Mandatory Greenhouse Gas Disclosure Impacts on Firms and Workers

Terry S. Moon* Xiomara Pulido† Jalal Sani‡ Daniel Xu§

September 10, 2025

Abstract

This paper assesses the effects of Greenhouse Gas (GHG) disclosure on both firm and worker outcomes. We exploit a federal mandatory disclosure policy implemented in Canada in 2004, along with federal and provincial changes in reporting thresholds in later years, to define first-time reporting firms within two years of each unanticipated reform as the treated group. Using administrative firm-balance sheet data, we compare the outcomes of affected firms with those of never-treated firms that are similar in size and age, and operate within the same industry and province. After the reform, treated firms that disclose GHG emissions significantly increase their hiring and capital investment relative to matched control firms. Subsequently, their total expenses and revenue increase, with a moderate rise in profitability. Furthermore, by linking employee-level information, we find that incumbent workers at treated firms experience a large increase in their annual earnings and retention rates. Notably, treated firms increase debt financing and reduce cash distributions to shareholders. The rise in employment and investment, financed through increased borrowing, is concentrated among more financially constrained firms. Taken together, these results are consistent with a model predicting that disclosing GHG emissions can lower the cost of capital by reducing information frictions under tighter environmental regulations (e.g., carbon taxes). Our findings suggest that more widespread GHG disclosure, mandated and enforced by the government, can bring benefits to disclosing firms, despite the potential regulatory and private costs.

JEL Classifications: G11, G14, G28, Q54
Keywords: Mandatory greenhouse gas disclosure; corporate investment; information asymmetry; cost of capital; carbon taxes

*Vancouver School of Economics, University of British Columbia (email: tsmoon@mail.ubc.ca)
†Vancouver School of Economics, University of British Columbia (email: xiomarapulido26@gmail.com)
‡Gies College of Business, University of Illinois at Urbana-Champaign (email: jsani@illinois.edu)
§Department of Economics, Duke University (email: daniel.xu@duke.edu)

1 Introduction

Climate change and the associated environmental risks are among the central policy topics. Accordingly, prior research extensively explores the effectiveness of market-based emission policies (e.g., Fowlie et al., 2012, 2016; Chen et al., 2024; Acharya et al., 2025). To complement these policies, regulators around the world have adopted various climate-related disclosure mandates (Greenstone et al., 2023). However, potential costs and benefits of mandatory climate disclosures remain widely debated, as highlighted in the Securities and Exchange Commission's (SEC) recent rule-making attempt, which sparked a flood of supporting and opposing comment letters.[1]

At the heart of the debate lies an important economic question of how mandating climate-related disclosure would affect firms and their workers. Prior research documents that investors may prefer investing in green firms because of non-pecuniary (e.g., social or ethical) motives or because these stocks hedge against climate risks (Riedl and Smeets, 2017; Hartzmark and Sussman, 2019; Engle et al., 2020; Bauer et al., 2021; Alekseev et al., 2024; Giglio et al., 2025),[2] and that investors value climate risk disclosures (Flammer et al., 2021; Ilhan et al., 2023). In the first part of our paper, we build a model which highlights a novel mechanism: disclosure helps investors evaluate how firms' costs and profits respond when environmental regulations (e.g., carbon taxes) tighten. Without regulations, emissions do not impose direct costs on firms, making emissions disclosure less relevant for investors. Once regulations become stricter, however, firms with greater emission efficiency face disproportionately lower carbon taxes, relative to less emission efficient firms. Disclosure allows investors to distinguish such firms, reducing information frictions and the cost of capital (Diamond and Verrecchia, 1991; Gao, 2010; Easley and O'Hara, 2004; Lambert et al., 2007), which may lead disclosing firms to increase investment and hiring. On the other hand, investors may perceive the information as a negative signal and revise their assessment of the firm's emission efficiency downward, which can raise the cost of capital and reduce investment.

Despite the importance of assessing the effects of disclosure mandates on firms and workers, empirically analyzing such effects faces identification and data challenges. Exploiting exogenous variation in disclosure across firms requires regulatory changes and a suitable control group. However, most disclosure regulations are uniformly mandated on comparable firms and rarely change

[1] The SEC's proposed rules in 2022, aimed at mandating climate-related disclosures, received a staggering 24,000 comment letters. While a group of supporters argued that it would provide investors with reliable information and reduce the cost of capital, others opposed it due to concerns about compliance costs and the disclosure of proprietary information (details). Although the final rule, adopted in 2024, was less strict than initially proposed, it still prompted several lawsuits from state officials, business groups, and listed companies, leading the SEC to voluntarily stay the climate disclosure rules pending judicial review (details).

[2] In 2020, U.S. sustainable investments totaled $17.1 trillion, representing nearly 33 percent of total U.S. assets under professional management, according to the Forum for Sustainable and Responsible Investment (SIF, 2020). This represents more than a 25-fold increase since 1995, with a compound annual rate of 14 percent.

significantly over time (e.g., Minnis, 2011; Greenstone et al., 2006). Furthermore, we need detailed firm balance-sheet data to find a comparable control group, but such firm-level information is not easily accessible for unregulated (mostly private) firms.

We address these challenges and assess the effects of mandatory climate-related disclosure on firms' investment and performance as well as workers' labor market outcomes. To do so, we leverage a series of regulatory disclosure changes that affected different groups of firms over time and use matched employer-employee data from tax records covering the universe of firms and workers in Canada from 2001 to 2021. Specifically, we exploit the Greenhouse Gas Reporting Program (GHGRP), introduced in Canada in 2004, which required facilities emitting 100 kilotonnes (kt) or more of greenhouse gases (GHGs) in carbon dioxide (CO2) equivalent per year to disclose their emissions. The reporting threshold was subsequently lowered to 50 kt in 2009 and 10 kt in 2017. We use these regulatory changes to identify treated firms as those with at least one facility exceeding the reporting threshold, focusing on firms affected for the first time within two years of the policy change. For identification, we use a matched difference-in-differences design, comparing treated firms to control firms of similar size and age, operating in the same province and industry, but were always below the reporting threshold and never disclosed GHG emissions during our sample period. To ensure that treated firms above the reporting threshold are comparable to matched control firms below the threshold, we show that their key outcomes, such as investment, sales, and profitability, trended similarly prior the reform. Importantly, the majority of treated and control firms are private companies in our setting; therefore, being able to access their financial information from tax records is crucial for our empirical design.

We begin our analysis by examining the impact of GHG disclosure mandates on firms' employment and capital investments. We find that treated firms increased their employment and capital stock by 11.8 percent and 6.7 percent, respectively, after disclosing GHG emissions for the first time, relative to matched control firms that never report their GHG emissions during our sample period. Next, we evaluate firm performance and find that treated firms experienced slight improvements relative to control firms. Specifically, treated firms' total revenue and total expenses increased by 13.3 percent and 12.9 percent, respectively. We also observe a positive but statistically insignificant increase in profitability, as measured by returns on assets (ROA), with treated firms experiencing a 1.2 percentage point rise. Overall, our findings suggest that GHG disclosure regulations contribute to firm growth and moderately enhanced performance.

Furthermore, we examine the effects of GHG disclosure regulations on worker-level outcomes. Our findings show that mandatory GHG disclosure leads to higher earnings of 8.8 log points for workers at treated firms, relative to matched workers at control firms. Additionally, the probability of treated workers moving to another firm declined by 4.1 percentage points relative to control

workers, suggesting higher employee retention.

Our results are consistent with the idea that mandatory disclosure of GHG emissions influences firms' investment decisions by affecting their cost of capital. GHG disclosure regulations can provide investors with comparable, high-quality information, and reduce information asymmetry and acquisition costs. As a result, mandatory disclosure can reduce the cost of capital, especially under tighter regulation environments such as carbon tax policies implemented across three major provinces in Canada. As mentioned earlier, the majority of firms in our sample are private and not subject to financial reporting regulations, and do not publicly disclose their information. Thus, their operational activities are generally opaque to external stakeholders, making it difficult to assess their size, performance, or GHG emission levels. Accordingly, the GHGRP can reveal a significant amount of information about these firms. Prior theoretical studies show that increased public disclosure reduces the amount of private information available to insiders and informed traders, thereby mitigating the information disadvantage of uninformed investors and lowering adverse selection costs. This, in turn, can reduce a firm's cost of capital (Easley and O'Hara, 2004). Additionally, disclosure mitigates information asymmetry between market makers and informed investors, reducing the extent to which large trades reveal private information. This effect enhances market liquidity, increases demand from institutional investors, and ultimately lowers the cost of capital (Diamond and Verrecchia, 1991). Furthermore, higher-quality information on GHG emissions can decrease a firm's assessed covariances with other firms' cash flows, reducing its cost of capital (Gao, 2010; Lambert et al., 2007). The reduced cost of capital increases the net present value of investment projects, thereby increasing investment opportunities for the firm.

To explore the cost-of-capital channel, we test two sets of predictions. The first set examines the capital structures of treated firms. First, reduced information asymmetry would improve access to cheaper external financing, such as debt financing. Second, as a lower cost of capital expands investment opportunities, we expect a decline in cash distributions to shareholders, leading to higher retained earnings. Third, the model developed by Diamond and Verrecchia (1991) suggests that disclosure lowers the cost of capital by attracting larger (i.e., institutional) investors. Our findings are consistent with these predictions. Specifically, treated firms experienced a 9.2% increase in debt financing and a 12.2 cents increase in retained earnings per dollar of total assets after the GHG disclosure regulation, relative to control firms. Additionally, institutional ownership in treated firms rose by 3.4 percentage points following the regulation.

In the second set of tests, we examine heterogeneity in firms' responses based on financing constraints. If GHG disclosure regulations impact investment by reducing the cost of capital, we would expect stronger responses from more financially constrained firms. We measure financing constraints using the leverage ratio (total liabilities divided by total assets), measured prior to

the reform, based on the idea that firms with higher baseline leverage may face greater difficulties in raising external capital. Our results show that more financially constrained firms exhibit larger increases in employment and capital stock through increased borrowing compared to less-constrained firms. Additionally, we find that these firms experience a larger increase in total revenue following the disclosure regulation. Overall, these heterogeneous effects are consistent with the cost-of-capital channel, suggesting that GHG disclosure mitigates financing constraints.

This paper makes four main contributions. First, we contribute to the literature on the effects of mandatory GHG disclosures on firm-level outcomes. Prior research examines the impacts of GHG disclosure on firms' incentives to reduce emissions (Jouvenot and Krueger, 2019; Downar et al., 2021; Tomar, 2023; Yang et al., 2023), divestiture of polluting assets (Ecker and Keeve, 2023), and "greenwashing" (Grewal et al., 2024). Overall, these studies find that disclosure leads to reductions in emission levels. In addition, Jouvenot and Krueger (2019), Downar et al. (2021), and Tomar (2023) study other firm outcomes such as investment. Jouvenot and Krueger (2019) and Downar et al. (2021) investigate the 2013 reporting regulation change in the United Kingdom, which influenced the dissemination and aggregation of existing emissions information, while Tomar (2023) examines the 2010 reporting regulation change in the United States, which targeted facilities with little to no publicly available emissions data prior to the regulation. These studies exploit a single regulatory change across different countries by comparing firms in a country that implemented new disclosure requirements (in the U.K. or the U.S.) with firms in countries that did not introduce such regulations. Similar to Tomar (2023), our study focuses on regulatory changes that affected facilities without any emission information before the implementation of the regulation. We complement these studies by exploiting multiple regulatory changes within the same country and comparing affected firms to matched control firms, thereby addressing the potential concern about concurrent events that may confound cross-country comparisons. Thus, our findings contribute to the ongoing academic and policy debate on whether GHG disclosure mandates affect firms' investment and performance.

Second, we contribute to the extensive literature examining the impact of information disclosure on the cost of capital, which can affect firm-level outcomes (Stein, 2003; Roychowdhury et al., 2019). Survey evidence indicates that investors with social preferences incorporate firms' ESG practices and GHG emission levels into their capital allocation and investment decisions (Riedl and Smeets, 2017; Bauer et al., 2021; Flammer et al., 2021; Ilhan et al., 2023). Furthermore, GHG emission levels can influence the pricing of transition risk by investors.[3] Our study provides new

[3]Prior research on the pricing of carbon transition risk presents mixed findings. Bolton and Kacperczyk (2021b, 2023) document a positive association between higher carbon emissions and stock returns both in the United States and globally. In contrast, In et al. (2017), Duan et al. (2023), Lubos et al. (2022), and Zhang (2025) find that green assets yield higher returns in both equity and bond markets. Rather than examining the cross-sectional relationship

evidence that mandatory disclosure of GHG emissions can lower the cost of capital for disclosing firms and influence their investment decisions. Furthermore, our paper complements the study by Bolton and Kacperczyk (2021a); while they examine the effects of GHG disclosure on the cost of capital, we assess the impacts of GHG disclosure on real economic outcomes at the firm level.

Third, we contribute to the nascent literature on the impacts of climate-related disclosure on worker outcomes. Such disclosure can affect workers through two key channels. First, through the demand channel, the rise in corporate investment after mandatory disclosure can increase labor demand and raise wages of workers at affected firms in the presence of imperfect labor market competition. Second, information disclosure can influence labor supply through sorting, as workers may choose firms based on their environmental performance. Colonnelli et al. (2025) provide evidence from a field experiment in Brazil, showing that high-skilled workers prefer firms with higher ESG ratings. Similarly, Krueger et al. (2023) and Wang and Li (2025) examine how the cross-firm variation in the ESG ratings of employers affect salaries in Sweden and China, respectively. Furthermore, Flammer and Luo (2017) and Flammer and Kacperczyk (2019) find that corporate social responsibility enhances employee retention and engagement. We complement these studies by leveraging multiple regulatory changes to assess how *disclosure* of GHG emissions affects worker outcomes.

Fourth, our paper relates to the broader literature on the effects of environmental policies on firm- and employee-level outcomes. Prior research examines the consequences of market-based policies, including cap-and-trade programs and carbon taxes, as well as command-and-control policies (Fowlie et al., 2012; Walker, 2013; Acemoglu et al., 2016; Acharya et al., 2025; Chen et al., 2025; Curtis et al., 2025; Greenstone et al., 2025). We contribute to this literature by examining the consequences of an emerging environmental policy — mandatory GHG disclosure — that has been adopted in several countries. Unlike other environmental policies, a GHG disclosure policy does not directly regulate carbon emissions. Instead, it may indirectly affect firm behavior by shaping stakeholder decisions, such as those of investors and employees, under tighter environmental regulations such as carbon tax policies implemented in Canada during our sample period. Our study shows that without directly limiting carbon emissions, mandatory disclosure can have real impacts on firms' investment and financing decisions, which can affect their future emissions.

This paper is organized as follows. Section 2 provides institutional details on the GHG disclosure regulations. Section 3 presents our model. Section 4 describes our data, and Section 5 discusses our empirical strategy. Section 6 presents our main results, and Section 7 discusses potential mechanisms of our findings. Section 8 concludes.

between emission levels and returns, we assess the effects of shocks to the *information* regarding emission levels on firm-level outcomes.

2 Institutional Setting

This section provides institutional details regarding the greenhouse gas reporting program and policy changes in Canada relevant to our empirical design.

To develop a harmonized and efficient mandatory greenhouse gas reporting system, the federal Greenhouse Gas Reporting Program was established in March 2004 by the Department of Environment and Climate Change Canada (ECCC) under the authority of Section 46 of the Canadian Environmental Protection Act.[4] Starting in 2004, all facilities located in Canada emitting 100 kt or more of GHGs in CO2 eq per calendar year were required to report their emissions to the ECCC.[5] In July 2009, the ECCC announced that the GHG reporting threshold would be lowered to 50 kt starting in 2009, and in December 2016 announced a further reduction to 10 kt starting in 2017.[6] The program also encourages facilities that do not meet the reporting threshold to report voluntarily. Between 2004 and 2021, about 8 percent of facilities reporting under the GHGRP voluntarily disclosed emissions, accounting for only 0.3 percent of the total emissions reported. The GHGRP provides significant information about GHG emissions in Canada. For instance, 1,732 facilities reported an aggregated amount of 285 million metric tonnes (MT) CO2 eq in 2021, representing 41 percent of Canada's total GHG emissions.[7]

The program mandates that the company in charge of operating the facility as of December 31 of the calendar year (referred to in the program as the reporting company) must report the facility's GHGs for the entire calendar year by June 1 of the following year to the GHGRP. The reporting company is required to ensure the accuracy of its reports. Specifically, an authorized signing officer of the reporting company (e.g., CEO or plant manager) must sign a "Statement of Certification" to confirm that the information submitted is accurate and complete. In addition, the ECCC reviews the reports to ensure completeness and compliance with the requirements and to identify potential errors. Compliance with the Act is mandatory. Penalties for a first offense (e.g., providing false or misleading information) include fines of up to 1 million Canadian dollars (CAD) or three years of imprisonment for an individual who commits the offense and 6 million CAD for a corporation.[8]

The ECCC publishes data obtained from facility reports, which include information about the facility's name; unique ID; address; the reporting year, and the name; address; and business number

[4]For more details, click the link here.

[5]The program defines a facility as an integrated facility, a pipeline transportation system, or offshore installation. Specifically, an integrated facility includes "all buildings, equipment, structures, on-site transportation machinery, and stationery items that are located on a single site, on multiple sites or between multiple sites that are owned or operated by the same person or persons and that function as a single integrated site." For more details, click the link here.

[6]For more details, click the link here for the 2009 change and here for the 2016 change.

[7]The total GHG emission was 698 MT CO2 eq in 2021. For more details, click the link here.

[8]For more details, click the link here.

of both the reporting company and its parent company. Furthermore, the data contains information on emission levels for 23 gases, broken down by the gas type, such as carbon dioxide (CO2).

In addition to the federal GHGRP, several provinces in Canada have regulations requiring facilities to monitor and report information on their GHG emissions to provincial governments. Most provincial reporting thresholds were greater than or equal to the federal threshold. However, three provinces had a reporting threshold below the federal threshold. Specifically, the reporting threshold in Ontario was 25 kt starting in 2010 and decreased to 10 kt in 2016. Both British Columbia and Quebec began publishing GHG emission data in 2012, with a reporting threshold of 10 kt.[9]

The announcement dates of federal and Ontario disclosure mandates were relatively close to their implementation dates, reducing concerns about anticipation effects. The 2004 federal regulation was announced in March 2004, requiring firms to report their 2004 GHG emissions by June 1, 2005. The announcement dates for the 2009 and 2017 federal-level reporting threshold changes were July 2009 and December 2016, respectively. The Ontario GHG reporting mandate was announced in December 2009 and became effective in 2010. In December 2015, the government of Ontario announced a reduction in its reporting threshold, which took effect in 2016. The GHG reporting mandates for British Columbia and Quebec were announced in December 2009 and November 2007, respectively. However, GHG datasets for these two provinces only became available starting in 2012. Due to the long lag between the announcement date and the first year the GHG dataset became publicly available, we drop treated firms in these two provinces to mitigate concerns over the anticipation effects of the mandates.

Panel A of Figure 1 summarizes the changes in reporting thresholds over time. Panel A of Table A1 in the Appendix A reports the number of facilities that reported their emission levels, the number of unique first-time facility reporters, and the number of unique first-time firm reporters. Note that the number of unique firms in this table represents firms from the GHG data that are matched with our firm balance sheet data, which includes almost all firms from the GHG data.

In 2008, British Columbia implemented a revenue-neutral carbon tax on the purchase and use of fossil fuel and on CO2 emissions. Quebec also implemented a carbon tax policy in 2007, which was replaced by a cap-and-trade system in 2013. In 2007, Alberta implemented the Specified Gas Emitters Regulation strategy, which required that plants which emitted more than 100 kilo tons of CO2 per year to reduce their emissions by 12 percent. These environmental regulations across three major provinces raised the awareness for investors to consider transition risks when deciding which Canadian firms to invest in, making carbon emissions information even more valuable.

[9]British Columbia mandates facilities that emit 10 kt or more GHGs, in CO2 equivalent, per calendar year to report their emissions starting in 2010. Quebec mandates facilities with that emit 10 kt or more GHGs, in CO2 eq, per calendar year to report their emissions before 2012. However, the published data starts in 2012.

3 Model

Environmental regulations such as carbon taxes require firms to adopt to regulatory tightening. However, not all firms are alike: some are more "productive efficient" while others are more "emission efficient". At first glance, two firms may look identical in terms of their output and profitability under the baseline regulation. Under the tighter regulation, their differences become relevant for firm owners and investors when making their investment choices. The purpose of our model is to formalize this logic. Specifically, it seeks to capture how emission efficiency affects firms' cost of capital under different regulatory intensities (e.g., carbon tax) and to show that regulation disproportionately benefits more energy-efficient firms even when they appear identical under the loose regulation. We highlight the role of information asymmetries that complicate firm valuation and also extend the analysis to financing frictions where the efficiency determines both the need for external funds and the enforceability of repayment. By connecting technology, regulation, market performance, and financing into a unified framework, the model clarifies how disclosure policies and environmental regulations can reshape firm dynamics.

3.1 Technology and Unit Cost

We begin with a CES production function where output depends on a variable input v and emissions e, with Hicks-neutral productivity $z > 0$ and emission efficiency $z_e > 0$:

$$y \;=\; z\Big[(1-\alpha_e)v^{\frac{\gamma-1}{\gamma}} + \alpha_e(z_e e)^{\frac{\gamma-1}{\gamma}}\Big]^{\frac{\gamma}{\gamma-1}},$$

The substitution elasticity $\gamma > 0$, and share is denoted $\alpha_e \in (0,1)$. Variable input v has price $w > 0$ and emissions e face shadow cost (or tax) $t_e > 0$.

Cost minimization gives the unit (per efficiency-adjusted output) cost index

$$\begin{aligned} C_w(z_e, t_e) &= \left[(1-\alpha_e)\left(\frac{w}{1-\alpha_e}\right)^{1-\gamma} + \alpha_e\left(\frac{t_e}{\alpha_e z_e}\right)^{1-\gamma}\right]^{\frac{1}{1-\gamma}} \\ &= \Big[A + K\, t_e^{1-\gamma} z_e^{\gamma-1}\Big]^{\frac{1}{1-\gamma}}, \end{aligned}$$

where

$$A \equiv (1-\alpha_e)\left(\frac{w}{1-\alpha_e}\right)^{1-\gamma} > 0, \qquad K \equiv \alpha_e^{\gamma} > 0.$$

This establishes the technological foundation: productivity and efficiency shape the unit cost of producing output under regulatory intensity t_e (e.g., carbon tax).

3.2 Prices, Demand, and Reduced-Form Profits

Assume inverse demand is isoelastic: $p(y) = y^{-1/\eta}$ with $\eta > 1$. Standard constant-markup analysis implies equilibrium output and profits proportional to a power of z/C_w:

$$y(z, z_e, t_e) \;=\; \tilde{\eta}\, z^{\eta-1}\, C_w(z_e, t_e)^{1-\eta}, \qquad \pi(z, z_e, t_e) \;=\; \tilde{\eta}_\pi\, z^{\eta-1}\, C_w(z_e, t_e)^{1-\eta}, \tag{1}$$

for positive constants $\tilde{\eta}, \tilde{\eta}_\pi$ that do not depend on (z, z_e, t_e).

Comparative Statics in z_e and t_e We next examine how cost and profitability change with emission efficiency and regulatory intensity. For empirically relevant values $0 < \gamma < 1$, the cost function is submodular in (z_e, t_e): the benefit of higher emission efficiency is larger when regulation is tighter. As a result, relative profitability of efficient firms increases with regulation. This comparative statics result is central: regulation acts as a magnifier of efficiency differences.

$$\frac{\partial C_w}{\partial z_e} = -K\, t_e^{1-\gamma}\, z_e^{\gamma-2}\, C_w(z_e, t_e)^{\gamma} \;<\; 0,$$

and

$$\frac{\partial^2 C_w}{\partial z_e\, \partial t_e} = -K z_e^{\gamma-2}\Big[(1-\gamma) t_e^{-\gamma} C_w^{\gamma} + \gamma K\, t_e^{1-2\gamma} z_e^{\gamma-1} C_w^{2\gamma-1}\Big] \;<\; 0.$$

This proves that C_w is *submodular* in (z_e, t_e): cost savings from higher z_e are larger when regulation is tighter. Writing the cost ratio

$$R(t) \;\equiv\; \frac{C_w(z_e^H, t)}{C_w(z_e^L, t)} \quad \text{with } z_e^H > z_e^L,$$

one has $R'(t) < 0$ for $0 < \gamma < 1$. Hence for $\eta > 1$, the relative profitability $\pi_H/\pi_L = R(t)^{1-\eta}$ increases with t.

3.3 Information: Two Ex-Ante Same-Output Firms

In practice, investors may not directly observe productivity and emission efficiency. Suppose investors observe current output y but not (z, z_e) separately. We start with two firms, H and L, with $z_e^H > z_e^L$, chosen (via appropriate z_H, z_L) to have *the same output* at $t = 1$:

$$y_H(1) \;=\; y_L(1). \tag{2}$$

Both face the same w and demand.

[Calibration under $t = 1$] Let z_H, z_L equate outputs at $t = 1$. Using (1) and (2),

$$\frac{z_H^{\eta-1}}{z_L^{\eta-1}} = \left(\frac{C_w(z_e^L, 1)}{C_w(z_e^H, 1)}\right)^{1-\eta}. \tag{$*$}$$

[Tightening regulation favors the more efficient firm] Fix $t > 1$. With z_H, z_L from Lemma $*$ and $z_e^H > z_e^L$,

$$\frac{y_H(t)}{y_L(t)} = \left(\frac{C_w(z_e^H, t)}{C_w(z_e^L, t)} \cdot \frac{C_w(z_e^L, 1)}{C_w(z_e^H, 1)}\right)^{1-\eta} = \left(\frac{R(t)}{R(1)}\right)^{1-\eta} > 1,$$

and likewise $\frac{\pi_H(t)}{\pi_L(t)} = \left(\frac{R(t)}{R(1)}\right)^{1-\eta} > 1$, provided $0 < \gamma < 1$ and $\eta > 1$.

Proof. From (1) and ($*$),

$$\frac{y_H(t)}{y_L(t)} = \frac{z_H^{\eta-1}}{z_L^{\eta-1}} \cdot \frac{C_w(z_e^H, t)^{1-\eta}}{C_w(z_e^L, t)^{1-\eta}} = \left(\frac{C_w(z_e^L, 1)}{C_w(z_e^H, 1)}\right)^{1-\eta} \cdot \left(\frac{C_w(z_e^H, t)}{C_w(z_e^L, t)}\right)^{1-\eta}.$$

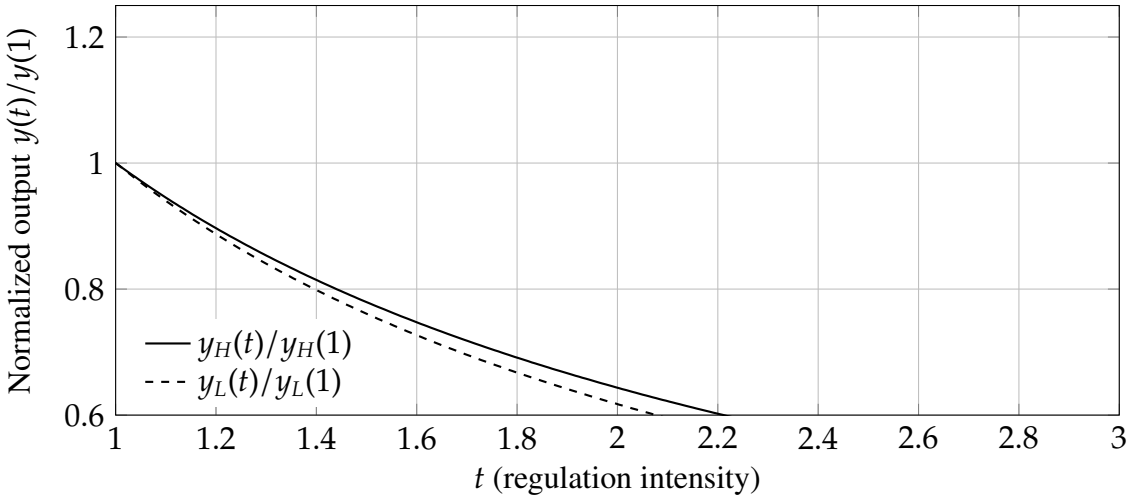
Since $R'(t) < 0$ when $0 < \gamma < 1$, we have $R(t) < R(1)$ for $t > 1$. Because $1 - \eta < 0$, $(R(t)/R(1))^{1-\eta} > 1$. □

[Regulatory risk and expected value] If $t = 1$ with probability $(1-p)$ and $t > 1$ with probability $p \in (0, 1]$, then with $y_H(1) = y_L(1)$,

$$\mathbb{E}[y_H] - \mathbb{E}[y_L] = p\,[y_H(t) - y_L(t)] > 0, \qquad \mathbb{E}[\pi_H] - \mathbb{E}[\pi_L] = p\,[\pi_H(t) - \pi_L(t)] > 0.$$

Illustration: Comparative Statics Figure The figure below plots *normalized outputs* $y_i(t)/y_i(1)$ for $i \in \{H, L\}$ under illustrative parameters with $0 < \gamma < 1$. Because we calibrate $y_H(1) = y_L(1)$, both curves start at 1 when $t = 1$. As t rises above 1, the high-efficiency firm H expands more:

$$\frac{y_H(t)}{y_H(1)} = \left(\frac{C_w(z_e^H, t)}{C_w(z_e^H, 1)}\right)^{1-\eta} > \frac{y_L(t)}{y_L(1)} = \left(\frac{C_w(z_e^L, t)}{C_w(z_e^L, 1)}\right)^{1-\eta}.$$



Notes: Parameters are illustrative. For $0 < \gamma < 1$ and $\eta > 1$, the H curve lies weakly above the L curve for $t > 1$, reflecting that tighter regulation amplifies the value of higher z_e.

3.4 Financing with One-Period Debt and Limited Enforcement

So far, we have focused on profitability. We now extend the model to financing constraints. Firms must borrow to cover input costs, but lenders only get repaid if profits are enforceable. With limited enforcement, borrowing capacity is tied to worst-case profits under tight regulation.

Timing. (i) Lenders and the firm contract at date 0 on a non-contingent one-period loan of face value B with gross interest $1 + r$. (ii) Regulation is realized: $t \in \{1, T\}$ with $T > 1$, occurring with probability $p \in (0, 1]$. (iii) The firm chooses (y, v, e) taking t and prices (w, t) as given. (iv) Profits realize, debt is repaid if incentive-compatible; otherwise the borrower can renege and keep a fraction $(1 - \lambda) \in [0, 1)$ of realized profits while the lender can seize the remaining share λ.

Operating cost that must be financed. Only the variable input v must be prepaid with debt. Let $C_w(z_e, t) = \left[A + K\, t^{1-\gamma} z_e^{\gamma-1}\right]^{1/(1-\gamma)}$ denote the unit cost index (with $A \equiv (1 - \alpha_e)\left(\frac{w}{1-\alpha_e}\right)^{1-\gamma}$ and $K \equiv \alpha_e^{\gamma}$). The CES cost shares imply

$$s_e(z_e, t) = \frac{K\, t^{1-\gamma} z_e^{\gamma-1}}{A + K\, t^{1-\gamma} z_e^{\gamma-1}}, \qquad s_v(z_e, t) = 1 - s_e(z_e, t) = \frac{A}{A + K\, t^{1-\gamma} z_e^{\gamma-1}}. \tag{3}$$

Thus the *financed* operating outlay per unit output is

$$\frac{wv}{y} \;=\; s_v(z_e, t)\,\frac{C_w(z_e, t)}{z} \;=\; \frac{A}{A + Kt^{1-\gamma} z_e^{\gamma-1}} \cdot \frac{C_w(z_e, t)}{z} \;=\; \frac{A\, C_w(z_e, t)^{\gamma}}{z}, \tag{4}$$

since $C_w^{\gamma-1} = \left[A + Kt^{1-\gamma} z_e^{\gamma-1}\right]^{-1}$. Therefore, to produce y units in state t, the firm must raise

$$B_t(y; z_e) \;=\; \frac{A\, C_w(z_e, t)^{\gamma}}{z}\; y. \tag{5}$$

Profits and enforceability. With isoelastic demand and constant markup (see earlier),

$$\pi(z, z_e, t) \;=\; \tilde{\eta}_\pi\, z^{\eta-1}\, C_w(z_e, t)^{\,1-\eta} \quad (\eta > 1). \tag{6}$$

Under limited enforcement, the borrower repays only if the repayment $(1 + r)B$ does not exceed the *enforceable* surplus $\lambda\, \pi(z, z_e, t)$ *ex post*. To guarantee repayment in all states, the contract must satisfy the worst-state enforceability:

$$(1 + r)\, B \;\le\; \lambda\, \min\left\{\pi(z, z_e, 1), \pi(z, z_e, T)\right\} \;=\; \lambda\, \pi(z, z_e, T), \tag{7}$$

since $T > 1$ raises unit costs and lowers profits.

Borrowing capacity and state-contingent operation. Because B is raised at date 0 (before t is known) but production y is chosen after t realizes, feasibility in state t requires $B \ge B_t(y; z_e)$ from (5). Combining with (7) yields a state-t output upper bound:

$$y(z, z_e, t) \;\le\; \frac{z}{A\, C_w(z_e, t)^{\gamma}} \cdot \frac{\lambda\, \pi(z, z_e, T)}{1 + r} \;=\; \frac{\lambda\, \tilde{\eta}_\pi}{1 + r}\, \frac{z^{\eta}}{A}\, \frac{C_w(z_e, T)^{\,1-\eta}}{C_w(z_e, t)^{\gamma}}. \tag{8}$$

Hence in the *tight state* $t = T$,

$$y(z, z_e, T) \;\le\; \frac{\lambda\, \tilde{\eta}_\pi}{1 + r}\, \frac{z^{\eta}}{A}\, C_w(z_e, T)^{\,1-\eta-\gamma}. \tag{9}$$

When the unconstrained optimum would exceed (8), the borrowing limit binds and (8) holds with equality.

[Higher z_e relaxes finance and raises output under regulation] Let $T > 1$ and $0 < \gamma < 1 < \eta$. Then in the finance-constrained region, the tight-state output bound (9) is strictly increasing in z_e. Moreover, the *gain* from raising z_e is larger at $t = T$ than at $t = 1$.

Proof. From (9), $\partial y(z, z_e, T)/\partial z_e \propto \partial C_w(z_e, T)^{\,1-\eta-\gamma}/\partial z_e$. Since $1 - \eta - \gamma < 0$ and C_w decreases in z_e, the derivative is positive. The *magnification* at T follows from submodularity: $\partial^2 C_w/\partial z_e\, \partial t < 0$ (for $0 < \gamma < 1$) implies C_w is more sensitive to z_e when t is higher, so $C_w(z_e, T)^{\,1-\eta-\gamma}$ falls faster in z_e than $C_w(z_e, 1)^{\,1-\eta-\gamma}$, yielding a larger output response at T. □

[Two ex-ante same-output firms and tightening to T] Let firms H and L have $z_e^H > z_e^L$. Choose (z_H, z_L) so that $y_H(1) = y_L(1)$ when $t = 1$ and financing is set competitively under (7). Then upon tightening to $t = T$, the realized outputs satisfy $y_H(T) > y_L(T)$ (and likewise profits), with the gap strictly increasing in T.

Discussion. Limited enforcement forces lenders to cap non-contingent debt at the worst-state enforceable surplus (7). Because the financed share of operating costs scales with $C_w(z_e, t)^\gamma$ while enforceable profits scale with $C_w(z_e, T)^{1-\eta}$, a higher z_e simultaneously *reduces financing needs per unit of output* and *raises worst-state cash flows*. Submodularity ($\partial^2 C_w / \partial z_e \, \partial t < 0$) makes this advantage larger precisely when regulation tightens ($t = T$), delivering $y_H(T) > y_L(T)$ even if firms were ex-ante indistinguishable in output at $t = 1$.

3.5 Model Summary

The model delivers several insights. Unit costs decline with emission efficiency, and the benefit is larger under stricter regulations. Market profitability reflects this asymmetry: efficient firms gain relatively more when regulation tightens. Even if two firms look similar under baseline conditions, a future regulation risk creates divergence in valuation and performance. Financing constraints magnify this effect, as efficiency lowers financing needs and raises repayment capacity.

Overall, the model demonstrates how mandatory carbon disclosure policies and environmental regulations such as carbon taxes disproportionately favor emission-efficient firms, altering competitive dynamics and shaping investment flows. Specifically, our model predicts that under a tighter environmental regulation (e.g., carbon tax policies described in Section 2), firms affected by mandatory carbon disclosure policies may expand and become profitable relative to otherwise similar firms that do not disclosure carbon emissions after the reform.

4 Data and Outcome Variables

In this section, we describe our main datasets used for empirical analyses. The first key dataset consists of disclosure information on GHG at the establishment level, which we aggregate at the firm level using unique business IDs. The second main dataset consists of employer-employee matched data, which contains detailed firm balance-sheet information as well as worker-level information on their earnings, job histories, and demographics from tax records. We merge these two datasets at the firm level using a unique firm ID.

4.1 Greenhouse Gas Emission Disclosure Data

We obtain our data from the ECCC which publishes facility-level GHG emissions data on Canada's Open Government webpage. We refer to this dataset as the GHG dataset, which includes GHG emission levels. To ensure compliance with Statistics Canada's guidelines, we converted the emission levels into bins, with each bin containing a minimum number of observations. This transformation prevents the identification of individual firms after Statistics Canada merges the GHG dataset with the Canadian Employer-Employee Dynamics Database (CEEDD). As a result, we do not have access to precise emission levels but only to emission ranges (e.g., 10 kt–15 kt). In addition, the GHG dataset contains the facility's name; unique ID; and address, and the name and address of the company that owns and operates the facility as of December 31 of the calendar year ("reporting company"). Importantly, the dataset also includes the business number of the reporting company, which can be used to merge GHG dataset with the CEEDD. The business number is a nine-digit number the Canada Revenue Agency assigns to companies for tax-related purposes. To maximize the number of high-quality merges, we manually verify the accuracy of business numbers and augmented the dataset by identifying and adding missing business numbers for reporting firms. Specifically, approximately 90 percent of reporting companies in the GHG dataset include a business number. To ensure accuracy, we verify each business number by cross-referencing it with Canada's Business Registries.[10]

Moreover, we obtain GHG emission data for facilities reporting under province-level regulations from the respective provincial webpage. Specifically, we obtained GHG datasets for facilities in Ontario from the Ontario Data Catalogue for facilities in British Columbia from the Government of British Columbia webpage and for facilities in Quebec from the open data portal of the Quebec government. Following the procedure described above, Statistics Canada merged these datasets with the CEEDD.

4.2 Employer-Employee Matched Data

For our analyses, we use the CEEDD, a matched employee-employer dataset that covers the universe of workers and companies in Canada from 2001 to 2021. This database is based on both individual (T1) and corporate (T2) tax return records, merged with job-level information from T4

[10]Specifically, we compare the company name listed in the GHG dataset with the name associated with the business number in Canada's Business Registries. If the names do not match or the business number is missing from the GHG dataset, we conduct an additional search using the company name in Canada's Business Registries to identify and confirm the correct business number. Statistics Canada first used the reporting year and business number of the reporting company from the GHG dataset to merge the GHG dataset with company-year observations in the CEEDD. If no match is found, it then uses the company name, province, and city from the GHG dataset—and, if necessary, the postal code—to identify the corresponding company in the CEEDD.

employee tax records and Record of Employment (ROE) data and firm-level information from the National Accounts Longitudinal Micro-data File (NALMF). To prevent the disclosure of confidential information, Statistics Canada requires researchers to round estimates and observation counts.

The main outcome variables used in the firm-level analysis are employment, capital stock, total revenue, total expenses, returns on assets, total liabilities, retained earnings (scaled by total assets), and share of institutional shareholders. Employment is defined as the average number of employees reported in the T4s. We use the reported book value of tangible assets, net of depreciation, as a measure of capital stock. We compute the amount of depreciated tangible assets in each year based on yearly changes in accumulated depreciation from their balance sheets. Total revenue and total expenses are the sum of all revenue and expense amounts in a given year, respectively. Returns on assets are defined as the company's net income divided by total assets. Total liabilities are defined as the sum of all current and long-term liabilities. We use the company's retained earnings scaled by total assets.

Furthermore, we link the above dataset to the ownership dataset, which contains information about both individuals and corporate (institutional) ownership of their companies. The ownership dataset derives from tax files T2 Schedule 50, which are required for private corporations with any shareholder holding 10 percent or more of the corporation's shares. We look at both individual and institutional shareholders with direct ownership (excluding ownership via intermediary companies) of the firms. For each owner, the dataset contains the number of incorporated businesses owned, the identifier of these businesses, and shares owned by each individual and corporate owner in percentage. We use this information to compute the share of institutional ownership of a given firm each year.

At the worker level, the key outcome variable is annual earnings aggregated across all employers in a given year. While we include earnings across all employers, we associate workers with the "dominant" employer from whom the employee receives the highest pay in the year. We define a job transition as an indicator equal to one if a worker's employer in a given year is different from the employer from last year.

All outcome variables are winsorized at the fifth and ninety-fifth percentiles except for worker-level earnings, which are winsorized at the first and ninety-ninth percentiles. Our results are robust to different levels of winsorization.

5 Empirical Strategy

This section describes our empirical design and provides descriptive statistics on our matched sample. To define a treated firm, we take the following procedure. As described in Section 2, there have been three federal-level policy changes regarding the GHG disclosure. Under the first reform, implemented in 2004, we identify a firm as a treated firm if at least one of its establishments emitted more than 100 kt GHG in 2004 and therefore appears in our emission database. To increase our sample size, we also identify a firm as a treated firm if at least one of its facilities emitted more than 100 kt GHG in 2005. However, we drop firms of which at least one facility emitted more than 100 kt GHG in 2006, 2007, or 2008; these later-treated firms would likely attenuate our difference-in-differences estimates due to the time gap between the first reform year and when they reveal this information.[11] More importantly, given the time gap since the announcement date, these firms had some time to adjust their emission levels, making the reform "less exogenous" to these treated firms. By contrast, both treated firms and their matched control firms (below the threshold, as explained in the next subsection) in 2004 and 2005 had little time to adjust their emission levels, supporting the idea that this reform was a shock to this set of firms.

Under the second reform in 2009, we identify a firm as treated if at least one of its establishments emitted more than 50 kt GHG in 2009 or in 2010. Among these treated firms, some also become treated at the provincial level (in 2010, Ontario or in 2012, British Columbia or Quebec). Furthermore, most of the firms treated in 2004 continue to report their emission level afterward. For these firms that either become treated again at the provincial level or continue to report their emission at the federal level, we focus on their first event of disclosing this information.[12] Similar to the 2004 reform, we drop firms of which at least one facility emitted more than 50 kt GHG from 2011 to 2016.

Finally, under the third reform in 2017, we identify a firm as a treated firm if at least one of its establishments emitted more than 10 kt GHG in 2017 or in 2018. Again, we focus on the treated firms that disclose GHG emissions for the first time, meaning that if a firm has reported its GHG emissions before 2017, it is not considered as treated again in 2017 even if it emitted more than 10 kt GHG. Furthermore, we drop firms of which at least one facility emitted more than 10 kt GHG in 2019, 2020, or 2021.

For the firms treated by the reform in 2010 Ontario (i.e., at least one facility in Ontario disclosed GHG emissions for the first time in 2010), we include them as part of the treated group in the 2009 federal reform. However, we drop firms treated by the reform in 2012 British Columbia or Quebec

[11] Our results remain qualitatively similar even if we include this small number of firms as part of our treated firms.

[12] While exploiting these multiple treatments for a given firm over time may yield an interesting heterogeneity, we do not find differential responses based on this distinction.

or in 2016 Ontario because some of these firms were already treated by the 2009 federal reform. Lastly, we drop a small share of firms whose facilities emitted below the regulation threshold, but who voluntarily disclose this information.

To estimate the effects of GHG disclosure on firm-level outcomes, we implement a matched difference-in-differences design by estimating a regression of the following form:

$$Y_{jt} = \sum_{k=-4}^{4} \beta_k \mathbb{1}(t_j = t^* + k) \times Treat_j + \tau_t + \psi_j + u_{jt} \tag{10}$$

where Y_{jt} is an outcome variable for firm j at year t, $Treat_j$ is an indicator for a treated firm j, $\mathbb{1}(t_j = t^* + k)$ indicates a policy change occurred k years in the past (or future) relative to the period of the policy t^*, τ_t are year fixed effects that vary by the year of the policy, ψ_j are firm fixed effects, and u_{jt} is an error term. The standard errors are clustered at the firm level. We use control firms that are always below the threshold and, therefore, never disclose their GHG emissions throughout our sample period. To ensure that control firms are comparable to our treated firms, we match them based on their sizes, industry, and province (described in detail in the next subsection). To maximize our sample size and efficiency, we stack the three policy changes and estimate a single difference-in-differences model, although running three separate regressions yields qualitatively similar results in each of these cases. Furthermore, most of the treated firms and their matched control firms are private companies in our analysis sample, so our results remain nearly unchanged even if we focus our analyses exclusively on private firms.

To assess worker-level impacts, we estimate a similar matched difference-in-difference design of the following form:

$$y_{it} = \sum_{k=-4}^{4} \beta_k \mathbb{1}(t_i = t^* + k) \times Treat_{ij} + \tau_t + \omega_i + \mu_{it} \tag{11}$$

where y_{it} is an outcome variable for incumbent worker i at year t, $Treat_{ij}$ is an indicator for a worker i at treated firm j, ω_i are worker fixed effects, and all other variables are defined as in Equation (10). The standard errors are two-way clustered at the worker and firm level.

The key identifying assumption is that outcomes for treated firms and workers and for control firms and workers would have trended similarly in the absence of the policy change. A potential concern is that outcomes of firms above the regulation threshold and, therefore, disclose GHG emissions may evolve differently relative to those of firms below the threshold and do not reveal this information. One way to determine the direction of the potential bias is by comparing outcomes for treated firms and workers to the control firms and workers in the years prior to the

policy change. Parallel pre-trends in firm-level and worker-level outcomes help alleviate the aforementioned concerns. While verifying parallel pre-trends is reassuring for a causal interpretation, contemporaneous shocks that occur with the reform could still bias our results. For example, firms affected by the policy may be concentrated in a particular industry (i.e., energy) and an industry-specific shock could coincide with the reform and differentially affect treated firms relative to control firms. To absorb any industry- or province-specific shock that may coincide with the policy, we include 4-digit industry dummies interacted with year dummies as well as province by year fixed effects as control variables. Furthermore, we match control firms within the same industry and province and exploit within-industry and within-province variation in treatment. We describe how we match our treated firms with control firms in the next subsection.

5.1 Matched Samples

Before performing a matching procedure between treated firms and potential control firms, we make the following restrictions. First, we drop a small share of firms that have missing observations for the key variables used for matching, such as employment, total revenue, total payroll, and firm age, measured one year prior to the reform. We then match each firm one year prior to reform to a control firm in the same province and 4-digit NAICS industry. A firm is a potential control firm for a treated firm j if: (1) the firm is always below the reporting threshold (and therefore never discloses GHG emissions) during our sample period, and (2) the firm is in the same decile bin of employment, total revenue, total payroll, and firm age one year prior to the reform of firm j.[13] Of all the possible counterfactual firms for a given treated firm, we choose the firm with the closest propensity score (one-to-one matching within a caliper), which is estimated by predicting treatment using a linear probability model with a quadratic function in employment, total revenue, total payroll, and firm age in year $[t-1]$. This matching strategy is similar to a number of recent papers implementing a dynamic difference-in-differences design (Goldschmidt and Schmieder, 2017; Smith et al., 2019). The matching strategy finds a control firm in about 70 percent of all treated firms. Choosing one counterfactual control firm per treated firm in a given year ensures that the treated and control groups are comparable on the matched variables. We construct an unbalanced panel of firms that extends 4 years prior to and 4 years after the reform.

To construct the worker-level sample, we extract all workers who were continuously employed in the matched firms during the entire pre-event period (4 years). This tenure restriction is chosen to

[13] While this specification yields parallel pre-trends on the key outcomes, which are important for our identification strategy, our main results remain qualitatively similar when we use different bin sizes or other related firm characteristics for matching. For example, we can make the matching criteria more restrictive so that the treated and control groups look even more comparable; we still find similar results with a smaller sample size. Results based on other variation in matching can be provided upon request.

obtain a sample of workers with attachment to the treated firms and is similar to tenure restrictions used in the mass layoff literature (Jacobson et al., 1993; von Wachter et al., 2009; Lachowska et al., 2020). Additionally, we restrict workers to have at least 4,000 CAD in annual earnings and drop multiple job holders to ensure that we study full-time workers with stable income and attached to their firms (Card et al., 2013; Sorkin, 2018), given that we do not observe work hours in our data. For each worker in a treated firm, we choose a worker in any of the matched control firms in the same 4-digit industry, province, worker age (five-year) bin, gender, and within-sector earnings (ventile) bin. If more than one match is found, we choose the worker with the closest propensity score to the treated worker (one-to-one matching within a caliper), where the propensity score is estimated by predicting treatment using a linear probability model with a quadratic function in worker age and earnings. To compute earnings in the worker-level data, we aggregate annual earnings across all employers if a worker is employed at more than one firm in a given year (due to job transitions).

Since we use a matched control group that is never treated, the specifications above do not suffer from the identification issues that arise in conventional event-study designs (Borusyak et al., 2021) with staggered timing (Goodman-Bacon, 2021). Identification here comes from differences in always-treated and never-treated units over time, not from units coming in and out of treatment.

5.2 Descriptive Statistics

We close this section with descriptive statistics of our analysis sample. Panel A of Table 1 shows the averages for key variables across firms, comparing treated firms to the set of matched control firms one year before the event. On average, treated firms are comparable to their control firms, in terms of employment, capital stock, total revenue, and expenses. Importantly, as we show in Section 6, treated firms and their control firms share parallel pre-trends on these variables, implying that they exhibited similar patterns not only in terms of employment and capital stock, but also with regards to sales and profitability, prior to the reform. The dominant sectors are mining, oil and gas extraction, and manufacturing (about 85 percent of our firm sample).

Panel C of Table 1 shows average worker characteristics in our analysis sample one year before the event. Annual earnings are 66,651 CAD and 67,322 CAD among workers at treated firms and workers at the matched control firms, respectively. Therefore, annual earnings of treated workers are comparable to those of control workers on average. The age and gender compositions are similar between treated workers and their matched control workers.

6 Results

This section reports estimation results from the difference-in-differences models in Section 4 and presents additional tests supporting the interpretations of the results.

6.1 Employment and Capital

We first examine the impacts of the GHG disclosure policy on firms' investment in employment and capital. Figure 2 plots estimates of β_k from equation (10) on employment and capital stock. Panels (a) and (b) show that treated firms followed a similar pattern as control firms before the policy change for each of these outcomes. After the reform, we observe significant increases in these outcomes for treated firms relative to control firms, suggesting that disclosing GHG emissions led treated firms to increase demand for both labor and capital.

Table 2 presents the difference-in-differences estimates on these firm-level outcomes. Columns (1) and (2) show that treated firms' employment and capital stock increased by 11.8 percent and 6.7 percent, respectively, suggesting a large and significant response in the demand for labor and capital. Overall, these results show that disclosing GHG emissions led these firms to hire more workers and increase investment in capital, relative to matched control firms, after the reform.

6.2 Sales, Expenses, and Profitability

Next, we examine whether the increased investment led to higher sales and profitability for the treated firms. Figure 3 plots estimates of β_k from equation (10) on total revenue, total expenses, and returns on assets. Panels (a) – (c) show that treated firms followed a similar pattern as control firms before the reform for each of these outcomes. After the policy change, we find significant increases in these outcomes for treated firms, suggesting that disclosing GHG emissions led these firms to increase total expenses, but experience a rise in sales and profitability.

Table 3 presents the difference-in-differences estimates on these firm-level outcomes. Columns (1) and (2) show that treated firms' total revenue and expenses increased by 13.3 percent and 12.9 percent per year, respectively, after the reform. Furthermore, returns on assets increased by 1.2 percentage points (about 3.3 percent increase), suggesting that firms became moderately more productive after the reform. In Section 6, we explore potential mechanisms behind the increases in firms' investment, sales, and profitability after disclosing GHG emissions due to the policy change.

6.3 Worker-level Earnings and Job Transitions

Our firm-level results highlight that disclosing GHG emissions after the federal regulation led to positive outcomes for the affected firms. Before we explore potential mechanisms behind these responses, we examine whether the increased labor demand also led to higher wages for the workers at treated firms. To estimate how the GHG disclosure policy affects worker earnings, we use employee-level data to track the same workers over time and control for changes in worker compositions within firms. In Figure 4, we plot $\hat{\beta}_k$ estimated from equation (11) on workers' annual earnings in logarithm. As the figure shows, earnings of treated workers trended similarly with those of control workers in the years prior to the policy change, but increased significantly after the reform. Column (1) of Table 4 shows that workers' annual earnings at treated firms increased by 8.8 percent on average relative to those of workers at control firms after the reform. Column (2) of Table 4 shows that the probability of moving to another firm decreased by 4.1 percentage points, for treated workers relative to control workers, meaning that the retention rate increased. In the next section, we explore potential mechanisms behind these firm- and worker-level results and provide economic interpretations of our findings.

7 Economic Interpretations and Potential Mechanisms

In this section, we examine potential mechanisms behind increases in investment, sales, and profitability of firms affected by the GHG disclosure policy.

7.1 Information Friction and Cost of Capital Channel

As described in Section 3, mandatory disclosure of GHG emissions can lead to higher corporate investment of disclosing firms by lowering the information frictions and thereby lowering the cost of capital. The model suggests that disclosing this information can also increase the cost of capital if it contains a net negative signal (i.e., higher than expected emission level relative to firm performance). Given that it is difficult to measure investors' expectation regarding individual firms' environmental risks, it is challenging to measure which firms would face a higher cost of capital after disclosing GHG emissions. Instead, if we find that an average firm increases investment after disclosing GHG emissions, we can infer that the disclosure led to a net reduction in the cost of capital by reducing information frictions for a given quality of the signal on average.

On the other hand, one empirically testable prediction from the model is that we would expect to see increases in external financing, changes in the composition of shareholders, and cash

distributions to shareholders of affected firms. Specifically, to the extent that higher-quality information reduces information frictions and increases corporate investment, we would expect the firm to more easily raise capital at a lower cost which leads to an increase in debt financing (via corporate bond issuance or borrowing from banks) and reduces the internal cash distributions to shareholders. We use retained earnings (scaled by total assets) to measure distributions to shareholders. Since retained earnings are the cumulative net income minus the cumulative distributions to shareholders, an increase in retained earnings captures a reduction in distributions to shareholders, relative to the firm's profit. We also examine the share of institutional shareholders since the rise in institutional ownership indicates increased stock liquidity and lower cost of capital (e.g., Diamond and Verrecchia, 1991). Figure 5 and Table 5 show that total liabilities and the share of institutional ownership increased by 9.2 log points and 3.4 percentage points (about 5 percent) for treated firms relative to control firms on average after disclosing GHG emissions. Furthermore, retained earnings increased by 12.2 cents per dollar of total assets (about 21 percent increase) after the reform. These results are consistent with the predictions from the model that disclosing GHG emissions can result in an overall reduction in the cost of capital, leading treated firms to increase their debts and share of institutional owners and reduce cash distributions to shareholders.

Moreover, another testable prediction from the model is that we would see larger responses among firms that were more financially constrained prior to the reform. To proxy firms' financial constraints, we use firms' leverage ratio (total liabilities divided by total assets) and cut firms based on the median value of the ratio measured in the year before the reform. We define firms as constrained if they have high debts relative to their total assets (above the median) before the reform. Figure 6 and Table 6 show these estimates separately for less-constrained firms (below the median) and more-constrained firms (above the median). We find much larger and statistically significant increases in employment and capital stock among more-constrained firms relative to less-constrained firms, financed by larger increases in total liabilities on average. Subsequently, we find larger increases in their total revenue among more-constrained firms after the reform. These heterogeneous responses based on the pre-reform measure of financial constraints suggest that lowering the cost of capital via reducing information frictions is likely the key mechanism behind our firm-level results.

7.2 Why Do Firms Not Voluntarily Disclose GHG Emissions?

Bolton et al. (2021, p.1) note, "The overwhelming majority of listed companies around the world still does not disclose their GHG emissions, and even fewer privately held companies do so." Our results so far are consistent with the idea that disclosing GHG emissions lowers the cost of capital

and brings net positive outcomes for the affected firms on average. A natural question is why firms do not voluntarily disclose this information. We next discuss three key reasons why firms may not provide voluntary GHG disclosure.

First, one argument for disclosure regulations is their comparability benefits. Firms disclosing under the same regulation provide more comparable reports, making it easier and less costly for investors and other stakeholders to collect and interpret information and to compare firms (Zingales, 2009; Leuz and Wysocki, 2016). The greater comparability reduces costs for investors to collect and process information, thereby reducing the cost of capital (e.g., Hail et al., 2010). The ECCC states that the goal of the federal GHG Reporting Program introduced in 2004 is to develop "a harmonized and efficient" reporting system. Similarly, the SEC's climate-related disclosure rule states: "The Commission proposed the climate-related disclosure rules in order to elicit more consistent, comparable, and reliable information for investors to enable them to make informed assessments of the impact of climate-related risks" (SEC, 2024, p.36). The SEC also notes that although some firms voluntarily provide climate-related information, their "disclosures currently vary considerably in terms of coverage, location, and presentation across companies, making it difficult for investors to navigate through different information sources and filings to identify, compare, and analyze climate-related information" (SEC, 2024, p.626). In addition to requiring more consistent and comparable reporting, GHG disclosure regulations require firms to adopt a common methodology to measure their GHG emission levels. In the absence of disclosure regulations, coordinating and agreeing on a common set of rules for measuring and disclosing GHG emissions is highly costly for firms, even if all firms are willing to provide this information voluntarily.

Second, when firms are uncertain about investor preferences and reactions, they reduce voluntary disclosure in equilibrium (Bond and Zeng, 2022). Mandatory GHG disclosure enables investors to compare and rank firms based on their GHG emissions. It also allows firms to assess their GHG performance *relative* to their peers (Tomar, 2023). To the extent that investors compare a firm's GHG emissions relative to its peers, mandatory disclosure reduces firms' uncertainty about investor preferences and responses to GHG disclosure. In the absence of disclosure regulation, such uncertainty may lead firms to lower their voluntary disclosure.

Third, disclosures can have direct and proprietary costs. GHG disclosures impose significant direct costs (i.e., compliance costs) on firms. Such costs include the costs related to developing or maintaining systems for tracking, measuring, and aggregating GHG emissions as well as the costs related to preparing and presenting the resulting disclosures (reporting costs). More importantly, disclosure can lead to substantial managerial opportunity costs. Since a large portion of these costs are fixed, GHG disclosures can be particularly burdensome for smaller firms. Furthermore, GHG disclosure can reveal commercially sensitive or proprietary information to competitors, which can

put firms at a competitive disadvantage (Bhattacharya and Ritter, 1983; Verrecchia, 1983). Specifically, as discussed in Section 2, the GHG Reporting Program requires firms to disclose detailed information about the location of their facility as well as detailed information on GHG emission levels for 23 gases, broken down by gas type. Such information can reveal the location of a firm's operations, its production level in each location, and its emission efficiency. Such information, combined the firm's financial reporting such as sales and profitability, can help competitors identify profitable investment opportunities, which can, in turn, affect entry and competitors' output at the expense of the disclosing company. In addition, competitors might clean insights into the disclosing firm's manufacturing processes from the breakdown of gases composing a company's emissions.[14]

7.3 Other Potential Mechanisms

While our results seem most consistent with the information friction and the cost-of-capital channel, another mechanism that potentially explains the increase in real investment. After mandatory GHG disclosure is implemented, affected firms may have greater incentives to invest in cleaner technology (e.g., solar panels or more energy-efficient machines) to increase demand and reduce potential pressure from investors with social preferences for more sustainable firms (e.g., Riedl and Smeets, 2017; Bauer et al., 2021; Ilhan et al., 2023; Aghamolla and An, 2024). This response will be stronger especially among firms that cannot scale back their production to reduce aggregate GHG emissions. To test this potential channel, we plan to examine whether treated firms increased their investment in green technology using a dataset on investment across different asset classes.[15] Note that this mechanism does not necessarily contradict the cost-of-capital channel and, in fact, can be complementary. For example, disclosing GHG emissions can lower the cost of capital, which can encourage firms to invest in tangible assets, including green assets.

8 Conclusion

In this paper, we study the effects of mandatory GHG disclosure on firms' investment, employment, and capital structure, as well as their workers' labor market outcomes. Specifically, we leverage a series of regulatory disclosure changes that affected different groups of firms over time in Canada, and the administrative matched employer-employee data to identify firms that were above the re-

[14] A survey of the Retail Industry Leaders Association's members shows their concern with the SEC's climate-related disclosure because reporting GHG on gas-by-gas basis can allow competitors to glean insights into the firm's operational and manufacturing processes. For more details, click the link here.

[15] This dataset will be available in May 2025.

porting threshold and mandated to report their GHG emissions for the first time (treated firms). These firms are matched with control firms of the same size and age bins, province, and industry, which were always below the reporting threshold and, therefore, never disclosed GHG emissions during our sample period.

Using a matched difference-in-differences design, we find that treated firms significantly increase employment and capital stock after the reform relative to control firms. Subquently, treated firms exhibit larger sales and a moderate increase in profitability. Furthermore, incumbent workers at treated firms earn higher earnings after the disclosure policy compared to matched employees at control firms. Additionally, treated employees are less likely to move to another firm, suggesting higher retention.

Overall, our results suggest that mandatory disclosure of GHG emissions impacts corporate investment by providing investors with relevant information for assessing disclosing firms' climate risk, thereby reducing information asymmetry and lowering the firm's cost of capital. To further explore this cost-of-capital mechanism, we develop and test two sets of predictions. First, we find that treated firms experience an increase in debt financing, retained earnings, and institutional investor ownership, suggesting that reduced information asymmetry facilitates external financing, attracts larger investors, and motivates firm managers to retain more earnings rather than distributing them to shareholders. Second, we show that these effects are concentrated among more financially constrained firms, as they increase their employment and capital stock through higher debt financing compared to less constrained firms. Overall, these results are consistent with the idea that disclosing GHG emissions can lower the cost of capital by reducing information frictions.

Prior research exploring the impact of mandatory GHG disclosure on firm-level outcomes primarily relies on cross-country variation, comparing firms in a country that implemented new disclosure requirements with firms in countries that did not (e.g., Jouvenot and Krueger, 2019; Downar et al., 2021; Tomar, 2023). We complement these studies by leveraging multiple regulatory changes that allow us to examine firms within the same country, addressing potential concerns about concurrent events that may confound cross-country comparisons.

Our paper also relates to the extensive literature examining the impact of information disclosure on the cost of capital and corporate investment by providing new evidence on the effects of GHG disclosure. Investors may prefer to invest in low-emission stocks either because they derive utility from holding such stocks or because these stocks hedge climate risk. Our results indicate that disclosing GHG emissions not only benefits investors in their capital allocation and hedging decisions but also facilitates growth by reducing the cost of capital. Overall, our findings imply that GHG disclosure, mandated and enforced by the government, may bring net benefits to disclosing firms, despite the associated regulatory and private costs.

References

Acemoglu, D., Akcigit, U., Hanley, D., and Kerr, W. (2016). Transition to clean technology. *Journal of political economy*, 124(1):52–104.

Acharya, V. V., Giglio, S., Pastore, S., Stroebel, J., Tan, Z., and Yong, T. (2025). Climate transition risks and the energy sector. Technical report, CESifo.

Aghamolla, C. and An, B.-J. (2024). Mandatory vs. voluntary esg disclosure, efficiency, and real effects. Working paper.

Alekseev, G., Giglio, S., Maingi, Q., Selgrad, J., and Stroebel, J. (2024). A quantity-based approach to constructing climate risk hedge portfolios. *Working Paper*.

Bauer, R., Ruof, T., and Smeets, P. (2021). Get real! individuals prefer more sustainable investments. *The Review of Financial Studies*, 34(8):3976–4043.

Bhattacharya, S. and Ritter, J. R. (1983). Innovation and communication: Signalling with partial disclosure. *The Review of Economic Studies*, 50:331–346.

Bolton, P. and Kacperczyk, M. (2021a). Carbon disclosure and the cost of capital. Working paper.

Bolton, P. and Kacperczyk, M. (2021b). Do investors care about carbon risk? *Journal of Financial Economics*, 142:517–549.

Bolton, P. and Kacperczyk, M. (2023). Global pricing of carbon-transition risk. *The Journal of Finance*, 78(6):3677–3720.

Bolton, P., Reichelstein, S. J., Kacperczyk, M. T., Leuz, C., Ormazabal, G., and Schoenmaker, D. (2021). Mandatory corporate carbon disclosures and the path to net zero. *Management and Business Review*, 1(3):21–28.

Bond, P. and Zeng, Y. (2022). Silence is safest: Information disclosure when the audience's preferences are uncertain. *Journal of Financial Economics*, 145:178–193.

Borusyak, K., Jaravel, X., and Spiess, J. (2021). Revisiting event study designs: Robust and efficient estimation. *arXiv preprint arXiv:2108.12419*.

Card, D., Heining, J., and Kline, P. (2013). Workplace heterogeneity and the rise of west german wage inequality. *The Quarterly Journal of Economics*, 128(3):967–1015.

Chen, Q., Chen, Z., Liu, Z., Suárez Serrato, J. C., and Xu, D. Y. (2025). Regulating conglomerates: Evidence from an energy conservation program in china. *American Economic Review*, 115(2):408–447.

Chen, Q., Ryan, N., and Xu, D. Y. (2024). Adverse selection in carbon offset markets: Evidence from the clean development mechanism in china. Technical report, Mimeo.

Colonnelli, E., McQuade, T., Ramos, G., Rauter, T., and Xiong, O. (2025). Polarizing corporations: Does talent flow to "good" firms? Working paper.

Curtis, E. M., Miao, W., Soliman, F. S., Serrato, J. C. S., and Xu, D. Y. (2025). The geographical leakage of environmental regulation: Evidence from the clean air act. Technical report.

Diamond, D. W. and Verrecchia, R. E. (1991). Disclosure, liquidity, and the cost of capital. *The Journal of Finance*, 46(4):1325–1359.

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

Duan, T., Li, F. W., and Wen, Q. (2023). Is carbon risk priced in the cross section of corporate bond returns? *Journal of Financial and Quantitative Analysis*, pages 1–35.

Easley, D. and O'Hara, M. (2004). Information and the cost of capital. *The Journal of Finance*, 59(4):1553–1583.

Ecker, F. and Keeve, T. (2023). Managing corporate emission disclosures through divestitures. Working paper.

Engle, R. F., Giglio, S., Kelly, B., Lee, H., and Stroebel, J. (2020). Hedging climate change news. *The Review of Financial Studies*, 33(3):1184–1216.

Flammer, C. and Kacperczyk, A. (2019). Corporate social responsibility as a defense against knowledge spillovers: Evidence from the inevitable disclosure doctrine. *Strategic Management Journal*, 40(8):1243–1267.

Flammer, C. and Luo, J. (2017). Corporate social responsibility as an employee governance tool: Evidence from a quasi-experiment. *Strategic Management Journal*, 38(2):163–183.

Flammer, C., Toffel, M. W., and Viswanathan, K. (2021). Shareholder activism and firms' voluntary disclosure of climate change risks. *Strategic Management Journal*, 42(10):1850–1879.

Fowlie, M., Holland, S. P., and Mansur, E. T. (2012). What do emissions markets deliver and to whom? evidence from southern california's nox trading program. *American Economic Review*, 102(2):965–993.

Fowlie, M., Reguant, M., and Ryan, S. P. (2016). Market-based emissions regulation and industry dynamics. *Journal of Political Economy*, 124(1):249–302.

Gao, P. (2010). Disclosure quality, cost of capital, and investor welfare. *The Accounting Review*, 85(1):1–29.

Giglio, S., Maggiori, M., Stroebel, J., Tan, Z., Utkus, S., and Xu, X. (2025). Four facts about esg beliefs and investor portfolios. *Journal of Financial Economics*, 164:103984.

Goldschmidt, D. and Schmieder, J. F. (2017). The rise of domestic outsourcing and the evolution of the german wage structure. *The Quarterly Journal of Economics*, 132(3):1165–1217.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, 225(2):254–277.

Greenstone, M., Leuz, C., and Breuer, P. (2023). Mandatory disclosure would reveal corporate carbon damages. *Science*, 381(6660):837–840.

Greenstone, M., Oyer, P., and Vissing-Jorgensen, A. (2006). Mandated disclosure, stock returns, and the 1964 securities acts amendments. *The Quarterly Journal of Economics*, 121(2):399–460.

Greenstone, M., Pande, R., Ryan, N., and Sudarshan, A. (2025). Can pollution markets work in developing countries? experimental evidence from india. *The Quarterly Journal of Economics*, page qjaf009.

Grewal, J., Richardson, G., and Wang, J. (2024). Effects of mandatory carbon reporting on greenwashing. Working paper.

Hail, L., Leuz, C., and Wysocki, P. (2010). Global accounting convergence and the potential adoption of ifrs by the us (part i): Conceptual underpinnings and economic analysis. *Accounting horizons*, 24(3):355–394.

Hartzmark, S. M. and Sussman, A. B. (2019). Do investors value sustainability? a natural experiment examining ranking and fund flows. *The Journal of Finance*, 74(6):2789–2837.

Ilhan, E., Krueger, P., Sautner, Z., and Starks, L. T. (2023). Climate risk disclosure and institutional investors. *The Review of Financial Studies*, 36:2617–2650.

In, S. Y., Park, K. Y., and Monk, A. (2017). Is "being green" rewarded in the market? an empirical investigation of decarbonization risk and stock returns. *International Association for Energy Economics (Singapore Issue)*, 46(48):46–48.

Jacobson, L. S., LaLonde, R. J., and Sullivan, D. G. (1993). Earnings losses of displaced workers. *The American Economic Review*, 83(4):685–709.

Jouvenot, V. and Krueger, P. (2019). Mandatory corporate carbon disclosure: Evidence from a natural experiment. Working paper.

Krueger, P., Metzger, D., and Wu, J. (2023). The sustainability wage gap. *Finance Working Paper*, (718/2020). Working paper.

Lachowska, M., Mas, A., and Woodbury, S. A. (2020). Sources of displaced workers' long-term earnings losses. *American Economic Review*, 110(10):3231–3266.

Lambert, R., Leuz, C., and Verrecchia, R. E. (2007). Accounting information, disclosure, and the cost of capital. *Journal of Accounting Research*, 45(2):385–420.

Leuz, C. and Wysocki, P. D. (2016). The economics of disclosure and financial reporting regulation: Evidence and suggestions for future research. *Journal of Accounting Research*, 54(2):525–622.

Lubos, P., Stambaugh, R. F., and Taylor, L. A. (2022). Dissecting green returns. *Journal of Financial Economics*, 146:403–424.

Minnis, M. (2011). The value of financial statement verification in debt financing: Evidence from private us firms. *Journal of accounting research*, 49(2):457–506.

Riedl, A. and Smeets, P. (2017). Why do investors hold socially responsible mutual funds? *The Journal of Finance*, 72(6):2505–2549.

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

SEC (2024). The enhancement and standardization of climate-related disclosures for investors: Release nos. 33-11275; 34-99678; file no. s7-10-22. *https://www.sec.gov/files/rules/final/2024/33-11275.pdf*.

SIF (2020). Report on us sustainable and impact investing trends. *SIF report*.

Smith, M., Yagan, D., Zidar, O., and Zwick, E. (2019). Capitalists in the twenty-first century. *The Quarterly Journal of Economics*, 134(4):1675–1745.

Sorkin, I. (2018). Ranking firms using revealed preference. *The Quarterly Journal of Economics*, 133(3):1331–1393.

Stein, J. C. (2003). Agency, information, and corporate investment.

Tomar, S. (2023). Greenhouse gas disclosure and emissions benchmarking. *Journal of Accounting Research*, 61(2).

Verrecchia, R. E. (1983). Discretionary disclosure. *Journal of Accounting and Economics*, 5:179–194.

von Wachter, T., Song, J., and Manchester, J. (2009). Long-term earnings losses due to mass layoffs during the 1982 recession: An analysis using us administrative data from 1974 to 2004. Manuscript.

Walker, W. R. (2013). The transitional costs of sectoral reallocation: Evidence from the clean air act and the workforce. *The Quarterly journal of economics*, 128(4):1787–1835.

Wang, X. and Li, X. (2025). Esg performance, employee income and pay gap: Evidence from chinese listed companies. *working paper*.

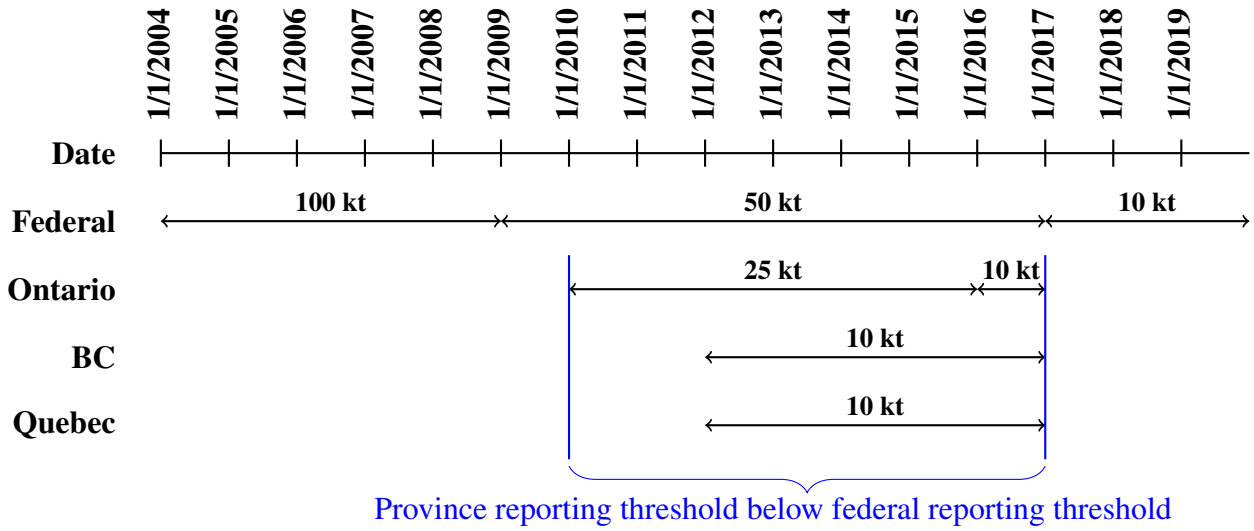
Yang, L., Muller, N. Z., and Liang, P. J. (2023). The real effects of mandatory csr disclosure on emissions: Evidence from the greenhouse gas reporting program. *Working Paper*.

Zhang, S. (2025). Carbon returns across the globe. *The Journal of Finance*, 80(1):615–660.

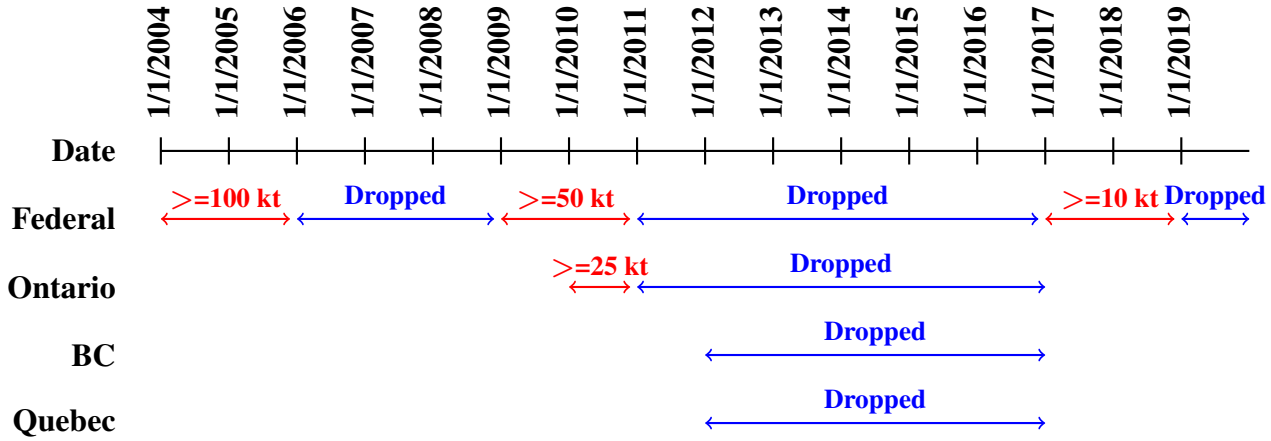
Zingales, L. (2009). The future of securities regulation. *Journal of Accounting Research*, 47(2):391–425.

Figure 1: Changes in Reporting Threshold over time and Policy Variation

(a) Changes in GHG Reporting Threshold Over Time



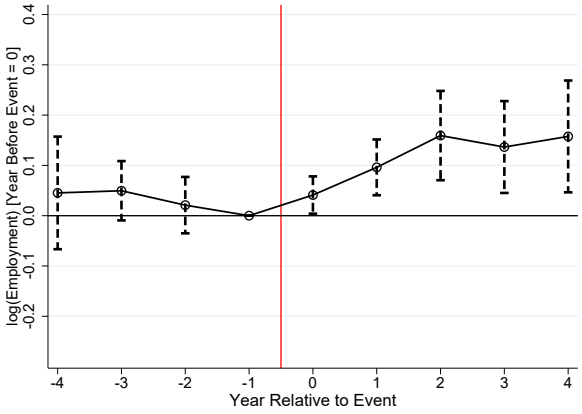
(b) Diagram Explaining the Policy Variation



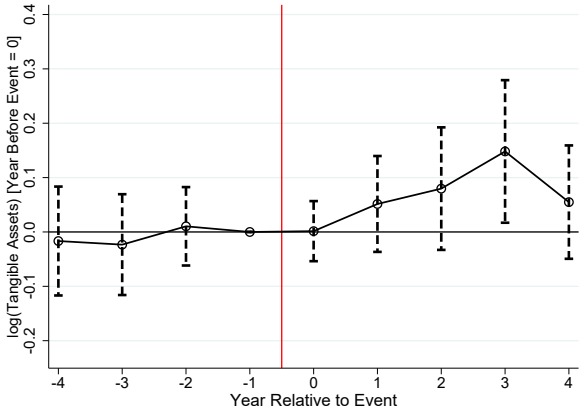
Notes: Panel A presents the reporting threshold for the federal Greenhouse Gas Reporting Program (GHGRP) and for provincial regulations when the provincial threshold is lower than the federal threshold. Panel B illustrates our approach to identifying treated firms. Specifically, we define treated firms as those with at least one facility that exceeds the reporting threshold during the year the mandate is implemented and the subsequent year. The red segments in the figure indicate periods when new emission thresholds were introduced under federal or provincial greenhouse gas (GHG) reporting requirements. However, we focus only on firms that are treated for the first time. That is, if a firm was already subject to disclosure requirements in an earlier regulatory change (e.g., treated in 2004) and later exceeds a lower emissions threshold in a subsequent regulatory change (e.g., 2009), it is not considered newly treated in that later period. Additionally, we exclude firms in years marked in blue, as these later-treated firms would likely attenuate our difference-in-differences estimates due to the time gap between the policy change year and when they disclose their emissions. BC stands for British Columbia.

Figure 2: Firms' Employment and Capital Stock

(a) log(Employment)



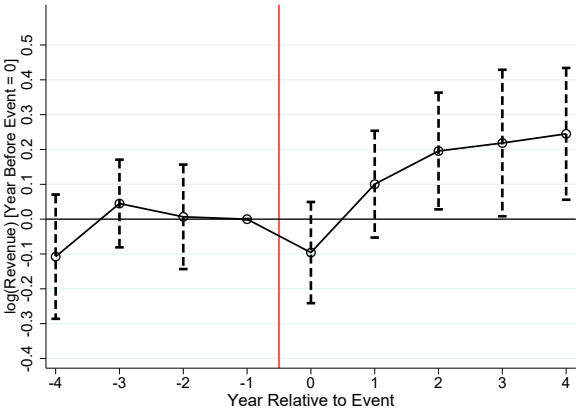
(b) log(Tangible Assets)



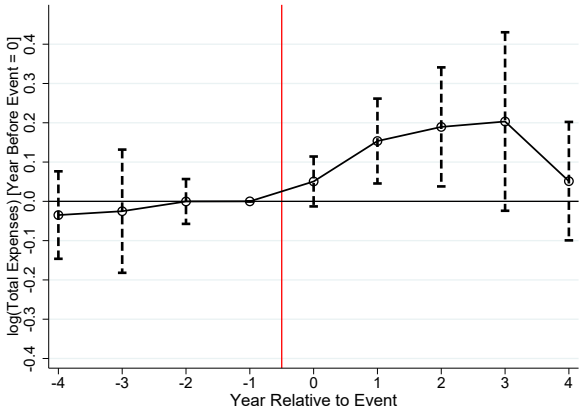
Notes: These figures present event-study estimates of the impact of GHG disclosure mandates on employment and tangible assets. Employment is measured as the average number of employees reported in employee tax records and Record of Employment data. The reported book value of tangible assets, net of accumulated depreciation, serves as our measure of capital stock. The dashed vertical lines represent 95 percent confidence intervals, with standard errors clustered at the firm level. We stack several policy changes and estimate a single difference-in-differences model (more detail in Section 4). The policy change is implemented in year 0, and the coefficient is normalized to zero in year -1.

Figure 3: Firms' Revenue and Profitability

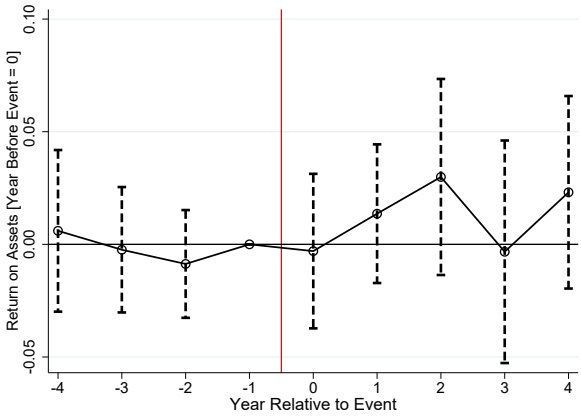
(a) log(Total Revenue)



(b) log(Total Expenses)



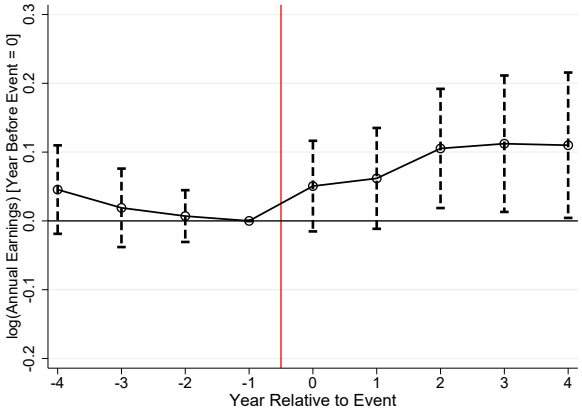
(c) Return on Assets



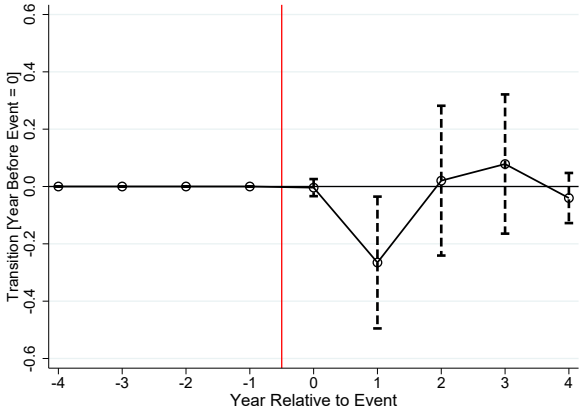
Notes: These figures present event-study estimates of the impact of GHG disclosure mandates on total revenue, total expenses, and return on assets. Total revenues represent the sum of all revenue sources in a given year, and total expenses reflect the sum of all expenses incurred within the same period. Return on assets (ROA) is calculated as net income divided by total assets. The dashed vertical lines represent 95 percent confidence intervals, with standard errors clustered at the firm level. We stack several policy changes and estimate a single difference-in-differences model (more detail in Section 4). The policy change is implemented in year 0, and the coefficient is normalized to zero in year -1.

Figure 4: Worker-level Outcomes

(a) log(Annual Earnings)



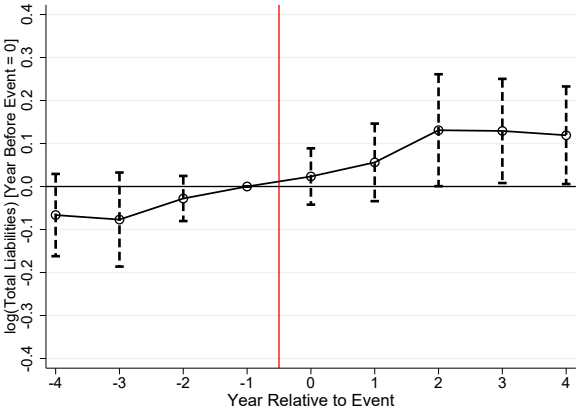
(b) Transition



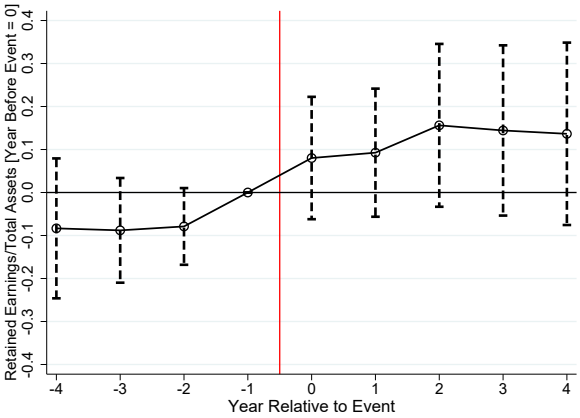
Notes: These figures present event-study estimates of the impact of GHG disclosure mandates on worker-level outcomes, including annual earnings and job transitions. Worker annual earnings are aggregated across all employers in a given year. Job transition is defined as an indicator variable equal to one if a worker's employer in a given year differs from their employer in the previous year. The dashed vertical lines represent 95 percent confidence intervals, with standard errors clustered at the firm and worker levels. We stack several policy changes and estimate a single difference-in-differences model (more detail in Section 4). The policy change is implemented in year 0, and the coefficient is normalized to zero in year -1.

Figure 5: Firms' Capital Structure

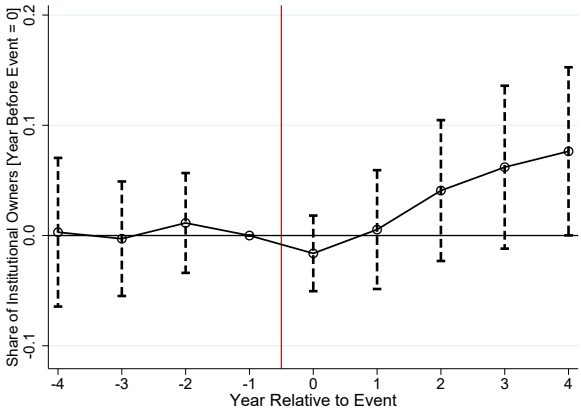
(a) log(Total Liabilities)



(b) Retained Earnings / Total Assets

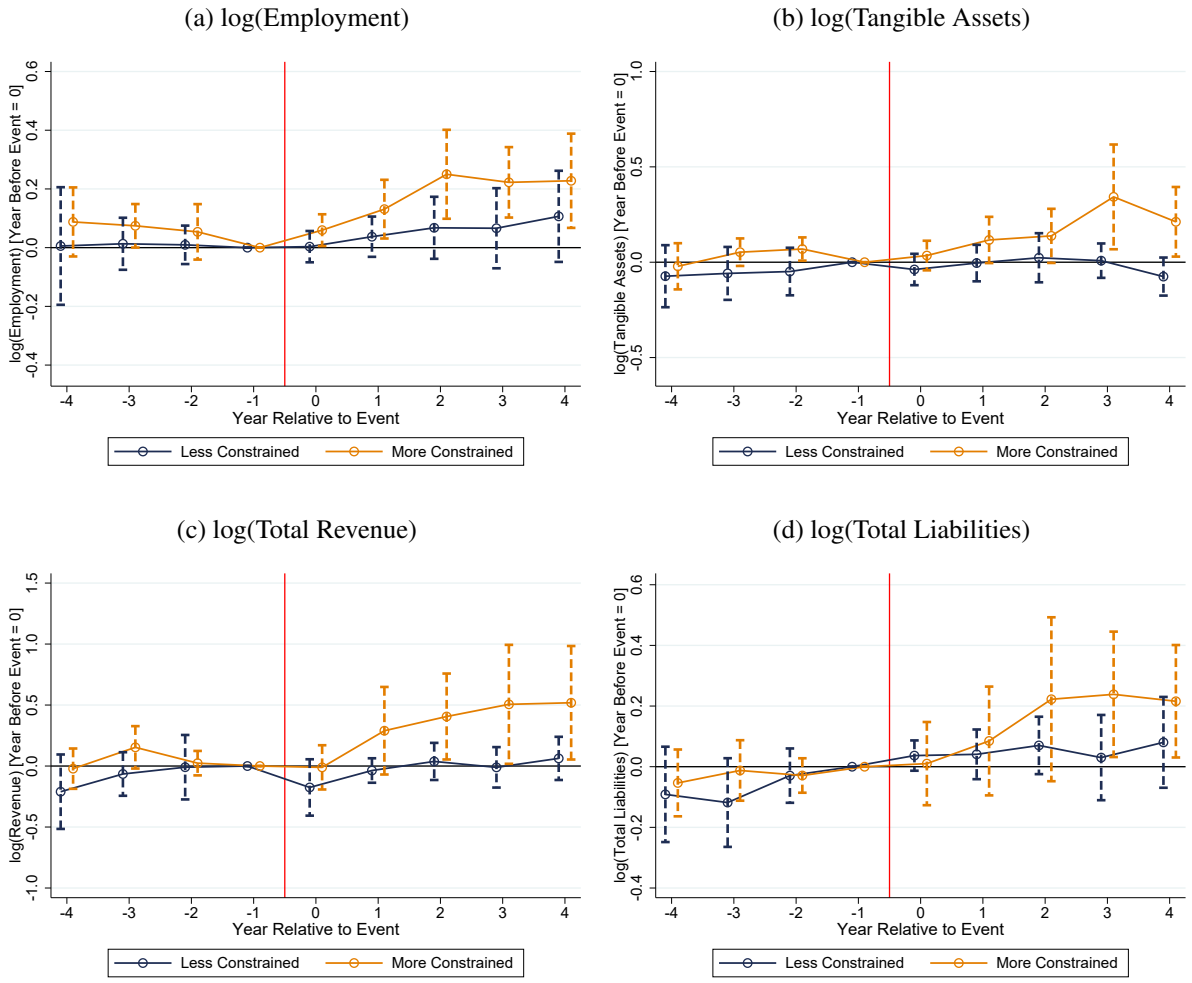


(c) Share of Institutional Owners



Notes: These figures present event-study estimates of the impact of GHG disclosure mandates on total liabilities, retained earnings scaled by total assets, and share of institutional ownership. Total liabilities are defined as the sum of all current and long-term liabilities. The share of institutional ownership represents the percentage of a firm's equity held by institutional investors in a given year. The dashed vertical lines represent 95 percent confidence intervals, with standard errors clustered at the firm level. We stack several policy changes and estimate a single difference-in-differences model (more detail in Section 4). The policy change is implemented in year 0, and the coefficient is normalized to zero in year -1.

Figure 6: By Financial Constraints Based on Pre-reform Leverage Ratio



Notes: These figures present event-study estimates of the impact of GHG disclosure mandates on firm-level outcomes, separately for less-financially constrained firms (navy lines) and more-financially constrained firms (orange lines). Firm-level outcomes include employment, tangible assets, total revenues, and total liabilties. We define less- (more-) financially constrained firms as those with below- (above-) median pre-reform measure of leverage ratio. The leverage ratio is defined as total liabilities divided by total assets. Employment is measured as the average number of employees reported in employee tax records and Record of Employment data. The reported book value of tangible assets, net of accumulated depreciation, serves as our measure of capital stock. Total revenues represent the sum of all revenue sources in a given year. Total liabilities are defined as the sum of all current and long-term liabilities. The dashed vertical lines represent 95 percent confidence intervals, with standard errors clustered at the firm level. We stack several policy changes and estimate a single difference-in-differences model (more detail in Section 4). The policy change is implemented in year 0, and the coefficient is normalized to zero in year -1.

Table 1: Descriptive Statistics on Firms and Workers (at $t = -1$)

	(1) Treated	(2) Control
Panel A: Descriptive Stats on Firms		
Employment	30.217	27.779
Total Payroll (in thousands)	1092.804	1050.855
Tangible Assets (in thousands)	1687.075	1513.027
Total Revenue (in thousands)	2580.133	2535.103
Total Expenses (in thousands)	2327.900	2295.204
Total Liabilities (in thousands)	2336.136	2049.281
Return on Assets	0.03	0.053
Share of Institutional Owners	0.473	0.444
Number of Firms	240	240
Panel B: Sector Composition		
Agriculture, Forestry, Fishing and Hunting	0.077	0.074
Mining, Quarrying, and Oil and Gas Extraction	0.322	0.322
Manufacturing	0.442	0.446
Transportation and Warehousing	0.030	0.029
Professional, Scientific, and Technical Services	0.009	0.008
Educational Services	0.069	0.066
Health Care and Social Assistance	0.017	0.021
Other Services	0.034	0.034
Panel C: Descriptive Stats on Workers		
Earnings	66,651.02	67,322.38
Male	0.833	0.834
Age	44.93	45.21

Notes: This table reports descriptive statistics on firms and workers, measured one year prior to the policy reform. Panel A reports firm characteristics, including employment, average payrolls, total assets, tangible assets, total revenue, total expenses, total liabilities, retained earnings, dividends, return on assets, and share of institutional ownership. Panel B reports firm composition across sectors. Panel C reports worker characteristics, including worker earnings, age, and gender. Columns (1) and (2) report these statistics for treated and control firms, respectively. Employment is the average number of employees. Average payroll is the average worker earnings at a given firm. Total assets reflect the book value of all current, tangible, intangible, and other long-term assets. The reported book value of tangible assets, net of accumulated depreciation, serves as our measure of capital stock. Total revenues represent the sum of all revenue sources, and total expenses reflect the sum of all expenses. Total liabilities are defined as the sum of all current and long-term liabilities. Retained earnings are defined as retained earnings scaled by total assets. Dividend payouts are the dividend payments to shareholders on their stock holdings. Return on assets (ROA) is net income divided by total assets. The share of institutional ownership represents the percentage of equity held by institutional investors. Worker annual earnings are aggregated across all employers in a given year. Job transition is defined as an indicator variable equal to one if a worker's employer in a given year differs from their employer in the previous year.

Table 2: Difference-in-differences Estimates on Firms' Employment and Investment

	(1) Log(Employment)	(2) Log(Tangible Assets)
Treat × Post	0.1181***	0.0671*
	(0.0322)	(0.0384)
Adj. R Squared	0.595	0.698
Mean at t=-1 (treated)	30.217	1,687.08
Number of Observations	7,065	6,890

Notes: This table presents difference-in-differences estimates of the impact of GHG disclosure mandates on employment and tangible assets. Treated firms are defined as those with at least one facility that exceeds the reporting threshold during the year the mandate is implemented and the following year. Matched control firms are identified based on the following criteria. A firm is considered a potential control for a treated firm j if: (1) it remains below the reporting threshold throughout the sample period and thus never discloses GHG emissions, and (2) it operates in the same province, belongs to the same four-digit NAICS industry, and falls within the same decile bin of employment, total revenue, total assets, and age one year prior to the regulatory change affecting firm j. Post is an indicator variable equal to one for the year of the policy change and all subsequent years. Employment is the average number of employees. The reported book value of tangible assets, net of accumulated depreciation, serves as our measure of capital stock. All models include firm, industry-by-year, and province-by-year fixed effects. The standard errors are clustered at the firm level.

Table 3: Difference-in-differences Estimates on Firms' Revenue and Profitability

	(1) Log(Total Revenue)	(2) Log(Total Expenses)	(3) Return on Assets
Treat × Post	0.1328**	0.1296**	0.0121
	(0.0635)	(0.0526)	(0.0151)
Adj. R Squared	0.489	0.662	0.390
Mean at t=-1 (treated)	2,580.13	2,327.90	0.031
Number of Observations	6,525	6,670	7,265

Notes: This table presents difference-in-differences estimates of the impact of GHG disclosure mandates on total revenue, total expenses, and return on assets. Treated firms are defined as those with at least one facility that exceeds the reporting threshold during the year the mandate is implemented and the following year. Matched control firms are identified based on the following criteria. A firm is considered a potential control for a treated firm j if: (1) it remains below the reporting threshold throughout the sample period and thus never discloses GHG emissions, and (2) it operates in the same province, belongs to the same four-digit NAICS industry, and falls within the same decile bin of employment, total revenue, total assets, and age one year prior to the regulatory change affecting firm j. Post is an indicator variable equal to one for the year of the policy change and all subsequent years. Total revenues represent the sum of all revenue sources in a given year, and total expenses reflect the sum of all expenses incurred within the same period. Return on assets (ROA) is calculated as net income divided by total assets. All models include firm, industry-by-year, and province-by-year fixed effects. The standard errors are clustered at the firm level.

Table 4: Difference-in-differences Estimates on Worker-level Outcomes

	(1) Log(Annual Earnings)	(2) Job Transitions
Treat × Post	0.0880**	-0.0411
	(0.0381)	(0.0343)
Adj. R Squared	0.454	0.203
Mean at t = -1 (treated)	66,651.02	0
Number of Observations	563,560	567,740

Notes: This table presents difference-in-differences estimates of the impact of GHG disclosure mandates on worker-level outcomes, including annual earnings and job transitions. Treated firms are defined as those with at least one facility that exceeds the reporting threshold during the year the mandate is implemented and the following year. Matched control firms are identified based on the following criteria. A firm is considered a potential control for a treated firm j if: (1) it remains below the reporting threshold throughout the sample period and thus never discloses GHG emissions, and (2) it operates in the same province, belongs to the same four-digit NAICS industry, and falls within the same decile bin of employment, total revenue, total assets, and age one year prior to the regulatory change affecting firm j. Post is an indicator variable equal to one for the year of the policy change and all subsequent years. Worker annual earnings are aggregated across all employers in a given year. Job transition is defined as an indicator variable equal to one if a worker's employer in a given year differs from their employer in the previous year. All models include worker, industry-by-year, and province-by-year fixed effects. The standard errors are clustered at the firm and worker levels.

Table 5: Difference-in-differences Estimates on Firms' Capital Structure

	(1) Log(Total Liabilities)	(2) Retained Earnings / Total Assets	(3) Share of Institutional Ownership
Treat × Post	0.0917**	0.1219*	0.0337
	(0.0410)	(0.0740)	(0.0255)
Adj. R Squared	0.574	0.582	0.728
Mean at t=0 (treated)	2336.136	0.021	0.473
Number of Observations	7,230	7,255	7,335

Notes: This table presents difference-in-differences estimates of the impact of GHG disclosure mandates on total liabilities, retained earnings scaled by total assets, and share of institutional ownership. Treated firms are defined as those with at least one facility that exceeds the reporting threshold during the year the mandate is implemented and the following year. Matched control firms are identified based on the following criteria. A firm is considered a potential control for a treated firm j if: (1) it remains below the reporting threshold throughout the sample period and thus never discloses GHG emissions, and (2) it operates in the same province, belongs to the same four-digit NAICS industry, and falls within the same decile bin of employment, total revenue, total assets, and age one year prior to the regulatory change affecting firm j. Post is an indicator variable equal to one for the year of the policy change and all subsequent years. Total liabilities are defined as the sum of all current and long-term liabilities. The share of institutional ownership represents the percentage of a firm's equity held by institutional investors in a given year. All models include firm, industry-by-year, and province-by-year fixed effects. The standard errors are clustered at the firm level.

Table 6: By Financial Constraints based on Pre-Reform Leverage Ratio

	Log(Employment)	Log(Tangible Assets)	Log(Total Revenue)	Log(Total Liabilities)
Treat × Post × Less Constrained	0.0562	-0.0173	-0.0245	0.0516
	(0.0443)	(0.0388)	(0.0645)	(0.0404)
Treat × Post × More Constrained	0.1782***	0.1689***	0.3418**	0.1544**
	(0.0461)	(0.0589)	(0.1421)	(0.0762)
Adj. R Squared (Less Constrained)	0.545	0.670	0.310	0.546
Number of Observations (Less Constrained)	3,405	3,280	3,365	3,465
Adj. R Squared (More Constrained)	0.620	0.748	0.670	0.598
Number of Observations (More Constrained)	3,345	3,290	3,280	3,460

Notes: This table presents difference-in-differences estimates of the impact of GHG disclosure mandates on firm-level outcomes, separately for less-financially constrained firms and more-financially constrained firms. Firm-level outcomes include employment, tangible assets, total assets, and total liabilities. We define less- (more-) financially constrained firms as those with below- (above-) median pre-reform measure of leverage ratio. The leverage ratio is defined as total liabilities divided by total assets. Treated firms are defined as those with at least one facility that exceeds the reporting threshold during the year the mandate is implemented and the following year. Matched control firms are identified based on the following criteria. A firm is considered a potential control for a treated firm j if: (1) it remains below the reporting threshold throughout the sample period and thus never discloses GHG emissions, and (2) it operates in the same province, belongs to the same four-digit NAICS industry, and falls within the same decile bin of employment, total revenue, total assets, and age one year prior to the regulatory change affecting firm j. Post is an indicator variable equal to one for the year of the policy change and all subsequent years. Employment is measured as the average number of employees reported in employee tax records and Record of Employment data. The reported book value of tangible assets, net of accumulated depreciation, serves as our measure of capital stock. Total revenues represent the sum of all revenue sources in a given year. Return on assets (ROA) is calculated as net income divided by total assets. All models include firm, industry-by-year, and province-by-year fixed effects. The standard errors are clustered at the firm level.

Appendix A

Table A1: **GHG Dataset Appendix**

Panel A: Annual Breakdown of GHG Datasets			
Year	**# of reporting facilities**	**# first-time facilities**	**# first-time firms**
2004	**326**	**326**	**157**
2005	**337**	**28**	**22**
2006	345	27	30
2007	351	19	21
2008	348	15	22
2009	**537**	**180**	**80**
2010	**573**	**85**	**61**
2011	581	39	47
2012	849	293	126
2013	879	104	34
2014	907	102	30
2015	903	83	41
2016	1,058	205	109
2017	**1,699**	**1,022**	**178**
2018	**1,759**	**240**	**52**
2019	1,763	176	41
2020	1,737	124	41
2021	1,732	75	43
Total	16,684	3,143	1,135

Panel B: Sample Selection				
Treatment Period	**# of first-time firms reporters**	**# of voluntary first-time reporters**	**# of mandatory first-time reporters**	**# of firms with balance-sheet info at t = -1**
2004-2005	179	12	167	117
2009-2010	141	20	121	65
2017-2018	230	20	210	175
Total	550	52	498	357
# unique treated firms eligible for matching				357
# of treated firms in our sample				240
Match rate				0.672

Notes: This table summarizes our original GHG emission datasets under federal and provincial disclosure mandates (Panel A) and our sample selection steps (Panel B). Panel A reports the number of facilities reporting their GHG emissions (*# of reporting facilities*), the number of unique first-time facility reporters (*# of first-time facilities*), and the number of unique first-time firm reporters (*# of first-time firms*) by year. The number of first-time firm reporters is smaller than the number of first-time facility reporters because a firm can have more than one facility. Our treatment years are 2004, 2005, 2009, 2010, 2017, and 2018, as discussed in Section 4 and Figure 1. Panel B presents the steps we take to create our final sample. Specifically, we drop voluntary reports and firms without balance sheet information for the year before the regulation change, as we need this information for our matching procedure. Finally, we drop treated firms for which we could not identify a properly matched control firm.