Mandatory Greenhouse Gas Disclosure and Spillover Impacts on Corporate Investment

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Abstract

This paper studies mandatory Greenhouse Gas (GHG) emissions disclosure and its spillover impacts on firms' investment in the aggregate economy. We exploit the 2004 federal mandatory disclosure policy implemented in Canada, along with federal and provincial changes in reporting thresholds in later years, to identify the treated group as the first-time reporting firms within two years of each unanticipated reform. Following disclosure, treated firms increase capital investment and hiring relative to matched control firms below the threshold, accompanied by rises in total expenses and revenue. These expansions are financed through increased borrowing at lower interest rates and are concentrated among more financially constrained firms and more energy-efficient firms. Overall, these results are consistent with mandatory GHG disclosure reducing investor uncertainty about firms' environmental risks and lowering their cost of capital. Importantly, the disclosure policy may also affect non-disclosing firms below (but close to) the reporting threshold through information spillovers: lenders can update their beliefs about the energy efficiency of non-disclosing firms. To estimate the aggregate impacts of the disclosure policy that account for these general equilibrium effects, we build and estimate a structural model that embeds information spillovers with Bayesian updating. We find large increases in aggregate capital and reductions in emissions following the disclosure policy, driven largely by a reallocation of market share toward both disclosing energy-efficient firms and non-disclosing firms just below the reporting threshold. Our findings suggest that government-mandated environmental disclosure can induce investment by lowering firms' cost of capital despite compliance costs and that accounting for information spillovers to non-disclosing firms is crucial for accurately capturing its aggregate impacts.

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1 Introduction

Climate change and the associated environmental risks have motivated a wide range 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 to provide investors with information on environmental risks (Greenstone et al., 2023). Prior research documents that investors value climate-related disclosures because such disclosures help them better evaluate and price their exposure to environmental risks (Engle et al., 2020; Flammer et al., 2021; Alekseev et al., 2024; Ilhan et al., 2023; Giglio et al., 2025).[1] To the extent that climate-related disclosures impact how investors assess expected returns, mandatory disclosure policies can influence firms' investment and real outcomes. Importantly, disclosure policies lead to information spillovers to non-disclosing firms, which can subsequently affect their outcomes in general equilibrium. This paper examines how mandating greenhouse gas disclosure affects firms' financing, investment, and hiring decisions and allocative efficiency within and across industries, accounting for information spillovers to non-disclosing firms in the aggregate economy.

The effects of mandatory greenhouse gas (GHG) disclosures on firm outcomes are theoretically ambiguous. Accordingly, we develop a model that highlights a distinct information mechanism through which mandatory disclosure affects firms. Investors do not directly observe firms' emission efficiency and instead infer it from coarse outcomes such as sales, which confound productivity and emission technology. As a result, firms with different emission efficiencies may face similar financing conditions in the absence of disclosure. Thus, investors face uncertainty about firms' underlying emission efficiency and therefore require higher expected returns as compensation for bearing that uncertainty, increasing the firm's cost of capital (e.g., Diamond and Verrecchia, 1991). Mandatory disclosure reshapes this information friction. By fully revealing emission efficiency for disclosing firms, disclosure reduces investor uncertainty and aligns financing costs with true environmental performance, thereby affecting disclosing firms' cost of capital (Diamond and Verrecchia, 1991; Gao, 2010; Easley and O'Hara, 2004; Lambert et al., 2007). Importantly, disclosure also affects firms that do not disclose. Observing non-disclosure itself conveys information by ruling out high-emission realizations, truncating investors' posterior beliefs toward cleaner technologies and reducing uncertainty. As a result, even non-disclosing firms experience changes in their cost of capital. Because financing conditions depend on both expected exposure to environmental costs and uncertainty, disclosure generates heterogeneous investment and hiring

[1] 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 growth rate of 14 percent. Investors may also use climate-related disclosures to identify and invest in "clean" firms for non-pecuniary reasons, such as social or ethical preferences (Riedl and Smeets, 2017; Hartzmark and Sussman, 2019; Bauer et al., 2021).

responses across firms, even when they are ex ante observationally similar and operate under the same regulatory environment. Ultimately, one needs to examine this question empirically.

Despite the importance of assessing the effects of mandatory GHG disclosures on firms, 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 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 by exploiting a sequence of regulatory changes that differentially expanded disclosure requirements across firms over time in Canada. Combining this variation with firm balance sheet information from tax records covering the universe of firms from 2001 to 2021 allows us to identify the effects of mandatory GHG disclosures on firms' investment and performance. 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. Since each reform was announced only a few months before its implementation, the affected firms had little time to manipulate their emissions, making this reform plausibly exogenous for those treated within the first two years. 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 below the reporting threshold and did not disclose GHG emissions within the two years of the reform. 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 to the reform. Importantly, the majority of treated and control firms are private in our setting; therefore, being able to access their financial information is crucial for our empirical design.

We begin our analysis by examining the impacts of GHG disclosure mandates on firms' production inputs. We find that treated firms increased their employment, average payrolls, capital stock, and total expenses by 16.1 log points, 14.7 log points, 26.1 log points, and 36.3 log points respectively, after disclosing GHG emissions for the first time, relative to matched control firms that did not report their GHG emissions. Subsequently, their total revenue and returns on assets rise by 38.2 log points and 1.1 percentage points on average relative to their matched control firms.

In terms of their financing, we find that affected firms experience a 0.42-percentage points reduction in average interest rates on their long-term debts, and 21.3 log points increase in their total liabilities. These results are consistent with the mandatory GHG disclosure lowering the cost of capital for affected firms, leading to increases in their investment and sales on average relative to non-disclosing firms after the reform.

We further corroborate our findings by assessing stock market responses to the mandatory disclosure among publicly listed firms. We conduct event-study analyses around the federal reforms, where we match each listed treated firm to a listed control firm with similar market value and performance within the same sector before the event. Relative to matched control firms that do not disclose, treated firms earn cumulative abnormal returns of 5.9 percentage points within the first half month of disclosing their GHG emissions. Overall, the stock market responses are consistent with our findings that mandatory disclosure had net positive impacts on firms' investment.

Since non-disclosing firms may be also affected by disclosure mandates in general equilibrium, we use our model to simulate the investment responses and estimate the aggregate impacts of mandatory GHG disclosures on the aggregate investment, sales, and productivity accounting for potential spillover effects. Our model is calibrated to match seven data moments using the joint distribution of productivity and emission efficiency and the substitution elasticity. The model fits the main patterns in the data well, especially the negative correlation between revenue and emission efficiency and the dispersion of log revenue. We find that mandatory disclosure leads lenders to update beliefs about firms' environmental efficiency and reprice capital accordingly, producing a "green expansion" in which average capital rises by 15.05 percent while aggregate emissions fall by 6.75 percent. At the same time, the price index declines by 4.70 percent, indicating that consumers benefit as market share shifts toward cleaner, more efficient producers. An important part of this mechanism is that disclosure affects not only firms that must report, but also some firms *just below the threshold*, because lenders infer from their non-disclosure that they are cleaner.

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, reducing their information asymmetry and uncertainty. 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 performance or GHG emission levels. 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). 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 conduct two sets of tests motivated by our model. The first set examines heterogeneity based on financing constraints. If the GHG disclosure regulation affects investment by lowering the cost of capital, we would expect stronger responses among more financially constrained firms. We proxy for financial constraints using the firm's leverage ratio (total liabilities divided by total assets), building on the idea that firms with higher baseline leverage may face greater difficulties in raising external capital. Consistent with this prediction, we find that high-leverage firms exhibit larger increases in hiring and sales and a larger reduction in interest rates compared to low-leverage firms. Overall, these heterogeneous effects are consistent with the cost-of-capital channel, suggesting that GHG disclosure mitigates financing constraints.

In the second set of tests, we examine heterogeneity by firms' post-reform energy efficiency, measured as total revenue divided by total emissions in the reform year. Our model predicts stronger effects among more energy-efficient firms. Disclosure reduces investor uncertainty for all treated firms, lowering the risk premium and therefore the cost of capital for both high- and low-efficiency firms. However, disclosure also reveals which firms are relatively more efficient: more energy-efficient firms receive a favorable belief update that further lowers their cost of capital, whereas less efficient firms receive an unfavorable update that partially offsets the benefit from lower uncertainty. Consistent with these predictions, we find that more energy-efficient firms show larger increases in employment and sales and a larger reduction in interest rates compared to less energy-efficient firms. Overall, these heterogeneous effects align with the model's predictions and support the cost-of-capital and information channel.

Since we find positive effects of mandatory GHG disclosures on firm outcomes, a natural question arises: why do firms not voluntarily report GHG emissions? Firms may not voluntarily disclose GHG emissions even when such information is valuable to investors because voluntary reporting lacks the key features that make mandatory GHG disclosure most useful: standardization and credibility. First, in the absence of a common mandate, disclosures can vary widely in scope, format, and measurement methodology; by contrast, mandatory disclosure standardizes reporting and measurement, producing more comparable emissions information that lowers investors' costs of collecting and processing information (Zingales, 2009; SEC, 2024). Second, mandatory GHG disclosure is reinforced by regulatory enforcement: oversight helps verify compliance and enhances credibility by subjecting disclosures to review and potential penalties for misreporting, whereas voluntary reports are typically not subject to comparable verification, limiting their perceived reliability. Finally, measuring and reporting emissions imposes direct reporting costs that have a substantial fixed component and are therefore likely more burdensome for smaller firms that fall below the reporting threshold.

This paper makes three main contributions. First, we contribute to the growing literature evaluating the effects of mandatory GHG disclosures on firm outcomes. What sets our paper apart is the combination of a clean empirical design, detailed balance-sheet data on both private and listed firms, and a structural model that captures information spillovers to non-disclosing firms and quantifies the policy's aggregate effects. Prior research examines the impacts of GHG disclosure on firms' emissions and investment (Jouvenot and Krueger, 2019; Downar et al., 2021; Tomar, 2023; Yang et al., 2023), divestiture of polluting assets (Ecker and Keeve, 2023), cost of capital (Bolton and Kacperczyk, 2021a), and "greenwashing" (Grewal et al., 2024). For example, Jouvenot and Krueger (2019) and Downar et al. (2021) investigate the reporting regulation change in the United Kingdom, which influenced the dissemination and aggregation of existing emissions information, while Tomar (2023) examines the reporting regulation change in the United States, which targeted facilities with little 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. We advance this literature by exploiting multiple regulatory changes and comparing affected firms to matched control firms within the same country, thereby addressing potential concerns about concurrent events that may confound cross-country comparisons. Besides studying listed firms, we examine private companies in which information on GHG emissions as well as financial statements are opaque to investors. This sharply contrasts prior studies primarily focusing on listed firms whose emissions can be indirectly inferred from publicly available financial statements (Gipper et al., 2026).[2] We also extend this literature by examining the aggregate effects of mandatory disclosure and the information spillovers to non-disclosing firms using a structural model.

Second, we contribute to the large literature studying how information disclosure affects firms' cost of capital and investment (Stein, 2003; Roychowdhury et al., 2019). Prior studies also study both the real and regulatory costs of mandatory financial disclosures (Iliev, 2010; Ewens et al., 2024). What distinguishes our paper is the focus on environmental disclosures and the use of a

[2]While revealing GHG emissions may still provide new information for investors, as evidenced by stock price responses (Choy et al., 2025), investors already have a lot of information about listed firms prior to the GHG disclosure. By contrast, disclosing GHG emissions by private firms may reveal information beyond just emissions. Investors may infer the size and efficiency of disclosing private firms, and may update their beliefs about size and efficiency even for non-disclosing private firms below the regulatory cutoff (see Section 3 for details).

structural model that allows us to capture information spillovers to non-disclosing firms. Prior research also examines investors' pricing of environmental risks. These studies explore how emissions levels are priced in financial markets,[3] and how investors incorporate firms' ESG practices and GHG emissions into capital allocation decisions (Riedl and Smeets, 2017; Bauer et al., 2021; Flammer et al., 2021; Ilhan et al., 2023). While it is valuable to assess how investors respond to emissions information among disclosing firms, it neglects how investors also learn about non-disclosing firms. Accounting for information spillovers to non-disclosing firms may be important to understand the aggregate impacts of disclosure mandates on investment in the economy. Our contribution is to show that mandatory disclosure affects firms through an information channel that reshapes investor beliefs and financing conditions. Disclosure lowers the cost of capital for firms whose emission-efficiency is revealed to be high, leading to increased investment. Importantly, disclosure also affects firms that do not disclose: non-disclosure itself conveys information by truncating investor beliefs about emissions efficiency, thereby altering expected risk and uncertainty. We use our model simulations to decompose and quantify the importance of these information spillovers to non-disclosing firms when evaluating the aggregate impacts of the disclosure policy.

Third, our paper relates to the broader literature on the effects of environmental policies on firm-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, 2016; Walker, 2013; Acemoglu et al., 2016a; Shapiro and Walker, 2018; Aghion et al., 2016; Acharya et al., 2025; Chen et al., 2025; Colmer 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.

[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, while In et al. (2017), Duan et al. (2023), Lubos et al. (2022), and Zhang (2025) find that greener assets earn higher returns. Rather than studying the cross-sectional pricing of emissions, we focus on shocks to the *information environment* surrounding emissions.

2 Institutional Setting

This section provides institutional details regarding the Greenhouse Gas Reporting Program and policy changes in Canada relevant to our empirical design.

2.1 Mandatory Disclosure Policy

To develop a harmonized and efficient mandatory greenhouse gas reporting system, the federal Greenhouse Gas Reporting Program was established in 2004 by the Department of Environment and Climate Change Canada (ECCC) under the Canadian Environmental Protection Act. Starting in 2004, all facilities located in Canada emitting 100 kt or more of GHGs in CO2-equivalent per calendar year were required to report their emissions to the ECCC.[4] 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.[5] The program also encourages facilities that do not meet the reporting threshold to report voluntarily, although voluntary reporters are not audited by the ECCC. Between 2004 and 2021, about 10 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 equivalent in 2021, representing 41 percent of Canada's total GHG emissions.[6]

The program mandates that the company in charge of operating the facility at the end 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 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., chief executive officer or 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.[7]

[4]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 stationary 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, see the information here.

[5]For more details, see the information here for the 2009 change and here for the 2016 change.

[6]The total GHG emission was 698 MT CO2 eq in 2021 (see the information here for more details).

[7]For more details, see the information here.

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.[8]

The publicly available data on facilities reporting under federal regulations are considerably broader in scope than the data published by provinces. The federal GHGRP publishes more than 100 fields per facility, including the facility's name, unique ID, address, geographic coordinates, NAICS industry classification, and reporting year, as well as identifying information about both the reporting company and its parent company (e.g., name, address, business number, and percent ownership). Furthermore, the federal data contain emission levels for 23 gases, broken down by gas type, such as carbon dioxide (CO2). In contrast, Quebec publishes only 15 fields, reporting total emissions – along with a separate figure for biomass-related emissions – but without a breakdown by individual gas type and without information on the parent company. Ontario's disclosures are similarly limited, covering only 8 gas types and omitting parent company information. Thus, there is less information about GHG emissions of firms that are required to report under provincial disclosure mandates but fall below the federal threshold than firms above the federal threshold (e.g., Ontario facility with 30 kt GHG emissions in 2015 vs. Ontario facility with 60 kt in 2015).

2.2 Announcement Dates and Anticipation Concerns

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 Ontario government 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

[8] 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 for both provinces start in 2012. In email correspondences with British Columbia's Ministry of Environment and Climate Change Strategy, we learned that the absence of publicly available data for 2010 and 2011 was partly due to low awareness of reporting requirements and incomplete data during the program's early years.

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 as well as the number of unique first-time facility reporters. The total number of unique firms that report for the first time is 1,140 during our sample period.

2.3 Environmental Regulations and Transition Risks

In 2008, British Columbia implemented a revenue-neutral carbon tax policy on the purchase and use of fossil fuels 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 emitting more than 100 kilotonnes of CO2 per year to reduce their emissions by 12 percent. In 2016, Alberta introduced the Climate Leadership Implementation Act, imposing a carbon levy beginning at CA$20 per tonne in 2017 and rising to CA$30 per tonne in 2018, until it was repealed in June 2019 following a change in government. Ontario adopted a cap-and-trade system through the Climate Change Mitigation and Low-Carbon Economy Act of 2016, which took effect in January 2017 and covered large industrial emitters, fuel distributors, and electricity importers, linking with Quebec and California in 2018, before being cancelled in October 2018 after the election of a new provincial government. In 2019, the federal government implemented the Greenhouse Gas Pollution Pricing Act, establishing a national carbon pricing backstop that applied to provinces lacking an equivalent provincial system, with an initial fuel charge of CA$20 per tonne of CO2 equivalent set to increase by CA$10 per tonne annually. These environmental regulations across Canadian provinces and at the federal level raised awareness among investors to consider transition risks when deciding which Canadian firms to invest in, making carbon emissions information more valuable.

3 Model

Disclosure of GHG emissions affects firms through the information available to financial markets. Prior to a disclosure policy, investors do not observe firms' emission efficiency or emission intensity directly and instead rely on coarse outcomes such as firms' revenue when valuing firms and pricing capital. As a result, firms that differ in emission efficiency may appear observationally identical under a given regulatory regime, even though they rely on different emission technologies. The purpose of the model is to formalize how disclosure reshapes market beliefs, financing conditions, and real outcomes in this environment. We develop a framework in which sales serve as a noisy signal of both productivity and emission efficiency, while mandatory disclosure alters the inference problem faced by lenders by either fully revealing emission efficiency or truncating posterior beliefs for non-disclosing firms. By embedding disclosure, Bayesian learning, and financing friction into a standard production and demand structure, the model clarifies how the disclosure reallocates capital toward cleaner firms through information channels, even when firms are ex ante indistinguishable.

3.1 Technology and Unit Cost

We begin with a production technology that combines capital and variable input. The variable input itself is a CES aggregator which depends on regular input v, such as labor and material, and emissions e, with Hicks-neutral productivity $z > 0$ and emission efficiency $z_e > 0$:

$$q = z k^{\alpha} \left(\left[(1-\alpha_e) v^{\frac{\gamma-1}{\gamma}} + \alpha_e (z_e e)^{\frac{\gamma-1}{\gamma}} \right]^{\frac{\gamma}{\gamma-1}} \right)^{1-\alpha}, \qquad \alpha \in (0,1),\ \alpha_e \in (0,1),\ \gamma > 0.$$

The factor shares are denoted as $\alpha, \alpha_e \in (0,1)$. Regular variable input v has price $w > 0$ and emissions e face shadow cost (or tax) $t_e > 0$.

Cost minimization gives the unit variable cost index $C_w(z_e)$, where

$$C_w(z_e) = \left[(1-\alpha_e) w^{1-\gamma} + \alpha_e \left(\frac{t_e}{z_e} \right)^{1-\gamma} \right]^{\frac{1}{1-\gamma}}.$$

CES expenditure shares satisfy

$$s_e(z_e) \equiv \frac{t_e e}{t_e e + wv} = \frac{\alpha_e \left(\frac{t_e}{z_e} \right)^{1-\gamma}}{(1-\alpha_e) w^{1-\gamma} + \alpha_e \left(\frac{t_e}{z_e} \right)^{1-\gamma}} \tag{1}$$

This establishes the technological foundation: productivity and emission efficiency shape the variable cost of producing output, where

$$C(q; z, z_e) = \underbrace{\frac{u^{\alpha} C_w(z_e)^{1-\alpha}}{\alpha^{\alpha} (1-\alpha)^{1-\alpha}}}_{C(z_e)} \left(\frac{q}{z} \right)$$

3.2 Prices, Demand, and Profits

The standard CES demand system implies

$$q(z, z_e) \; = \; (P^{-1}Y)\left(\frac{p(z, z_e)}{P}\right)^{-\eta},$$

where the CES price index is

$$P \; \equiv \; \left(\int p(z, z_e)^{1-\eta}\, dG(z, z_e)\right)^{\frac{1}{1-\eta}}.$$

We normalize $Y P^{\eta-1} = 1$ in the initial equilibrium.

Sales and Profits Given the iso-elastic inverse demand: $p(q) = q^{-1/\eta}$ with $\eta > 1$. Standard constant-markup analysis implies equilibrium sales and profits:

$$y(z, z_e) \; = \; \tilde{\eta}\left(\frac{z}{C(z_e)}\right)^{\eta-1} \quad , \; \pi(z, z_e) \; = \; \eta^{-1}\tilde{\eta}\left(\frac{z}{C(z_e)}\right)^{\eta-1}$$

where $\tilde{\eta} \equiv \left(\frac{\eta-1}{\eta}\right)^{\eta-1}$.

Input Demands From cost minimization, expenditure on each input equals its cost share times total cost. Since profits are $\varpi = \frac{1}{\eta}y$, total cost is $TC = \frac{\eta-1}{\eta}y$. Capital expenditure is the share α of total cost, and variable cost is the share $1 - \alpha$.

Capital demand:

$$k(z, z_e)^* = \frac{\alpha(\eta - 1)}{\eta \cdot u} y(z, z_e)$$

Emissions demand:

$$e(z, z_e)^* = \frac{s_e(z_e)(1 - \alpha)(\eta - 1)}{\eta \cdot t_e} y(z, z_e)$$

Regular input demand:

$$v(z, z_e)^* = \frac{(1 - s_e(z_e))(1 - \alpha)(\eta - 1)}{\eta \cdot w} y(z, z_e)$$

3.3 Linear–Gaussian Learning

The model is more complicated when the public does not directly observe (z, z_e). Assume they have the unbiased joint Gaussian prior

$$(\ln z, \ln z_e) \sim \mathcal{N}\left(\begin{pmatrix} \mu_z \\ \mu_{z_e} \end{pmatrix}, \Sigma\right), \qquad \Sigma = \begin{pmatrix} \sigma_z^2 & \sigma_{z,z_e} \\ \sigma_{z,z_e} & \sigma_{z_e}^2 \end{pmatrix}.$$

Equilibrium sales satisfy

$$y(z, z_e) = \tilde{\eta}\left(\frac{z}{C(z_e)}\right)^{\eta-1}.$$

Taking logs and absorbing constants into a gives

$$\ln y = a + (\eta - 1)\ln z - (\eta - 1)\ln C(z_e).$$

To obtain a linear-Gaussian signal, we can write the first order expansion with respect to the prior mean

$$\ln C(z_e) \approx c_0 - (1-\alpha)s_e(\mu_{ze})\ln z_e,$$

so that

$$\ln y = a' + (\eta - 1)\ln z + b\ln z_e, \qquad b \equiv (\eta - 1)(1-\alpha)s_e(\mu_{z_e}). \tag{2}$$

Define the observable size signal

$$\hat{y} \equiv \ln y - a' = (\eta - 1)\ln z + b\ln z_e. \tag{3}$$

Before disclosure, the public can only observe $\hat{y}$ but not $(\ln z, \ln z_e)$. By Gaussian projection,

$$\ln z_e \mid \hat{y} \sim \mathcal{N}\left(\mu_{z_e|\hat{y}},\, \sigma^2_{z_e|\hat{y}}\right). \tag{4}$$

The conditional mean in (4), given $\mu_{\hat{y}} = (\eta - 1)\mu_z + b\mu_{z_e}$, is

$$\mu_{z_e|\hat{y}} = \mu_{z_e} + \beta^*(\hat{y} - \mu_{\hat{y}}), \qquad \beta^* \equiv \frac{Cov(\ln z_e, \hat{y})}{Var(\hat{y})} = \frac{(\eta-1)\sigma_{z,z_e} + b\sigma_{z_e}^2}{(\eta-1)^2\sigma_z^2 + b^2\sigma_{z_e}^2 + 2(\eta-1)b\sigma_{z,z_e}}.$$

The conditional variance is

$$\sigma^2_{z_e|\hat{y}} = \sigma^2_{z_e} - \frac{Cov(\ln z_e, \hat{y})^2}{Var(\hat{y})} = \sigma^2_{z_e} - \frac{\left((\eta-1)\sigma_{z,z_e} + b\sigma_{z_e}^2\right)^2}{(\eta-1)^2\sigma_z^2 + b^2\sigma_{z_e}^2 + 2(\eta-1)b\sigma_{z,z_e}}.$$

Equations (2)–(4) characterize Bayesian updating of emission efficiency under normal learning when only firm sales are observed.

We assume that the rental cost of capital depends on the conditional mean and variance of log emission efficiency to reflect a wedge of potential concern for tighter regulations:

$$r(\hat{y}) = r_0 + r_1 \mu_{z_e|\hat{y}} + r_2 \sigma^2_{z_e|\hat{y}}, \quad r_1 < 0, r_2 > 0$$

where $u(\hat{y}) = r(\hat{y}) + \delta$.

3.4 Two-Period Model

To clarify our model intuitions, we start with a two-period model, where in period 0, all firms have the same interest rate r_0 and optimally choose their production decisions. In period 1, the public lenders update their beliefs about the firm and adjust rental rate.

3.4.1 No Disclosure

Under no disclosure, the only information available for the public is each firm's period 0 output $y_0(z, z_e)$. As a result, the period 1 can be computed as $r(\hat{y}_0)$ as in section 3.3.

3.4.2 Mandatory Disclosure

When firms are mandated to disclose emissions in period 1 whenever $\ln e_0 > \ln \bar{e}$, the information environment changes discretely. In this case, firms whose realized emissions exceed the threshold must disclose $\ln e_0$ in addition to the size signal $\ln y_0$.

We can write the model emission input share as

$$s_e(z_e) \equiv \frac{\eta\, t_e}{(1-\alpha)(\eta - 1)} \cdot \frac{e_0}{y_0}.$$

Solving for emission efficiency yields the closed-form inversion:

$$z_e = \frac{t_e}{w} \left(\frac{\alpha_e}{1-\alpha_e} \cdot \frac{1 - s_e(z_e)}{s_e(z_e)} \right)^{\frac{1}{1-\gamma}}.$$

Therefore, disclosure of $\ln e_0$ together with y_0 (equivalently $\hat{y}_0$) identifies z_e. As a result,

$$Var(\ln z_e \mid \hat{y}_0, \ln e_0) = 0.$$

The lender's posterior belief collapses, and the period-1 rental rate for disclosing firms becomes

$$r_1^D(z_e) = r_0 + r_1 \ln z_e,$$

Relative to the no-disclosure case, disclosure removes the variance component of the risk premium and replaces the posterior mean with the true emission efficiency.

For non-disclosing firms, the market observes y_0 and the event $\{\ln e_0 \leq \ln \bar{e}\}$. Since y_0 is observed, this event imposes an upper bound on emission intensity and therefore on the implied share:

$$s_e(z_e) \leq \bar{s}(y_0) \equiv \frac{\eta\, t_e}{(1-\alpha)(\eta-1)} \cdot \frac{\bar{e}}{y_0}. \tag{5}$$

Because $s_e(z_e)$ is strictly decreasing in z_e, the restriction is equivalent to a lower bound on z_e:

$$\underline{z}_e(y_0) = \frac{t_e}{w}\left(\frac{\alpha_e}{1-\alpha_e} \cdot \frac{1-\bar{s}(y_0)}{\bar{s}(y_0)}\right)^{\frac{1}{1-\gamma}}$$

Recall the *pre-truncation* posterior from the learning block: $\mu_{z_e|\hat{y}_0}$ and $\sigma^2_{z_e|\hat{y}_0}$. Non-disclosure adds the constraint $\ln z_e \geq \ln \underline{z}_e(y_0)$.

Let κ denote the standardized cutoff and let $\phi(\cdot)$ and $\Phi(\cdot)$ be the standard normal pdf and cdf:

$$\kappa \equiv \frac{\ln \underline{z}_e(y_0) - \mu_{z_e|\hat{y}_0}}{\sigma_{z_e|\hat{y}_0}}, \qquad \lambda(\kappa) \equiv \frac{\phi(\kappa)}{1-\Phi(\kappa)}.$$

Then the *truncated* posterior mean and variance are

$$\mu^{ND}_{z_e|\hat{y}_0} = \mu_{z_e|\hat{y}_0} + \sigma_{z_e|\hat{y}_0}\lambda(\kappa),$$
$$(\sigma^{ND}_{z_e|\hat{y}_0})^2 = (\sigma_{z_e|\hat{y}_0})^2\left(1 + \kappa\,\lambda(\kappa) - \lambda(\kappa)^2\right).$$

These expressions reflect that non-disclosure shifts beliefs toward cleaner firms and reduces uncertainty, but does not fully reveal z_e.

Accordingly, the period-1 rental rate for non-disclosing firms is

$$r_1^{ND}(\hat{y}_0) = r_0 + r_1\mu^{ND}_{z_e|\hat{y}_0} + r_2(\sigma^{ND}_{z_e|\hat{y}_0})^2. \tag{6}$$

3.5 Industry Equilibrium

Firms are heterogeneous in productivity z and emission efficiency z_e, drawn from a joint log-normal distribution $G(z, z_e)$. Given total industry expenditure Y and factor prices (w, r_0), a two-period equilibrium consists of:

1. *Period-0 allocations* $\{k_0(z, z_e), v_0(z, z_e), e_0(z, z_e), y_0(z, z_e)\}$ such that each firm maximizes profits given the initial cost of capital $u_0 = r_0 + \delta$.

2. *Disclosure status* determined by the threshold rule: firm (z, z_e) discloses if and only if $e_0(z, z_e) > \bar{e}$.

3. *Period-1 beliefs* updated according to Bayesian learning, with truncation for non-disclosing firms.

4. *Period-1 cost of capital* given by:

$$u_1(z, z_e) = \begin{cases} r_0 + r_1 \ln z_e + \delta & \text{if } e_0(z, z_e) > \bar{e} \\ r_0 + r_1 \mu^{ND}_{z_e|\hat{y}_0} + r_2 (\sigma^{ND}_{z_e|\hat{y}_0})^2 + \delta & \text{if } e_0(z, z_e) \leq \bar{e} \end{cases} \tag{7}$$

5. *Period-1 allocations* $\{k_1(z, z_e), v_1(z, z_e), e_1(z, z_e), y_1(z, z_e)\}$ such that each firm maximizes profits given $u_1(z, z_e)$.

6. *Price index* satisfies:

$$P_t = \left[\int p_t(z, z_e)^{1-\eta} \, dG(z, z_e) \right]^{\frac{1}{1-\eta}}, \quad t \in \{0, 1\} \tag{8}$$

The model isolates disclosure as the central informational channel through which environmental regulation affects firm outcomes. When emission efficiency is unobserved, financial markets infer it imperfectly from sales, conflating productivity and cleanliness. Mandatory disclosure changes this inference problem: disclosure collapses uncertainty for emitting firms, while non-disclosure truncates beliefs toward cleaner types. These belief updates translate directly into heterogeneous costs of capital, leading otherwise ex-ante similar firms to diverge in investment, output, and emissions. Regulation thus reallocates capital not only through production costs, but by reshaping what markets learn about firms' underlying emission efficiency.

Discussion and Extension Our baseline model treats disclosure as exogenous, but in practice firms can endogenously select into disclosure by adjusting their input choices. In particular, firms whose unconstrained emissions $e^*(z, z_e)$ exceed the threshold $\bar{e}$ may choose to distort production and cap emissions at $\bar{e}$ in order to avoid disclosure. This introduces a trade-off between financial and real distortions. By avoiding disclosure, firms pool with other non-disclosers and face a capital user cost based on truncated beliefs, u^{ND}, rather than the type-specific cost $u^D(z_e)$. For firms with low emission efficiency z_e, disclosure reveals their "dirtiness," leading to a high capital cost $u^D(z_e)$. In contrast, non-disclosure allows these firms to benefit from more favorable financing conditions through pooling. This constitutes the *financial benefit of capping emissions*.

However, avoiding disclosure requires setting emissions at $e = \bar{e}$, which constrains the firm's input mix. This effectively removes emissions as a flexible input and induces decreasing returns to the remaining inputs. The resulting distortion reduces output and profits relative to the unconstrained optimum. Importantly, the magnitude of this distortion is increasing in firm productivity z. Thus, high-productivity firms incur a larger real cost from capping emissions.

Combining these forces, the model would predict a selection pattern in (z, z_e) space. Among firms whose unconstrained emissions exceed the threshold, relatively dirty and low-to-moderate productivity firms optimally distort their inputs to avoid disclosure, as the financial benefit from pooling dominates the real distortion cost. In contrast, highly productive firms—regardless of their emission efficiency—choose to disclose, since the output losses from constraining emissions outweigh any financing advantage from pooling. Clean firms (high z_e) also disclose, as their type-specific capital cost $u^D(z_e)$ is already favorable.

Overall, allowing for endogenous disclosure introduces a margin of strategic input distortion, which attenuates the allocative effects of disclosure policies. While disclosure still reallocates capital toward cleaner firms, it also induces some dirty firms to inefficiently reduce emissions in order to remain pooled, generating a misallocation of inputs within the economy. In equilibrium, the set of non-disclosing firms is not simply those below the emissions threshold, but includes a subset of firms that endogenously bunch at $\bar{e}$, with the extent of bunching decreasing in productivity. A possible extension of our model accounts for this endogenous disclosure and would predict more muted spillover effects on non-disclosing firms in general equilibrium (in progress).

4 Data and Outcome Variables

In this section, we describe our main datasets. The first dataset consists of disclosure information on GHG at the establishment level, which we aggregate at the firm level using unique business numbers. The second dataset is based on corporate tax returns with detailed firm balance-sheet information. We merge these two datasets at the firm level using a unique business number.

4.1 Greenhouse Gas Emission Disclosure Data

We obtain our emission data from the ECCC, which publishes facility-level GHG emissions on Canada's Open Government website. 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 firm balance sheet data. 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 the GHG dataset with the balance sheet data.[9]

Moreover, we obtain GHG emission data for facilities reporting under province-level regulations from the respective provincial webpage. The GHG datasets for facilities in Ontario, in British Columbia and in Quebec are from the Ontario Data Catalogue, from the Government of British Columbia website, and from the Quebec open data portal, respectively. Following the procedure described above, Statistics Canada merged these datasets with the firm balance sheet data.

4.2 Firm Balance Sheet Data

For our analyses, we use the Canadian Employer-Employee Dynamics Database (CEEDD), a matched employee-employer dataset that covers the universe of firms and workers 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 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.

[9] 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. 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. We first use 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 firm balance sheet data. If no match is found, we then use the company name, province, city, and postal code from the GHG dataset identify the corresponding company.

The main outcome variables used in the firm-level analysis are employment, average payrolls, tangible assets, total expenses, total revenue, returns on assets, interest rates, and total liabilities. Employment is defined as the average number of employees reported in the T4s. Average payrolls are defined as total payrolls divided by the number of workers. We use the reported book value of tangible assets, net of depreciation, as a measure of capital stock. 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. Interest rates are computed as total interest and bank charges divided by total liabilities, which capture how much a company is paying in terms of interest payments per dollar of debts in a given year.

All outcome variables are winsorized at the fifth and ninety-fifth percentiles following the disclosure rules by Statistics Canada.

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 are 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 GHG 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.[10] However, we include firms of which at least one facility emitted more than 100 kt GHG in 2006, 2007, or 2008 as part of potential matched control firms. 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 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.[11]

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.

[10] In Appendix B, we show results from robustness tests where we define treated firms using only the first year of the reform (instead of two consecutive years).

[11] If some of these matched control firms include those that disclose GHG emissions in later years, then our difference-in-differences estimates would be attenuated, assuming that these firms would be similarly impacted by the reform as treated firms.

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. Similar to the 2004 reform, we include firms of which at least one facility emitted more than 50 kt GHG from 2011 to 2016 as part of potential matched control firms.

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 include firms of which at least one facility emitted more than 10 kt GHG in 2019, 2020, or 2021 as part of potential matched control firms.

For the firms treated by the reform in 2010 Ontario, we include them as part of the treated group in the 2009 federal reform. However, we drop firms treated by the 2007 Quebec reform and the 2010 British Columbia reform because the GHG data for these provinces became available only starting in 2012. Given the long lag between the reform year and the first year the GHG data become publicly available, we exclude treated firms in these provinces to mitigate concerns about anticipation effects. No firm is treated for the first time by the 2016 Ontario reform. Lastly, we drop a small share of firms whose facilities emitted below the regulation threshold, but who voluntarily disclose this information. In Section 7.3, we discuss why there is a limited number of voluntary-disclosing firms in the data.

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:

$$Y_{jt} = \sum_{k=-4}^{4} \beta_k \mathbb{1}(t_j = t^* + k) \times Treat_j + \tau_t + \psi_j + \phi_{st} + \pi_{pt} + u_{jt} \tag{9}$$

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. Additionally, we include industry by year fixed effects (ϕ_{st}) and province by year fixed effects (π_{st}). The standard errors are clustered at the firm level. We use control firms that are below the threshold and, therefore, do not disclose their GHG emissions within two years of the reform. To ensure that control firms are comparable to our treated firms, we match them based on their sizes and industry (see 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.

The key identifying assumption is that outcomes for treated firms and control firms 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 to those of control firms in the years prior to the policy change. Parallel pre-trends in firm-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 (e.g., manufacturing) 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 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

We describe our sample selection and construction of our matched sample. We first start with the full sample of firms (1,140) that disclose GHG emission information in any given year between 2004 and 2021. We merge these firms with our firm balance sheet data and drop roughly 1 percent of these firms that did not file corporate tax returns. Then we drop about 10 percent of these firms as they voluntarily disclose this information (i.e., their emission is below the regulatory threshold).

Before performing a matching procedure between treated firms and potential control firms, we make the following restrictions. First, among roughly 1,000 firms that become required to disclose GHG emissions and with balance sheets information, we drop about 34 percent of these firms that have missing observations for the key variables used for matching, such as total assets, total revenue, average payrolls, profit margins (total revenue minus total expenses divided by total revenue), and listing status measured one year prior to the reform. As explained in the previous section, we focus on firms that become mandated to disclose GHG emissions within the first two years of the reform, so we drop additional 35 percent of these firms that become mandated to report after 2 years of the reform, leaving us with roughly 310 treated firms eligible for matching.

We then match each treated firm with a control firm in the same 4-digit NAICS industry in the year prior to reform. A firm is a potential control firm for a treated firm j if: (1) the firm is below the reporting threshold (and therefore does not disclose GHG emissions) within the first two years of the reform, and (2) the firm is in the same 12-quantile bins of total revenue and 6-quantile bins of profit margins in the year prior to the reform of firm j.[12] 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 total assets, total revenue, average payrolls, profit margins, employment, 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 87 percent of treated firms eligible for matching. 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.

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.[13] On average, treated firms are larger than their matched control firms, in terms of their sizes and total liabilities. While we match them based on size bins, treated firms in the *highest* quantiles are larger than their counterparts in the control group, leading to noticeable differences between the two groups *on average*. 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 their sizes, but also with regard to capital structures and profitability, prior to the reform. The dominant sectors are mining, oil and gas extraction, and manufacturing (about three quarters of our firm sample).

[12] 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 variations in matching can be provided upon request.

[13] There are 250 unique control firms and 270 unique treated firms in our sample. This difference arises mainly because a given control firm can be matched to treated firms in more than one reform. For example, a control firm may be matched to one treated firm in the 2004 reform and to a different treated firm in the 2009 reform.

6 Results

This section reports estimation results from the difference-in-differences models in Section 5 and presents additional tests supporting the interpretations of the results.

6.1 Employment and Capital

We first examine the impacts of the GHG disclosure mandates on firms' hiring and investment decisions. Figure 2 plots the event-study estimates of β_k from Equation (9) for employment, average payrolls, capital stock, and total expenses. Across panels (a)–(d), treated and control firms exhibit similar trends during the pre-reform period. Following the implementation of the mandates, we observe significant increases in these outcomes for treated firms relative to matched control firms, consistent with mandatory GHG disclosure increasing demand for labor and capital as well as raising expenses that accompany expansion, such as higher material inputs, payroll-related costs, and other costs needed to support business expansions.

Table 2 reports the corresponding difference-in-differences estimates. Relative to matched control firms, treated firms increase employment by 16.1 log points, average payroll by 14.7 log points, tangible assets by 26.1 log points, and total expenses by 36.3 log points after the policy. Overall, these results suggest that mandatory GHG disclosure leads to economically large increases in firm hiring and investment for treated firms after the reform.

6.2 Sales, Profitability, and Capital Structure

Next, we examine whether the increases in hiring and investment translate into higher sales and profitability for treated firms. Figure 3 plots the event-study estimates of β_k from Equation (9) for total revenue and return on assets (ROA). In both panels (a) and (b), treated and control firms exhibit similar trends in the pre-reform period. Following the implementation of the mandates, we observe a significant increase in total revenue for treated firms relative to matched control firms.

Table 3 reports the corresponding difference-in-differences estimates. Column (1) implies that, relative to matched controls, treated firms increase total revenue by 38.2 log points in the post period. Column (2) shows that treated firms experience a 1.1 percentage-point increase in ROA (albeit statistically not significant). Overall, these results suggest that mandatory GHG disclosure is associated with economically large increases in revenue and profitability.

One empirically testable prediction from the model is that we would expect to see changes in external financing (e.g., debts) and interest rates. Specifically, to the extent that higher-quality emission information reduces information friction, we would expect treated firms to more easily raise capital at lower costs, which would lead to an increase in debt financing (via borrowing from banks). We test this prediction and report results in Figure 3 and Table 3. Panels (c) and (d) of Figure 3 plot the event-study estimates. Treated and control firms exhibit similar pre-reform trends, but after the policy change, we observe an increase in total liabilities for treated firms relative to matched control firms. We also observe a statistically significant decline in interest rates. Columns (3) and (4) of Table 3 show that total liabilities increase by 21.3 log points and interest rates decrease by 0.42 percentage points for treated firms relative to control firms, on average, after becoming mandated to disclose GHG emissions. These results are consistent with the predictions from our model that disclosing GHG emissions can result in an overall reduction in the cost of capital, leading treated firms (mostly private) to increase their debt financing.

6.3 Stock Market Reaction

We corroborate our findings so far by assessing stock market responses to the mandatory disclosure of GHG emissions. Our main results are based on a pooled sample of private and publicly listed firms. For the subset of publicly listed treated firms, we can additionally observe how investors react to the mandatory disclosure of GHG emissions. We conduct event study analyses around the disclosure dates of the 2004, 2009, and 2017 federal reforms, matching each listed treated firm to a comparable listed control firm and measuring cumulative abnormal returns (CARs) based on a market model. We summarize the main findings here and report the sample construction, the estimating equation, the table of results, and the event-study figures in Appendix C.

Table C2 shows positive and significant market responses to mandatory disclosure. Relative to their matched controls, treated firms earn cumulative abnormal returns of 1.7 percentage points over the $[0, 2]$ window and 5.9 percentage points over the $[0, 14]$ window. Consistent with the efficiency prediction of our model, the reaction is concentrated among high energy-efficiency firms. Figure C1 plots the difference in CARs between treated firms and control firms over event time: the difference is flat before the disclosure date, consistent with parallel pre-trends, sharply increases after the event, and persists without reversing in the medium run. These stock market responses are consistent with the decline in interest rates and the increases in investment that we document among our treated firms, which include both private and publicly listed firms. The fact that investors positively react to the mandatory GHG disclosure reinforces our interpretation that mandatory disclosure lowers the cost of capital and facilitates investment.

6.4 Quantitative Model Predictions

While our results so far suggest that the policy led to positive outcomes for disclosing firms relative to their matched control group, our difference-in-differences estimates are potentially biased due to information spillovers that could positively affect some of our control firms just below the threshold. While we do a robustness test in Appendix B where we drop these non-disclosing firms (below, but close to the threshold partially affected by the policy) to address this bias, we need to account for these information spillovers in order to estimate the aggregate impacts of the policy on investment, emissions, and market share in the economy. Next, we turn to our model simulations to test how the disclosure mandates affect the aggregate capital, emissions, and market share in the aggregate economy, accounting for information spillovers and other general equilibrium effects.

Panel A of Table 4 reports the seven data moments targeted by the method of moments estimation alongside the model-simulated counterparts. The estimation identifies four key parameters that govern the relationship between observed firm size and their emission efficiency: σ_z^2, $\sigma_{z_e}^2$, σ_{z,z_e}, and γ. Revenue y moments pin down the productivity variance σ_z^2, while e/y moments of disclosing firms discipline the emission-efficiency variance $\sigma_{z_e}^2$ and the substitution elasticity γ. We pick the model's disclosure threshold such that disclosing firms account for the same share of total sales as in the data. The correlation between $\ln y$ and $\ln(e/y)$ among disclosing firms identifies the covariance σ_{z,z_e}. Note that our model simulation, similar to that in the data, accounts for the selection of disclosing firms when we calculate this moment.

The model matches the revenue-emissions correlation closely and also reproduces the log revenue dispersion well. The only discrepancy lies in the e/y distribution among disclosing firms, where the model compresses the right tail (p90−p50 of 2.41 versus 3.70 in the data) while simultaneously overstating the left tail in magnitude, and understates overall dispersion (Std of 1.96 versus 2.60). This reflects the selection inherent in the disclosure threshold $\bar{e}$, which truncates the observed sample. The data's e/y distribution among disclosing firms is right-skewed, and the truncated lognormal parametric structure produces a shape that cannot fully capture that skewness with only four free parameters. An implication for the policy counterfactual is that the compressed e/y dispersion understates heterogeneity in emission intensity, so the reallocation effects reported in Table 4 Panel B are likely conservative.

Panel B of Table 4 reports aggregate outcomes before and after the mandatory disclosure regulation. In period 0, all firms face a common cost of capital; in period 1, the market has observed the output for all firms and the emissions for disclosing firms. Lenders then update beliefs for all firms, set their interest rate, and reallocate capital toward emission-efficient producers. Several features of the results stand out. First, the disclosure threshold selects high-emission firms which, despite the negative correlation between productivity and emission intensity, tend to be the largest producers. Second, disclosure generates a "green expansion": average capital rises by 15.05 percent as the resolution of uncertainty lowers risk premia for all firms by more than it raises costs for dirty ones, while average emissions fall by 6.75 percent. The reallocation operates almost entirely through the capital margin, consistent with disclosure affecting the cost of capital in our empirical analysis. Third, the regulation reshuffles market share across firms. The 4.70 percent decline in the price index confirms that this reallocation favors energy-efficient producers, benefiting consumers.

Information Spillover to Non-disclosing Firms A novel feature of our model is that lenders update their beliefs not only about disclosing firms, but also about *non-disclosing* firms. Figure 4 illustrates this mechanism. We divide firms into four groups. First, note that the firms in the lower-right region (both red and blue) are those that lie above the disclosure threshold and therefore become subject to mandatory disclosure. As already shown in Table 4, the blue region consists of firms that are both highly productive and relatively emission-efficient. These firms benefit from mandatory disclosure because the policy allows lenders to recognize their favorable fundamentals more precisely. By contrast, firms in the yellow region lose, since disclosure reveals that they are relatively emission-inefficient, worsening lenders' assessments and reducing their market share.

Notably, firms in the green region—although not themselves subject to mandatory disclosure—also expand their market share. The reason is that lenders learn from the selection rule induced by the disclosure policy. Conditional on observed sales, firms that remain right below the disclosure threshold are inferred to be relatively emission-efficient, since otherwise they would have been more likely to fall into the disclosing group. These firms therefore benefit from an information spillover: even without directly revealing their own emissions, they are re-evaluated more favorably by lenders. This indirect effect further reinforces market share reallocation toward cleaner and more productive producers. As Figure 4 suggests, this spillover to non-disclosing firms can be quantitatively important and, in some parameterizations, may be a central driver of the aggregate allocative-efficiency gains generated by mandatory disclosure.

Aggregate Implications To understand the aggregate responses to the policy, we decompose the changes in sales share, capital, and emissions into contributions by firm group. Figure 4 presents this decomposition. Each firm's emissions are proportional to the product of its emission intensity $s_e(z_{e,i})$ and its output y_i. Because output depends on the user cost of capital u_i, the policy-induced change in emissions for each firm traces back to a change in its cost of capital, Δu_i. There are three distinctive forces. First, the removal of the risk premium that lenders charge under pooling, which lowers the cost of capital for all firms. This channel is visible from Panel (c) of Figure 4 where all group's capital stock go up in varying degrees. However, this is not to be misunderstood as all firm's sales grow: Panel (b) makes it clear that the cleaner firms gain a significant amount of market share from the less energy-efficient disclosing firms as well as firms which do not disclose but are far from the threshold. Second, the revelation of the firm's true emission efficiency upon disclosure raises the cost of capital for less energy-efficient firms, but lowers it for cleaner firms. Third, the updating of lenders' belief about non-disclosing firms via a truncation effect lowers the cost of capital by a magnitude that depends on their distance from the disclosure threshold. The changes in aggregate emissions is then a weighted sum of firm-level responses, where each firm's weight is its initial share in total emissions.

As Panel (d) of Figure 4 shows, mandatory disclosure reduces aggregate emissions by 6.75 percent, driven almost entirely by firms that lose their market share from the policy. More importantly, disclosure breaks the "pooling" of interest rates by linking each firm's cost of capital to its true or inferred emission efficiency z_e. Firms that gain are more emission-efficient (high z_e): they get cheaper capital and expand, but add almost no emissions because their $s_e(z_e)$ is low. Firms that do not gain are less emission-efficient (low z_e): their cost of capital barely falls or rises, they lose market share, and their contraction removes a large amount of emissions because their $s_e(z_e)$ is high. This asymmetry is the key driver of the reduction in aggregate emission intensity: the same characteristic that causes a firm to lose from the policy (low z_e) also makes it a heavy emitter (high s_e) initially.

Taken together, the results from our model simulations suggest that accounting for information spillovers from the mandatory disclosure policy is crucial for understanding the aggregate impacts of this policy on investment, reallocation, and emissions in the economy. In the next section, we explore potential mechanisms behind our reduced-form findings and model predictions.

7 Economic Interpretations and Potential Mechanisms

In this section, we explore potential mechanisms behind the responses of firms affected by the mandatory 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 information friction and thereby 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., when the disclosed emission is higher than the expected emission level). Given that it is difficult to measure investors' expectations 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 treated firm increases hiring and investment after disclosing GHG emissions, we can infer that the disclosure led to a net reduction in the cost of capital by reducing information friction for a given quality of the signal on average.

A testable prediction from our model is that we would expect larger responses among firms that were more financially constrained prior to the reform. To proxy for financial constraints, we use the firm's leverage ratio (total liabilities divided by total assets), building on the idea that firms with higher baseline leverage may face greater difficulties in raising external capital. We split the sample at the median leverage ratio into high- and low-leverage firms and estimate the difference-in-differences model separately for each subsample. Table 5 reports the results. The point estimates for employment and average payroll are larger among high-leverage firms (22.9 and 25.8 log points, respectively) than among low-leverage firms (10.0 and 6.6 log points), and only the high-leverage estimates are statistically significant. Tangible assets increase for both groups, though the point estimate is larger for high-leverage firms. While the differences across the subsample are suggestive of a stronger response among more financially constrained firms, we note that the sample sizes in each subsample are small and the difference in the coefficient estimates is not statistically different from zero. Nonetheless, the pattern of point estimates is consistent with the cost-of-capital channel, in which reducing information friction has a larger impact on firms that are more dependent on external financing.

7.2 Heterogeneity by Energy Efficiency

Next, we examine whether we find stronger responses among treated firms that are ex-post more energy efficient. As predicted by our model in Section 3, we would expect to find larger responses among more energy-efficient firms as they would experience a larger reduction in the cost of capital. By contrast, affected firms that are ex-post less energy efficient may experience a smaller reduction (and possibly an increase) in the cost of capital as investors update their beliefs about these firms after the mandatory disclosure.

To test whether we find different responses based on energy efficiency, we first define energy efficiency as total revenue divided by total emissions in a given year, i.e., a more energy-efficient firm would generate more sales relative to their emission in a year. To ensure that we are comparing energy efficiencies among similarly productive firms, we first create tercile bins in firms' profitability measured in the reform year. Then within each profitability bin, we create tercile bins in firms' energy efficiency measured in the reform year and classify high energy-efficient firms as those in the top tercile and low energy-efficient firms as those in the bottom tercile. In this way, we compare more energy-efficient firms with less energy-efficient firms within each productivity bin (i.e., holding fixed firms' profitability). Even though the energy efficiency is measured in the year of the reform, this measure is not likely to be vulnerable to the issue of "endogenous to the treatment" given that firms had little time to manipulate their emission prior to reporting it in the first year of the reform, as discussed in section 2. Furthermore, we observe firms' emissions only after the mandatory disclosure, so we can create this measure only after the reform.

We split the sample into high- and low-energy-efficiency firms and estimate the difference-in-differences model separately for each subsample. Table 6 shows the estimates for the key firm-level outcomes separately for high energy-efficient firms and for low-energy efficient firms. The point estimates are consistently larger for more energy-efficient firms across all outcomes: tangible assets increase by 31.4 log points (statistically significant at the 5 percent level) for more efficient firms compared to 15.2 log points (not statistically significant) for less efficient firms, and interest rates decline by 0.75 percentage points (significant) for more efficient firms compared to 0.12 percentage points (not significant) for less efficient firms. While the pattern of point estimates is suggestive, the sample sizes in each subsample are small and the difference in the coefficient estimates is not statistically different from zero. Nonetheless, the direction of the differences is consistent with a mechanism in which mandatory disclosure reduces information friction about firms' emissions, lowers the cost of capital more for high energy-efficiency firms, and thereby facilitates their growth, while generating weaker responses among lower energy-efficiency firms.

Figure 7 shows how energy efficiencies for treated firms evolve over time, separated by the reform cohorts. The first (reform) year is normalized to be zero for firms across different cohorts. As discussed above, energy efficiencies are measured in thousand Canadian dollars in total revenue per kilo tonnes of GHG emissions; for example, the energy efficiency of one means that the firm generates a thousand dollars in total revenue per kt of GHG emissions. Then we take the log of this measure so that any difference is in terms of log points. To be part of this sample, firms have to continue to report their emission; if they stop reporting it, that means either they shut down or they are below the current threshold and their energy efficiency cannot be measured in that year. Panel A shows the changes in energy efficiency over time for all mandatorily disclosing firms and Panel B shows the same for those in our matched sample. In both figures, we observe similar patterns. For the first cohort treated by the 2004 reform, we see that the energy efficiency gradually increases over time after the reform, until it peaks after 15 years. For the second cohort treated firms, we see faster increases in their energy efficiencies. For the last treated cohort, we do not see much changes given that we only have 4 years of emission data after the 2017 reform. Overall, this figure shows that treated firms that were mandated to report their GHG emission gradually improve their energy efficiency over time, consistent with the idea that once they disclose emissions, firms have incentives to improve efficiencies to reduce their cost of capital.

7.3 Why Firms Do Not Voluntarily Disclose GHG Emissions

In our sample, the share of firms that voluntarily report their GHG emissions is small (around 10 percent). 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 despite its potential net benefits. We next discuss four key reasons why firms may not voluntarily disclose GHG emissions.

First, one major benefit for disclosure regulations is their comparability. Firms disclosing under the same regulation provide more comparable reports, making it easier and less costly for investors and stakeholders to collect and interpret information and to compare firms (Zingales, 2009; Leuz and Wysocki, 2016), 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 are highly costly for firms, even if all firms are willing to provide this information voluntarily.

Second, another benefit of mandatory GHG disclosure is regulatory enforcement. Regulatory oversight helps verify compliance and enhances credibility by subjecting disclosures to review and potential penalties for misreporting. As noted in Section 2, compliance with the reporting program is mandatory: the ECCC reviews reports for completeness and adherence to reporting requirements, flags potential errors, and imposes significant penalties for non-compliance (e.g., up to three years of imprisonment for managers who commit the offense). By contrast, voluntary disclosures are not subject to verification or enforcement, which can limit their credibility and usefulness to investors. While third-party assurance of firm-disclosed GHG emissions can improve the quality of voluntary disclosures, it still remains an imperfect substitute for regulatory enforcement. Assurance is heterogeneous and subject to considerable discretion: during our sample period, no common standard governed assurance of emission levels, and different providers applied a variety of standards, levels of rigor, and scopes of coverage. As a result, firms retain substantial discretion over the quality and standard of assurance they obtain and which metrics to apply (Gipper et al., 2025, 2026), limiting the credibility of the resulting disclosures. Accordingly, third-party assurance of emissions remains uncommon even among large firms in recent years. For example, Gipper et al. (2025, Table 1) find that, among S&P 500 firms issuing an ESG report between 2010 and 2020, only about 33 percent of them obtain assurance on their Scope 1 GHG emissions.

Third, 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 energy efficiency *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.

Fourth, disclosures can have direct and proprietary costs. GHG disclosures impose significant direct compliance costs on firms. Such costs include those 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 information such as sales and profitability, can help competitors identify profitable investment opportunities, which can, in turn, affect entry and sales of competitors at the expense of the disclosing company. In addition, competitors might get insights into the disclosing firm's manufacturing processes from the breakdown of gases consisting of a company's emissions.[14]

7.4 Other Potential Mechanisms

While our results seem most consistent with the information friction and the cost-of-capital channel, another mechanism can potentially explain 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 examine whether treated firms increased their investment in green technology using a dataset on investment across different asset classes. 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. While we find increases in green-technology investment for treated firms relative to matched control firms in our sample, the magnitude is economically small and statistically indistinguishable from zero, mainly because the share of firms that invest in green technology is small in our matched sample.

8 Conclusion

In this paper, we study the effects of mandatory GHG disclosure on firms' investment, hiring, and capital structure. Specifically, we exploit a series of regulatory disclosure changes that affected different groups of firms over time in Canada, and the administrative data to identify firms that were above the reporting 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 profitability bins within the same industry, which were below the reporting threshold and, therefore, did not disclose GHG emissions.

Using a difference-in-differences design, we find that treated firms significantly increase employment and capital stock after the reform relative to their matched control firms. Subsequently, treated firms exhibit increases in sales and profitability. Overall, our results suggest that mandatory

[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, see the information here.

disclosure of GHG emissions affects corporate investment by providing investors with relevant information to assess disclosing firms' environmental risks, thereby reducing their expected returns and firms' cost of capital. Consistent with a lower cost of capital, we find that treated firms experience an increase in debt financing and a reduction in interest rates.

To further explore this cost-of-capital mechanism, we conduct two sets of heterogeneity tests. First, we use the firm's leverage ratio as a proxy for financing constraints and find that the effects of mandatory GHG disclosure are stronger for high-leverage firms than for low-leverage firms, consistent with the idea that disclosing GHG emissions can reduce financing constraints, with larger effects for firms facing greater constraints during the pre-disclosure regulation period. Second, we examine heterogeneity by firms' post-reform energy efficiency, measured as total revenue divided by total emissions in the reform year. Absent GHG disclosure, investors do not directly observe firms' emission efficiency and may therefore price the environmental risks of firms with different emission efficiencies similarly. However, because GHG disclosures reveal which firms are relatively more or less energy-efficient, investors may update their beliefs favorably for high-efficiency firms and unfavorably for low-efficiency firms, resulting in lower (higher) expected returns for high-efficiency (low-efficiency) firms. Consistent with these predictions, we find that the point estimates on tangible assets and interest rates are larger for more energy-efficient firms than for less energy-efficient firms. Overall, the pattern of results is directionally consistent with the cost-of-capital channel.

Importantly, since non-disclosing firms may be also affected by disclosure mandates in general equilibrium, we use our model to simulate the investment responses and estimate the aggregate impacts of mandatory GHG disclosures on the aggregate investment, sales, and productivity accounting for potential spillover effects. We find that mandatory disclosure leads lenders to update beliefs about firms' environmental efficiency and reprice capital accordingly, producing a "green expansion". A novel part of this mechanism is that disclosure affects not only firms that must report, but also some firms *just below the threshold*, because lenders infer from their non-disclosure that they are cleaner.

Our results indicate that disclosing GHG emissions not only benefits investors in their capital allocation and hedging decisions, but also facilitates firm 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.

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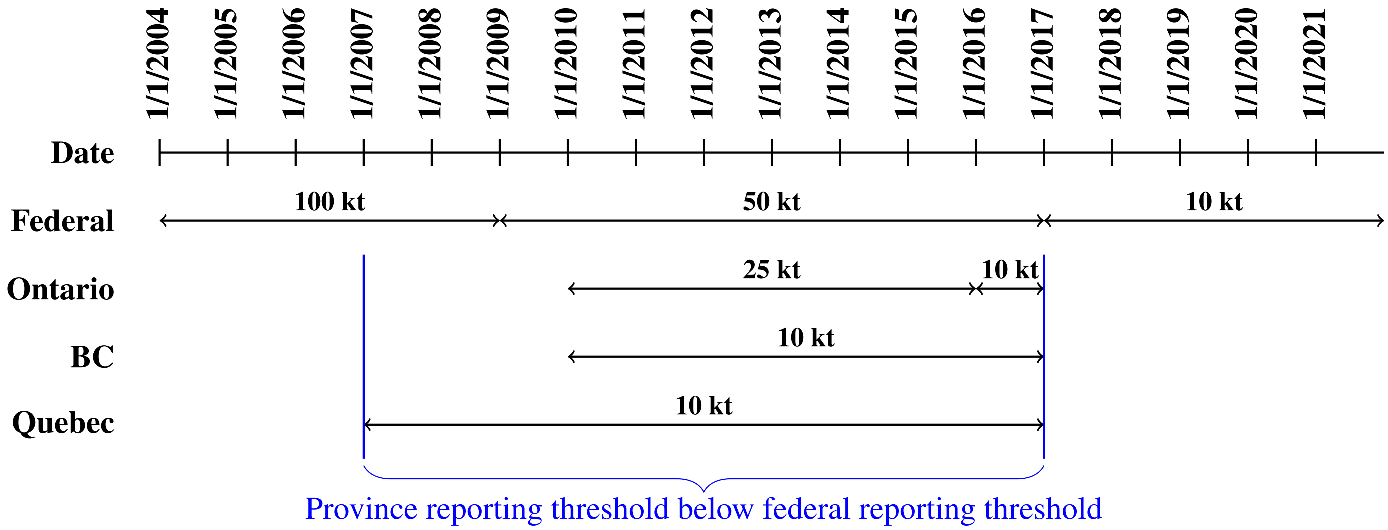
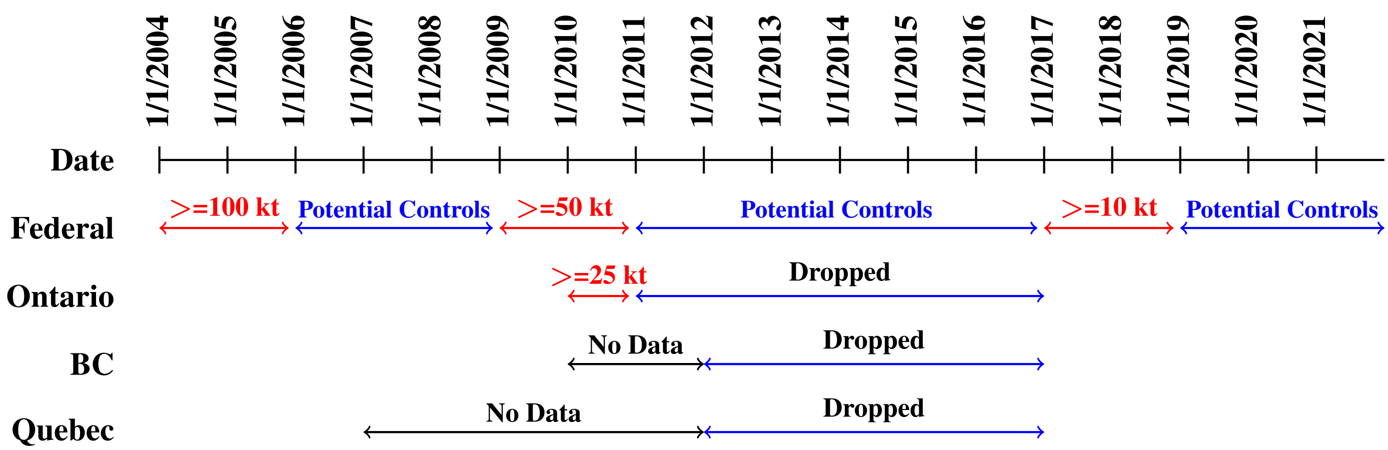
Figure 1: **Changes in Reporting Threshold over time and Policy Variation**

(a) Changes in GHG Reporting Threshold Over Time

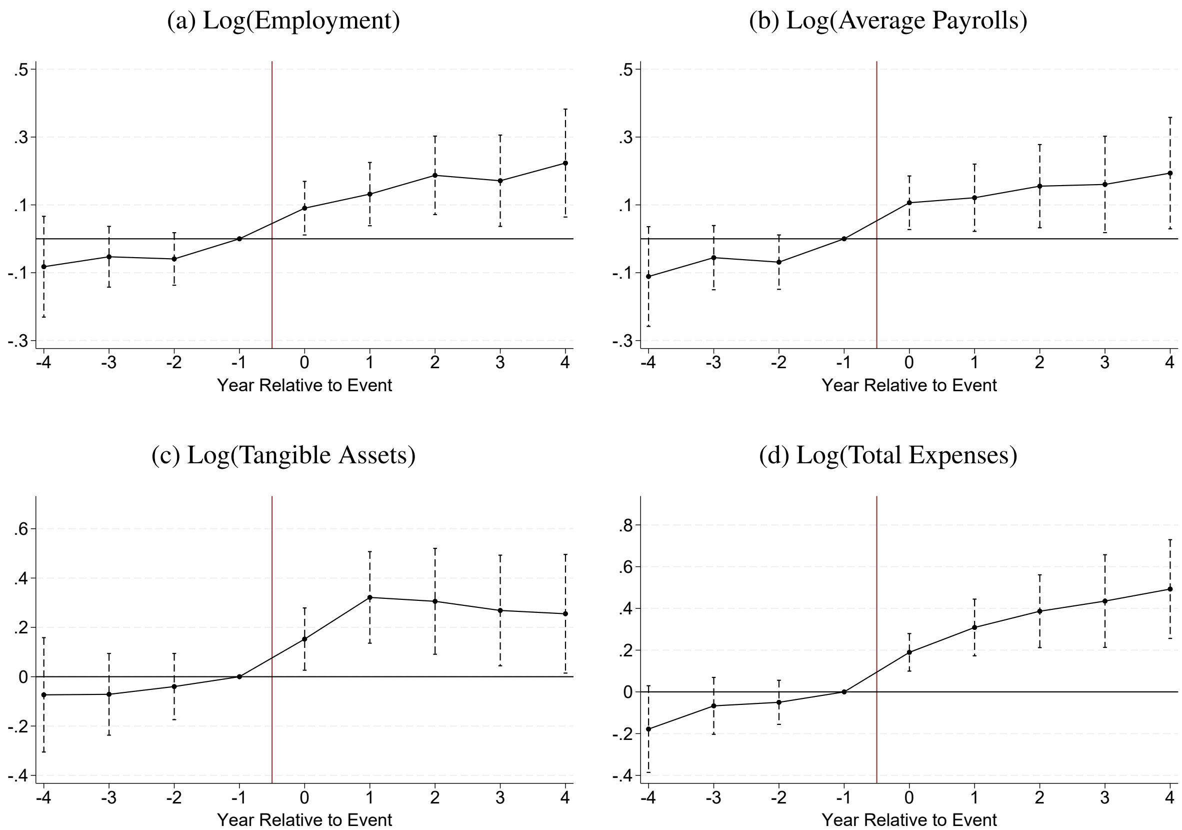


(b) Identifying treated and potential control firms



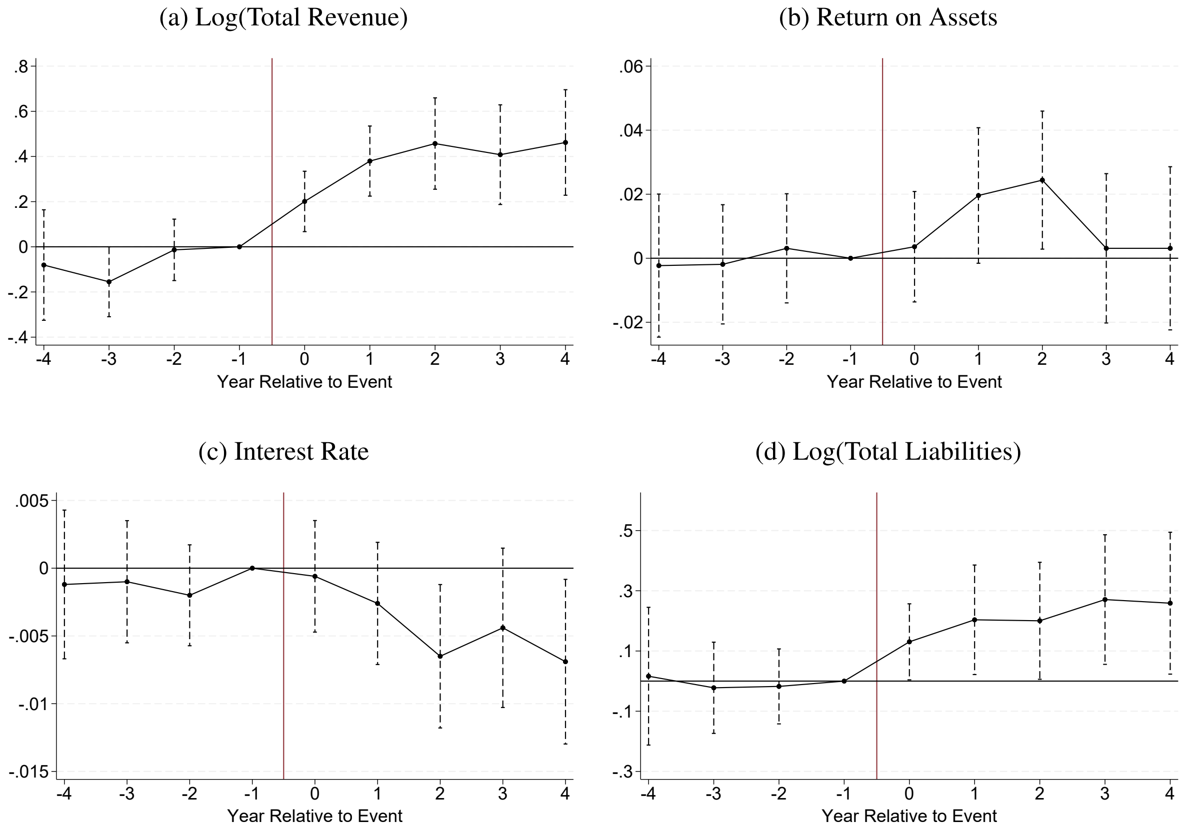
Notes: Panel A shows the emissions-reporting thresholds under the federal Greenhouse Gas Reporting Program (GHGRP) as well as under provincial GHG reporting regimes when a province's threshold is below the federal threshold (Ontario, British Columbia, and Quebec), as described in Section 2. Panel B summarizes how we identify treated and potential control firms, as described in Section 5.

Figure 2: **Mandatory Disclosure Impacts on Employment, Investment, and Expenses**



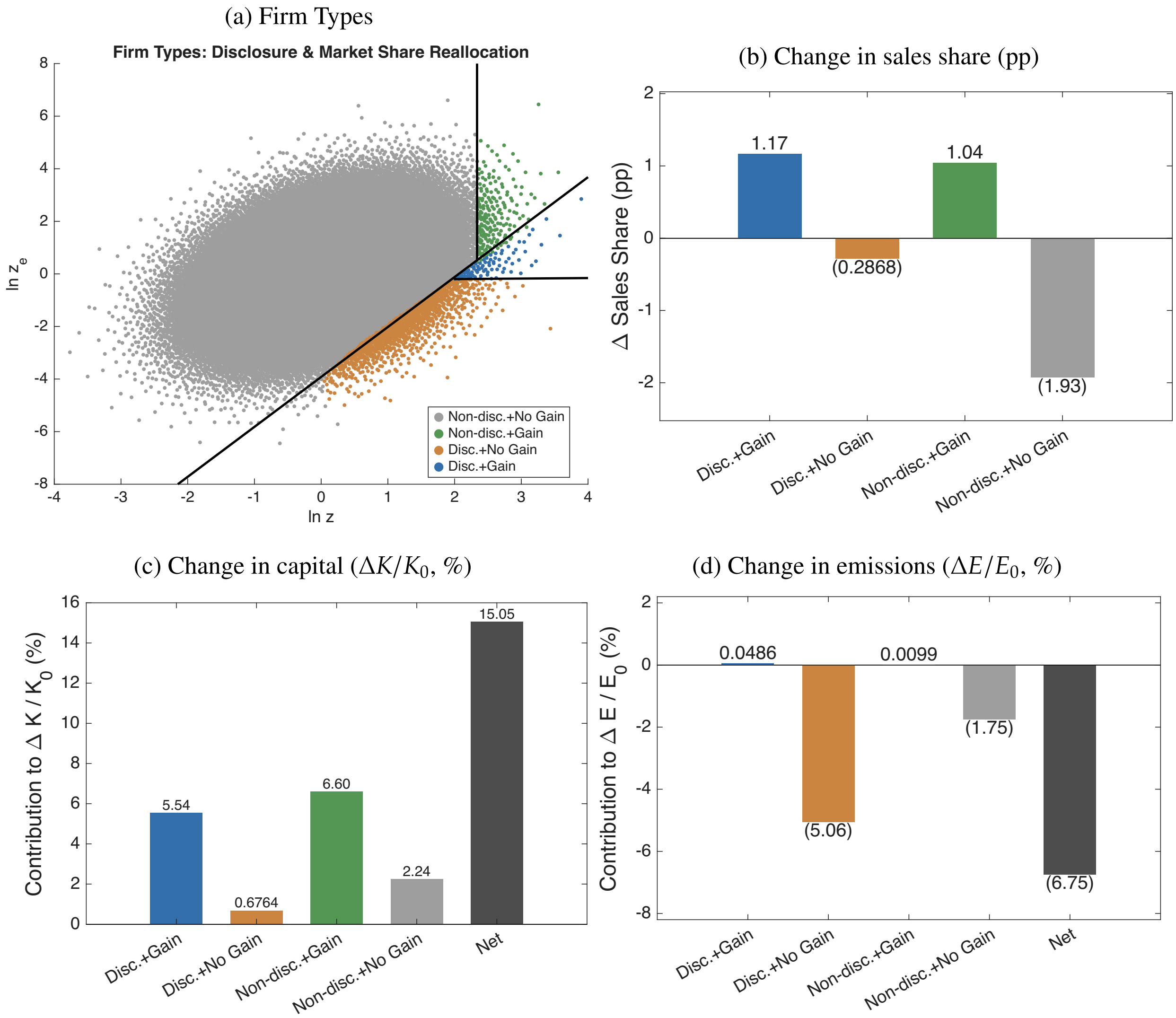
Notes: These figures present event-study estimates of the impact of GHG disclosure mandates on employment, average payroll, tangible assets, and total expenses. Employment is measured as the average number of employees reported in employee tax records and Record of Employment data. Average payroll is measured as total annual payroll divided by the average number of employees in the firm-year. Tangible assets are measured as the reported book value of tangible fixed assets, net of accumulated depreciation, and serve as our measure of capital stock. Total expenses are measured as total expenses reported in firms' financial statements. 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 5). The policy change is implemented in year 0, and the coefficient is normalized to zero in year -1.

Figure 3: Mandatory Disclosure Impacts on Revenue, ROA, and Capital Structures



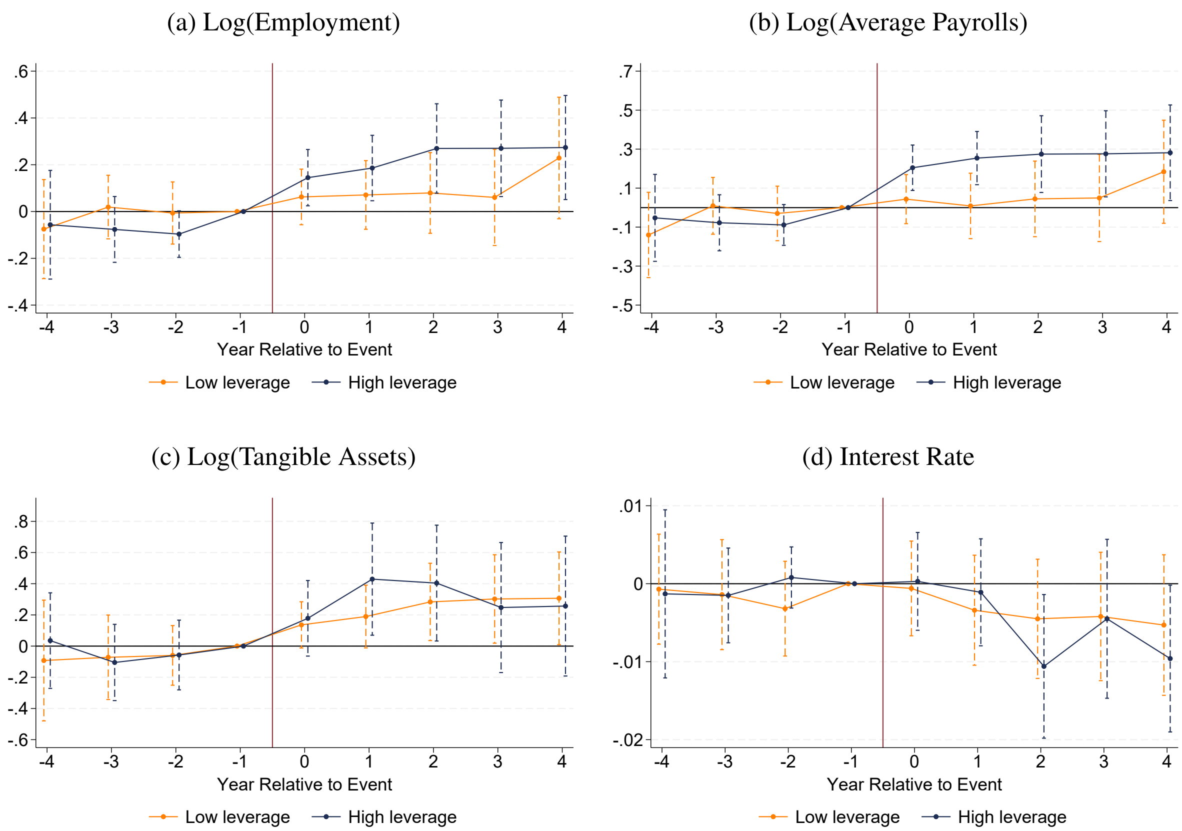
Notes: These figures present event-study estimates of the impact of GHG disclosure mandates on total revenue, return on assets, interest rate, and total liabilities. Total revenue is the sum of all revenue sources in a given year. Return on assets (ROA) is calculated as net income divided by total assets. Interest rate is measured as interest expense divided by total debt. Total liabilities represent the book value of all liabilities outstanding at the end of the fiscal 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 5). The policy change is implemented in year 0, and the coefficient is normalized to zero in year -1.

Figure 4: **Firm Types and Decomposition of Sales Share, Capital, and Emissions**



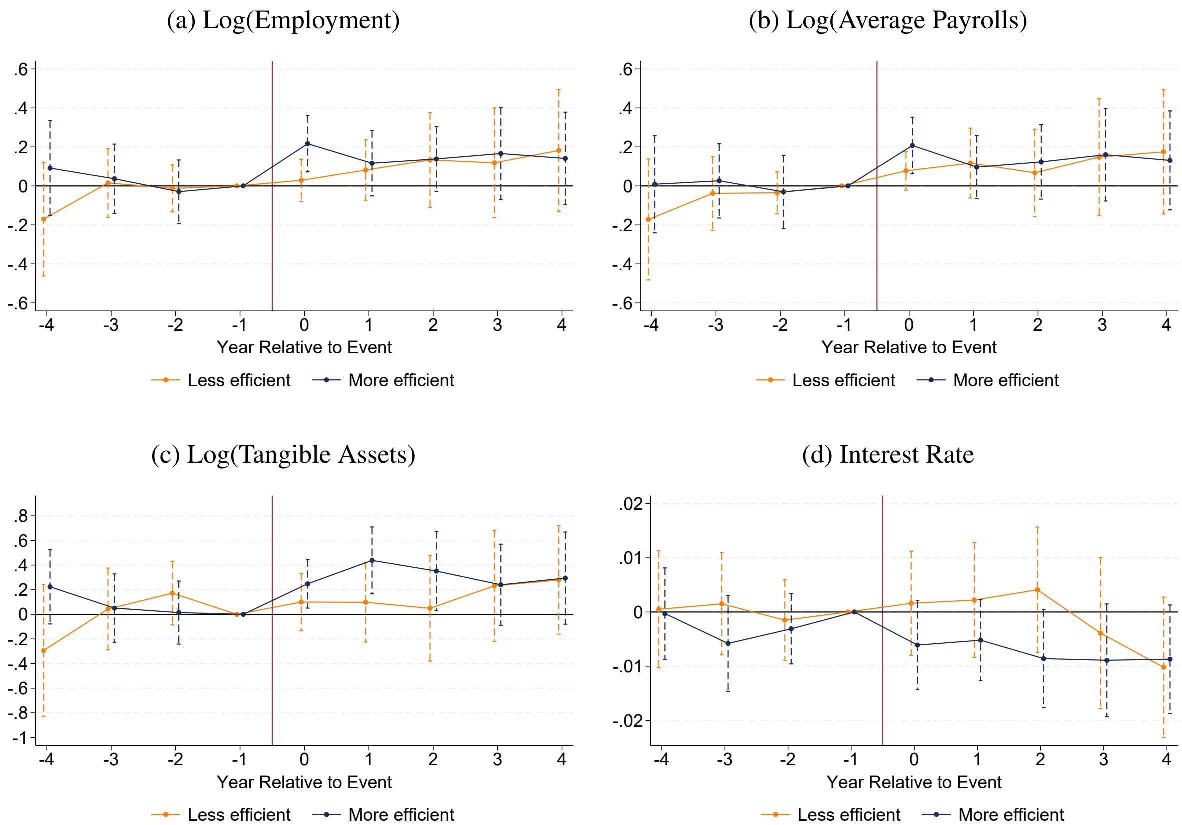
Notes: Firms are classified into four groups based on whether they disclose (Disc.) or do not disclose (Non-disc.) emissions, and whether their profit increases after the policy (Gain) or not (No Gain). Panel (a) describes these four types of firms in the joint distributions of sales (x-axis) and emission efficiency (y-axis). Panel (b) shows the change in each group's share of total industry sales (in percentage points); the four bars sum to zero by construction. Panels (c) and (d) decompose the aggregate percentage change in capital ($\Delta K/K_0$) and emissions ($\Delta E/E_0$), respectively; in each case the four group bars sum to the Net bar.

Figure 5: **Mandatory Disclosure Impacts on Firm Outcomes by Leverage Ratio**



Notes: These figures present event-study estimates of the heterogeneous impact of GHG disclosure mandates on firm outcomes separately for those with high and low leverage ratio. Leverage ratio is defined as total liabilities divided by total assets, measured at $t = -1$, and high (low) leverage firms are those above (below) the sample median. We estimate two separate regressions, one using the sample of high-leverage firms and the other using the sample of low-leverage firms, and plot separate lines for the estimated effects. Employment is measured as the average number of employees reported in employee tax records and Record of Employment data. Average payroll is measured as total annual payroll divided by the average number of employees in the firm-year. Tangible assets are measured as the reported book value of tangible fixed assets, net of accumulated depreciation, and serve as our measure of capital stock. Interest rate is measured as interest expense divided by total debt. 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 5). The policy change is implemented in year 0, and the coefficient is normalized to zero in year -1.

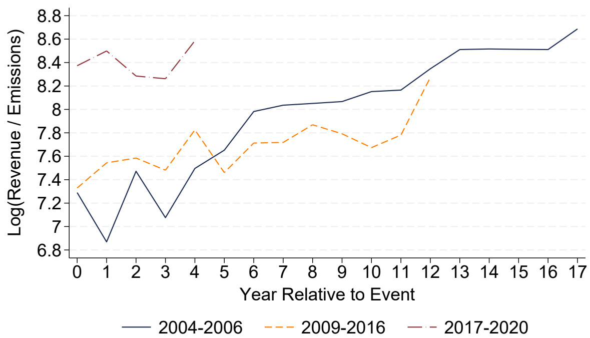
Figure 6: **Mandatory Disclosure Impacts on Firm Outcomes by Energy Efficiency**



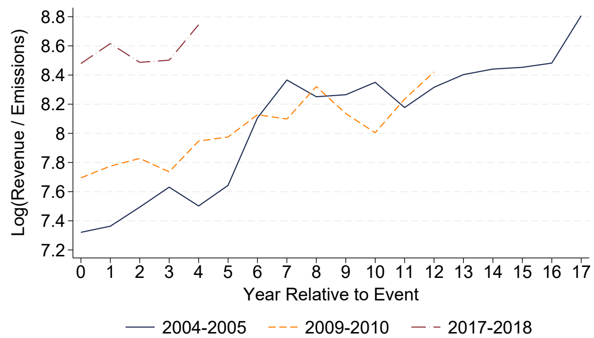
Notes: These figures present event-study estimates of the heterogeneous impact of GHG disclosure mandates on firm outcomes separately for those with low and high energy efficiency firms. Energy efficiency is measured as total revenue divided by total GHG emissions, expressed in thousands, and is measured at $t = -1$; high (low) energy efficiency firms are those above (below) the sample median. We estimate two separate regressions, one using the sample of high-energy-efficiency firms and the other using the sample of low-energy-efficiency firms, and plot separate lines for the estimated effects. Employment is measured as the average number of employees reported in employee tax records and Record of Employment data. Average payroll is measured as total annual payroll divided by the average number of employees in the firm-year. Tangible assets are measured as the reported book value of tangible fixed assets, net of accumulated depreciation, and serve as our measure of capital stock. Interest rate is measured as interest expense divided by total debt. 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 5). The policy change is implemented in year 0, and the coefficient is normalized to zero in year -1.

Figure 7: **Energy Efficiency Over Time**

(a) All Mandatorily Disclosing Firms



(b) Matched Sample



Notes: This figure plots energy efficiency for firms first treated by the three federal GHGRP reporting reforms. Energy efficiency is measured as total revenue divided by total GHG emissions, expressed in thousands. For each federal reform, we define a firm as treated if its first mandatory federal disclosure occurs in the implementation year of the reform or in the subsequent year (2004–2005, 2009–2010, and 2017–2018). The three lines correspond to these three treated cohorts. Year 0 denotes the reform implementation year (2004, 2009, or 2017, depending on the cohort). The figure reports average energy efficiency for each cohort by year relative to the implementation year, including the implementation year (0) and subsequent years.

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

	(1) Treated	(2) Control
Panel A: Descriptive Stats on Firms		
Total Revenue (in million)	533.18	126.22
Total Expenses (in million)	459.05	106.43
Employment	818	250
Average Payroll (in million)	4.79	1.31
Tangible Assets (in million)	606.56	124.54
Total Liabilities (in million)	768.77	192.96
Returns on Assets	0.028	0.059
Interest Rates	0.027	0.024
Number of Unique Firms	270	250
Panel B: Sector Composition		
Sector 1	0.03	0.03
Sector 2	0.37	0.36
Sector 3	0.45	0.44
Sector 4	0.03	0.04
Sector 5	0.12	0.12

Notes: This table reports descriptive statistics on firms, measured one year prior to the reform ($t = -1$). Panel A reports firm characteristics. Panel B reports the sector composition of firms. Columns (1) and (2) report statistics for treated and matched control firms (Panels A and B), respectively. Sector 1 is agriculture, forestry, fishing, and hunting; Sector 2 is mining, quarrying, and oil and gas extraction, and utilities; Sector 3 is manufacturing; Sector 4 is Whole Sale and Retail Trade, Transportation and Warehousing; and Sector 5 is Information, Professional, Scientific, and Technical Services. Average payroll is total annual payroll divided by employment. Employment is the average number of employees in the firm-year. Tangible assets are the book value of tangible fixed assets, net of accumulated depreciation, and serve as our measure of capital stock. Total expenses are the sum of all expenses incurred within the year. Total revenue is the sum of all revenue sources in a given year. Return on assets (ROA) is net income divided by total assets. Interest rate is interest expense divided by total debt. Total liabilities are the book value of all current and long-term liabilities.

Table 2: **Diff-in-Diff Estimates on Employment, Average Payrolls, Investment, and Expenses**

	(1) Log(Employment)	(2) Log(Av. Payroll)	(3) Log(Tangible Assets)	(4) Log(Total Expenses)
Treat x Post	0.1607***	0.1473***	0.2607***	0.3625***
	(0.0501)	(0.0531)	(0.0867)	(0.0724)
Observations	7330	7330	7140	7350
Treated Firms	270	270	250	260
Control Firms	250	250	230	240
Adj. R Squared	0.940	0.941	0.907	0.912
Mean at t=-1 (treated)	818	4.79	606.56	459.05

Notes: This table presents difference-in-differences estimates of the impact of GHG disclosure mandates on firms' employment, average payroll, tangible assets, and total expenses. Treat is an indicator equal to one for treated firms, and zero for control firms, as described in Section 5. Employment is the average number of employees in the firm-year. Average payroll is total annual payroll divided by employment. Tangible assets are the book value of tangible fixed assets, net of accumulated depreciation, and serve as our measure of capital stock. Total expenses are the sum of all expenses incurred within the year.

Table 3: **Diff-in-Diff Estimates on Revenue, ROA, and Capital Structures**

	(1) Log(Total Revenue)	(2) Returns on Assets	(3) Interest Rates	(4) Log(Total Liabilities)
Treat x Post	0.3815***	0.0107	-0.0042**	0.2128***
	(0.0768)	(0.0085)	(0.0021)	(0.0817)
Observations	7290	7550	5840	7510
Treated Firms	260	270	210	260
Control Firms	240	250	190	250
Adj. R Squared	0.902	0.444	0.430	0.913
Mean at t=-1 (treated)	533.18	0.028	0.027	768.77

Notes: This table presents difference-in-differences estimates of the impact of GHG disclosure mandates on total revenue, return on assets, interest rates, and total liabilities. Treat is an indicator equal to one for treated firms, and zero for control firms, as described in Section 5. Total revenue is the sum of all revenue sources in a given year. Return on assets (ROA) is net income divided by total assets. Interest rate is interest expense divided by total debt. Total liabilities are the book value of all current and long-term liabilities.

Table 4: **SMM Estimation Results and Aggregate Statistics**

Panel A: SMM Moment Fit and Parameter Identification

Moment		Data	Model	Parameter(s) identified
p90–p50 $\ln y$	[all firms]	2.80	3.42	$\sigma_z^2 = 0.79$
p10–p50 $\ln y$	[all firms]	-3.50	-3.44	σ_z^2
Std($\ln y$)	[all firms]	2.50	2.68	σ_z^2
p90–p50 $\ln(e/y)$	[disclosing]	3.70	2.41	$\sigma_{z_e}^2 = 2.32,\ \gamma = -0.73$
p10–p50 $\ln(e/y)$	[disclosing]	-2.10	-2.69	$\sigma_{z_e}^2,\ \gamma$
Std($\ln e/y$)	[disclosing]	2.60	1.96	$\sigma_{z_e}^2,\ \gamma$
Corr($\ln y, \ln e/y$)	[disclosing]	-0.85	-0.89	$\sigma_{z,z_e} = 0.51$

Panel B: Aggregate Statistics — Pre- vs. Post-Disclosure Regulation

Statistic	Period 0	Period 1	% Change
Price index P	0.4115	0.3922	-4.70
Avg. capital $\bar{k}$	21.7431	25.0158	15.05
Avg. emissions $\bar{e}$	0.1370	0.1278	-6.75
Revenue share (disclosers)	27.91%	28.79%	3.16

Notes. Panel A: Data moments are computed from the cross-section of log revenue (all firms) and log emissions-to-revenue (disclosing firms only). Model moments are simulated at the SMM optimum using $N = 2^{16}$ Sobol draws. The estimation is over-identified: seven moments pin down four parameters (σ_z^2, $\sigma_{z_e}^2$, σ_{z,z_e}, γ). The correlation moment receives a weight of 10 in the diagonal weight matrix; all other moments receive unit weight. *Panel B:* Period 0 is the pre-regulation equilibrium with common user cost of capital. Period 1 reflects the post-disclosure equilibrium with firm-specific user costs. Averages are computed over $N = 2^{16}$ simulated firms. Disclosure share is the fraction of firms whose period-0 emissions exceed the threshold $\bar{e}$.

Table 5: **Diff-in-Diff Estimates on Firm Outcomes By Leverage Ratio**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log(Employment)		Log(Av. Payroll)		Log(Tangible Assets)		Interest Rates	
	Low Lev.	High Lev.	Low Lev.	High Lev.	Low Lev.	High Lev.	Low Lev.	High Lev.
Treat x Post	0.1004	0.2289***	0.0662	0.2582***	0.2435**	0.3032*	-0.0036	-0.0051
	(0.0793)	(0.0757)	(0.0870)	(0.0809)	(0.0980)	(0.1671)	(0.0032)	(0.0032)
Observations	3630	3340	3630	3340	3510	3260	2920	2560
Treated Firms	140	130	140	130	130	120	110	100
Control Firms	130	120	130	120	120	110	100	90
Adj. R Squared	0.937	0.957	0.933	0.957	0.903	0.907	0.442	0.482
Mean at t=-1 (treated)	654	1018	3.85	5.95	569.67	725.89	0.026	0.028

Notes: This table presents difference-in-differences estimates of the impact of GHG disclosure mandates on firm outcomes separately for firms with high and low leverage ratios. Leverage is measured as total liabilities divided by total assets at $t = -1$; high (low) leverage firms are those above (below) the sample median. We estimate two separate regressions, one using the sample of high-leverage firms and the other using the sample of low-leverage firms. Treat is an indicator equal to one for treated firms, and zero for control firms, as described in Section 5. Employment is the average number of employees in the firm-year. Average payroll is total annual payroll divided by employment. Tangible assets are the book value of tangible fixed assets, net of accumulated depreciation, and serve as our measure of capital stock. Interest rate is interest expense divided by total debt.

Table 6: **Diff-in-Diff Estimates on Firm Outcomes By Energy Efficiency**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log(Employment)		Log(Av. Payroll)		Log(Tangible Assets)		Interest Rates	
	Less eff.	More eff.	Less eff.	More eff.	Less eff.	More eff.	Less eff.	More eff.
Treat x Post	0.1089	0.1556*	0.1170	0.1436	0.1517	0.3143**	-0.0012	-0.0075**
	(0.0969)	(0.0838)	(0.0990)	(0.0894)	(0.1577)	(0.1250)	(0.0044)	(0.0036)
Observations	1940	2440	1940	2440	1870	2430	1380	1940
Treated Firms	90	90	90	90	80	90	60	70
Control Firms	70	80	70	80	70	80	50	60
Adj. R Squared	0.941	0.954	0.943	0.951	0.916	0.912	0.477	0.446
Mean at t=-1 (treated)	432	1341	2.33	8.30	408.94	952.78	0.028	0.027

Notes: This table presents difference-in-differences estimates of the impact of GHG disclosure mandates on firm outcomes separately for low and high energy efficiency firms. Energy efficiency is measured as total revenue divided by total GHG emissions, expressed in thousands, and is measured at $t = -1$; high (low) energy efficiency firms are those above (below) the sample median. We estimate two separate regressions, one using the sample of high energy efficiency firms and the other using the sample of low energy efficiency firms. Treat is an indicator equal to one for treated firms, and zero for control firms, as described in Section 5. Employment is the average number of employees in the firm-year. Average payroll is total annual payroll divided by employment. Tangible assets are the book value of tangible fixed assets, net of accumulated depreciation, and serve as our measure of capital stock. Interest rate is interest expense divided by total debt.

ONLINE APPENDIX:

Mandatory Greenhouse Gas Disclosure and Spillover Impacts on Corporate Investment

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A Additional Data and Sample Descriptions

This appendix presents additional details on the GHG emission disclosure data and compares the characteristics of firms that disclose mandatorily and of firms that disclose voluntarily.

A.1 Additional Descriptions of GHG Data

Table A1 provides additional details on the GHG emission disclosure data described in Section 2. The underlying data are obtained from facility-level GHG emissions reports published by the ECCC and provincial agencies under the federal and provincial disclosure mandates, respectively. A facility is defined as an individual plant, pipeline system, or offshore installation, while a firm is the reporting company that owns or operates one or more facilities. Panel A reports the annual breakdown of this facility-level reporting from 2004 to 2021. For each year, the table shows: (i) the total number of facilities that reported their GHG emissions, (ii) the number of facilities reporting for the first time, and (iii) the number of firms reporting for the first time. Because a single firm can operate multiple facilities, the number of first-time reporting firms is generally smaller than the number of first-time reporting facilities. The treatment years used in our empirical design are 2004–2005 (following the introduction of the federal GHGRP at 100 kt in 2004), 2009–2010 (following the federal threshold reduction to 50 kt in 2009), and 2017–2018 (following the further reduction to 10 kt in 2016). In each treatment window, there is a sharp increase in the number of first-time reporters consistent with the threshold reductions bringing new facilities into the reporting regime. In 2021 alone, 1,732 facilities reported their GHG emissions. Over the entire 2004–2021 period, there are 3,143 unique facilities in total. We merge these facilities with our firm-level data obtained from CEEDD and identify roughly 1,000 unique firms with balance sheet information at $t = -1$ (the year before the reform is implemented).

A.2 Characteristics of Mandatory and Voluntary Disclosing Firms

Table A2 compares the characteristics of firms subject to GHG reporting requirements with those that disclose GHG information voluntarily, measured one year prior to the reform. Panel A shows that firms subject to the reporting requirement are substantially larger than those who disclose voluntarily across all measures: their average total revenue is 611 million CAD compared to 149 million CAD for voluntarily-disclosing firms, and they employ on average 968 workers compared to 406 workers. Similarly, firms subject to the disclosure mandates have larger tangible assets (671 million versus 149 million CAD) and total liabilities (1,023 million versus 243 million CAD). Despite these size differences, the two groups have similar profitability, with returns on assets of 2.6

and 2.4 percent, respectively. Interest rates are slightly lower for firms subject to the mandate (2.5 versus 3.0 percent), consistent with larger firms generally having better access to credit markets at lower rates. Panel B shows that the sector composition also differs between the two groups.

Firms that voluntarily disclose account for less than 10 percent of firms that report their GHG emissions, consistent with the costs of reporting and the lower benefits for firms not subject to disclosure requirements. As noted in Section 7.3, disclosure can impose significant direct costs (e.g., measuring and aggregating GHG emissions and preparing emission reports), proprietary costs from revealing sensitive information to competitors (e.g., Bhattacharya and Ritter, 1983), and managerial opportunity costs. Because a large portion of these costs is fixed, GHG disclosure can be particularly burdensome for smaller firms. Firms below the reporting threshold tend to be smaller and are therefore less likely to provide GHG information. In addition, a key benefit of mandatory disclosure is regulatory oversight, which helps with compliance and enhances the credibility of GHG reports. In contrast, disclosures provided outside the regulatory framework are not subject to the same level of verification or enforcement, limiting their credibility and usefulness to investors and, in turn, reducing firms' incentives to disclose.

Why do relatively smaller firms provide voluntary disclosures? First, consider relatively larger firms that are below the reporting threshold. Although investors do not directly observe these firms' emissions, they can infer energy efficiency, measured as revenue divided by emissions, based on observed revenues. Specifically, if a non-reporting firm has high revenue, investors can conclude that its emission efficiency must be high, since the firm generates substantial outputs while emitting below the threshold (Section B.2 provides further details). These firms gain the benefits of their high energy efficiency without incurring the costs of disclosure and thus have less incentive to provide voluntary reports. Second, consider smaller firms below the threshold. Among these firms, some gain greater advantages from voluntary disclosure, particularly those with relatively low emissions. Consistent with this intuition, we find that the energy efficiency, measured as log(revenue/emissions), of firms reporting voluntarily is nearly identical to that of firms subject to mandatory reporting (7.5 versus 7.6, respectively). This suggests that smaller firms with comparable efficiency levels are more likely to report voluntarily, likely aiming to distinguish themselves from less efficient, higher-emitting firms in the eyes of investors. However, Figure A1 shows that both the number of firms that disclose voluntarily and their survival rate (i.e., probability of these firms continuing to disclose voluntarily in subsequent years after doing so for the first time) decrease over time, consistent with the idea that it is costly (especially for smaller firms) to disclose GHG emissions and only energy-efficient firms have incentives to do so.

Table A1: **GHG Dataset**

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

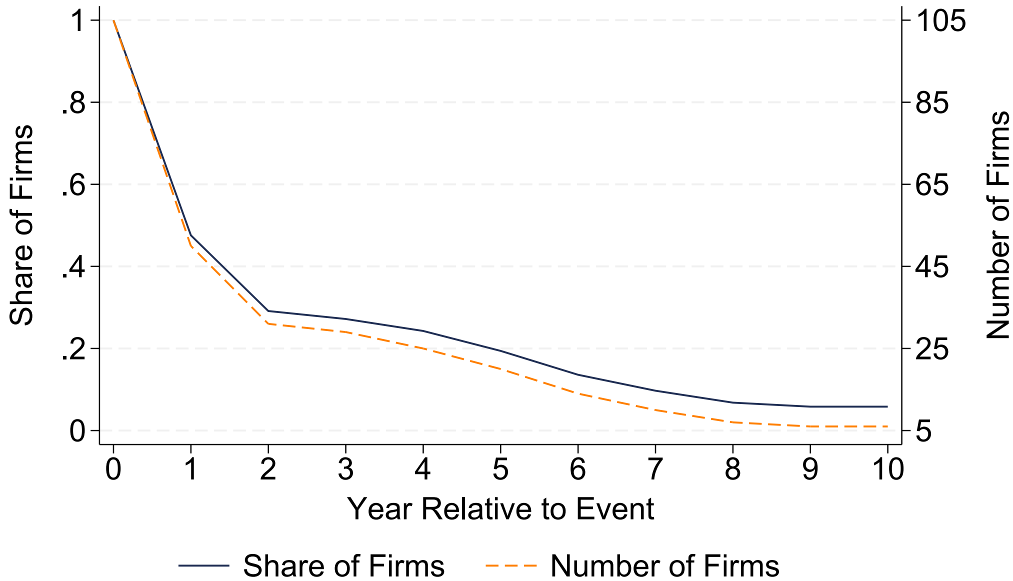
Notes: This table summarizes our original GHG emission datasets under federal and provincial disclosure mandates. The table reports the number of facilities reporting their GHG emissions (*# of reporting facilities*) and the number of unique first-time facility reporters (*# of first-time facilities*) by year. The number of first-time facility reporters reflects facilities reporting for the first time in the sample period.

Table A2: Descriptive Statistics on Mandatorily and Voluntarily Disclosing Firms (at $t = 0$)

	(1) Mandatory	(2) Voluntary
Panel A: Descriptive Stats on Firms		
Total Revenue (in million)	610.51	149.33
Total Expenses (in million)	500.43	140.04
Employment	968	406
Average Payroll (in million)	5.68	1.85
Tangible Assets (in million)	670.49	148.78
Total Liabilities (in million)	1022.82	242.60
Returns on Assets	0.026	0.024
Interest Rates	0.025	0.030
Number of Unique Firms	700	100
Panel B: Sector Composition		
Sector 1	0.07	0.00
Sector 2	0.34	0.21
Sector 3	0.38	0.52
Sector 4	0.04	0.07
Sector 5	0.16	0.18

Notes: This table reports descriptive statistics for firms subject to mandatory disclosure requirements and those that voluntarily report their GHG emissions, measured one year prior to the reform ($t = 0$). Panel A reports firm characteristics. Panel B reports the sector composition of firms. Column (1) reports statistics for mandatorily disclosing firms and Column (2) for voluntarily disclosing firms. Sector 1 is agriculture, forestry, fishing, and hunting; Sector 2 is mining, quarrying, and oil and gas extraction, and utilities; Sector 3 is manufacturing; Sector 4 is Whole Sale and Retail Trade, Transportation and Warehousing; and Sector 5 is Information, Professional, Scientific, and Technical Services. Total revenue is the sum of all revenue sources in a given year. Employment is the average number of employees in the firm-year. Average payroll is total annual payroll divided by employment. Tangible assets are the book value of tangible fixed assets, net of accumulated depreciation, and serve as our measure of capital stock. Total liabilities are the book value of all current and long-term liabilities. Return on assets (ROA) is net income divided by total assets. Interest rate is interest expense divided by total debt.

Figure A1: **Share and Number of Voluntarily Disclosing Firms Over Time**



Notes: This figure shows both the number and share of firms that voluntarily disclose GHG emissions. The share of firms indicates the survival rate – the probability that firms continue to disclose voluntarily in subsequent years after doing so for the first time.

B Robustness tests

This appendix presents results from robustness tests. Section B.1 restricts the treatment definition to only the first year of each reform. Section B.2 addresses potential spillover effects by dropping similar-sized control firms near the reporting threshold.

B.1 Restricting Treatment to the First Year of the Reform

As discussed in Section 5, our main specification defines treated firms as those that first reported their GHG emissions within the first two years of each reform. A potential concern is that firms reporting in the second year may have had some time to adjust their emissions after the regulation. To address this, we re-estimate our main specification restricting treatment to firms that first reported in only the first year of each reform (i.e., 2004, 2009, and 2017 for the federal mandates).

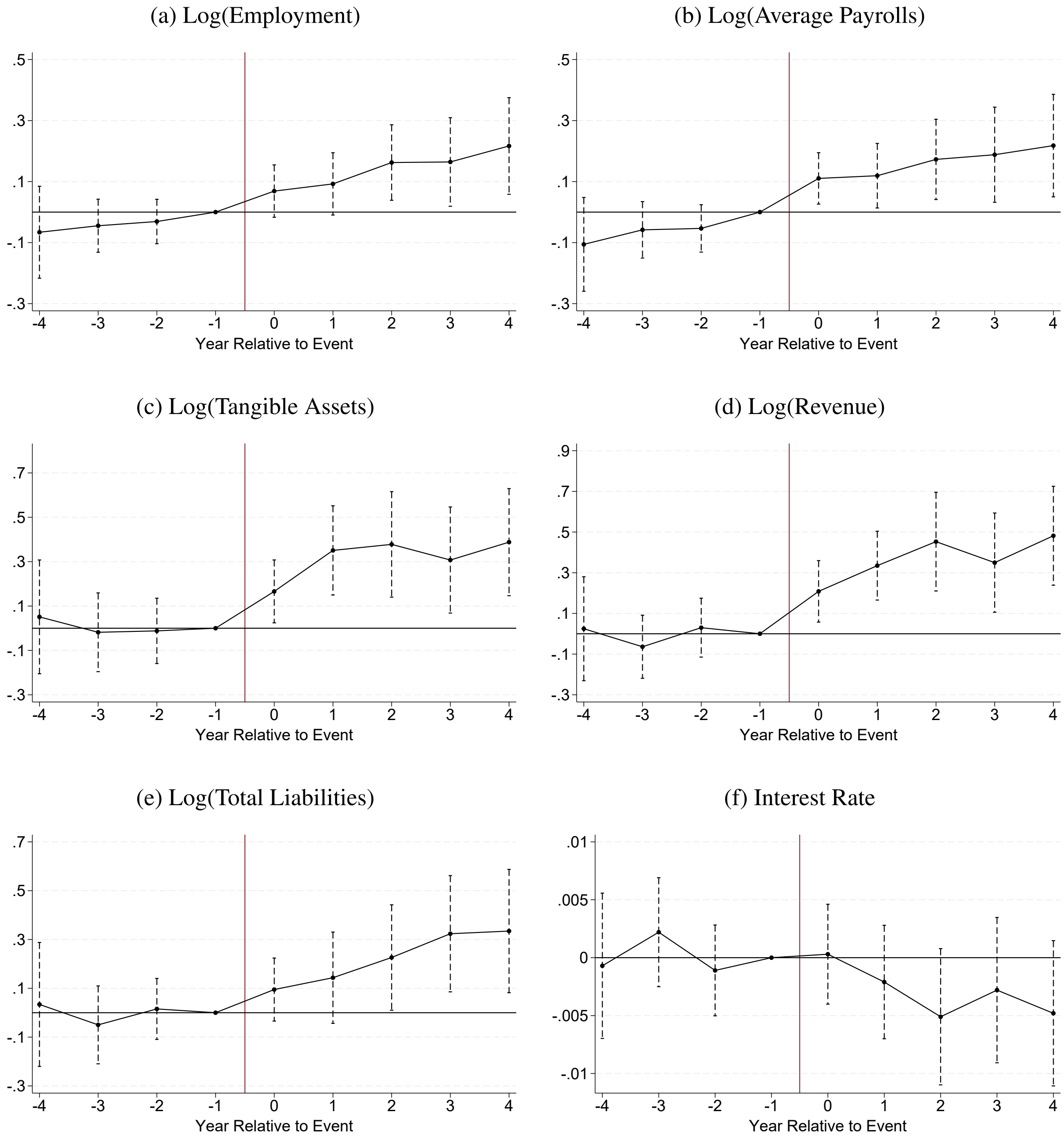
Figure B1 plots the event-study estimates for employment, average payrolls, tangible assets, total revenue, total liabilities, and interest rates. Across all panels, treated and control firms exhibit similar pre-reform trends. Following the reform, we observe significant increases in employment, payrolls, investment, revenue, and liabilities for treated firms relative to matched controls. Table B1 reports the corresponding difference-in-differences estimates. The coefficients imply that treated firms increase employment by 14.1 log points, average payrolls by 16.2 log points, tangible assets by 31.8 log points, total revenue by 36.6 log points, and total liabilities by 22.4 log points. The estimated effect on interest rates is negative, albeit not statistically significant. These results are qualitatively similar to our main estimates, suggesting that anticipation effects in the second year of each reform are unlikely to drive our findings.

B.2 Dropping Similar-sized Firms Below the Cutoff

A potential concern with our difference-in-differences design is that control firms just below the reporting threshold may be "partially treated" by the disclosure mandate through information spillovers. Although investors do not directly observe the emission levels of non-reporting firms, they can observe these firms' revenues and profits and know that their emissions are below the threshold (e.g., below 100 kt in 2004). If a non-reporting firm has high revenue, investors can infer that its emission efficiency—measured as revenue divided by emissions—must be high, since the firm generates a substantial amount of outputs while emitting below the threshold. In other words, non-disclosure combined with high revenue places a tight lower bound on emission efficiency. Thus, investors may update their belief upward about these firms' emission efficiency, reducing their cost of capital—even though these firms do not disclose (see the truncated posterior in Section 3, Equations 5–6). Unlike disclosing firms, for which emission efficiency is fully revealed and uncertainty is eliminated, non-disclosing firms still face residual uncertainty about their true emission efficiency. To the extent that partially treated firms experience any reduction in their cost of capital, including them in the control group would attenuate our estimated treatment effect. To address this concern, we identify potentially partially treated firms in the control group as those operating in the same industry as treated firms and whose revenue and profit margins are at least 90 percent of those of the treated firms. Specifically, for each treatment year we define a bandwidth around the reporting threshold (100–120 kt for the 2004–2005 reforms, 50–60 kt for 2009–2010, and 10–20 kt for 2017–2018) and flag control firms in the same sector whose revenue and profitability are at least 90 percent of the revenue and profitability of the treated firms. We then drop these partially treated firms from the control group and re-run our matching procedure on the remaining firms.

Figure B2 plots the event-study estimates and Table B2 reports the corresponding difference-in-differences estimates. The results are qualitatively similar to our main estimates: treated firms increase employment by 19.8 log points, average payrolls by 20.1 log points, tangible assets by 33.4 log points, total revenue by 40.1 log points, and total liabilities by 37.1 log points. The estimated decline in interest rates is 0.54 percentage points and statistically significant. The sample size is smaller than in the main specification (210 treated firms versus 270) because removing spillover firms from the pool of potential controls reduces the number of comparable firms available for matching. Despite the smaller sample, the estimates become a bit larger in magnitude, suggesting that spillover effects from firms near the threshold likely introduce the attenuation bias in our difference-in-differences results. Furthermore, Figure B3 and Table B3 report similar estimates where we simply exclude these spillover firms from our matched control group (instead of re-doing the matching) while keeping the original set of treated firms. We find very similar results when we just drop these spillover firms from our control group.

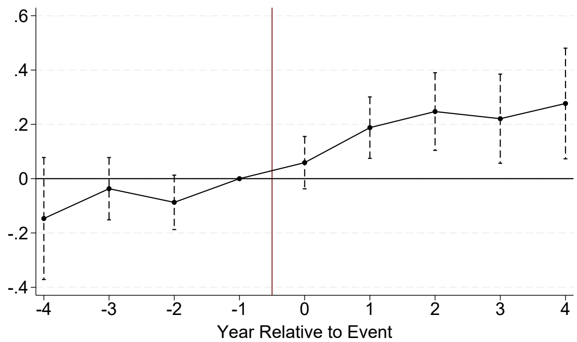
Figure B1: **Mandatory Disclosure Impacts on Firms Using Only First Year of Reform**



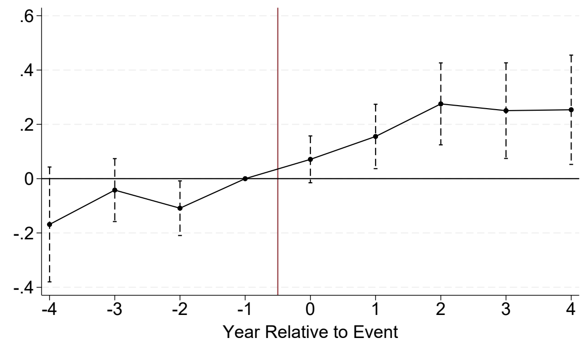
Notes: These figures examine the robustness of our results to restricting treated firms to a narrower set of firms—those that first reported in only the first year of each reform (i.e., 2004, 2009, and 2017 for the federal mandates; 2010 for Ontario), rather than the first two years. These figures present event-study estimates of the impact of GHG disclosure mandates on employment, average payroll, tangible assets, total revenue, total liabilities, and interest rates. Employment is measured as the average number of employees reported in employee tax records and Record of Employment data. Average payroll is measured as total annual payroll divided by the average number of employees in the firm-year. Tangible assets are measured as the reported book value of tangible fixed assets, net of accumulated depreciation, and serve as our measure of capital stock. Total revenue is the sum of all revenue sources in a given year. Total liabilities are the book value of all current and long-term liabilities. Interest rate is measured as interest expense divided by total debt. 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 5). The policy change is implemented in year 0, and the coefficient is normalized to zero in year −1.

Figure B2: **Mandatory Disclosure Impacts Dropping Similar-sized Firms Below Cutoff (Re-matching)**

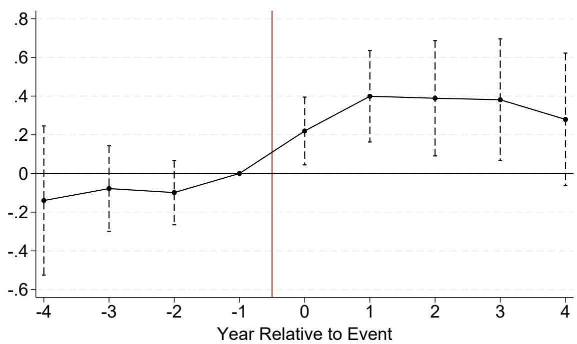
(a) Log(Employment)



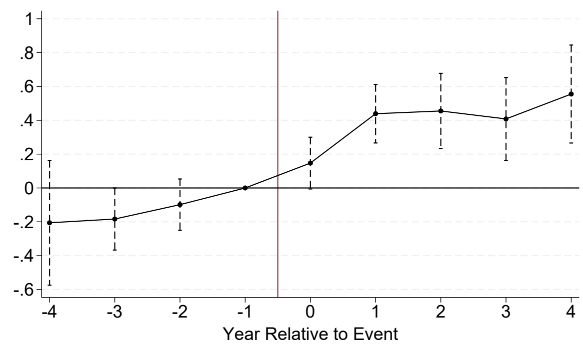
(b) Log(Average Payrolls)



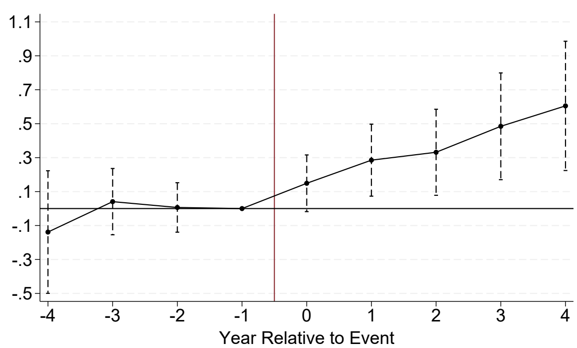
(c) Log(Tangible Assets)



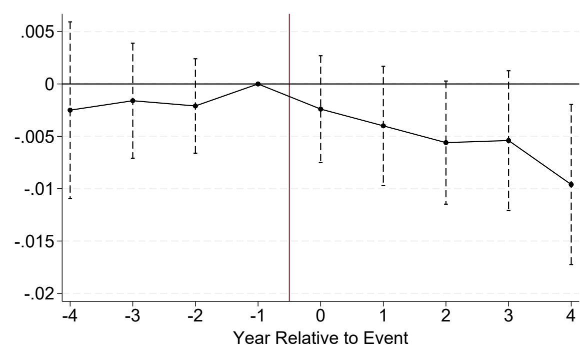
(d) Log(Revenue)



(e) Log(Total Liabilities)

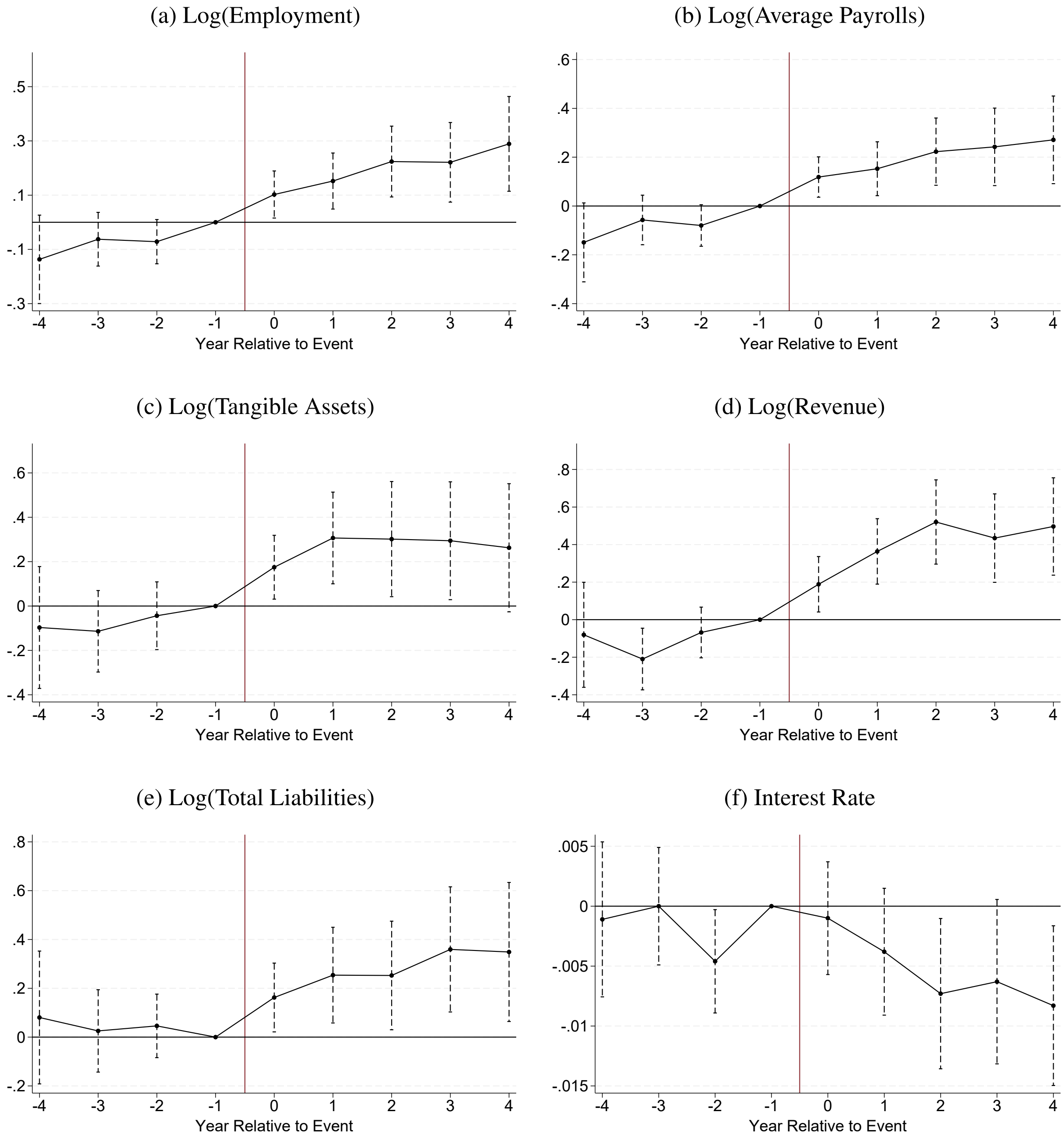


(f) Interest Rate



Notes: These figures examine the robustness of our results to dropping a subset of firms from the potential control group. Specifically, we drop "partially treated firms" identified as control firms in the same industry as treated firms near the cutoff whose revenue and profit margins are at least 90 percent of those of the treated firms. We drop these partially treated firms from the potential control group and re-run our matching procedure on the remaining firms. These figures present event-study estimates of the impact of GHG disclosure mandates on employment, average payroll, tangible assets, total revenue, total liabilities, and interest rates. Employment is measured as the average number of employees reported in employee tax records and Record of Employment data. Average payroll is measured as total annual payroll divided by the average number of employees in the firm-year. Tangible assets are measured as the reported book value of tangible fixed assets, net of accumulated depreciation, and serve as our measure of capital stock. Total revenue is the sum of all revenue sources in a given year. Total liabilities are the book value of all current and long-term liabilities. Interest rate is measured as interest expense divided by total debt. 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 5). The policy change is implemented in year 0, and the coefficient is normalized to zero in year -1.

Figure B3: Mandatory Disclosure Impacts Dropping Similar-sized Firms Below Cutoff



Notes: These figures examine the robustness of our results to dropping a subset of firms from the potential control group. Specifically, we drop "partially treated firms" identified as control firms in the same industry as treated firms near the cutoff whose revenue and profit margins are at least 90 percent of those of the treated firms. These figures present event-study estimates of the impact of GHG disclosure mandates on employment, average payroll, tangible assets, total revenue, total liabilities, and interest rates. Employment is measured as the average number of employees reported in employee tax records and Record of Employment data. Average payroll is measured as total annual payroll divided by the average number of employees in the firm-year. Tangible assets are measured as the reported book value of tangible fixed assets, net of accumulated depreciation, and serve as our measure of capital stock. Total revenue is the sum of all revenue sources in a given year. Total liabilities are the book value of all current and long-term liabilities. Interest rate is measured as interest expense divided by total debt. 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 5). The policy change is implemented in year 0, and the coefficient is normalized to zero in year -1.

Table B1: **Diff-in-Diff Estimates on Firm outcomes using only the first year of the reform**

	(1) Log(Employment)	(2) Log(Av. Payroll)	(3) Log(Tang. Assets)	(4) Log(Tot. Revenue)	(5) Log(Tot. Liabilities)	(6) Interest Rate
Treat x Post	0.1410***	0.1617***	0.3180***	0.3656***	0.2245**	-0.0029
	(0.0536)	(0.0573)	(0.0944)	(0.0840)	(0.0878)	(0.0022)
Observations	6330	6330	6140	6290	6480	5010
Treated Firms	230	230	220	230	230	180
Control Firms	220	220	210	220	220	170
Adj. R Squared	0.945	0.946	0.916	0.906	0.914	0.430
Mean at t=-1 (treated)	869	5.13	713.24	554.70	819.37	0.026

Notes: This table examines the robustness of our results to restricting treated firms to a narrower set of firms—those that first reported in only the first year of each reform (i.e., 2004, 2009, and 2017 for the federal mandates; 2010 for Ontario), rather than the first two years. The table presents difference-in-differences estimates of the impact of GHG disclosure mandates on firms' employment, average payroll, tangible assets, total revenue, total liabilities, and interest rates. Treat is an indicator equal to one for treated firms, and zero for control firms, as described in Section 5. Employment is the average number of employees in the firm-year. Average payroll is total annual payroll divided by employment. Tangible assets are the book value of tangible fixed assets, net of accumulated depreciation, and serve as our measure of capital stock. Total revenue is the sum of all revenue sources in a given year. Total liabilities are the book value of all current and long-term liabilities. Interest rate is interest expense divided by total debt.

Table B2: **Diff-in-Diff Estimates After Dropping Similar-sized Firms Below Cutoff (Re-matching)**

	(1) Log(Employment)	(2) Log(Av. Payroll)	(3) Log(Tang. Assets)	(4) Log(Tot. Revenue)	(5) Log(Tot. Liabilities)	(6) Interest Rate
Treat x Post	0.1984***	0.2012***	0.3337***	0.4008***	0.3711***	-0.0054**
	(0.0630)	(0.0646)	(0.1218)	(0.0886)	(0.1126)	(0.0025)
Observations	5050	5050	4950	5140	5220	4230
Treated Firms	210	210	190	210	200	170
Control Firms	170	170	160	170	170	140
Adj. R Squared	0.938	0.940	0.902	0.900	0.915	0.439
Mean at t=-1 (treated)	949.97	5621.29	803237.90	604312.60	896184.10	0.029

Notes: This table examines the robustness of our results to dropping a subset of firms from the potential control group. Specifically, we drop "partially treated firms" identified as control firms in the same industry as treated firms near the cutoff whose revenue and profit margins are at least 90 percent of those of the treated firms. We drop these partially treated firms from the potential control group and re-run our matching procedure on the remaining firms. This table presents difference-in-differences estimates of the impact of GHG disclosure mandates on firms' employment, average payroll, tangible assets, total revenue, total liabilities, and interest rates. Treat is an indicator equal to one for treated firms, and zero for control firms, as described in Section 5. Employment is the average number of employees in the firm-year. Average payroll is total annual payroll divided by employment. Tangible assets are the book value of tangible fixed assets, net of accumulated depreciation, and serve as our measure of capital stock. Total revenue is the sum of all revenue sources in a given year. Total liabilities are the book value of all current and long-term liabilities. Interest rate is interest expense divided by total debt.

Table B3: Diff-in-Diff Estimates After Dropping Similar-sized Firms Below Cutoff

	(1) Log(Employment)	(2) Log(Av. Payroll)	(3) Log(Tang. Assets)	(4) Log(Tot. Revenue)	(5) Log(Tot. Liabilities)	(6) Interest Rate
Treat x Post	0.1977***	0.2015***	0.2680**	0.4007***	0.2754***	-0.0053**
	(0.0555)	(0.0589)	(0.1041)	(0.0845)	(0.0960)	(0.0023)
Observations	6420	6420	6250	6380	6580	5170
Treated Firms	260	260	240	260	260	200
Control Firms	200	200	180	190	200	150
Adj. R Squared	0.941	0.943	0.907	0.908	0.917	0.425
Mean at t=-1 (treated)	784.13	4540.61	643719.50	501170.10	745430.40	0.027

Notes: This table examines the robustness of our results to dropping a subset of firms from the potential control group. Specifically, we drop "partially treated firms" identified as control firms in the same industry as treated firms near the cutoff whose revenue and profit margins are at least 90 percent of those of the treated firms. This table presents difference-in-differences estimates of the impact of GHG disclosure mandates on firms' employment, average payroll, tangible assets, total revenue, total liabilities, and interest rates. Treat is an indicator equal to one for treated firms, and zero for control firms, as described in Section 5. Employment is the average number of employees in the firm-year. Average payroll is total annual payroll divided by employment. Tangible assets are the book value of tangible fixed assets, net of accumulated depreciation, and serve as our measure of capital stock. Total revenue is the sum of all revenue sources in a given year. Total liabilities are the book value of all current and long-term liabilities. Interest rate is interest expense divided by total debt.

C Stock Market Reaction to Mandatory GHG Disclosure

In this appendix, we assess the stock market responses to the public disclosure of mandatory GHG emissions, focusing on the subset of publicly listed firms. The underlying idea mirrors our model. Publicly listed firms are mandated to file financial reports, but before the GHG disclosure, investors do not observe a firm's emission efficiency and instead infer it from noisy signals based on the firm characteristics contained in financial reports, such as revenues and operating costs. Mandatory GHG disclosure has two effects. First, it reduces investor uncertainty about emission efficiency, lowering the risk premium and the cost of capital for the disclosing firm. Second, it updates investor beliefs about the level of emission efficiency, revising those beliefs upward or downward. For firms revealed to be cleaner than the market had inferred, this mean correction is positive and pushes the cost of capital down further. For firms revealed to be dirtier than expected, the mean correction is negative and works against the uncertainty effect, and when it dominates, the cost of capital rises on net. We examine the average market reaction for treated firms relative to comparable non-disclosing firms, as well as a heterogeneity test for high versus low energy-efficiency firms.

We examine the federal reform cohorts, namely the 2004, 2009, and 2017 cohorts. A treated firm is a listed firm with at least one facility that first crosses the federal threshold in the reform year and therefore appears in the GHGRP dataset. We match each treated firm to a listed control firm using a propensity-score procedure similar to the one described in Section 5.1: one-to-one nearest-neighbor matching within a caliper, within the same two-digit NAICS industry and quartile bins of total revenue and profit margin, where the propensity score is estimated from a linear probability model with quadratics in total revenue, profit margin, and market value measured in the reform year. Control firms are listed Canadian firms that are not subject to mandatory federal disclosure as of the reform year, including never-treated and not-yet-treated firms.

We match our GHG dataset with Compustat and identify 155 treated first-time reporters over the three cohorts. Next, we drop 30 firms with missing variables required for matching, such as total revenue, profit margin, market value, and two-digit NAICS measured in the reform year. Out of these 125 remaining treated firms, we match 83 firms (with 66 percent match rate). The final sample consists of 83 treated firms matched to 76 unique control firms – a control firm can be matched in more than one cohort –, for 166 firm-cohort observations. Table C1 reports summary statistics on this matched sample of listed firms. Consistent with the main matched sample in Section 5.1, treated firms are somewhat larger than their matched controls on average: the average market value is \$4.0 billion for treated firms versus \$3.1 billion for controls, and the average revenue is \$3.5 billion versus \$2.0 billion.

The event date is April 28, 2006, for the 2004 cohort, February 8, 2011, for the 2009 cohort, and April 12, 2019, for the 2017 cohort. We measure abnormal returns using the market model, following Campbell et al. (1997) and Acemoglu et al. (2016b). The abnormal return for firm i on day t is

$$AR_{t,i} = R_{t,i} - \left(\hat{\alpha}_i + \hat{\beta}_i R_{m,t}\right),$$

where $R_{t,i}$ is firm i's actual return on day t and $R_{m,t}$ is the return on the market on day t. The parameters $\hat{\alpha}_i$ and $\hat{\beta}_i$ are estimated firm by firm from

$$R_{t,i} = \alpha_i + \beta_i R_{m,t} + \varepsilon_{t,i},$$

over a pre-event estimation window of 250 trading days ending 90 days before the event date, and we keep firms with at least 30 return observations in the estimation window. Although there is no generally agreed-upon length for the estimation window, a length of 250 days corresponds to roughly one year of trading and has been used in other studies (e.g., Acemoglu et al., 2016b; Jayachandran, 2006; Li and Lie, 2006). Next, for each firm, we calculate the cumulative abnormal return as follows,

$$CAR_{i,\tau} = \sum_{s=-42}^{\tau} AR_{i,s},$$

which we normalize to zero on the day before the event date ($\tau = -1$).

To study the market reaction to GHG disclosure, we estimate the following model:

$$CAR_{i,h} = \delta\, Treat_i + \mu_c + \varepsilon_{i,h},$$

where $CAR_{i,h}$ is firm i's cumulative abnormal return over the post-event window $[0, h]$, $Treat_i$ equals one for treated firms and zero for their matched controls, and μ_c denotes cohort fixed effects. We estimate the model separately for a short window ($h = 2$) and a longer window ($h = 14$), with one observation per firm-cohort, weighting by firm market value and clustering standard errors at the firm level. The coefficient δ measures the average post-event abnormal return of treated firms relative to their controls. Table C2 reports the results. Columns (1) and (2) show a positive and significant market reaction: treated firms earn an abnormal return of 1.7 percentage points over $[0, 2]$ and 5.9 percentage points over $[0, 14]$ relative to their controls. The magnitude of the estimated market reaction is comparable to prior research. For instance, Choy et al. (2025) study the market reaction to GHG disclosure in the U.S. and, in Panel C of their Table 1, report an average three-day raw return of about 1.1 percentage points around EPA GHGRP data releases.[15]

[15]Our market reaction is slightly larger (1.7% versus 1.1%). Choy et al. (2025) pool market reactions across all reporting dates from 2012 to 2024, and thus include firms beyond their first-time reporting. By contrast, we isolate firms' *first-time* mandatory disclosure, when the information content can be the largest.

Next, we examine the efficiency prediction of our model (Section 3). We split treated firms at the within-cohort median of energy efficiency, measured as total revenue divided by total emissions, into high- and low-efficiency groups. For each treated firm, we keep the same matched control as in the pooled sample and assign the control to the same efficiency group as its treated firm. Columns (3) and (4) report the results for high-efficiency firms and columns (5) and (6) for low-efficiency firms. The market reaction is concentrated among high-efficiency firms, which earn abnormal returns of 2.1 and 7.0 percentage points over the two windows, respectively. For low-efficiency firms, the market reaction is small and statistically insignificant.

We also plot the difference in CAR between treated firms and control firms over event time for the pooled sample and the two efficiency groups. Specifically, we estimate the difference in CAR between treated firms and control firms at each event day from a dynamic event-study regression of firm-level CAR on event-time indicators interacted with a treated dummy, fit by weighted least squares with market-value weights and standard errors clustered by firm. These estimated differences, together with their 95 percent confidence intervals, are what we plot in Figure C1. Panel (a) plots the treated-minus-control CAR for all treated firms. The difference is flat around zero before the event date, consistent with parallel pre-trends, and rises sharply after $\tau = 0$, with treated firms outperforming their controls; the difference persists through the end of the window rather than reversing. Panel (b) plots the treated-minus-control CAR for high-efficiency firms and Panel (c) for low-efficiency firms. The high-efficiency CAR jumps up after $\tau = 0$ and stays elevated with no subsequent reversal, whereas the low-efficiency CAR shows no comparable jump.

Table C1: **Descriptive Statistics on Treated and Matched Control Firms (Publicly Listed)**

NAICS		(1) Treated	(2) Control
Panel A: Descriptive Stats on Firms			
	Market Value (in million)	4,023.05	3,114.69
	Total Revenue (in million)	3,524.38	1,952.52
	Profit Margin	0.031	0.022
	Total Assets (in million)	5,575.82	4,020.65
	Number of Unique Firms	83	76
Panel B: Industry Composition (2-digit NAICS)			
21	Mining, quarrying, and oil and gas extraction	39	38
32	Manufacturing (wood product, paper, printing, chemical, and non-metallic mineral product)	18	13
33	Manufacturing (primary metal, computer and electronic product, and transportation equipment)	10	10
22	Utilities	6	5
31	Manufacturing (food)	4	4
48	Transportation and warehousing (pipeline)	2	2
56	Administrative and support and waste management and remediation services	2	2
45	Retail trade	1	1
51	Information and cultural industries	1	1

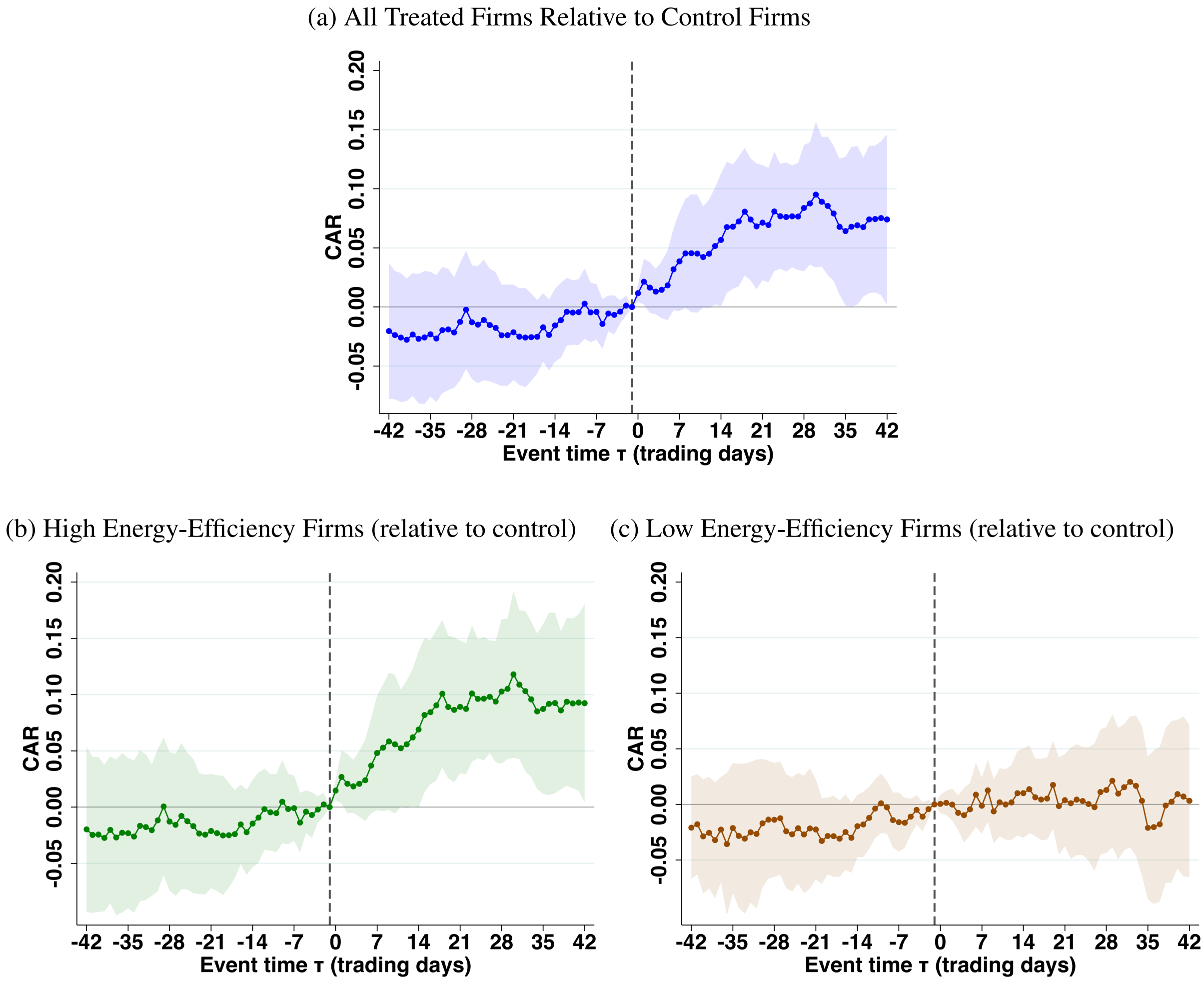
Notes: This table reports descriptive statistics for publicly listed treated firms and their matched control firms, for the market reaction sample pooling the 2004, 2009, and 2017 federal reform cohorts into a single sample. Panel A reports firm characteristics and Panel B reports the number of firms in each two-digit NAICS sector. Column (1) reports means for treated firms and Column (2) for matched control firms. Treated firms are matched one-to-one to control firms by nearest propensity score within two-digit NAICS industry and quartile bins of total revenue and profit margin, where the propensity score is estimated from a linear probability model with quadratics in total revenue, profit margin, and market value. Market Value is the firm's market value of equity. Total Revenue is the sum of all revenue sources in the year. Profit Margin is pre-tax income divided by total revenue. Total Assets is the book value of total assets. Market Value, Total Revenue, and Total Assets are in millions of dollars. All variables are measured in the reform year (the matching year). Because some control firms are matched across more than one cohort, the number of unique control firms (76) is smaller than the number of treated firms (83).

Table C2: **Cumulative Abnormal Returns Around Mandatory GHG Disclosure**

	All firms		High efficiency		Low efficiency	
	(1)	(2)	(3)	(4)	(5)	(6)
	CAR[0,2]	CAR[0,14]	CAR[0,2]	CAR[0,14]	CAR[0,2]	CAR[0,14]
Treat	0.0172*	0.0586**	0.0209*	0.0699**	0.0032	0.0179
	(0.0094)	(0.0274)	(0.0115)	(0.0339)	(0.0072)	(0.0188)
Observations	166	166	80	80	86	86
Treated Firms	83	83	40	40	43	43
Control Firms	83	83	40	40	43	43
Adj. R Squared	0.245	0.148	0.269	0.163	0.123	0.069

Notes: This table reports the stock market reaction to mandatory GHG disclosure. The dependent variable is the cumulative abnormal return, $CAR[0,n]$, defined as the sum of a firm's abnormal returns from the event date through n trading days afterward. The event date (day 0) is the GHG disclosure date for each of the three reforms in our sample, the 2004, 2009, and 2017 federal reforms, which we pool into a single sample. There is one observation per firm-cohort. Treated firms are matched one-to-one to control firms by nearest propensity score within two-digit NAICS industry and quartile bins of total revenue and profit margin, where the propensity score is estimated from a linear probability model with quadratics in total revenue, profit margin, and market value. $Treat$ is an indicator equal to one for treated firms and zero for their matched controls. Columns (1) and (2) use all firms; columns (3) and (4) restrict to high energy-efficiency firms and columns (5) and (6) to low energy-efficiency firms, where treated firms are split at the within-cohort median of energy efficiency and each matched control is assigned to its treated firm's group. Abnormal returns are computed from the market model, defined as the firm's return less its expected return from a market-model regression estimated over a pre-event window of 250 trading days ending 90 days before the event date. All regressions are weighted by firm market value and include cohort fixed effects. Standard errors, in parentheses, are clustered at the firm level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Figure C1: **Stock Market Reaction to Mandatory GHG Disclosure**



Notes: This figure plots the stock market reaction to mandatory GHG disclosure. Each panel plots the difference in cumulative abnormal returns (CAR) between treated firms and their matched controls at each event day, normalized to zero on the day before the event date ($\tau = -1$), which the vertical dashed line marks. The event date (day 0) is the GHG disclosure date for each of the three reforms in our sample, the 2004, 2009, and 2017 federal reforms, which we pool into a single sample. Treated firms are matched one-to-one to control firms by nearest propensity score within two-digit NAICS industry and quartile bins of total revenue and profit margin, where the propensity score is estimated from a linear probability model with quadratics in total revenue, profit margin, and market value. Panel (a) plots the treated-minus-control CAR for all firms; Panel (b) restricts to high energy-efficiency firms and Panel (c) to low energy-efficiency firms, where treated firms are split at the within-cohort median of energy efficiency and each matched control is assigned to its treated firm's group. Abnormal returns are computed from the market model, defined as the firm's return less its expected return from a market-model regression estimated over a pre-event window of 250 trading days ending 90 days before the event date. The treated-minus-control difference at each event day is estimated from a dynamic event-study regression weighted by firm market value. The shaded area shows 95 percent confidence intervals from standard errors clustered at the firm level.