11, 2012. Figure 1 chronicles the relevant events visually. The US Program requires facilities to report their GHG emissions, by specific gas, coming from any

and facilities in determining their GHG footprints and identifying opportunities to reduce emissions (e.g., through energy audits or other forms of assistance)."

[5] `Opensecrets.org` and the `Center for American Progress Action Fund` found that the energy sector spent over $500 million from January 2009 to June 2010, primarily to lobby against climate change legislation.

of 41 source categories. Examples of GHGs are carbon dioxide (CO_2), methane, and nitrous oxide—CO_2 is the chief GHG emitted by facilities. Examples of source categories include stationary combustion, paper and pulp production, and cement manufacturing. Disclosure thresholds are specific to the source categories, but 25,000T CO_2e is the most common threshold for most facilities.[6] Depending on the source category, facilities must disclose additional information at the sub-facility level (i.e., unit or process level). For example, 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 detailed emissions calculations. Thus, the US Program provides very granular data and is highly prescriptive.

[Figure 1 about here.]

US EPA does not require third-party verification of submitted data. Instead, reports must be self-certified by facilities. Nonetheless, the US Program incorporates features to promote its data quality. During the submission process, the US Program's electronic reporting platform provides real-time feedback about potential errors to reporting facilities. Reports received by the US EPA are then subjected to a series of electronic checks to flag potential errors. After manually reviewing these flags, US EPA can follow up with a facility to ascertain the reason for any irregularity. In addition, the US Program is given force by the Clean Air Act, a major piece of federal legislation. This lets US EPA levy penalties of up to $37,500

[6] To verify whether or not a facility falls below a threshold, US EPA requires a basic measurement submission, which typically involves pen-and-paper calculations based on the amount of resources consumed during a process, and standard conversion factors. This calculation does not require the heat or carbon content of fuels to be physically measured, nor the installation of measurement devices.

US EPA prescribes measurement methods for US Program facilities. For example, CO_2 emissions from stationary combustion of fossil fuels can be measured using one of four measurement tiers, which increase in their degrees of accuracy and cost. Larger combustion units must use higher measurement tiers. Tier 1 involves multiplying a mass of combusted fuel by a default high heating value and emission factor, provided by US EPA, to arrive at a CO_2 figure. A high heating value is the amount of energy released per unit of mass of a particular fuel combusted. An emission factor is the mass of a pollutant (in this instance CO_2) emitted per unit of energy released from fuel combustion. Tier 2 is similar to Tier 1, except that the facility determines the high heating value through periodic sampling and testing. Tier 3 is also similar to Tier 1, except the facility measures and uses the carbon content of a fuel instead of using a high heating value and an emission factor. Tier 4 involves using continuous emissions monitoring systems to directly measure CO_2 emissions. These systems range in cost from $25,000 to $75,000 [Singh and Bacher, 2014].

per day of a violation, which includes failure to report emissions, failure to retain records needed for report verification, and report falsification. That said, US EPA has yet to take an enforcement action regarding the US Program and did not list the US Program in its 'National Enforcement Initiatives.'

As described above, the US Program was implemented primarily to inform future emissions policy decisions, with a side benefit of generating a conversation around GHG emissions. As such, US EPA makes these data publicly accessible in multiple formats with varying complexity. Facility Level Information on GreenHouse gases Tool (FLIGHT) is an interactive map-based-tool geared towards novice users. Facility-level data are also available in spreadsheet format, which I use in this paper. Advanced users can access the totality of US Program data by querying US EPA's Envirofacts database. US EPA makes an additional spreadsheet available that contains the names of facilities' highest-level US owners, though combining this with emissions information and ensuring consistency across names is a cumbersome process. From direct communications with US EPA, the GHG portion of Envirofacts received over 100,000 page-views from January 2013 to June 2020, indicating US Program data are frequently accessed.

3 Related Literature and Hypothesis Development

Non-financial/CSR disclosure mandates span many areas, including workplace safety, healthcare, and mineral extraction rights [Johnson, 2020; Kolstad, 2016; Rauter, 2020]. Their goal is to create an 'action-cycle' whereby disclosed information becomes embedded in the decisions of disclosers and users. The responses of users can, in turn, feed back into disclosers' decisions [Hombach and Sellhorn, 2019; Weil et al., 2006]. As discussed in Section 2.1, while the impetus for the US Program is to gather information for potential future GHG regulation, US EPA also recognizes the potential for an 'action-cycle' to emerge. Potential users are legislators, regulators, the public, investors, customers, and, as will be discussed, competitor facilities.

In the environmental domain, Chen et al. [2018] show that CSR reports mandated by stock exchanges lead Chinese firms to reduce toxic SO_2 and wastewater emissions. Examining more granular and technical disclosures, Fetter [2018] likewise shows fracking firms reduce the toxicity of their chemical mixtures. Bennear and Olmstead [2008] and Delmas et al. [2010] show that utilities improve their environmental performance following mandatory disclosures to customers. US EPA's TRI is perhaps the most studied pollution disclosure rule, and is credited for a dramatic fall in toxic emissions [Weil et al., 2006; Graham and Miller, 2001]. The TRI and US Program share a number of similarities—both are administrated by US 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 targeted disclosure in the previously cited studies suggests that disclosure can play a role in curbing GHG emissions too. This leads to the first hypothesis, expressed in alternate form:

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

A number of factors, however, push against H1. First, the costs of reducing GHG emissions following disclosure might swamp the benefits of doing so, especially given that firms have natural incentives to minimize costs such as fuel. Regarding the US Program specifically, its 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 provides an incomplete picture of a firm's global emissions. Thus, the potential for external pressure at the firm-level (e.g., Chen et al. [2018]; Christensen et al. [2017]; Rauter [2020]) loses some force.[7]

[7] Online Appendix A describes the comment letters US EPA received after proposing a mandatory reporting rule and before publishing a final rule. These provide a glimpse at potentially interested stakeholders. I focus on the 766 letters related to general stationary fuel combustion, the largest emissions source. The vast majority of these letters are from manufacturers and manufacturing associations 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 advocacy organizations jointly submitted seven letters pushing for more comprehensive emissions measurement and reporting. A small number of state governmental agencies submitted letters requiring clarification of the rules, and a small number of

More generally, the economics of GHG externalities set climate change in stark contrast to other environmental disclosure settings. Emissions from the TRI and the prior cited work are toxic, making their effects (e.g., illness, smog, acid rain) salient and likely to trigger outrage. They also act locally, keeping the coordination costs of stakeholder action (e.g., shaming, litigation) surmountable [Coase, 1960]. GHG emissions, however, are largely non-toxic. Further, climate change damages, though systemic and multifarious, are relatively slow-moving, occurring over decades. GHG emissions also impose a global externality, with similarly global coordination problems: each emitter contributes only marginally to the global temperature and affected stakeholders are dispersed both in space and time. On top of this, quantifying the social damages that a facility's emissions cause is fraught with uncertainty [Barnett, Brock, and Hansen, 2020]. Weighed against the cited work, these factors highlight the importance of empirically testing the relation between GHG disclosure and emissions.

Attention then naturally falls to the mechanism through which emissions reductions occur. Using a regression discontinuity design, Cao et al. [2019] provide evidence of CSR-mimicking between competitors. Ioannou et al. [2016] study self-reported CDP data and find that target-setting plays an important role in firms' GHG emissions reductions. Connecting these ideas with the US Program's significant granularity, it emerges that facilities (and the science/engineering consulting firms that serve them) 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 that:

H2: *Facilities use their peers' GHG emissions disclosures for benchmarking (i.e., to better assess their own GHG-performance).*

Benchmarking, and consequent emissions reductions, could be driven by pressure from stakeholders with environmentally friendly preferences (e.g., Friedman and Heinle, 2016). These stakeholders have shown a concern for relative CSR performance [Clarkson et al.,

GHG consulting firms made recommendations for measurement methods. No letters were from the investing community.

2015; Hartzmark and Sussman, 2019]. For example, Fung and O'Rourke [2000], describe how journalists and environmentalists fixated on the worst TRI performers. The literature also argues that firms can take cues from their peers' disclosures [Foster, 1989; Dye, 1990; Shroff, Verdi, and Yu, 2014], and that firms occasionally forgo NPV-positive energy-efficiency improvements, possibly due to incomplete information [McKinsey Global Energy and Materials, 2009; Gerarden, Newell, and Stavins, 2017]. Thus, profit and efficiency motives might also drive benchmarking. Grennan and Swanson [2020] and Fetter et al. [2020] highlight such behavior in fracking and hospital settings.

External pressure and profit/efficiency motives are not mutually exclusive, and could interact; for example, outsider pressure could lead firms to discover profitable improvements, making it difficult to disentangle the two. Therefore, H2 considers benchmarking generally. Further, emissions reductions following benchmarking might conceivably occur in two ways (which again, are not mutually exclusive). First, finding a 'red-flag' after benchmarking might spur a facility to engage in a technological search. Second, US Program data themselves might reveal clues about the type of change facilities can make. I do not hypothesize which of these causal chains operate. In either case, observing peers' data helps to assess relative GHG-performance, which then leads to emissions reductions.

4 GHG Emissions Reductions Following Disclosure

4.1 Control Sample

There a range of factors, beyond disclosure, that could cause facilities' GHG emissions to decline. These include changes in customer demand, the relative prices of inputs, and an evolution in production technologies. Thus, a credible estimation of the effects of the US Program must account for these factors. To this end, I use Canadian facilities as my control group. Canadian facilities emitting over 100,000T (50,000T) CO_2e have been reporting to Environment Canada's GHG Reporting Program since 2004 (2009). During my sample period, the Canadian Program required disclosures by specific GHG and general category (e.g., 'stationary combustion', 'industrial processes', 'venting', 'flaring'). Unit or process level disclosures are not required, and thus, while still granular, the Canadian Program is coarser than the US Program. Nevertheless, the scope of both reporting programs allows the examination of the US Program's effect across a wide-range of industries.

The United States and Canada are both developed countries that share geographic proximity, cultural similarities, an integrated network of oil and gas pipelines, and both experienced recent shale energy booms. Thus, I expect that a large portion of the non-disclosure-based shocks to GHG emissions affect both countries similarly. Accordingly, I assume that Canadian facilities' GHG emissions provide a suitable counterfactual for US facilities' emissions, with respect to the US Program's effects.

To provide support for the parallel-trends assumption underlying the difference-in-differences estimation, Figure 2a plots mean logged GHG emissions by country and year. The two countries' emissions share similar changes from 2010 to 2011, after which US facilities' emissions steadily decline. To assess parallel trends going further back, I estimate US facilities' CO_2 emissions for the years 2008 to 2013. I defer detailing the estimation process for now, but an interested reader can jump to Section 6.1.1 for an overview. Figure 2b shows that the trends in both countries' CO_2 emissions remain parallel.[8]

[Figure 2 about here.]

In terms of benchmarking, Canadian facilities are suitable as a control group because

[8]To maintain visual consistency, I use a balanced panel to abstract from the effects of facility entry and exit that are otherwise captured by regression controls. I also exclude Massachusetts' facilities, which began emissions reporting and disclosure a year earlier than other US facilities. The main regression tests do not impose these restrictions.

To provide further support for the suitability of the Canadian control group, Online Appendix Figure A3 shows that yearly percent changes in the countries' GDPs move together, with this co-movement holding during the US financial crisis. Although Canada did not experience a deep banking crisis as the United States did, the United States is by far Canada's largest trading partner: in 2014, its trade in goods with the United States was over eight times that with the European Union, its next-largest trading partner. Online Appendix Figure A4 also shows that public interest in climate change in the two countries, measured using `Google Trends`, moved together during the sample window. This reduces the concern that US facilities faced differential variation in public sentiment towards climate change than did Canadian facilities.

of their geographic distance from US facilities. Research shows that geographic closeness reduces barriers to information flows [Coval and Moskowitz, 2001; Ellison, Glaeser, and Kerr, 2010; Engelberg, Ozoguz, and Wang, 2018]. Thus, facilities arguably have a better contextual understanding of peers that are located in the same region as them—an understanding that includes these peers' pricing decisions, quantity of goods produced, regulatory environment, and other local shocks faced. This information can enable benchmarking; conversely, a lack of contextual knowledge, due to distance, can inhibit it. For example, the manager of a cement manufacturing facility might gauge how much cement a rival in the same region produces, as they compete in the same local market. When US Program data become public, the manager can then assess whether their own facility uses more or less fuel, per unit of cement produced, than the rival.

Regarding other candidate control groups, Matisoff [2013] reports that the US has a number of state-level Programs. Because the US Program is overseen by federal regulator, its data are arguably of higher quality [Agarwal, Lucca, Seru, and Trebbi, 2014]. Further, for the majority of these state programs, it is unclear when first data disclosure occurred, if at all. Indeed, Matisoff [2013] notes that "State Reporting data. . . may not be complete, up to date, or available online." Only California's and Massachusetts' Programs have their data readily available online and have emissions reporting thresholds less than or equal to those of the US Program. Massachusetts' facilities' 2009 emissions were first disclosed in 2011; I code their data accordingly. Although Californian facilities have disclosed emissions data throughout my sample period, environmental issues typically receive heightened prominence in California, raising concerns about comparability.[9] US power plants were also subject to reporting requirements prior to the US Program, as described in Section 1. An issue that raises comparability concerns, however, is that power plants face regulatory incentives and scrutiny distinct from those that non-power plants face (e.g., US EPA's Acid Rain

[9] For example, California alone has power under the US Clean Air Act to request a waiver to forego implementing federal automobile tailpipe emissions standards. California enforces its own, more stringent tailpipe standards, with other states able to opt in to Californian standards in lieu of federal standards.

Program, Cross-State Air Pollution Rule, and Mercury & Air Toxics Standards; and the Obama Administration's proposed Clean Power Plan).[10]

Lastly, some facilities' owners were already reporting firm-level GHG emissions to the Carbon Disclosure Project. The CDP is a not-for-profit organization that surveys large firms about their GHG emissions and emissions governance. Since 2003, surveys have been sent annually on behalf of investor signatories. S&P 500 firms and the 200 largest public Canadian firms receive CDP surveys. There are three features of the CDP that reduce its suitability to produce control facilities in this study. First, participation in the CDP is voluntary, which raises concerns about selection, as CDP participating firms might generally be more attentive to GHG emissions. Indeed, Qian and Schaltegger [2017] find that firms reduce GHG emissions subsequent to initiating voluntary CDP disclosure. Second, firms participating in the CDP are larger. Although one option might be to use non-participating S&P 500 firms to identify treatment facilities, this would arguable magnify the selection problem. Third, CDP facilities might still benchmark under the US Program. US Program disclosures are considerably more granular, being informative at the facility and sub-facility levels. CDP data are rarely informative in this way.

4.2 Research Design

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

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

i, j, and t represent facilities, four-digit NAICS industries, and years 2010 to 2013. GHG is logged GHG emissions. US indicates US-located facilities, and $\mathbb{1}_{\{t \geq 2012\}}$ indicates

[10]The combination of the Cross-State Air Pollution Rule and Mercury & Air Toxics Standards, both enacted in 2011, have lead to the phase-out of many old, carbon-intensive coal-fired US power plants that were grandfathered into the 1970 amendments of the Clean Air Act. This has contributed meaningfully to the transition away from the use of carbon-intensive coal in US power plants [Revesz and Lienke, 2016].

years 2012 and onward (2011 and onward for Massachusetts' facilities). GHG emissions are logged to account for scale differences across facilities; as such, β_3 approximates the percent change in emissions following the US Program's first data disclosure. X denotes control variables described below. I eventually add facility and four-digit NAICS-year interaction fixed effects. These are granular—an example of a four-digit industry is 3311: Iron and steel mills and ferro-alloy manufacturing. The estimation then compares US facilities to Canadian facilities, accounting for any industry-level shocks and persistent facility characteristics. I cluster standard errors by industry-year to account for cross-sectional shocks at the industry level.

I include logged GDP (value-added) 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 helps account for temporal and regional variation in natural gas prices faced by facilities. Natural gas is more CO_2-efficient than oil, and, given that US natural gas production increased after 2005, following technological advances in unconventional drilling techniques, a concern is that a fundamental change in the US natural gas market might lead US facilities to reduce their emissions. Although Canadian and US energy markets are highly integrated and the two countries' gas prices co-move closely, the concern remains that proximity to shale gas reserves affords US facilities differentially cheaper access to natural gas over my sample period. I use natural gas pricing-points from Dawn Ontario and AECO Storage in Canada's east and west, and Dominion South, Henry Hub, SoCal Border, and Kern River in the US' north-east, south, west and center. Lastly, I include the number of efficiency incentives and regulations that are both applicable to a facility and implemented at the federal or state levels. The regulations typically relate to building energy use, and the incentives typically exist as rebates for energy-efficiency improvements.

To explore year-by-year emissions differences between US and Canadian facilities, I estimate the following OLS regression model:

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

The $\beta_{3,k}$s track how US and Canadian facilities' emissions differ relative their difference in 2011 (the latest pre-disclosure year), after considering the effects of the other covariates.

I exclude plant facilities (NAICS code 2211) because they are subject to reporting requirements prior to the US Program and face substantially more regulatory incentives and scrutiny about GHG emissions than other facilities (as described in Section 4.1). Municipal waste facilities (NAICS code 5622) are also excluded. This is because these facilities emit methane, which results from the anaerobic breakdown of organic matter, a process that occurs incidentally, with its rate affected significantly by waste composition, and by ambient conditions including barometric pressure, amount of rainfall, and temperature [Héroux, Guy, and Millette, 2010; Santhosh, Lakshmikanthan, and Sivakumar Babu, 2017]. Fluctuations in these conditions can also induce long-term measurement error [Xu, Lin, Amen, Welding, and McDerrmitt, 2014]. These features make the measurement of precise GHG emissions from waste facilities particularly difficult. In contrast, the emitting processes in the other industries in the US Program are more directed and standardized, facilitating measurement. In addition, given the variability in ambient conditions across the US and Canada, it is uncertain if Canadian municipal waste facilities are a good control for their US counterparts.

I also exclude facilities located in California because, as described in Section 4.1, California is known for its aggressive regulatory stance towards environmental issues, raising concerns about comparability with the rest of the sample. Lastly, I exclude facilities owned by non-corporate entities, because benchmarking appeals to a notion of profit or shareholder value maximization. With non-corporate entities, it is unclear what the ultimate operating objective is (e.g., customer service, cost-minimization, profit-maximization, employment, social welfare). This last exclusion drops 940 observations—because of the fixed effects structure, 597 of these contribute directly the estimation of the US Program, 422 of these being Colleges, Universities, and Professional Schools.

4.3 Data and Descriptive Statistics

The bulk of the main dataset comes from US EPA's Greenhouse Gas Reporting Program and Environment and Climate Change Canada's Greenhouse Gas Reporting Program.[11] Natural gas price data come from SNL Financial and Alberta Energy Regulator, and data about efficiency incentives and regulations come from North Carolina State University's Database of State Incentives for Renewables & Efficiency and Natural Resources Canada's Directory of Energy Efficiency and Alternative Energy Programs.

Tables 1a and 1b show coverage across a wide range of industries and states/provinces. Though many industries have zero Canadian observations, 88% of the observations share an industry-year with one of more observations from the other country. The five most common industries (oil and gas extraction; pipeline transportation of natural gas; basic chemical manufacturing; pulp, paper, and paperboard mills; and petroleum and coal products manufacturing) account for 54% of the sample. The five most common states/provinces (Texas, Louisiana, Pennsylvania, Ohio, and Alberta) account for 32% of the sample. Table 2a shows that US facilities compose 89% of the sample and emit roughly half the amount of GHGs per facility ($e^{11.0-11.8} - 1 = -0.55$). These differences reflect both the larger size of the US industrial sector and the US Program's lower GHG reporting threshold. Table 2a also shows

[11] In 2011, GHGs from additional source categories (electronics manufacturing, fluorinated GHG production, magnesium production, industrial wastewater treatment, and the following related to petroleum and natural gas systems—offshore production, processing, transmission, compression, storage, and export) became applicable for reporting. I exclude emissions from these source categories, to maintain comparability across years and because a number of these categories were not included in the Canadian Program.

To code facility ownership type, I consult the parent company information provided by the US and Canadian Programs; however, only the highest parent company at the respective country levels are reported. Thus, I use Bloomberg.com research reports to manually identify the ultimate parent company. I identify a parent company as public if it has a GVKEY in the COMPUSTAT database, or has a quoted stock price. I identify an owner as non-corporate if the owner's name includes words such as 'county', 'state', 'city', or the facility is a university. I code the remaining facility owners as private. I only consider owners having more than 50% ownership of a facility. This process took between 50 to 100 hours of labor, highlighting the difficulty in cleanly aggregating US Program emissions to the firm-level. I caveat all results that use facility owner information by acknowledging this process is likely imperfect.

that US facilities faced a similar average gas price and fewer GHG efficiency regulations.

[Table 1 about here.]

[Table 2 about here.]

4.4 Emissions Reduction Findings

4.4.1 GHG Emissions Levels

Table 3 shows the results of estimating Model 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 GHG emissions variation; in this case, the difference-in-differences coefficient ends up more precisely estimated [DeHaan, 2020]. Moving to Column 3 shows that adding industry-year fixed effects reduces the estimated treatment effect appreciably, highlighting the importance of controlling for industry-level shocks. Moving to Column 4 shows that including control variables slightly increases the estimated treatment effect. Column 4 provides the primary result: consistent with H1, US facilities reduce GHG emissions by a relative 7.9% ($p = 0.014$) following the first disclosure of US Program data in 2012. This is 4.1% standard deviation change in US facilities' logged GHG emissions, and a 21% standard deviation change in US facilities logged GHG emissions residualized against fixed effects.[12]

[Table 3 about here.]

To explore the dynamics behind the GHG emissions reduction, Figure 3a plots the $\beta_{3,k}$s obtained from estimating Model 2.[13] Further supporting H1, the relative emissions differ-

[12] In line with DeHaan [2020], descriptive statistics for variables residualized against fixed effects are provided in Online Appendix Table A2. To provide a comparison for the estimated treatment effect, the concurrent papers—Downar et al. [2020], Grewal [2021], and Jouvenot and Krueger [2020]—estimate 14%, 11%, and 12% reductions for UK emitters. It should be noted, however, that they capture the effects of dissemination/aggregation of information that was already publicly available, for facilities that were already subject to a GHG emissions trading scheme. I estimate a treatment effect for facilities that largely had no emissions information available elsewhere, and that were largely not subject to emissions pricing.

[13] To ease exposition, I exclude Massachusetts' facilities from this test and the subsequent emissions dispersion test. These facilities began emissions reporting and disclosure a year earlier than other US facilities.

ential stays close to zero until the US Program's first disclosure, after which US facilities' display a relative fall. Figure 3a provides further support for the parallel trends assumption.

[Figure 3 about here.]

Online Appendix C.1 shows the GHG emissions reduction result is: present for industries for which it is difficult to offshore production (reducing concern that US emissions are merely shifted overseas); not driven by oil and gas facilities; not driven by the few states that have GHG emissions pricing; robust to including non-corporate facilities and municipal waste facilities (individually or jointly); not driven by facilities owned by firms with a higher likelihood financial misstatement; subject to minor Stable Unit Treatment Value Assumption violations very close to the border (consistent with a benchmarking story), and; persistent if extending the sample window forward to 2016. Online Appendix C.3 describes entry into, and exit from, the US Program.

Another concern might be that emissions reductions have long lead-time, and are unlikely to materialize in a year. It is true that longer lead-times 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 when moving from 2012 to 2013. However, less complex retrofits or replacements, maintenance, calibration and optimization, and other behavioral changes, can be implemented within a year. For example, Colket et al. [2012] report that a combustion control system and a sensor package retrofit for a 25 MMBtu dual fuel 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 GHG Emissions Intensity

Facilities could achieve lower GHG emissions by implementing GHG-reducing improvements, or by simply scaling back economic activity. To identify the former, I measure GHG-intensity by aggregating GHG emissions to the firm-level, dividing by Cost of Goods

Sold (COGS), and logging the result. This treats COGS as a proxy for the quantity of goods produced. I take weighted averages of the control variables, with weights based on each facility's 2011 GHG emissions. Thus, the *US* variable becomes continuous, ranging from zero to one. I also control for 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]. To avoid the effects of facility acquisitions and disposals, I require a balanced panel of facilities. All firm-level dependent variables are winsorized at 1 and 99%.

Column 5 of Table 3 shows that firms reduced their GHG-intensity following US Program disclosure and did not just scale back operations. The GHG percentage change minus the COGS percentage change is -22%, but this needs a major caveat: firm-level aggregation reduces the number of facilities contributing to the analysis by two thirds and adds noise by capturing overseas economic activity. These factors could drive the large coefficient estimate.

If facilities had implemented costly improvements that improved operational efficiency (e.g., less fuel used per good produced), I would expect capital expenditures and gross margins to increase. Columns 6 and 7 support this idea: capital expenditures increased by 3.4% of assets and gross margins increased by 4.3% for US Program firms. Given the caveat above, I view these last two tests as supporting the idea that firms reduced their GHG-intensities, and not as a precise quantification of the financial costs and benefits of these changes.

5 Benchmarking of GHG Emissions

This section tests H2 which states that facilities use their peers' GHG emissions to benchmark and assess their own GHG-performance.

5.1 Emissions Dispersion

In the Korean setting, Berger, Choi, and Tomar [2020] document an increase in profitability dispersion after firms begin withholding detailed cost disclosures. They argue that these disclosures lead to across-firm information flows. Grennan and Swanson [2020] show that the dispersion of negotiated prices paid by hospitals for supplies shrinks after they gain access to the purchase price history of their peers. If benchmarking under the US Program facilitates a convergence in practices, I expect there to be less dispersion in GHG emissions after disclosure.

I estimate the following differences-in-differences model:

$$GHG\_DISP_{it} = \beta_1 \mathbb{1}_{\{t \geq 2012\}t} + \beta_2 US_i + \beta_3 \mathbb{1}_{\{t \geq 2012\}t} * US_i + \gamma X_{jt} + \eta_i + \eta_t + \varepsilon_{it}. \quad (3)$$

i indexes (four-digit) industry-country and t indexes years 2010 to 2013. GHG_DISP is the standard deviation, or 90th-10th percentile difference, of raw GHG emissions (in 1,000T CO_2e). For controls, I use the standard deviation of percentage changes in year-lagged gas prices and the standard deviation of emissions reduction incentives and regulations. I include industry-country and year fixed effects. Columns 1 and 2 of Table 4 examine the standard deviation and 90th-10th percentile difference of GHG emissions. Both show reduced emissions dispersion within US industries after US Program disclosure, relative to Canadian industries ($p = 0.044$; $p = 0.038$). This emissions convergence is high-level evidence consistent with benchmarking.

[Table 4 about here.]

However, these tests do not show that 'poor GHG performers' reduced their emissions more than others. This is because the outcome variable is a summary statistic, and because facilities might have high emissions levels, despite emitting a low amount of GHGs per unit of goods produced. The next set of tests tackles this issue, highlights the key role of information observability, and addresses the alternative explanation of a naturally higher US rate of technological convergence.

5.2 Effect of Relative Carbon-Intensity

These next tests assess whether facilities emitting more GHGs per unit of goods produced (relative to their peers) reduce their emissions more after their peers' data become observable.

I begin by producing a facility-level measure of carbon-intensity. CO_2 is largely produced by burning fuels. Online Appendix B.1 describes the intuition, construction, and validation of this measure in detail, but in brief, 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 US EPA's National Emissions Inventory and Emissions Inventory System (US NEI/EIS), which provide information about US facilities' criteria and hazardous air pollutant emissions (not GHGs). Importantly, these data are available at the facility-process-pollutant level. For each four-digit NAICS industry, I use 2014 US NEI/EIS data to identify the most common non-fossil fuel combustion pollutant reported by facilities.[14] As an example, the Kraft process in the paper and pulp industry converts wood chips to wood pulp through a combination of chemical and mechanical treatment. The chemical treatment produces VOCs. The idea is that producing the same amount paper will produce the same amount of VOCs, even if the fossil fuel combustion processes that provide heat to the wood chips become more efficient. Thus, CO_2 emissions can be scaled by VOC emissions to produce a measure of carbon-intensity for paper and pulp facilities.

To create values for carbon-intensity, I scale a facility's CO_2 emissions by its emissions of its respective non-combustion pollutant that year. This emissions ratio is then normalized within industry-state, so that the highest value becomes one (most carbon-intense) and the lowest value becomes zero (most carbon-light). There are two reasons to normalize within

[14] 2008, 2011, and 2014 are comprehensive years characterized by lower emissions thresholds for reporting and, thus, cover more facilities. These years' data form part of US EPA's `National Emissions Inventories`, available online. Emissions data for the non-comprehensive years—2009, 2010, 2012, and 2013—are found in the US Emission Inventory System database and were provided directly to me by US EPA.

states. First, it might be more useful to benchmark against industry-peers that compete in local markets. Second, many states in the US directly regulate VOCs to a greater extent than federal regulations require [Chemical Watch, 2019].

To test whether carbon-intensity is related to CO_2 emissions reductions following US Program disclosure, I estimate the following OLS model for US facilities in the year 2012:

$$
\begin{aligned}
CH\_CO2\_2012_i = &\beta_1 CARBON\_INTNS\_2010_i + \beta_2 CARBON\_INTNS\_2011_i + \\
&\gamma X_i + \eta_j + \varepsilon_i. \qquad (4)
\end{aligned}
$$

CH_GHG_2012 is the percentage change in 2012 CO_2 emissions, relative to 2011 (winsorized at 1% and 99%), *CARBON_INTNS_t* is peer-normalized carbon-intensity in year t, X denotes control variables, and η_j are industry fixed effects. The controls are the year-lagged percentage change in gas price, and the change in efficiency incentives and regulations.

Column 3 of Table 4 provides results that are consistent with benchmarking. Carbon-intense facilities reduce their CO_2 emissions more in 2012, the year US Program data needed to compute 2010 carbon-intensity become public. This could, however, be driven by technological convergence or mean-reversion, and not disclosure. Under this alternative, I expect 2011 carbon-intensity to 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.

Consistent with a disclosure-benchmarking effect, Column 3 shows that 2010 carbon-intensity is significantly associated with 2012 CO_2 reductions, but 2011 carbon-intensity is not. This also pushes back against the idea that facilities my have been completely aware of their GHG-performance without benchmarking, and merely waited until disclosure in 2012 in order to determine the value attached to GHG-performance by external stakeholders. Under this alternative, I would expect 2011 carbon-intensity to better predict 2012 emissions reductions. Columns 5 and 6 limit the sample to facilities with above-industry-median carbon-intensity. The results become more striking: variation in carbon intensity has a larger effect on emissions reductions for those facilities that are already carbon-intense.

These tests support H2; however, there is another feature worth highlighting. Specifically, these tests exploit purely within-US facility variation and do not use the Canadian facilities as controls. This helps to 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

These next tests of H2 examine 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 US SEC's EDGAR database. Further, they show that such information-acquisition leads to mimicking in subsequent capital and R&D investment levels. 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.[15]

To test whether benchmarker facilities reduce their GHG emissions more than non-benchmarkers following US Program disclosure, I estimate the following OLS model:

$$\begin{aligned} GHG_{it} = &\beta_1 \mathbb{1}_{\{t \geq 2012\}t} + \beta_2 US_i + \beta_3 \mathbb{1}_{\{t \geq 2012\}t} * US_i + \\ &\beta_4 \mathbb{1}_{\{t \geq 2012\}t} * US_i * \mathbb{1}_{\{\text{BENCHMARKER}\}it} + \gamma X_{it} + \eta_i + \eta_{jt} + \varepsilon_{it}. \end{aligned} \tag{5}$$

$\mathbb{1}_{\{\text{BENCHMARKER}\}}$ indicates US benchmarker facilities, and β_4 captures an incremental treatment effect of the US Program for this cross-section. Column 1 of Table 5 shows this incremental effect is negative, but insignificantly so. This test uses few observations, however, because of the low-overlap between the Bernard et al. [2020] sample and the sample for this study. Bernard et al. [2020] also provide a more comprehensive EDGAR search dataset,

[15] I thank Darren Bernard, Terrence Blackburne, and Jacob Thornock for generously sharing their data with me.

which is five times larger but with a 60% firm-identifier accuracy. Column 2 provides the results when using this larger EDGAR-search sample. The coefficient estimate of β_4 remains economically similar, and gains statistical significance—benchmarkers reduce their emissions by 7.2% more than non-benchmarkers do. Column 3 estimates a treatment effect for each benchmarking quintile; facilities in the fourth and fifth quintiles of benchmarking activity have the largest emissions reductions.

[Table 5 about here.]

These results provide further support for the benchmarking mechanism. They also help to address two alternative economic stories. First, it could be suggested that facilities perhaps did not benchmark using US Program disclosures in 2012, but rather, began implementing emissions reduction changes in 2010 (due to anticipated external pressure), with these changes having long lead-times [Fiechter, Hitz, and Lehmann, 2018]. However, this explanation would not explain the observation that the emissions reduction is larger for benchmarking facilities. Second, as described in Section 5.2, it could be posited that facilities perhaps had no need for benchmarking, and rather, waited until disclosure to assess external stakeholders' preferences over GHG-performance. However, this explanation would need to suggest why benchmarker facilities are more responsive to external-pressure.

5.4 Process Convergence

This last set of benchmarking tests considers how the physical processes that facilities employ change after US Program disclosure. Returning to the process-level US NEI/EIS described in Section 5.2, I focus on the fossil fuel/waste combustion processes that facilities had in place in 2010 (the start of my main sample). Online Appendix B.1 describes how these processes are identified in the data. An example of a fuel combustion process is 'External Combustion Boilers: Commercial/Institutional: Bituminous/Subbituminous Coal: Pulverized Coal: Wet Bottom (Subbituminous Coal).' In the spirit of Fetter et al. [2020], I then compute the Jaccard similarity between each US facility and its (hypothetical) representative state-industry peer, as described below.

Let $\mathscr{P}$ denote the set of possible combustion processes. Let $a_{ip} = x_{ip}/n_i$, where $x_{ip} \in \{0,1\}$ denotes whether facility i uses process p, and n_i is the total number of processes used by facility i. Similarly, let $a'_{ip} = x'_{ip}/n'_i$, where x'_{ip} is the number of facility i's peers that use process p, and n'_i is the total number of processes used by facility i's peers (where each instance of a facility-process increments n'_i by 1). a_{ip} and a'_{ip} are the fractional prevalences of process p for facility i and facility i's representative peer. Let $A_i = \sum_{\mathscr{P}} a_{ip}\mathbb{1}(a'_{ip} > 0)$ be the prevalence-weighted share of the representative peer's processes used by facility i, and $A'_i = \sum_{\mathscr{P}} a'_{ip}\mathbb{1}(a_{ip} > 0)$ be the converse. Finally, the Jaccard similarity between facility i and its representative peer is:

$$s_i = \frac{A_i A'_i}{A_i + A'_i - A_i A'_i} \in [0,1] \tag{6}$$

This is, loosely, the ratio of the intersection of two sets over the union of those sets. $s_i = 1$ means maximal similarity.

The average similarities between US facilities and their representative peers in 2010 and 2013 are similar at 0.281 and 0.276 (p = 0.27). Row 1 of Table 6a, however, indicates that the average percentage change in similarity is a significantly positive 10% (p = 0.008). Given the insignificant average level change, the implication is that facilities with low initial similarity to peers became proportionally more similar to their peers (relative to facilities that were already fairly similar to their peers).[16] To investigate this further, I separate US facilities into three terciles based on their 2010 similarities with representative peers. I then estimate a different treatment effect of the US Program for each tercile using the main difference-in-differences framework. Consistent with the earlier argument, Column 1

[16]The distinction between level changes and percentage changes in Jaccard similarity ends up being important. If focusing on level changes, the results in this subsection become statistically insignificant. Some industries, however, might have a larger number of potential processes to choose from, giving them a different similarity 'baseline.' A focus on percentage changes takes such baselines into account.

of Table 6b shows that the emissions reduction is largest for facilities in peer-groups marked by low initial similarity, and fades as initial similarity grows. Benchmarking seems to be a more useful exercise when there is a pre-existing diversity in the processes used among peers. This result echoes Bernard et al. [2020], who find that the predictive power of information flows for future R&D mimicking is greater when the product similarity of firms is low.

[Table 6 about here.]

Benchmarking predicts that facilities shift their processes towards (away from) their less (more) carbon-intense peers. To examine this prediction, I re-introduce the carbon-intensity measure used in Section 5.2. Specifically, I consider a facility's 2010 representative peer, but after conditioning on the constituent peer facilities having a 2010 carbon-intensity-rank of ≤ 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 representative peer. Row 2 of Table 6a shows that the average percentage change when moving from the former similarity to the latter is 6.3% (p = 0.048). That is, after US Program disclosure, facilities become proportionally more similar to their carbon-light peers (as these peers were in 2010). 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). Row 3 of Table 6a shows that facilities become 3.4%, proportionally less similar to their carbon-intense representative peers (p = 0.037). These results echo Fetter et al. [2020], who show that fracking firms shift the chemical composition of their fracking fluids towards those of their more productive peers. I do not make a claim about whether the changes documented here result from a technological search that facilities embark on after benchmarking, or whether peers' US Program disclosures themselves also provide technological cues. By whichever mechanism, benchmarking appears to prompt facilities to make changes to operations.

Column 2 of Table 6b shows that although facilities that shift their processes towards their carbon-light representative peers have larger emissions reductions, this incremental effect is statistically insignificant. Column 3 yields the same inference when focusing on facilities that also become less like their carbon-intense representative peers. These results highlight the need for a discussion of caveats. First, there is surely variation within a process as defined by the US NEI/EIS data. Thus, some actual process changes will not appear in these data. Second, behavioral changes (e.g., maintenance, calibration and optimization) can be implemented within a process, however defined. These points might explain why an emissions reduction is observed for the broader sample of facilities examined, even if their processes did not change in the US NEI/EIS data. Another caveat is that the convergence results in Table 6a are not computed relative to 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 and color to the other benchmarking results.

6 Additional Analyses: GHG Emissions Responses Prior to US Program Disclosure

This paper has so far established that US Program disclosure is a key event driving emissions reductions and facilitating benchmarking. These next tests consider potential emissions responses prior to US Program disclosure.

US facilities knew as early as April 2009 that their 2010 emissions would eventually be publicly disclosed. Fiechter et al. [2018] show that EU firms increase their CSR activities in anticipation of mandatory disclosure under an EU CSR directive. US facilities might similarly anticipate external stakeholder pressure and act earlier to curb their 2010 emissions. Facilities might also have prior uncertainty about their own GHG emissions. Shroff [2017] shows that compliance with new accounting rules can lead firms to collect and/or process new internal information that is relevant for their investment choices. In this vein, mandatory emissions measurement might improve facilities' own-GHG information sets, which then leads to emissions reductions. Most US facilities were not previously required to measure their GHG emissions. Anecdotally, some firms in the TRI setting were unaware of their volume of toxic emissions prior to reporting [Graham, 2002]. Grennan and Swanson [2020] also consider effects prior to disclosure, but in their setting of hospitals, facilities were surely aware of the prices they paid, prior to their planned inclusion into a data-sharing agreement, making this an incomparable context.

6.1 Tests Using Estimated CO_2 Emissions

A key challenge for assessing emissions responses prior to US Program disclosure is the need to measure US facilities' GHG emissions before the US Program was implemented (e.g., in 2008 and 2009). To address this challenge, I leverage the atmospheric science literature and exploit physical relations underlying fossil fuel combustion processes to estimate US facilities' CO_2 emissions [Gurney et al., 2009].

6.1.1 Estimation Overview

When burning fossil fuels, most of the carbon ends up in CO_2, but some ends up in carbon monoxide (CO), due to incomplete combustion. Because of the toxicity of CO, facilities have been required to report their yearly process-level CO emissions to the US NEI/EIS since 2008.[17] Given a standardized industrial process, the same fuel burned under the same conditions should produce CO_2 and CO in constant proportions; this implication lies at the heart of the estimation.

Online Appendix B.2 details the CO_2 estimation, but by way of brief overview, the process is as follows. First, I estimate a Bayesian linear model relating US facilities' logged

[17] Using the Source Classification Code, one can separately observe, for example, a facility's CO emissions from using bituminous coal in an external combustion boiler with a cyclone furnace and those from using natural gas in a four-cycle lean burn internal combustion engine. Facilities are required to report emissions for all criteria pollutants (CO, SO_x, NO_x, PM, and VOCs) if any one of those pollutants exceeds its respective reporting threshold. CO thresholds are 1,000T and 2,500T, SO_x and NO_x thresholds are 100T and 2,500T, and PM and VOC thresholds are 100T and 250T in comprehensive and non-comprehensive years.

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 US NEI/EIS, and 3) a set of priors about the process-level CO_2-CO relations from the Vulcan Science Methods Documentation (see 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. The estimation relies heavily on US Program data and, hence, could not be conducted prior to the US Program. The Bayesian estimates explain 42% of the variation in out-of-sample actual values. Variation in equipment specification, CO abatement technology, fuel carbon content, and various other factors reduce the goodness of fit. For reference, naive OLS estimates explain 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 lets me assign sensible distributions to the parameters (e.g., no negative support) and incorporate priors that shrink noisy parameter estimates toward reasonable values.

6.1.2 Difference-in-Differences Results

To estimate the US Program's effect prior to disclosure, I augment Model 1 by extending the sample window back to 2008 and adding an indicator variable (and its interaction with US) denoting years 2010 onward. *CO2* is logged CO_2, estimated as above for US facilities, and as reported by Canadian facilities. I require a balanced panel for two reasons. First, the Canadian Program's reporting threshold fell from 100,000T to 50,000T CO_2e in 2009. Second, the availability of US facilities' CO_2 estimates is contingent on those facilities reporting CO; however, CO reporting thresholds are lower in 2008, 2011, and 2013. A balanced panel reduces the likelihood the results are affected by selection of facilities, driven by variation in the thresholds.

Column 1 of Table 7 reports no significant effects when only the difference-in-differences variables are estimated. After including control variables and facility and industry-year fixed effects, however, the coefficients of interest are estimated much more precisely, as shown in Column 2. Facilities reduce CO_2 emissions by 11.2% (p = 0.019) following the first disclosure of US Program data in 2012; however, no significant emissions response is observed in 2010 and 2011, when facilities begin measuring and reporting emissions. Figure 3b, the expanded sample-window analog of Figure 3a, likewise shows that the relative emissions differential between US and Canadian facilities stays close to zero until the US Program's first disclosure, after which US facilities' emissions fall relative to Canadian facilities' emissions. Importantly, Figure 3b also provides further support for the parallel trends assumption.

[Table 7 about here.]

6.2 Tests Using Carbon Disclosure Project Data

To assess pre-disclosure emissions responses without using estimated data, I turn to voluntarily-reported, firm-level, Carbon Disclosure Project (CDP) data. I manually collect data for the years 2008 to 2013, focusing on Scope 1 (on-site, non-vehicular) GHG emissions reported by firms that are part of the industries in my US Program sample. Using a balanced panel sidesteps issues of selection into reporting.

I estimate a firm-level version of the regression model used in Section 6.1.2. I use market capitalization, leverage, and market-to-book as controls, as in Section 4.4.2. Columns 3 and 4 of Table 7 provide the results obtained when using firm and year, and firm and industry-year, fixed effects. As before, there is evidence of a US Program disclosure effect, but no strong evidence of an emissions response prior to disclosure. That these results are obtained for firms already disclosing their firm-level GHG emissions, speaks to the nature of the two reporting programs. As described in Section 4.1, US Program disclosures are considerably more granular than CDP data, being informative at the facility and sub-facility levels. Thus the two programs are arguably not information substitutes from a facility-level benchmarking perspective.[18]

[18] Online Appendix C.4 explores facilities' emissions reductions by CDP participation and disclosure status,

6.3 Tests Based on Relative-Carbon Intensity

Lastly, I explore pre-disclosure emissions responses by exploiting purely within-US variation. Specifically, I estimate the following OLS model for US facilities in 2011:

$$CH\_CO2\_2011_i = \beta CARBON\_INTNS\_2010_i + \gamma X_i + \eta_j + \varepsilon_i. \tag{7}$$

This variant of Model 4 tests whether a facility's 2010-carbon intensity-rank predicts its emissions reductions in 2011 (relative to 2010). If measurement and/or reporting of GHG emissions leads to emissions reductions, a reasonable conjecture is that these reductions are larger for facilities that were previously more carbon-intense. Importantly, the information needed to compute 2010-carbon-intensity is not publicly observable in 2011, but is known by facilities themselves. Columns 5 and 6 of Table 7 provide no significant evidence that carbon-intensity-rank in 2010 predicts CO_2 emissions reductions in 2011, as would reasonably be expected if measurement and/or reporting drives emissions reductions.

6.4 Remarks About Emissions Responses Prior to US Program Disclosure

The results in in this section provide no significant evidence of GHG emissions responses prior to US Program disclosure. This is not to say that the US Program has zero or a negligible effect on pre-disclosure emissions. For instance, the estimate of $\mathbb{1}_{\{t \geq 2010\}} * US$ in Column 2 of Table 7, one of the more precise estimates, gives a 95% confidence interval of ~(-3.1%, 11%). Rather, I view these results as highlighting uncertainty around whether measurement/reporting of GHG emissions leads to emissions reductions. In contrast, these same tests support the notion of disclosure-driven effects. I leave it to future research to triangulate these findings and move towards a stronger claim about pre-disclosure emissions responses.

and by whether or not facilities had peers participating in the CDP. The same conclusion emerges.

7 Additional Analyses: Pressure and Incentives for GHG Emissions Reductions

To conclude, this section details exploratory and descriptive analyses that focus on specific pressures and incentives facilities might face when benchmarking GHG emissions.

7.1 Concern About Future Legislation

From Footnote 4 of 2.1, the US Program's main stated purpose was to act as a detailed dataset that could aid future potential GHG-related legislation and rule-making. Thus, the first pressure I examine is the threat of future GHG-related legislation.

Theoretical and empirical work, typically about product pricing, suggests that firms facing regulatory threat can self-regulate to avoid facing more stringent legislation in the future [Erfle and McMillan, 1990; Glazer and McMillan, 1992; Suijs and Wielhouwer, 2019]. The evidence in Maxwell, Lyon, and Hackett [2000] suggests that latent political pressure played a role in toxic emissions reductions following the US TRI's implementation. Such self-regulation might lead to GHG emissions reduction under the US Program. One the other hand, Sanchez, Matthews, and Fischbeck [2012] note that, "...the political reality is that the US has no national GHG commitments, has no pending action to ratify its Kyoto target, and has no formal national climate policy...we are left to wonder whether or not the US has put the cart ahead of the horse by creating (the) GHG Reporting Program prior to sustained momentum towards a climate policy..."

To test whether such self-regulation drives GHG emissions reductions, I exploit state-level variation in political support for progressive climate change legislation. Specifically, I measure senators' and representatives' propensity to legislate on GHG emissions by using their extant voting records with respect to climate change-progressive bills. These data come from the League of Conservation Voters' Scorecards. For each legislator-year, I compute the fraction of climate-change progressive bills, from 2008 to that year, that the legislator supported. The average is then taken across a states' senators or representatives.

To test whether facilities with climate change progressive legislators reduce their GHG emissions more than those without such legislators, I estimate the following OLS model:

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

$\mathbb{1}_{\{\text{CC LEGISLATOR}\}}$ indicates facilities whose state legislators have above-industry-median climate change progressiveness, as described above. β_4 captures an incremental treatment effect of the US Program for this cross-section. Column 1 of Table 8 shows that facilities with climate change progressive senators reduce their emissions by 5.1% more than those without. Column 2 reveals no statistically significant effect of representative progressiveness. In Column 3, I consider both legislators simultaneously and again find a statistically significant incremental emissions reduction when facilities' senators are climate change progressives. These results suggest that concern about future GHG legislation is a motive for emissions reductions.[19] The results also agree with Grewal [2021]'s concurrent finding that firms reporting higher perceived regulatory risk have larger emissions reductions following GHG disclosure in the UK. In Online Appendix C.5, I show that emissions reductions associated with senatorial progressiveness are not common to other facilities owned by the same firm. That is, when reacting to potential legislative threat, firms consider their individual facilities' visibility to legislators.

[Table 8 about here.]

Senators frequently turn over and can adjust their stances on issues. Therefore, I include their progressiveness score, *SENATE_CC_SCORE*, as an independent variable (setting its

[19] A possible explanation for senators' apparent influence is that there are fewer of them, and thus, each senator has a larger proportionate say in their branch of Congress. Additionally, the Waxman-Markey bill, which proposed a cap-and-trade system for US GHG emissions, passed the House but never made it to the Senate floor for a vote. Thus, the Senate might be the relevant political body when facilities self-regulate.

value to zero for Canadian facilities). I also include the interaction of *SENATE_CC_SCORE* and the post-US Program disclosure dummy. The use of facility fixed-effects makes this akin to a changes-specification. Column 4 provides the results. The coefficient estimate on *SENATE_CC_SCORE* shows that senatorial progressiveness changes are negatively associated with GHG emissions levels before US Program disclosure, but not significantly so. After disclosure, however, this association becomes larger and significant, such that a one-standard deviation change in *SENATE_CC_SCORE* (33.2%) implies an incremental GHG emissions reduction of 3.9%. That is, facilities become more responsive to legislative concern once they can benchmark using the disclosures of their peers.

7.2 Other Pressures and Incentives

From Footnote 4 of Section 2.1, US EPA also recognizes that US Program data might be used by other stakeholders and facilities to drive down emissions.[20] In Online Appendix C, I explore whether variation is emissions reductions is associated with exposure to capital markets, end-customers, and the general public. To speak to profit/efficiency motives for benchmarking, I examine whether variation in emissions reductions is associated with proxies for within-firm information frictions. In short, no strong conclusions emerge from these tests.

8 Conclusion

Climate scientists and economists have argued for more to be done to limit increases in global temperatures. In this paper, I explore whether the mandatory, granular disclosures of the US Greenhouse Gas Reporting Program (US Program) lead to emissions reductions.

[20] A US EPA presentation, Rand and Stewart [2012], is more specific. It states that the US Program data can help to: "Enable industries to compare their emissions to similar facilities and identify emissions reductions strategies; Provide states and localities with GHG emissions data from facilities within their borders and to compare with emissions in other areas; Educate the public about large sources of GHGs; Make GHG data available to the financial community leading to more informed investment decisions; Provide detail on GHG emissions by gas, sector, and location that can be used by the research community; Inform policy decisions at the local state or other level; Establish a baseline for facilities to track emissions over time; Help identify industry leaders."

Targeted disclosure mandates have shown promise in other arenas, but with the non-investor facing nature of the US Program, and with the extent of coordination problems around GHG emissions, it is questionable whether disclosure can be similarly effective in this context.

I find that US facilities, compared to similar Canadian facilities, reduce GHG emissions by 7.9% following the public disclosure of emissions data. In contrast to much of the related work, I estimate a treatment effect for facilities that had largely no other emissions information in the public domain. US Program data are granular, often providing details at the sub-facility level. Thus, they are plausibly informative to facilities' peers at an operational-level. I conduct a range of emissions and process-based tests that are consistent with facilities using the emissions data of their peers to set benchmarks.

While disclosure elicits a significant emissions response, I find no significant evidence of an emissions response to measurement/reporting, prior to disclosure. With respect to particular pressures and incentives, I find that concerns about future legislation are one factor motivating emissions reductions. The key takeaway is that mandatory, granular disclosure can help curb GHG emissions and facilitate benchmarking.

This paper also highlights avenues for future work. First, it focuses on the US Program from a benefits perspective. According to US EPA's regulatory impact analysis, the US Program's first year would result in compliance and administrative costs of $132 million (2006 USD), with subsequent ongoing yearly costs of $89 million [US Environmental Protection Agency, 2009b]. In my sample, 2010 US emissions are 690 million T CO_2e. Given a 7.9% emissions reduction, President Obama's $50/T social cost of carbon, and that emissions reductions persist, the US Program's emission reduction benefits appear to outweigh its direct compliance and administrative costs. Breuer, Leuz, and Vanhaverbeke [2020] and Fetter et al. [2020], however, show that disclosure can stifle innovation by disseminating proprietary information. This is potentially a deeper cost of the US Program. US EPA has deemed a number of non-emissions reporting items to be Confidential Business Information, and as such they are not disclosed (e.g., supplier and vendor information). Emissions data collected under the Clean Air Act, however, are not entitled to confidential treatment. Further work assessing the extent of innovation-dampening would allow a for more rounded view of the US Program's impact.

Second, this paper examines the effect of a single reporting/disclosure event. Thus, further research on other mandatory GHG disclosure settings will be valuable. The concurrent work in this area is very beneficial on this point, but given the complexity of the GHG issue and the range of potential stakeholders, there is still more to be done. In addition to the triangulation provided, variation in the institutional features of additional studies can highlight different economic mechanisms that affect emissions patterns.

Appendix: Variable Description

Facility-level Analysis

Variable Name	Description
CH_CO2_201X	The percentage change in CO_2 emissions in 201X relative to the prior year
CO2	CO_2 emissions in logged metric tons. In Table 7, these are estimated for US facilities as described in Appendix B.2.
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	90th minus 10th 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' facilities) and beyond
$\mathbb{1}_{\{\text{CROSS-SECTION}\}}$	Indicates US facilities in a cross-section as described in the relevant section and table description
CO2_INT_201X	CO_2 emissions over industry-specific non-combustion 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 level (logged value-added)
REGULATIONS	Number of emissions reduction incentives and regulations applicable to a facility and implemented at the federal or state 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	An indicator denoting a US facility

Firm-level Analysis

Variable Name	Description
GHG	Facility GHG emissions aggregated to the firm level (logged metric tons CO_2 equivalent)
GHG_INT	Firm-level GHG emissions over firm Cost of Goods Sold (logged)
CAPEX	Compustat item CAPX, over beginning total assets
GROSS_MARGIN	Gross profit over revenue
LEVERAGE	Total liabilities over total assets
MCAP	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.

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Figures and Tables

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

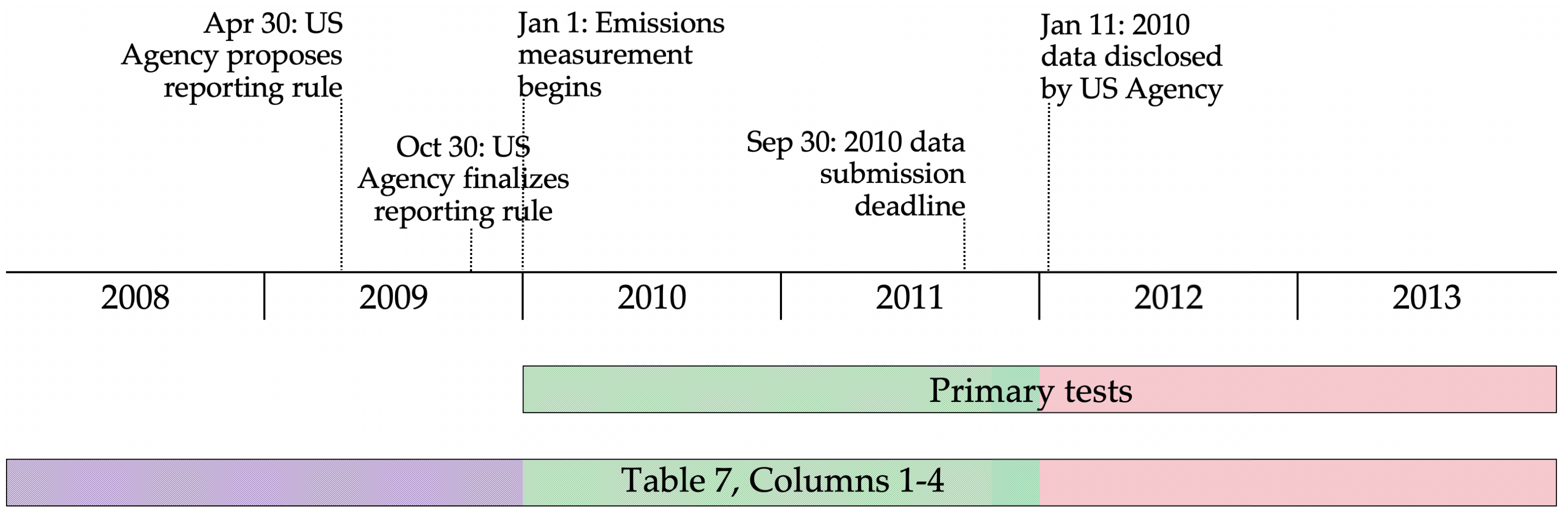
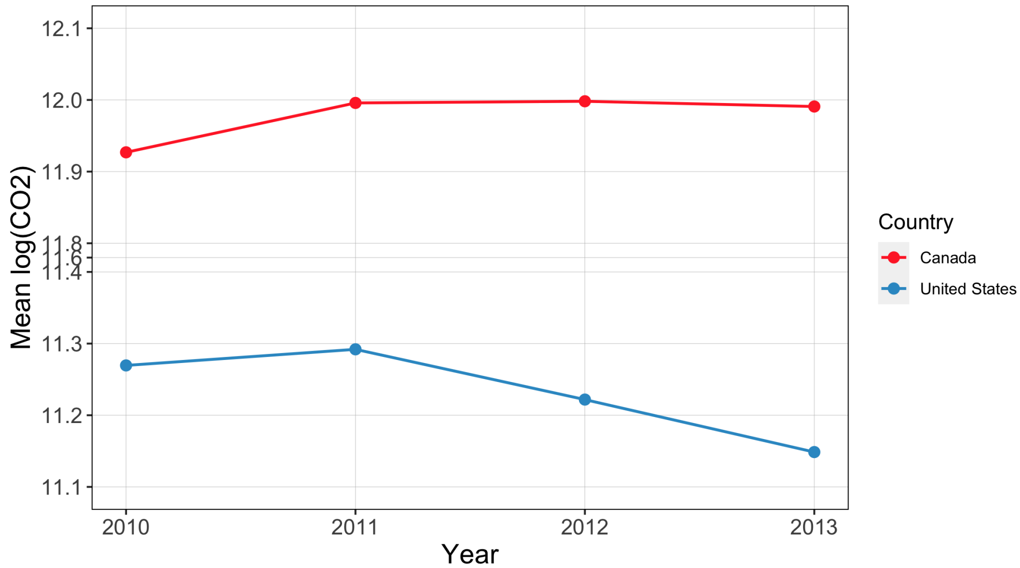
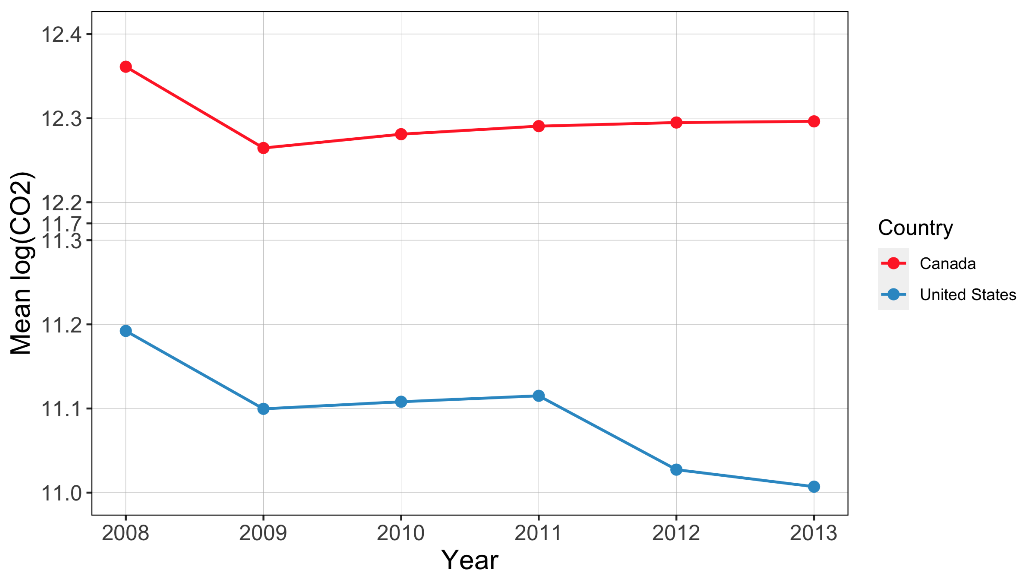


Figure 2: Average Logged Emissions for US and Canadian Facilities

(a) GHG Emissions



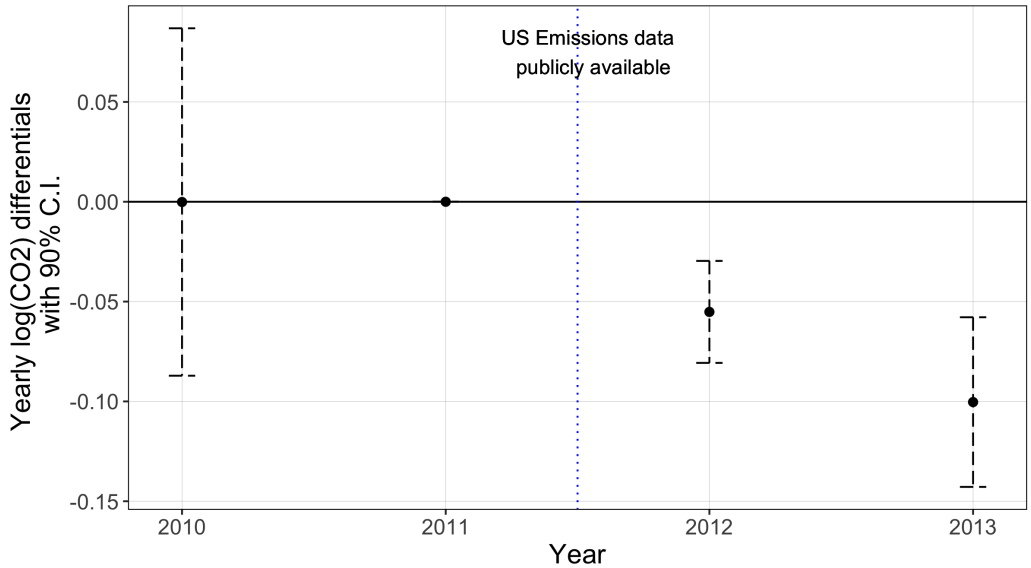
(b) CO_2 Emissions



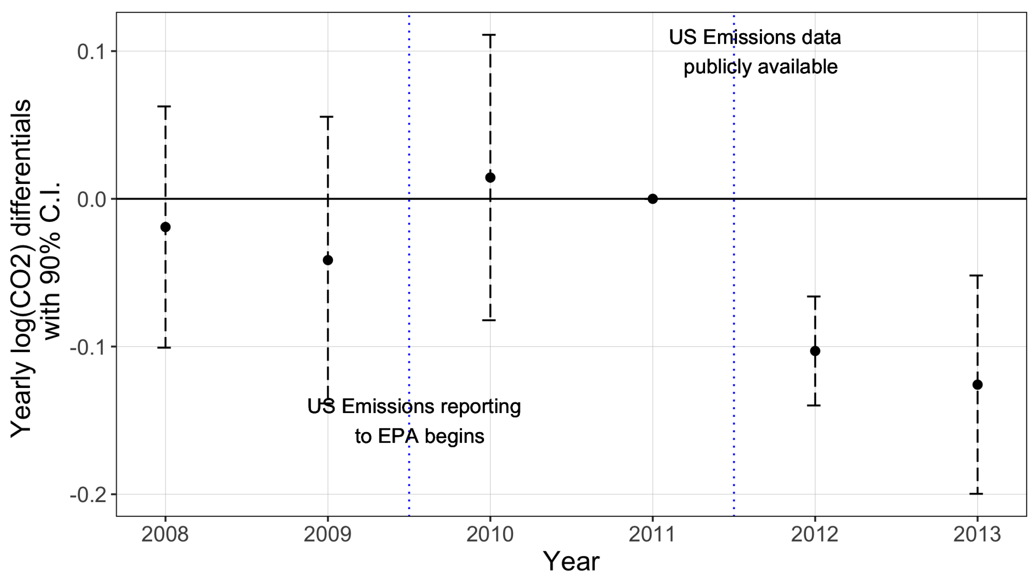
These figures plot average annual logged emissions for US and Canadian facilities that reported greenhouse gas (GHG) emissions to their national GHG Reporting Programs. Figure 2a uses reported GHG emissions; Figure 2b uses reported CO_2 emissions for Canadian facilities and estimated CO_2 for US facilities—the estimation is described in Online Appendix B.2. Data filters are described in Section 4.1. The Appendix provides variable definitions.

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

(a) Greenhouse Gases; Main sample



(b) CO_2 (estimated for US facilities); Table 3, Column 4 sample



These figures plot the β_ks obtained from estimating Model 2, described in Section **??**:

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

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 Online Appendix B.2. The sample comprises US and Canadian facilities that reported greenhouse gas (GHG) emissions to their national GHG Reporting Programs. Data filters are described in Section 4.2. The Appendix provides variable definitions.

Table 1a: Observation 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	194	382
Amusement Parks and Arcades	0	4	267
Animal Food Manuf.	0	14	54
Animal Slaughtering and Processing	0	304	36
Architectural, Engineering, and Related Services	0	7	37
Bakeries and Tortilla Manuf.	0	4	27
Basic Chemical Manuf.	117	1683	270
Beverage Manuf.	4	88	67
Business, Professional, Labor, Political, and Similar Orgs.	0	4	74
Cement and Concrete Product Manuf.	60	330	606
Clay Product and Refractory Manuf.	3	46	70
Coal Mining	60	96	80
Coating, Engraving, Heat Treating, and Allied Activities	0	16	73
Colleges, Universities, and Professional Schools	0	28	95
Commercial and Service Industry Machinery Manuf.	0	4	38
Computer and Peripheral Equipment Manuf.	0	8	6
Converted Paper Product Manuf.	0	19	65
Cutlery and Handtool Manuf.	0	4	51
Dairy Product Manuf.	0	55	31
Drugs and Druggists' Sundries Merchant Wholesalers	0	1	69
Electric Lighting Equipment Manuf.	0	4	43
Electrical Equipment Manuf.	0	13	2
Engine, Turbine, and Transmission Equipment Manuf.	0	22	33
Fabric Mills	0	8	97
Facilities Support Services	0	8	1050
Forging and Stamping	0	36	45
Foundries	12	153	62
Fruit and Vegetable Preserving and Specialty Food Manuf.	9	135	46
General Medical and Surgical Hospitals	0	32	35
Glass and Glass Product Manuf.	8	347	78
Grain and Oilseed Milling	17	339	233
Greenhouse, Nursery, and Floriculture Production	0	4	79
Hardware, and Plumbing/Heating/Supplies Wholesalers	0	4	101
Household and Institutional Furniture and Cabinet Manuf.	3	0	1
Household Appliance Manuf.	1	9	23
HVAC and Commercial Refrigeration Equip. Manuf.	0	4	55
Iron and Steel Mills and Ferroalloy Manuf.	46	495	676
Lessors of Real Estate	0	8	69
Lime and Gypsum Product Manuf.	49	350	288

Management of Companies and Enterprises	0	1	2
Metal Ore Mining	54	65	330
Metalworking Machinery Manuf.	0	8	53
Motor Vehicle and Parts and Supplies Wholesalers	0	8	448
Motor Vehicle Body and Trailer Manuf.	0	4	48
Motor Vehicle Manuf.	18	141	61
Motor Vehicle Parts Manuf.	1	59	33
Natural Gas Distribution	24	76	208
Nonferrous Metal (except Aluminum) Production	33	140	108
Nonmetallic Mineral Mining and Quarrying	40	171	181
Office Furniture (including Fixtures) Manuf.	20	4	8
Oil and Gas Extraction	487	2170	152
Other Chemical Product and Preparation Manuf.	6	61	122
Other Crop Farming	0	4	171
Other Electrical Equipment and Component Manuf.	0	12	77
Other Fabricated Metal Product Manuf.	0	15	23
Other Food Manuf.	0	87	57
Other General Purpose Machinery Manuf.	0	4	31
Other Investment Pools and Funds	0	3	16
Other Miscellaneous Manuf.	24	16	31
Other Nonmetallic Mineral Product Manuf.	7	147	80
Other Pipeline Transportation	0	7	21
Other Transportation Equipment Manuf.	0	3	1
Paint, Coating, and Adhesive Manuf.	0	12	1
Pesticide/Fertilizer/Other Agricultural Chemical Manuf.	36	198	635
Petroleum and Coal Products Manuf.	74	607	1087
Petroleum and Petroleum Products Merchant Wholesalers	0	9	29
Pharmaceutical and Medicine Manuf.	1	113	62
Pipeline Transportation of Crude Oil	0	20	71
Pipeline Transportation of Natural Gas	50	1872	55
Plastics Product Manuf.	11	53	74
Printing and Related Support Activities	0	12	22
Pulp, Paper, and Paperboard Mills	199	889	188
Railroad Rolling Stock Manuf.	0	8	46
Resin, Synthetic Rubber, and Synthetic Fiber Manuf.	12	343	300
Rubber Product Manuf.	3	78	40
Sawmills and Wood Preservation	3	21	11
Scheduled Air Transportation	0	8	47
Scientific Research and Development Services	0	31	62
Seafood Product Preparation and Packaging	0	8	36
Semiconductor and Other Electronic Component Manuf.	0	147	31
Ship and Boat Building	0	4	96
Soap, Cleaning Compound, and Toilet Preparation Manuf.	0	25	75
Steel Product Manuf. from Purchased Steel	8	51	78
Sugar and Confectionery Product Manuf.	8	109	188

Support Activities for Crop Production	0	4	49
Support Activities for Mining	0	310	46
Support Activities for Rail Transportation	0	4	50
Textile and Fabric Finishing and Fabric Coating Mills	0	31	45
Textile Furnishings Mills	0	24	39
Tobacco Manuf.	0	12	84
Traveler Accommodation	0	12	67
Utility System Construction	0	6	67
Veneer, Plywood, and Engineered Wood Product Manuf.	17	61	31
Water, Sewage and Other Systems	19	168	151
All	1587	13454	230

Table 1b: Observation Frequency by US State

State	US N
Alabama	432
Alaska	126
Arizona	118
Arkansas	380
Colorado	301
Connecticut	69
Delaware	32
Florida	191
Georgia	295
Hawaii	20
Idaho	112
Illinois	523
Indiana	426
Iowa	366
Kansas	284
Kentucky	291
Louisiana	1144

State	US N
Maine	57
Maryland	53
Massachusetts	94
Michigan	442
Minnesota	300
Mississippi	236
Missouri	184
Montana	74
Nebraska	159
Nevada	64
New Hampshire	16
New Jersey	132
New Mexico	202
New York	262
North Carolina	226
North Dakota	114
Ohio	569

State	US N
Oklahoma	397
Oregon	144
Pennsylvania	610
Rhode Island	12
South Carolina	226
South Dakota	85
Tennessee	266
Texas	1966
Utah	176
Vermont	16
Virginia	182
Washington	203
West Virginia	224
Wisconsin	281
Wyoming	372
All	13454

Table 1c: Observation Frequency by Canadian Province

Province	Canada N
Alberta	539
British Columbia	259
Manitoba	35
New Brunswick	37
Newfoundland and Labrador	29
Northwest Territories	16
Nova Scotia	29
Ontario	334
Prince Edward Island	4
Quebec	205
Saskatchewan	100
All	1587

These tables count the observations used in the primary regression analysis (Column 4 of Table 3) 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 greenhouse gas (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	1587	11.80	2.0	6.60	11.0	12.0	13.0	15.0
GDP	1587	11.90	0.3	11.00	12.0	12.0	12.0	12.0
GAS_PRICE	1587	1.29	0.2	0.89	1.1	1.3	1.5	1.6
REGULATIONS	1587	6.37	2.0	2.00	6.0	7.0	8.0	10.0

	US							
	N	Mean	SD	q1	q25	q50	q75	q99
GHG	13454	11.00	2.0	5.90	10	11.0	12.0	15.0
GDP	13454	13.90	0.8	13.00	13	14.0	14.0	15.0
GAS_PRICE	13454	1.30	0.2	0.89	1	1.4	1.4	1.5
REGULATIONS	13454	2.42	2.0	1.00	1	2.0	3.0	8.0

(b) Firm-level

	N	Mean	SD	q1	q25	q50	q75	q99
GHG_INT	1216	4.320	2.00	− 0.510	2.9000	4.5000	5.900	8.50
CAPEX	1216	0.029	0.08	− 0.048	− 0.0064	0.0093	0.041	0.36
GROSS_MARGIN	1216	0.319	0.20	0.020	0.1800	0.2700	0.420	0.84
US	1216	0.895	0.30	0.000	1.0000	1.0000	1.000	1.00
GDP	1216	13.900	0.90	12.000	13.0000	14.0000	14.000	15.00
GAS_PRICE	1216	1.310	0.20	0.890	1.1000	1.4000	1.400	1.50
REGULATIONS	1216	2.570	2.00	1.000	1.0000	2.0000	3.000	8.00
LEVERAGE	1216	0.087	0.08	− 0.092	0.0480	0.0800	0.120	0.30
MCAP	1216	24.800	50.00	0.057	1.7000	5.9000	26.000	210.00
MTB	1216	0.604	0.20	0.210	0.4900	0.5900	0.690	1.30

Table 2a describes the observations used in primary regression analysis (Column 3 of Table 3). Its sample spans 2010 to 2013 and comprises US and Canadian facilities that reported greenhouse gas (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: GHG Emissions and Intensity Related Outcomes Following Disclosure

	GHG	*GHG*	*GHG*	*GHG*	*GHG_INT*	*CAPEX*	*GROSS_MARGIN*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\mathbb{1}_{\{t\geq 2012\}} * US$	−0.302	−0.121***	−0.065**	−0.082**	−0.249***	0.034***	0.043***
	(0.255)	(0.039)	(0.028)	(0.034)	(0.079)	(0.010)	(0.010)
$\mathbb{1}_{\{t\geq 2012\}}$	0.067	0.136**	0.035	0.051			
	(0.175)	(0.061)	(0.067)	(0.067)			
US	−0.605***						
	(0.172)						
GDP				0.220	0.712***	−0.009	−0.051*
				(0.430)	(0.241)	(0.006)	(0.026)
GAS_PRICE				−0.021	−0.618	0.029	−0.282
				(0.350)	(0.929)	(0.074)	(0.180)
REGULATIONS				−0.007	0.001	−0.002	−0.001
				(0.008)	(0.019)	(0.002)	(0.003)
Year and Facility effects		Y					
Ind-Yr and Facility effects			Y	Y			
Ind-Yr and Firm effects					Y	Y	Y
Observations	15,041	15,041	15,041	15,041	1,216	1,215	1,216
Adjusted R^2	0.028	0.899	0.904	0.904	0.965	0.751	0.935

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

Columns 1 to 4 of this table show how US facilities' logged greenhouse gas (GHG) emissions change following the US GHG Reporting Program's first disclosure of emissions data in January 2012. Column 5 examines changes in carbon-intensity (firm-level GHG emissions divided by Cost of Goods Sold). To triangulate a reduction in GHG-intensity, Columns 6 and 7 explore how US firms' capital expenditures and gross margins change. 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 4.2 and 4.4.2. The Appendix provides variable definitions.

Table 4: Peer Benchmarking of Facility GHG Emissions Following Disclosure

	GHG_SD	GHG_P90_P10	CH_CO2_2012	CH_CO2_2012	CH_CO2_2012	CH_CO2_2012
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}_{\{t\geq 2012\}} * US$	−45.962**	−63.004**				
	(21.937)	(28.775)				
CARBON_INT_2010			−0.028**	−0.070*	−0.087***	−0.117***
			(0.012)	(0.042)	(0.018)	(0.042)
CARBON_INT_2011				0.055		0.041
				(0.047)		(0.046)
GAS_PRICE	−494.360	−671.100	1.502***	1.499***	1.494***	1.490***
	(338.720)	(444.313)	(0.027)	(0.026)	(0.033)	(0.033)
REGULATIONS	8.219	6.217	−0.003	−0.002	−0.006	−0.003
	(19.589)	(25.696)	(0.014)	(0.014)	(0.024)	(0.025)
Ind.-Country and Year effects	Y	Y				
Ind. effects			Y	Y	Y	Y
Inefficient facilities only					Y	Y
Observations	392	392	1,111	1,111	555	555
Adjusted R^2	0.961	0.960	0.258	0.259	0.182	0.182

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

This table provides evidence consistent with US facilities benchmarking their emissions against their peers' emissions. Columns 1 and 2 examine how the within-industry dispersion of US facility emissions changes following the US Greenhouse Gas (GHG) Reporting Program's first disclosure of emissions data in 2012. They examine the 90th−10th percentile emissions difference, and standard deviation of emissions, respectively. 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 responded to their 2010 within-industry-state rankings of emissions efficiency (CO_2 emissions scaled by a proxy for goods produced, which would become publicly known in 2012). 2010 US Program data were publicly disclosed in 2012. This sample comprises US facilities only. Data filters are described in Section 4. Data filters are described in Section 5.1 and 5.2. The Appendix provides variable definitions.

Table 5: The Effect of Being a 'Benchmarker' on Facility GHG Emissions Following Disclosure

	GHG	*GHG*	*GHG*
	(1)	(2)	(3)
$\mathbb{1}_{\{t\geq 2012\}} * US$	-0.034	-0.072	
	(0.034)	(0.044)	
$\mathbb{1}_{\{t\geq 2012\}} * US * \mathbb{1}_{\{\text{BENCHMARKER}\}}$	-0.077	-0.077^{*}	
	(0.066)	(0.043)	
$\mathbb{1}_{\{t\geq 2012\}} * US * \mathbb{1}_{\{\text{80th-100th pctile BENCHMARKER}\}}$			-0.249^{*}
			(0.138)
$\mathbb{1}_{\{t\geq 2012\}} * US * \mathbb{1}_{\{\text{60th-80th pctile BENCHMARKER}\}}$			-0.124^{**}
			(0.054)
$\mathbb{1}_{\{t\geq 2012\}} * US * \mathbb{1}_{\{\text{40th-60th pctile BENCHMARKER}\}}$			-0.095
			(0.074)
$\mathbb{1}_{\{t\geq 2012\}} * US * \mathbb{1}_{\{\text{20th-40th pctile BENCHMARKER}\}}$			-0.038
			(0.046)
$\mathbb{1}_{\{t\geq 2012\}} * US * \mathbb{1}_{\{\text{0th-20th pctile BENCHMARKER}\}}$			-0.108
			(0.068)
GDP	-0.063	-0.036	-0.151
	(0.286)	(0.604)	(0.600)
GAS_PRICE	-0.595	-0.951^{***}	-1.024^{***}
	(0.407)	(0.322)	(0.317)
REGULATIONS	0.001	-0.007	-0.006
	(0.007)	(0.009)	(0.010)
Ind-Yr and Facility effects	Y	Y	Y
Search data used	Actual	Predicted	Predicted
Observations	3,177	5,857	5,857
Adjusted R^2	0.957	0.934	0.923

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

This table explores whether a tendency of a facility's owner to access the information of its peers (i.e., whether it is a 'benchmarker') affects the reduction in that facility's logged greenhouse gas (GHG) emissions following the US GHG Reporting Program's first disclosure of emissions data in 2012. A facility's owner is classified as a benchmarker if it accessed 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 Section 5.3. The Appendix provides variable definitions.

Table 6a: Process Convergence Tests

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 6b: Mechanisms for GHG Emissions Reductions Following Disclosure

	GHG	*GHG*	*GHG*
	(1)	(2)	(3)
$\mathbb{1}_{\{t\geq 2012\}} * US * \mathbb{1}_{\{\text{LOW SIM.}\}}$	-0.192^{**}		
	(0.081)		
$\mathbb{1}_{\{t\geq 2012\}} * US * \mathbb{1}_{\{\text{MED. SIM.}\}}$	-0.119^{**}		
	(0.055)		
$\mathbb{1}_{\{t\geq 2012\}} * US * \mathbb{1}_{\{\text{HIGH SIM.}\}}$	-0.018		
	(0.044)		
$\mathbb{1}_{\{t\geq 2012\}} * US$		-0.102^{**}	-0.101^{**}
		(0.046)	(0.046)
$\mathbb{1}_{\{t\geq 2012\}} * US * \mathbb{1}_{\{\text{IMPROVE}\}}$		-0.070	-0.112
		(0.049)	(0.092)
Ind-Yr and Facility effects	Y	Y	Y
Observations	7,352	3,592	3,557
Adjusted R^2	0.944	0.954	0.951

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

These tables explores process changes at US facilities around US Program disclosure in 2012. Table 6a examines whether facilities become more (less) similar to their carbon-light (carbon-intense) peers following the disclosure of GHG emissions. Column 1 of Table 6b examines how the emissions reductions of a facility vary 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. Data filters are described in Section 5.4. The Appendix provides variable definitions.

Table 7: Assessing Emissions Reductions Following GHG Emissions Reporting (Prior to Disclosure)

	CO2	*CO2*	*GHG*	*GHG*	*CH_GHG_2011*	*CH_GHG_2011*
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}_{\{t \geq 2012\}} * US$	−0.133	−0.119**	−0.160*	−0.072		
	(0.284)	(0.046)	(0.082)	(0.154)		
$\mathbb{1}_{\{t \geq 2010\}} * US$	0.017	0.037	−0.050	0.042		
	(0.268)	(0.043)	(0.115)	(0.159)		
CARBON_INT_2010					−0.005	−0.004
					(0.014)	(0.025)
GDP		−0.152				
		(0.443)				
GAS_PRICE		−0.002			−0.013**	−0.006
		(0.009)			(0.006)	(0.013)
REGULATIONS		0.059			0.100	0.616
		(0.211)			(0.385)	(0.805)
Ind-Yr and Facility effects		Y				
Year and Firm effects			Y			
Ind-Yr and Firm effects				Y		
Industry effects					Y	Y
Inefficiency facilities only						Y
Observations	6,078	6,078	550	550	1,112	556
Adjusted R^2	0.079	0.864	0.986	0.984	0.007	−0.010

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

This table explores how US facilities' greenhouse gas (GHG) emissions change following commencement of emissions measurement and reporting to the US EPA under the US GHG Reporting Program. Columns 1 and 2 examine logged CO_2 emissions (estimated for US facilities using the routine described in Appendix B.2). Columns 3 and 4 examine logged Scope 1 GHG emissions voluntarily disclosed by large firms under the Carbon Disclosure Project. The samples in Columns 1 to 4 span 2008 to 2013, with Canadian observations forming the control. Columns 5 and 6 examine how the percentage change in US facilities' CO_2 emissions from 2010 to 2011 varied with their 2010 within-industry-state rankings of carbon intensity (CO_2 emissions scaled by a proxy for goods produced, which would not have been public in 2011). This sample comprises US facilities only. Data filters are described in Section 6. The Appendix provides variable definitions.

Table 8: The Effect of a Facility's Political Environment on GHG Emissions Following Disclosure

	GHG (1)	*GHG* (2)	*GHG* (3)	*GHG* (4)
$\mathbb{1}_{\{t\geq 2012\}} * US$	-0.057^{*}	-0.081^{**}	-0.059^{*}	-0.047
	(0.030)	(0.036)	(0.031)	(0.031)
$\mathbb{1}_{\{t\geq 2012\}} * US * \mathbb{1}_{\{\text{CC SENATE}\}}$	-0.052^{*}		-0.053^{*}	
	(0.029)		(0.032)	
$\mathbb{1}_{\{t\geq 2012\}} * US * \mathbb{1}_{\{\text{CC HOUSE}\}}$		-0.004	0.006	
		(0.020)	(0.023)	
$\mathbb{1}_{\{t\geq 2012\}} * SENATE\_CC\_SCORE$				-0.001^{**}
				(0.0004)
SENATE_CC_SCORE				-0.0003
				(0.001)
GDP	0.215	0.220	0.214	0.128
	(0.433)	(0.428)	(0.432)	(0.404)
GAS_PRICE	-0.054	-0.023	-0.050	-0.032
	(0.346)	(0.351)	(0.344)	(0.345)
REGULATIONS	-0.007	-0.007	-0.007	-0.006
	(0.008)	(0.008)	(0.008)	(0.007)
Ind-Yr and Facility effects	Y	Y	Y	Y
Observations	15,041	15,041	15,041	15,041
Adjusted R^2	0.904	0.904	0.904	0.904

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

This table explores whether political pressures affect emissions reductions. Specifically, it examines how the climate change views of legislators for a facility's state affect that facility's reduction in logged greenhouse gas (GHG) emissions following the US GHG Reporting Program's first disclosure of emissions data in 2012. Columns 1 to 3 assess the impact of state senators' and congressional district representatives' voting on bills that seek to curb climate change. Column 4 examines whether the relation between a facility's state senators' views on climate change, and that facility's logged GHG emissions, changes following emissions disclosure. Data filters are described in Section 7. The Appendix provides variable definitions.

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