Impacts of Mandatory Greenhouse Gas Disclosure on Firms

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Abstract

This paper studies the effects of mandatory Greenhouse Gas (GHG) emissions disclosure on firms' investment, employment, and subsequent growth. We exploit the 2004 federal mandatory disclosure policy implemented in Canada, along with federal and provincial changes in reporting thresholds in later years, to define first-time reporting firms within two years of each unanticipated reform as the treated group. Following disclosure, treated firms increase employment and capital investment relative to matched control firms below the threshold, accompanied by rises in total expenses, revenue and profitability. With employee-level data merged with firm-level data from tax records, we show that incumbent workers experience increases in their annual earnings and retention rates. 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 on firms' environmental risks and relaxing financing constraints. Our findings suggest that government-mandated environmental disclosure can induce investment by lowering firms' cost of capital despite compliance costs.

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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 subsequent growth. This paper examines how mandating greenhouse gas (GHG) disclosure affect firms' financing, investment, and employment decisions and ultimately allocative efficiency within and across industries.

The effects of mandatory 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, in the absence of disclosure, 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 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.

[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).

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 from 2001 to 2021. Combining this variation with matched employer–employee tax records covering the universe of firms and workers allows us to identify the effects of mandatory GHG disclosures on firms' investment and performance, as well as workers' outcomes. 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 19.8 log points, 20 log points, 27.3 log points, and 28.4 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 28.2 log points and 1 percentage points on average relative to their matched control firms. In terms of their financing, we find that affected firms experience a 1.9-percentage points reduction in average interest rates on their long-term debts, and 23.6 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. Since the non-disclosing firms can be also affected by disclosure mandates in the general equilibrium, we plan to use our model to simulate the investment responses and estimate the aggregate impacts of mandatory GHG disclosures on the aggregate investment, growth, and productivity accounting for potential spillover effects.

Furthermore, we examine the effects of GHG disclosure regulations on worker-level outcomes. Our findings show that mandatory GHG disclosure leads to increased earnings of 15.5 log points for workers at treated firms, relative to matched workers at control firms. Additionally, the probability of treated workers moving to another firm declined by 11.5 percentage points relative to control workers, suggesting higher employee retention.

Our results are consistent with the idea that mandatory disclosure of GHG emissions influences firms' investment decisions by affecting their cost of capital. GHG disclosure regulations can provide investors with comparable, high-quality information, 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 financing constraints using firms' listing status, building on the idea that publicly listed companies generally have better access to external capital than private firms. Consistent with this prediction, we find that private firms exhibit larger increases in employment and sales and a larger reduction in interest rates compared to listed 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 versus less 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 is: 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 four main contributions. First, we contribute to the growing literature on the effects of mandatory GHG disclosures on firms by leveraging a credible research design and using detailed balance sheet data of both private and listed firms to study a wide range of outcomes. 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), 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. Similar to Tomar (2023), our study focuses on regulatory changes that affected facilities without any emission information before the regulation. We advance this literature by exploiting multiple regulatory changes within the same country and comparing affected firms to matched control firms, thereby addressing potential concerns about concurrent events that may confound cross-country comparisons.

Second, we contribute to the literature studying how information disclosure affects firms' cost of capital and investment (Stein, 2003; Roychowdhury et al., 2019). Existing evidence shows that investors incorporate firms' ESG practices and greenhouse gas (GHG) emissions into capital allocation decisions, reflecting both social preferences and exposure to climate-related risks (Riedl and Smeets, 2017; Bauer et al., 2021; Flammer et al., 2021; Ilhan et al., 2023). Related work also examines how emissions levels are priced as transition risk in financial markets.[2] 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. By documenting how disclosure-induced changes in the information set translate into investment responses, our paper complements Bolton and Kacperczyk (2021a), which focuses on financial pricing effects. Instead, we highlight disclosure as a mechanism with real economy-wide consequences.

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

[2]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.

corporate social responsibility enhances employee retention and engagement. We complement these studies by leveraging multiple regulatory changes to assess how *disclosure* of GHG emissions affects workers' earnings and job transitions.

Fourth, our paper relates to the broader literature on the effects of environmental policies on firm- and employee-level outcomes. Prior research examines the consequences of market-based policies, including cap-and-trade programs and carbon taxes, as well as command-and-control policies (Fowlie et al., 2012, 2016; Walker, 2013; Acemoglu et al., 2016; 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.

2 Institutional Setting

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

To develop a harmonized and efficient mandatory greenhouse gas reporting system, the federal Greenhouse Gas Reporting Program was established in March 2004 by the Department of Environment and Climate Change Canada (ECCC) under the authority of Section 46 of the Canadian Environmental Protection Act.[3] Starting in 2004, all facilities located in Canada emitting 100 kt or more of GHGs in CO2 eq per calendar year were required to report their emissions to the ECCC.[4]

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

[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, click the link here.

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. Between 2004 and 2021, about 8 percent of facilities reporting under the GHGRP voluntarily disclosed emissions, accounting for only 0.3 percent of the total emissions reported. The GHGRP provides significant information about GHG emissions in Canada. For instance, 1,732 facilities reported an aggregated amount of 285 million metric tonnes (MT) CO2 eq in 2021, representing 41 percent of Canada's total GHG emissions.[6]

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

The ECCC publishes data obtained from facility reports, which include information about the facility's name; unique ID; address; the reporting year, and the name; address; and business number of both the reporting company and its parent company. Furthermore, the data contains information on emission levels for 23 gases, broken down by the gas type, such as carbon dioxide (CO2).

In addition to the federal GHGRP, several provinces in Canada have regulations requiring facilities to monitor and report information on their GHG emissions to provincial governments. Most provincial reporting thresholds were greater than or equal to the federal threshold. However, three provinces had a reporting threshold below the federal threshold. Specifically, the reporting threshold in Ontario was 25 kt starting in 2010 and decreased to 10 kt in 2016. Both British Columbia and Quebec began publishing GHG emission data in 2012, with a reporting threshold of 10 kt.[7]

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

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

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

[7]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 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.

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

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

In 2008, British Columbia implemented a revenue-neutral carbon tax on the purchase and use of fossil 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 which emitted more than 100 kilotonnes of CO2 per year to reduce their emissions by 12 percent. These environmental regulations across three major provinces raised the awareness among investors to consider transition risks when deciding which Canadian firms to invest in, making carbon emissions information more valuable.

3 Model

Environmental information disclosure affects firms through the information available to financial markets. Investors typically do not observe firms' emission efficiency or emissions intensity directly and instead rely on coarse outcomes such as firm size or sales 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 emissions 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 frictions into a standard production and demand structure, the model clarifies how the disclosure

reallocates capital toward cleaner firms through informational 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 is denoted $\alpha, \alpha_e \in (0,1)$. 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 share α of total cost, and variable cost is 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 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

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

The conditional variance is

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

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 emissions 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

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

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 prof-

its 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.

4 Data and Outcome Variables

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

4.1 Greenhouse Gas Emission Disclosure Data

We obtain our data from the ECCC, which publishes facility-level GHG emissions data on Canada's Open Government webpage. We refer to this dataset as the GHG dataset, which includes GHG emission levels. To ensure compliance with Statistics Canada's guidelines, we converted the emission levels into bins, with each bin containing a minimum number of observations. This transformation prevents the identification of individual firms after Statistics Canada merges the GHG dataset with the Canadian Employer-Employee Dynamics Database (CEEDD). As a result, we do not have access to precise emission levels but only to emission ranges (e.g., 10 kt–15 kt). In addition, the GHG dataset contains the facility's name; unique ID; and address, and the name and address of the company that owns and operates the facility as of December 31 of the calendar year ("reporting company"). Importantly, the dataset also includes the business number of the reporting company, which can be used to merge the GHG dataset with the CEEDD. The business number is a nine-digit number the Canada Revenue Agency assigns to companies for tax-related purposes. To maximize the number of high-quality merges, we manually verify the accuracy of business numbers and augmented the dataset by identifying and adding missing business numbers for reporting firms. Specifically, approximately 90 percent of reporting companies in the GHG dataset include a business number. To ensure accuracy, we verify each business number by cross-referencing it with Canada's Business Registries.[8]

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

4.2 Employer-Employee Matched Data

For our analyses, we use the Canadian Employer-Employee Dynamics Database (CEEDD), a matched employee-employer dataset that covers the universe of workers and companies in Canada from 2001 to 2021. This database is based on both individual (T1) and corporate (T2) tax return

[8]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 CEEDD. If no match is found, we then use the company name, province, and city from the GHG dataset—and, if necessary, the postal code—to identify the corresponding company in the CEEDD.

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.

The main outcome variables used in the firm-level analysis are average payrolls, employment, tangible assets, total expenses, total revenue, returns on assets, interest rates, and total liabilities. Average payrolls are defined as total payrolls divided by the number of workers. Employment is defined as the average number of employees reported in the T4s. We use the reported book value of tangible assets, net of depreciation, as a measure of capital stock. We compute the amount of depreciated tangible assets in each year based on yearly changes in accumulated depreciation from their balance sheets. Total revenue and total expenses are the sum of all revenue and expense amounts in a given year, respectively. Returns on assets are defined as the company's net income divided by total assets. Total liabilities are defined as the sum of all current and long-term liabilities. Interest rates are computed as total interest and bank charges divided by total long-term liabilities, which capture how much a company is paying in terms of interest payments per dollar of long-term debts in a given year.

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

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

5 Empirical Strategy

This section describes our empirical design and provides descriptive statistics on our matched sample. To define a treated firm, we take the following procedure. As described in Section 2, there have been three federal-level policy changes regarding the GHG disclosure. Under the first reform, implemented in 2004, we identify a firm as a treated firm if at least one of its establishments emitted more than 100 kt GHG in 2004 and therefore appears in our 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. 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. More importantly, given the time gap since the announcement date, these firms had some time to adjust their emission levels, making the reform "less exogenous" to these 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.[9]

Under the second reform in 2009, we identify a firm as treated if at least one of its establishments emitted more than 50 kt GHG in 2009 or in 2010. Among these treated firms, some also become treated at the provincial level (in 2010, Ontario or in 2012, British Columbia or Quebec). Furthermore, most of the firms treated in 2004 continue to report their emission level afterward. For these firms that either become treated again at the provincial level or continue to report their emission at the federal level, we focus on their first event of disclosing this information. 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 (i.e., at least one facility in Ontario disclosed GHG emissions for the first time in 2010), we include them as part of the treated group in the 2009 federal reform. However, we drop firms treated by the 2007 Quebec reform and the 2010 British Columbia reform because GHG data for these provinces are only available 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

[9] 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.

difference-in-differences design by estimating a regression of the following form:

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

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. 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, industry, and province (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. In Section 7.1, we present separate results for private firms and for publicly listed firms, where we find much larger responses among private firms.

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

$$y_{it} = \sum_{k=-4}^{4} \beta_k \mathbb{1}(t_i = t^* + k) \times Treat_{ij} + \tau_t + \omega_i + \mu_{it} \tag{10}$$

where y_{it} is an outcome variable for incumbent worker i at year t, $Treat_{ij}$ is an indicator for a worker i at treated firm j, ω_i are worker fixed effects, and all other variables are defined as in Equation (9). The standard errors are two-way clustered at the worker and firm level.

The key identifying assumption is that outcomes for treated firms and workers and for control firms and workers would have trended similarly in the absence of the policy change. A potential concern is that outcomes of firms above the regulation threshold and, therefore, disclose GHG emissions may evolve differently relative to those of firms below the threshold and do not reveal this information. One way to determine the direction of the potential bias is by comparing outcomes for treated firms and workers to the control firms and workers in the years prior to the policy change. Parallel pre-trends in firm-level and worker-level outcomes help alleviate the aforementioned concerns. While verifying parallel pre-trends is reassuring for a causal interpretation, contemporaneous shocks that occur with the reform could still bias our results. For example, firms affected by the policy may be concentrated in a particular industry (i.e., 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 province and exploit within-industry and within-province variation in treatment. We describe how we match our treated firms with control firms in the next subsection.

5.1 Matched Samples

Before performing a matching procedure between treated firms and potential control firms, we make the following restrictions. First, we drop a small share of firms that have missing observations for the key variables used for matching, such as total assets, total revenue, average payrolls, and profit margins (total revenue minus total expenses divided by total revenue), measured one year prior to the reform. We then match each firm one year prior to reform to a control firm in the same province and 4-digit NAICS industry. A firm is a potential control firm for a treated firm j if: (1) the firm is below the reporting threshold (and therefore do not disclose GHG emissions) within the first two years of the reform, and (2) the firm is in the same decile bin of total assets, total revenue, average payrolls, and profit marings one year prior to the reform of firm j.[10] 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 marings, 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 70 percent of all treated firms. Choosing one counterfactual control firm per treated firm in a given year ensures that the treated and control groups are comparable on the matched variables. We construct an unbalanced panel of firms that extends 4 years prior to and 4 years after the reform.

To construct the worker-level sample, we extract all workers who were continuously employed in the matched firms during the three years prior to the reform. This tenure restriction is chosen to obtain a sample of workers with attachment to the treated firms and is similar to tenure restrictions used in the mass layoff literature (Jacobson et al., 1993; von Wachter et al., 2009; Lachowska et al., 2020). Additionally, we restrict workers to have at least 4,000 CAD in annual earnings and drop

[10] 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.

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

5.2 Descriptive Statistics

We close this section with descriptive statistics of our analysis sample. Panel A of Table 1 shows the averages for key variables across firms, comparing treated firms to the set of matched control firms one year before the event. On average, treated firms are 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 much 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 78 percent of our firm sample).

Panel C of Table 1 shows average worker characteristics in our analysis sample one year before the event. Annual earnings are 68,739 CAD and 68,244 CAD among workers at treated firms and workers at the matched control firms, respectively. Therefore, annual earnings of treated workers are comparable to those of control workers on average. The age and gender compositions are similar between treated workers and their matched control workers.

6 Results

This section reports estimation results from the difference-in-differences models in Section 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 average payroll, employment, capital stock (measured using tangible assets), 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 (e.g., higher material inputs, payroll-related costs, and other operating costs needed to support business expansions).

Table 2 reports the corresponding difference-in-differences estimates. The coefficients imply that, relative to matched controls, treated firms increase average payroll by 20.0 log points, employment by 19.8 log points, tangible assets by 27.3 log points, and total expenses by 28.4 log points in the post period. Overall, these results suggest that mandatory GHG disclosure leads to economically large increases in firm hiring and investment after the reform.

6.2 Sales and Profitability

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). Across 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 treated firms increase total revenue by 28.2 log points in the post period. Column (2) shows that treated firms experiences a one percentage-points increase in ROA (albeit statistically not significant). Overall, these results suggest that mandatory GHG disclosure is associated with economically large increases in firm sales and profitability.

6.3 Worker-level Earnings and Job Transitions

Our firm-level results highlight that disclosing GHG emissions after the federal regulation facilitates growth for the affected firms. Before we explore potential mechanisms underlying these results, we examine whether the increased labor demand also led to higher wages for the work-

ers at treated firms. To estimate how the GHG disclosure policy affects worker earnings, we use employee-level data to track the same workers over time and control for changes in worker composition within firms. In Figure 4, we plot $\hat{\beta}_k$ estimated from Equation (10) for workers' log annual earnings. As Panel (a) of this figure shows, earnings of treated workers trended similarly with those of control workers in the years prior to the policy change, but increased after the reform. Similarly, Panel (b) shows a decrease in treated workers' job transitions after the reform.

Table 4 reports the corresponding difference-in-differences estimates. Column (1) shows that treated workers' annual earnings increased by 15.5 log points relative to those of control workers after the reform. Column (2) shows that the probability of moving to another firm decreased by 11.5 percentage points for treated workers relative to control workers, suggesting higher retention.

7 Economic Interpretations and Potential Mechanisms

In this section, we explore potential mechanisms behind the responses of firms affected by the GHG disclosure policy.

7.1 Information Frictions and Cost of Capital Channel

As described in Section 3, mandatory disclosure of GHG emissions can lead to higher corporate investment of disclosing firms by lowering the information frictions and thereby lowering the cost of capital. The model suggests that disclosing this information can also increase the cost of capital if it contains a net negative signal (i.e., 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 frictions for a given quality of the signal on average.

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 frictions, we would expect the firm to more easily raise capital at lower costs, which would lead to an increase in debt financing (via borrowing from banks). We examine 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 mandates, we observe an increase in total liabilities for treated firms relative to matched control firms. We also observe a decline in interest rates, although the estimates are not statistically significant. Columns (3) and (4) of Table 3 show that total liabilities increase by 23.6 log points and interest rates decrease by 1.9 percentage points for treated firms relative to control firms, on average, after mandatorily disclosing GHG emissions. These results are consistent with the predictions from the model that disclosing GHG emissions can result in an overall reduction in the cost of capital, leading treated firms to increase their debt financing.

Another testable prediction from the model is larger responses among firms that were more financially constrained prior to the reform. To proxy for financial constraints, we use the distinction between private firms and publicly listed firms, building on the idea that listed firms typically have better access to external capital and therefore face fewer financing constraints. We use this proxy because financing constraints are difficult to measure directly (Farre-Mensa and Ljungqvist, 2016), whereas listing status is directly observed in our data. Furthermore, information frictions are generally less severe among listed companies because they are required to publicly disclose financial and operational information frequently, and extensively, making it easier for investors to infer their emission efficiency prior to the reform. We split the sample into publicly listed and private firms and estimate our difference-in-differences model separately for each subsample. Table 5 reports estimates for key firm-level outcomes separately for listed and private firms. We find larger and statistically significant increases in average payroll, employment, and total revenue, as well as a larger decline in interest rates, among private firms relative to listed firms. These heterogeneous responses by listing status suggest that reducing information frictions—and thereby lowering the cost of capital—is likely a key mechanism behind our firm-level results.

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 only create this measure 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. We find larger and statistically significant increases in average payroll, employment, and total revenue, as well as a larger decline in interest rates, among firms with high energy efficiency. These heterogeneous responses are consistent with a mechanism in which mandatory disclosure reduces information frictions about firms' emissions, lowers the cost of capital more for high energy efficiency firms, and thereby facilitates their growth, while generating weaker responses among low energy efficiency firms.

Figure 5 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. Note that to be part of this sample, firms have to continue to report their emission; if they stop reporting it, that means they are below the current threshold and their energy efficiency cannot be measured in that year. For the first cohort treated by the 2004 reform (dark solid line), we see that the energy efficiency gradually increases over time since 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 the cost of capital.

7.3 Why Do Firms Not Voluntarily Disclose GHG Emissions?

In our sample, the share of firms that voluntarily report their GHG emissions is small (i.e., just under 8 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. We next discuss three key reasons why firms may not provide voluntary GHG disclosure.

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

Second, 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 can impose 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 the same level of verification or enforcement, which can limit their credibility and usefulness to investors.

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 GHG performance *relative* to their peers (Tomar, 2023). To the extent that investors compare a firm's GHG emissions relative to its peers, mandatory disclosure reduces firms' uncertainty about investor preferences and responses to GHG disclosure. In the absence of disclosure regulation, such uncertainty may lead firms to lower their voluntary disclosure.

Fourth, disclosures can have direct and proprietary costs. GHG disclosures impose significant direct costs (i.e., compliance costs) on firms. Such costs include the costs related to developing or maintaining systems for tracking, measuring, and aggregating GHG emissions as well as the costs related to preparing and presenting the resulting disclosures (reporting costs). More importantly, disclosure can lead to substantial managerial opportunity costs. Since a large portion of these costs are fixed, GHG disclosures can be particularly burdensome for smaller firms. Furthermore, GHG disclosure can reveal commercially sensitive or proprietary information to competitors, which can put firms at a competitive disadvantage (Bhattacharya and Ritter, 1983; Verrecchia, 1983). Specifically, as discussed in Section 2, the GHG Reporting Program requires firms to disclose detailed information about the location of their facility as well as detailed information on GHG emission levels for 23 gases, broken down by gas type. Such information can reveal the location of a firm's operations, its production level in each location, and its emission efficiency. Such information, combined the firm's financial reporting such as sales and profitability, can help competitors identify profitable investment opportunities, which can, in turn, affect entry and competitors' output at the expense of the disclosing company. In addition, competitors might clean insights into the disclosing firm's manufacturing processes from the breakdown of gases composing a company's emissions.[11]

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

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

will be stronger especially among firms that cannot scale back their production to reduce aggregate GHG emissions. To test this potential channel, we plan to examine whether treated firms increased their investment in green technology using a dataset on investment across different asset classes. Note that this mechanism does not necessarily contradict the cost-of-capital channel and, in fact, can be complementary. For example, disclosing GHG emissions can lower the cost of capital, which can encourage firms to invest in tangible assets, including green assets.

8 Conclusion

In this paper, we study the effects of mandatory GHG disclosure on firms' investment, employment, and capital structure, as well as their workers' labor market outcomes. Specifically, we leverage a series of regulatory disclosure changes that affected different groups of firms over time in Canada, and the administrative matched employer-employee data to identify firms that were above the 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 age bins, province, and industry, which were below the reporting threshold and, therefore, did not GHG emissions.

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

Overall, our results suggest that mandatory disclosure of GHG emissions affects corporate investment by providing investors with relevant information to assess disclosing firms' environmental risk, 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, building on the idea that publicly listed firms generally have better access to external capital than private firms, we use listing status as a proxy for financing constraints. We find that the effects are stronger for private firms than for public firms, suggesting that disclosing GHG emissions can lower the cost of capital by reducing information frictions. 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 versus less efficient, investors may update their beliefs favorably for high-efficiency firms and unfavorably for low-efficiency firms, resulting in lower (higher) expected returns (i.e., cost of capital) for high-efficiency (low-efficiency) firms. Consistent with these predictions, we find that more energy-efficient firms experience larger increases in employment and sales and a larger reduction in interest rates compared to less energy-efficient firms. Overall, these heterogeneous effects support the cost-of-capital channel.

We extend to the growing literature on the effects of mandatory GHG disclosures on firm-level outcomes by leveraging a credible research design and using detailed balance sheet data of both private and listed firms to study a wide range of outcomes. Our paper also relates to the extensive literature examining the impact of information disclosure on the cost of capital and corporate investment by providing new evidence on the effects of GHG disclosure. Our results indicate that disclosing GHG emissions not only benefits investors in their capital allocation and hedging decisions but also facilitates growth by reducing the cost of capital. Overall, our findings imply that GHG disclosure, mandated and enforced by the government, may bring net benefits to disclosing firms, despite the associated regulatory and private costs.

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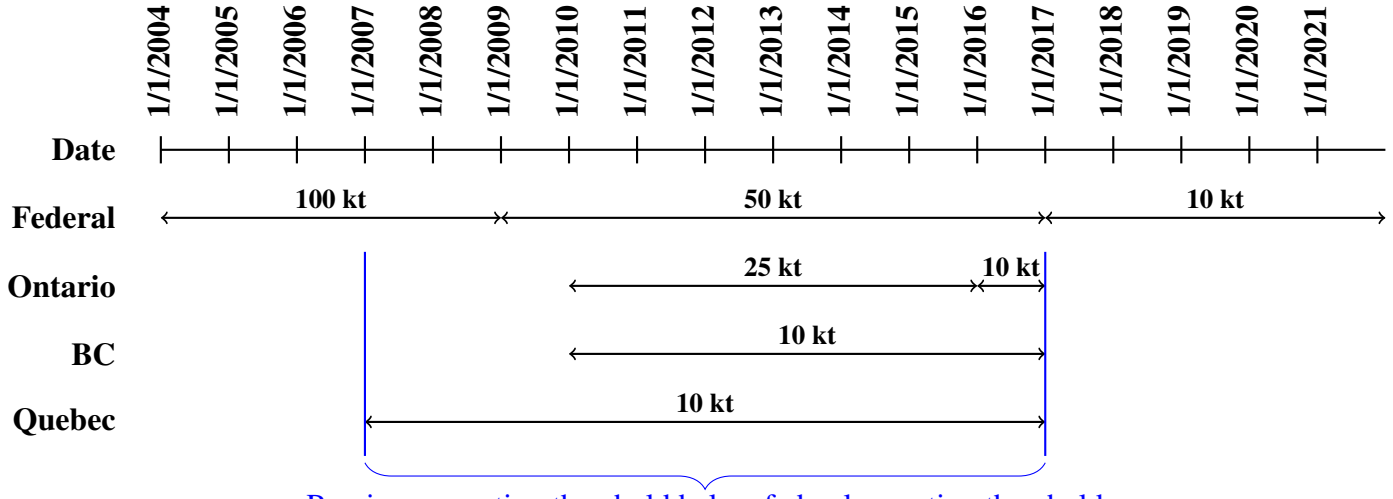
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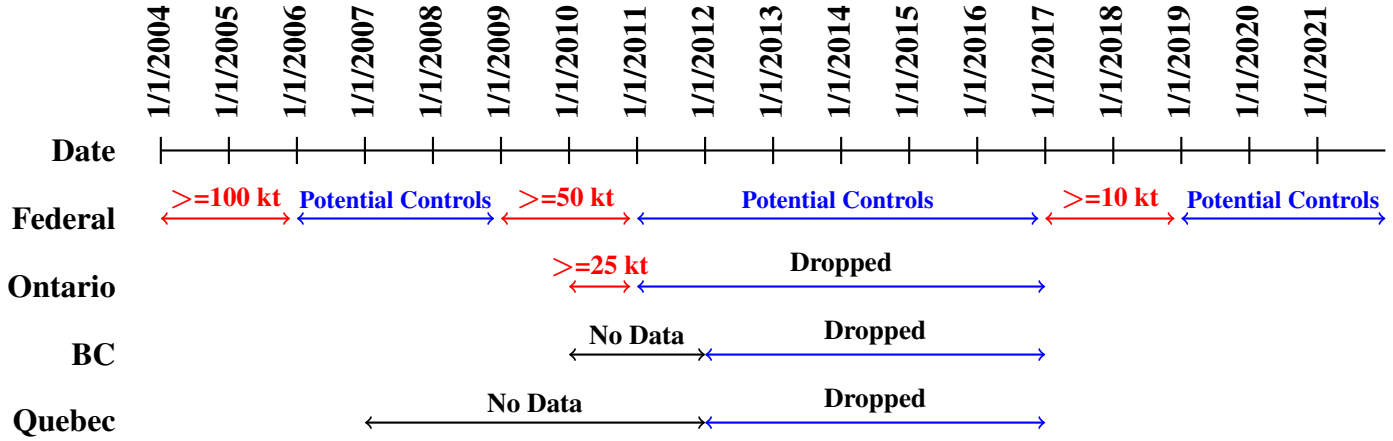
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Figure 1: **Changes in Reporting Threshold over time and Policy Variation**

(a) Changes in GHG Reporting Threshold Over Time



(b) Diagram Explaining the Policy Variation



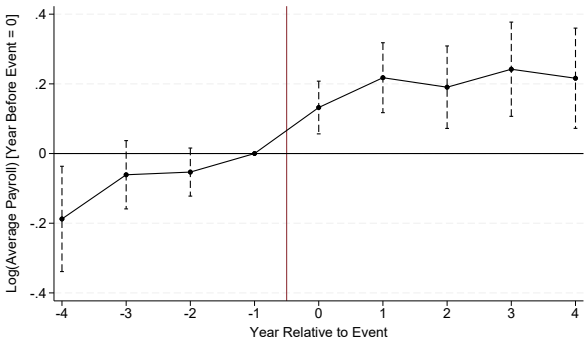
Notes:Panel A shows the emissions-reporting thresholds under the federal Greenhouse Gas Reporting Program (GHGRP) as well as under provincial GHG reporting regimes when a province's threshold is below the federal threshold (Ontario, British Columbia, and Quebec). Panel B summarizes how we identify treated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

Treated firms are those that disclose facility-level GHG emissions for the first time because at least one facility exceeds the relevant threshold in the implementation year of a threshold change or the subsequent year. Specifically, firms that cross the federal threshold (100 kt in 2004–2005, 50 kt in 2009–2010, 10 kt in 2017–2018) and firms that cross the Ontario threshold (25 kt in 2010). We focus only on firms that are treated for the first time. That is, if a firm was already subject to disclosure requirements in an earlier regulatory change and later exceeds a lower emissions threshold in a subsequent regulatory change, it is not considered newly treated in that later period. We exclude firms that report their GHG emissions voluntarily from our treated firms.

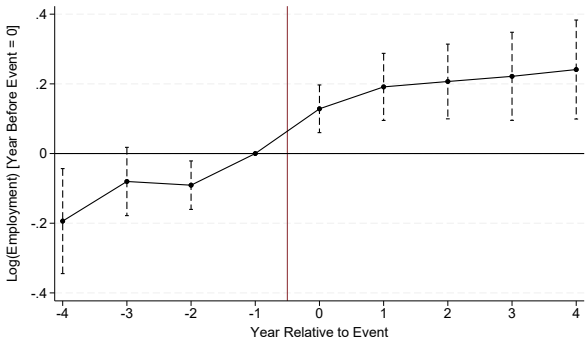
We match treated firms to control firms. The pool of potential control firms includes (i) never-treated firms, i.e., firms that are below the applicable reporting threshold and do not disclose within the first two years after each reform, and (ii) firms that cross the threshold only later (2006–2008; 2011–2016; 2019–2021). We do not use the second group as treated and use only as potential controls because these later reporters have more time between the announcement and their first disclosure, and thus their emissions (and other outcomes) may reflect endogenous adjustment to the policy. We exclude voluntary disclosers from the control pool and drop British Columbia and Quebec due to the long lag between mandate announcements and the first year of publicly available provincial data (2012), which raises anticipation concerns.

Figure 2: Firms' Average Payrolls, Employment, Investment, and Expenses

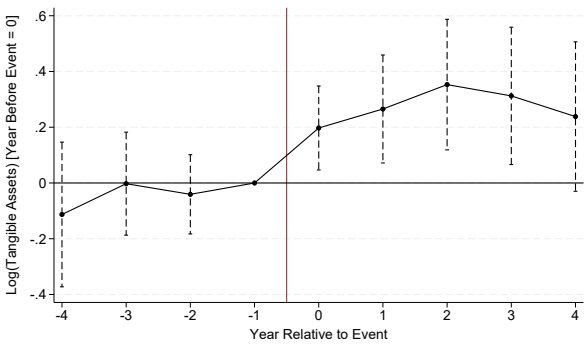
(a) Log(Average Payrolls)



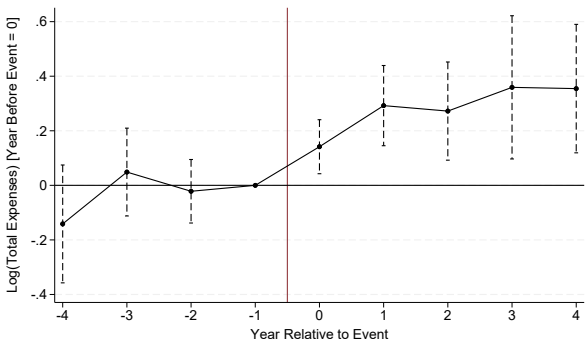
(b) Log(Employment)



(c) Log(Tangible Assets)



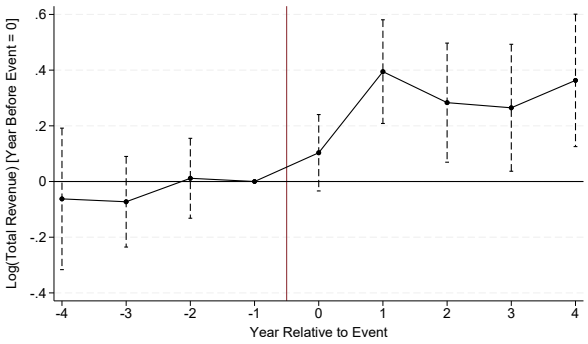
(d) Log(Total Expenses)



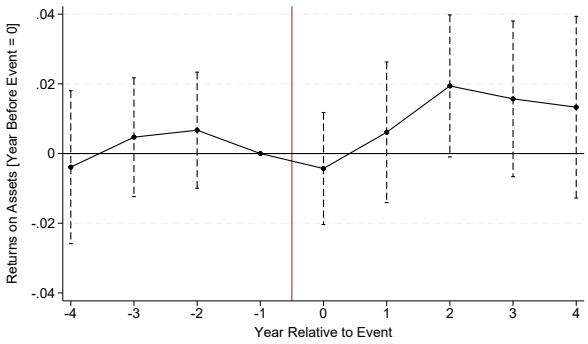
Notes: These figures present event-study estimates of the impact of GHG disclosure mandates on average payroll, employment, tangible assets, and total expenses. Average payroll is measured as total annual payroll divided by the average number of employees in the firm-year. Employment is measured as the average number of employees reported in employee tax records and Record of Employment data. 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: Firms' Revenue, ROA, and Capital Structures

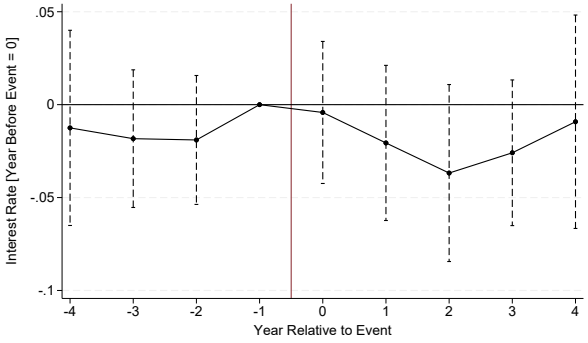
(a) Log(Total Revenue)



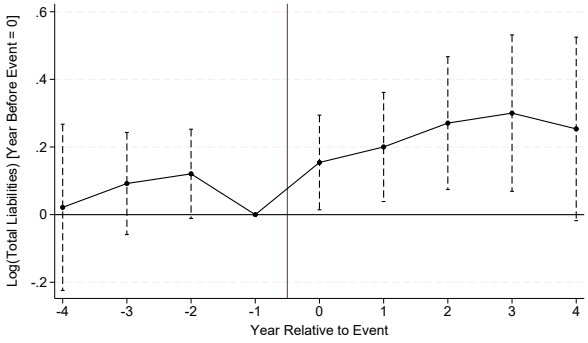
(b) Return on Assets



(c) Interest Rate



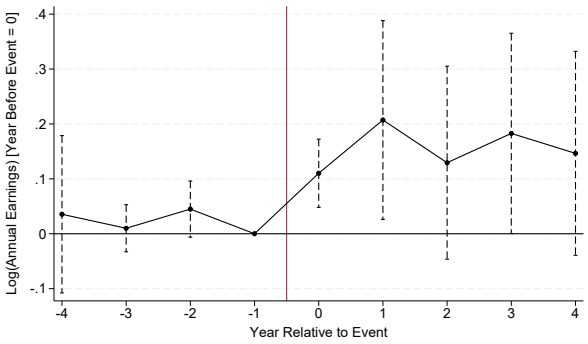
(d) Log(Total Liabilities)



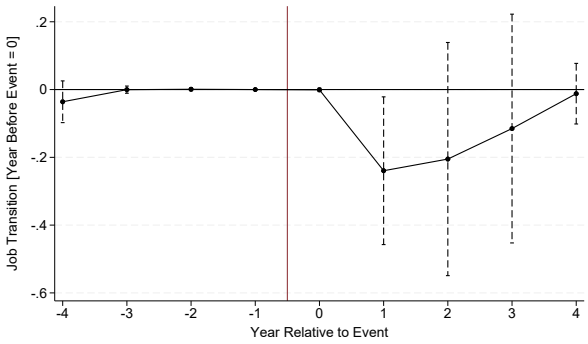
Notes: These figures present event-study estimates of the impact of GHG disclosure mandates on total revenue, return on assets, interest rate, and total liabilities. Total revenue is the sum of all revenue sources in a given year. Return on assets (ROA) is calculated as net income divided by total assets. 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: Worker-level Outcomes

(a) Log(Annual Earnings)

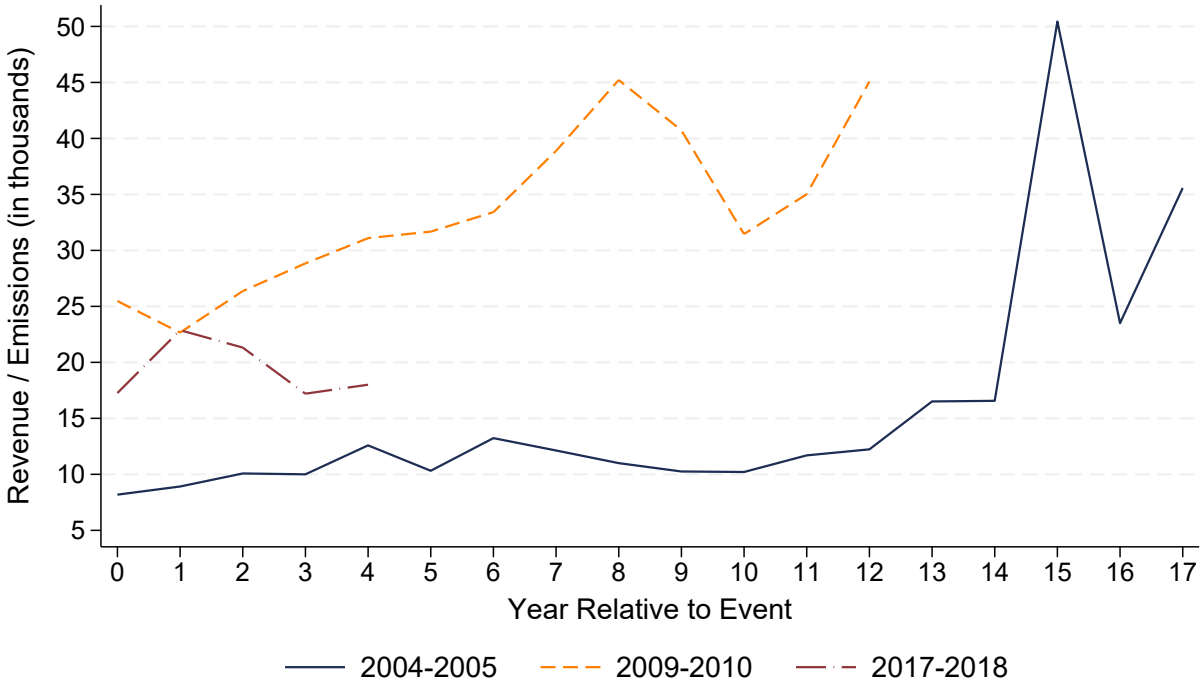


(b) Job Transition



Notes: These figures present event-study estimates of the impact of GHG disclosure mandates on worker-level outcomes, including annual earnings and job transitions. Annual earnings are measured as total labor earnings in a given year, aggregated across all employers. Job transition is an indicator equal to one if a worker's employer in a given year differs from their employer in the previous year. The dashed vertical lines represent 95 percent confidence intervals, with standard errors clustered at the firm and worker levels. We stack several policy changes and estimate a single difference-in-differences model (more details in Section 5). The policy change is implemented in year 0, and the coefficient is normalized to zero in year -1.

Figure 5: **Energy Efficiency over time**



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 and Workers (at $t = -1$)**

	(1) Treated	(2) Control
Panel A: Descriptive Stats on Firms		
Average Payroll (in thousands)	4,753.56	1,288.56
Employment	800.165	237.337
Tangible Assets (in thousands)	604,841.88	141,111.65
Total Expenses (in thousands)	512,037.54	133,154.02
Total Revenue (in thousands)	586,496.45	151,198.62
Returns on Assets	0.028	0.052
Interest Rates	0.059	0.077
Total Liabilities (in thousands)	726,204.73	210,502.69
Number of Unique Firms	210	195
Panel B: Sector Composition		
Agriculture, forestry, fishing and hunting	0.03	0.03
Mining, quarrying, and oil and gas extraction.	0.31	0.31
Manufacturing	0.47	0.45
Others	0.21	0.20
Panel C: Worker Characteristics		
Earnings	68,739.53	68,244.27
Age	45.918	45.617
Male	0.85	0.851
Number of Unique Workers	13185	13180

Notes: This table reports descriptive statistics on firms and workers, measured one year prior to the reform ($t = -1$). Panel A reports firm characteristics. Panel B reports the sector composition of firms. Panel C reports worker characteristics. Columns (1) and (2) report statistics for treated and matched control firms (Panels A and B) and workers (Panel C), respectively. 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. Worker earnings are total annual labor earnings, aggregated across all employers in a given year. Age is measured in years. Male is an indicator equal to one for male workers.

Table 2: **DiD Estimates on Firms' Average Payrolls, Employment, Investment, and Expenses**

	(1) Log(Ave. Payroll)	(2) Log(Employment)	(3) Log(Tangible Assets)	(4) Log(Total Expenses)
Treat × Post	0.1997***	0.1978***	0.2733***	0.2840***
	(0.0506)	(0.0478)	(0.0963)	(0.0779)
Adj. R Squared	0.935	0.939	0.896	0.897
Mean at t=-1 (treated)	4,753.56	800.165	604,841.88	512,037.54
Observations	5,890	5,890	5,795	5,875

Notes: This table presents difference-in-differences estimates of the impact of GHG disclosure mandates on firms' average payroll, employment, tangible assets, and total expenses. Treat is an indicator equal to one for treated firms. Post is an indicator equal to one for the policy implementation year (year 0) and all subsequent years. 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.

Treated firms are defined as those with at least one facility that exceeds the reporting threshold during the implementation year of a mandate or the subsequent year (above 100 kt in 2004–2005, above 50 kt in 2009–2010, and above 10 kt in 2017–2018 for the federal reforms; above 25 kt in 2010–2011 for the Ontario mandate). Each treated firm is matched to a control firm in the same province and four-digit NAICS industry, using decile bins of total assets, total revenue, average payroll, and profit margin measured one year prior to the reform. Potential control firms include (i) firms that are below the reporting threshold and do not disclose within the first two years after the reform; and (ii) firms that first exceed the threshold at least two years after the implementation year (2006–2008, 2011–2016, and 2019–2021). All specifications include firm fixed effects, four-digit industry-by-year fixed effects, and province-by-year fixed effects. Standard errors are clustered at the firm level.

Table 3: **DiD Estimates on Firms' Revenue, ROA, and Capital Structures**

	(1) Log(Total Revenue)	(2) Returns on Assets	(3) Interest Rates	(4) Log(Total Liabilities)
Treat × Post	0.2818***	0.0100	-0.0193	0.2358***
	(0.0822)	(0.0083)	(0.0180)	(0.0865)
Adj. R Squared	0.885	0.484	0.328	0.907
Mean at t=-1 (treated)	586,496.45	0.028	0.059	726,204.73
Observations	5,825	6,025	4,275	5,985

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. Post is an indicator equal to one for the policy implementation year (year 0) and all subsequent years. 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.

Treated firms are defined as those with at least one facility that exceeds the reporting threshold during the implementation year of a mandate or the subsequent year (above 100 kt in 2004–2005, above 50 kt in 2009–2010, and above 10 kt in 2017–2018 for the federal reforms; above 25 kt in 2010–2011 for the Ontario mandate). Each treated firm is matched to a control firm in the same province and four-digit NAICS industry, using decile bins of total assets, total revenue, average payroll, and profit margin measured one year prior to the reform. Potential control firms include (i) firms that are below the reporting threshold and do not disclose within the first two years after the reform; and (ii) firms that first exceed the threshold at least two years after the implementation year (2006–2008, 2011–2016, and 2019–2021). All specifications include firm fixed effects, four-digit industry-by-year fixed effects, and province-by-year fixed effects. Standard errors are clustered at the firm level.

Table 4: **DiD Estimates on Worker-level Outcomes**

	(1) Log(Annual Earnings)	(2) Job Transitions
Treat × Post	0.1552***	-0.1145**
	(0.0583)	(0.0479)
Adj. R Squared	0.417	0.224
Mean at t=-1 (treated)	68,739.53	0
Observations	440085	445515

Notes: This table presents difference-in-differences estimates of the impact of GHG disclosure mandates on worker-level outcomes, including annual earnings and job transitions. Treat is an indicator equal to one for treated workers (workers employed at treated firms) and zero for control workers. Post is an indicator equal to one for the policy implementation year (year 0) and all subsequent years. Annual earnings are total annual labor earnings, aggregated across all employers in a given year. Job transition is an indicator equal to one if a worker's employer in a given year differs from their employer in the previous year.

Treated firms are defined as those with at least one facility that exceeds the reporting threshold during the implementation year of a mandate or the subsequent year (above 100 kt in 2004–2005, above 50 kt in 2009–2010, and above 10 kt in 2017–2018 for the federal reforms; above 25 kt in 2010–2011 for the Ontario mandate). Each treated firm is matched to a control firm in the same province and four-digit NAICS industry using decile bins of total assets, total revenue, average payroll, and profit margin measured at $t = -1$. Potential control firms include (i) firms that are below the reporting threshold and do not disclose within the first two years after the reform; and (ii) firms that first exceed the threshold at least two years after the implementation year (2006–2008, 2011–2016, and 2019–2021). We define treated workers as incumbent workers employed at treated firms and control workers as workers employed at matched control firms. Each treated worker is matched to a control worker based on four-digit NAICS industry, province, five-year age bin, gender, and within-sector earnings ventile (measured at $t = -1$). All specifications include worker fixed effects, four-digit industry-by-year fixed effects, and province-by-year fixed effects. Standard errors are two-way clustered at the firm and worker levels.

Table 5: **DiD Estimates on Firm Outcomes By Private vs. Listed**

	(1) Log(Average Payroll)	(2) Log(Employment)	(3) Log(Total Revenue)	(4) Interest Rates
Treat × Post × Listed	0.0503	0.0331	0.0095	0.0028
	(0.2224)	(0.2210)	(0.3799)	(0.0060)
Treat × Post × Private	0.2379***	0.2294***	0.2401**	-0.0062***
	(0.0538)	(0.0531)	(0.1103)	(0.0024)
Adj. R Squared (Listed)	0.900	0.923	0.846	0.220
Observations (Listed)	625	625	645	595
Adj. R Squared (Private)	0.928	0.929	0.859	0.455
Observations (Private)	5,180	5,180	5,090	3,890

Notes: This table presents difference-in-differences estimates of the impact of GHG disclosure mandates on firm outcomes separately for publicly listed and private firms.

We estimate two separate regressions, one using the sample of publicly listed firms and the other using the sample of private firms. Listed is an indicator equal to one for publicly listed firms, and Private is an indicator equal to one for private firms. We estimate two separate regressions, one using the sample of publicly listed firms and the other using the sample of private firms. Treat is an indicator equal to one for treated firms. Post is an indicator equal to one for the policy implementation year (year 0) and all subsequent years. Average payroll is total annual payroll divided by employment. Employment is the average number of employees in the firm-year. Total revenue is the sum of all revenue sources in a given year. Interest rate is interest expense divided by total debt.

Treated firms are defined as those with at least one facility that exceeds the reporting threshold during the implementation year of a mandate or the subsequent year (above 100 kt in 2004–2005, above 50 kt in 2009–2010, and above 10 kt in 2017–2018 for the federal reforms; above 25 kt in 2010–2011 for the Ontario mandate). Each treated firm is matched to a control firm in the same province and four-digit NAICS industry, using decile bins of total assets, total revenue, average payroll, and profit margin measured one year prior to the reform. Potential control firms include (i) firms that are below the reporting threshold and do not disclose within the first two years after the reform; and (ii) firms that first exceed the threshold at least two years after the implementation year (2006–2008, 2011–2016, and 2019–2021). All specifications include firm fixed effects, four-digit industry-by-year fixed effects, and province-by-year fixed effects. Standard errors are clustered at the firm level.

Table 6: **DiD Estimates on Firm Outcomes By Energy Efficiency**

	(1) Log(Average Payroll)	(2) Log(Employment)	(3) Log(Total Revenue)	(4) Interest Rates
Treat × Post × Low energy efficiency	0.1084	0.1240	-0.1291	-0.0034
	(0.0925)	(0.0940)	(0.2210)	(0.0047)
Treat × Post × High energy efficiency	0.2208**	0.2042**	0.4668***	-0.0078**
	(0.0976)	(0.0962)	(0.1405)	(0.0034)
Adj. R Squared (Low energy efficiency)	0.919	0.915	0.806	0.403
Observations (Low energy efficiency)	1,795	1,795	1,745	1,220
Adj. R Squared (High energy efficiency)	0.927	0.934	0.885	0.403
Observations (High energy efficiency)	1,875	1,875	1,875	1,470

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. Post is an indicator equal to one for the policy implementation year (year 0) and all subsequent years. Average payroll is total annual payroll divided by employment. Employment is the average number of employees in the firm-year. Total revenue is the sum of all revenue sources in a given year. Interest rate is interest expense divided by total debt.

Treated firms are defined as those with at least one facility that exceeds the reporting threshold during the implementation year of a mandate or the subsequent year (above 100 kt in 2004–2005, above 50 kt in 2009–2010, and above 10 kt in 2017–2018 for the federal reforms; above 25 kt in 2010–2011 for the Ontario mandate). Each treated firm is matched to a control firm in the same province and four-digit NAICS industry, using decile bins of total assets, total revenue, average payroll, and profit margin measured one year prior to the reform. Potential control firms include (i) firms that are below the reporting threshold and do not disclose within the first two years after the reform; and (ii) firms that first exceed the threshold at least two years after the implementation year (2006–2008, 2011–2016, and 2019–2021). All specifications include firm fixed effects, four-digit industry-by-year fixed effects, and province-by-year fixed effects. Standard errors are clustered at the firm level.

Appendix A

Table A1: **GHG Dataset Appendix**

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

Panel B: Sample Selection				
Treatment Period	**# of first-time firms reporters**	**# of voluntary first-time reporters**	**# of mandatory first-time reporters**	**# of firms with balance-sheet info at t = -1**
2004–2005	179	12	167	117
2009–2010	141	20	121	65
2017–2018	230	20	210	175
Total	550	52	498	357
# unique treated firms eligible for matching				357
# of treated firms in our sample				210
Match rate				0.59

Notes: This table summarizes our original GHG emission datasets under federal and provincial disclosure mandates (Panel A) and our sample selection steps (Panel B). Panel A reports the number of facilities reporting their GHG emissions (*# of reporting facilities*), the number of unique first-time facility reporters (*# of first-time facilities*), and the number of unique first-time firm reporters (*# of first-time firms*) by year. The number of first-time firm reporters is smaller than the number of first-time facility reporters because a firm can have more than one facility. Our treatment years are 2004, 2005, 2009, 2010, 2017, and 2018, as discussed in Section 5 and Figure 1. Panel B presents the steps we take to create our final sample. Specifically, we drop voluntary reports and firms without balance sheet information for the year before the regulation change, as we need this information for our matching procedure. Finally, we drop treated firms for which we could not identify a properly matched control firm.