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The Impact of Carbon Disclosure Mandates on Emissions and Financial Operating Performance

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The Impact of Carbon Disclosure Mandates on Emissions and Financial Operating Performance[*]

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ABSTRACT

We examine whether a disclosure mandate for greenhouse gas emissions creates stakeholder pressure for firms to subsequently reduce their emissions. For UK-incorporated listed firms such a mandate was adopted in 2013. Using a difference-in-differences design, we find that firms affected by the mandate reduced their emissions – depending on the specification – by an incremental 14-18% relative to a control group. This reduction was accompanied by an average 9% increase in production costs. At the same time, the treated firms were able to increase their sales by an almost compensating amount. Taken together, our findings provide no indication that the disclosure requirement led to a significant deterioration in the financial operating performance of the treated firms, despite the significant carbon footprint reduction following the disclosure mandate.

Keywords: Disclosure of non-financial information; mandatory disclosure, greenhouse gas emissions; real effects

JEL Classification: Q28, Q40, M41, M48

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1. INTRODUCTION

Climate change is widely regarded as one of the most vexing societal challenges of our time. The root cause of this challenge arguably is the fundamental externality that arises when economic subjects do not internalize the full social cost of their carbon dioxide (CO_2) emissions. The continuing mismatch between the private and social costs of CO_2 emissions has arguably resulted in an unabated global market failure.[1] Policymakers have begun to counteract this mismatch either by imposing a price on carbon emissions or through direct regulations of emissions.[2] At the same time, relatively little is known about the effectiveness of a mere disclosure mandate that requires firms to report their carbon footprint without any further obligation to subsequently reduce these emissions.[3]

A central axiom in management accounting, dating back to Drucker (1954), states: "what gets measured, also gets managed". Inside firms, this linkage is plausible and well-documented to the extent that the variable of interest is included in the firm's performance evaluation and incentive system.[4] The linkage between measurement and subsequent improvements becomes more tenuous in a market setting where firms disclose the variable of interest to the general public. It appears plausible that a mere requirement to report carbon emissions will "incentivize" firms to improve upon their subsequent emissions, even though such improvements are costly, because firms also anticipate being "rewarded" by external stakeholders for these improvements.

Numerous studies on Corporate Social Responsibility (CSR) activities and the attendant Environmental Social and Governance (ESG) scores have observed that improvements in outcome variables are often difficult to establish because there is no commonly accepted measurement framework, e.g., labor practices.[5] The lack of a well-defined measurement framework also makes it difficult, if not practically impossible, to have subsequent outcomes verified by third parties.[6] In these respects, greenhouse gas emissions are relative exceptions as they involve straightforward physical measurements, the delineation of scope (e.g., so-called

[1] The so-called Stern Review (Stern, 2006) famously refers to climate change as "the greatest market failure ever seen".
[2] See Ellerman et al. (2010), Anderson and Di Maria (2011), Bel and Joseph (2015), Murray and Maniloff (2015), and Martin, Muûls, and Wagner (2016).
[3] As discussed in more detail below, contemporaneous papers by Tomar (2019), Jouvenot and Krueger (2020) and Grewal (2019) also examine the real effects of carbon disclosures mandates.
[4] See, for instance, Young and O'Byrne (2001), Kaplan and Anderson (2007).
[5] See, for instance, Christensen et al. (2019), Fiechter, Lehmann, and Hitz (2019), and Grewal and Serafeim (2020) for a comprehensive assessment of the many recent studies on corporate sustainability management.
[6] Kanodia (2007) develops a model framework in which the designer faces a tradeoff in terms of setting a precision standard for disclosures due to the real (investment) effects that emerge in the ensuing market equilibrium.

Scope 1 emissions) and an aggregate score for multiple greenhouse gases in order to arrive at a measure of CO_2 equivalents.

Our main hypothesis is that mandatory reporting of carbon footprints creates "stakeholder pressure" for firms to subsequently "manage" – in the spirit of Drucker (1954) – their carbon emissions. Firms arguably will do so because multiple stakeholder groups, including customers, employees and investors, regard corporate emissions as a negative firm attribute. In that sense, a disclosure mandate may create a "pillory" with regard to a firm's carbon footprint. As firms can report improvements with regard to this publicly reported "bad" in the future, they will arguably improve their standing with different stakeholder groups.[7] We examine this interaction both in terms of subsequent corporate CO_2 emission levels as well as subsequent financial operating performance.[8]

The empirical approach in this paper derives from the 2013 Regulations of the UK Companies Act 2006 (Strategic Report and Directors' Report). This regulation imposed a mandate on publicly listed UK-incorporated firms to report their GHG emissions in their annual reports. Prior to this mandate, companies had an obligation to gather and report the emissions of their individual installations (i.e., production sites such as power plants or steel mills) regulated under the European Union Emissions Trading System (EU ETS) to a publicly available register.[9] Prior to 2013, there was no obligation for publicly listed UK firms to report their overall "carbon footprint" by aggregating their installation-level emissions as part of the annual corporate report. In that sense, the regulation arguably increased *transparency* as interested parties no longer need to trace emission figures at the installation level back to the parent companies owning the installation.

To examine the impact of the UK GHG emission disclosure mandate on subsequent emissions and financial performance, we adopt a difference-in-differences approach (DiD) surrounding the implementation of the 2013 Regulation under the Companies Act. We compare the difference between pre- and post-mandate emission data for affected firms with emission data for a sample of control firms. In our study, this comparison is made feasible by tracing the emissions of all installations, as reported to the central EU registry, back to the firms in our sample for a nine-year time window centered around the year 2013. The treatment group in our

[7] A particular firm attribute, like pollution, that is publicly known and viewed as a negative by investors is also the central feature in the models of Heinkel et al. (2001) and Friedman and Heinle (2008).

[8] Prior studies document real effects of disclosure regulation on investment (Cheng, Dhaliwal, and Zhang 2013; Biddle, Hilary, and Verdi 2009; Biddle and Hilary 2006; Graham, Hanlon, and Shevlin 2011; Shroff, Verdi, and Yu 2014; Shroff 2017), mine safety (Christensen, Floyd, Liu, and Maffett 2017), and managerial short-termism (Ernstberger, Link, Stich, and Vogler 2017; Kraft, Vashishtha, and Venkatachalam 2018; Granja 2018).

[9] The EU Transaction Log registers yearly emissions from all installations regulated under the EU Emissions Trading System.

DiD design consists of all installations, located in the UK or another European country, but ultimately owned by UK-incorporated and publicly listed companies.[10] The control group consists of installations ultimately owned by companies not subject to the 2013 Act, i.e., UK-incorporated firms that are not publicly listed as well as publicly listed firms in other EU countries.

Our results provide evidence of a significant reduction in GHG emissions following the disclosure mandate for firms in the treatment group relative to the control group. The effect is sizable insofar as firms subject to the mandate on average reduce their GHG emissions by an additional 14-18% between the pre- and the post-mandate period. The estimate varies somewhat depending on whether the outcome variable is specified as (the logarithm of) emissions at the individual installation level or the aggregate firm level. To ensure that emission reductions are not partly attributed to reductions in capacity utilization, we also examine the effects of the mandate on carbon intensity, defined as the ratio of CO_2 emissions to cost of goods sold (COGS). Our results here again yield a significant incremental effect in the range of an additional 8-9% reduction in carbon intensity for firms that were subject to the 2013 disclosure mandate.

In additional tests, we find that the emission reductions occur over several years. Furthermore, our analysis identifies an incremental reduction impact for those treated firms that had relatively high levels of CO_2 emissions prior to the mandate. A similar incremental effect is established for firms that are subject to more intense analyst coverage, an effect we attribute to the notion that greater analyst coverage results in greater visibility and thereby creates additional pressure for firms to reduce their emissions. Finally, we provide evidence that the aforementioned findings are robust to various sample specifications, i.e., alternative control groups, and propensity score matching.

Having established that the disclosure requirement had a real effect in terms of carbon emissions, the immediate next question concerns the impact of the requirement on the firms' financial operating performance. One would expect that abnormal efforts to reduce GHG emissions are accompanied by a corresponding increase in production costs. Firms can potentially reduce emissions through a range of measures, including the outsourcing of carbon intensive activities, input substitution, e.g., the use of a less carbon-intensive energy carrier for

[10] Our study focuses on data of all installations covered by the EU ETS, i.e. emissions within the 27 member states of the European Union and Norway. Data on installations in other countries is not available. Thus, we cannot rule out that substitutions between European and non-European installations influence our results. However, because the Companies Act requires disclosure of all corporate emissions unrelated of the respective country, we expect our results not to be severely biased. In this context, Dechezleprêtre, Genaioli, Martin, and Muûls (2014) find no carbon substitution effects across countries.

heating furnaces, or through capital investments that reduce the need for burning fossil fuels, e.g., energy efficiency measures. Any of these activities would lead to an increase in the firm's cost of goods sold. Our findings indicate that the treated firms did experience a significant differential increase of about 9% in their production costs (as measured by COGS), relative to the control group.

On the sales side, we find that the treated firms also achieved a 7% differential increase in their sales. This increase may, of course, simply reflect pricing power that allowed these firms to pass on the increase in their production costs. At the same time, and in line with our "stakeholder pressure" argument, the differential increase in sales for the treated firms could also reflect greater customer loyalty. By being able to credibly convey progress on a goal that is widely regarded as a central corporate social responsibility, the firms arguably improved their public image and their standing with customers.

For the treatment group in our sample, the incremental increases in both sales and cost of goods sold almost cancel each other. The corresponding net effect on the observed gross margins is small and furthermore this variable does not emerge as significant in our difference-in-differences analysis. Taken together, our findings provide no indication that the disclosure requirement for publicly listed UK listed firms led to a significant deterioration in their financial operating performance.

The magnitude of the reduction in emissions and the accompanying significant differential increase in both production costs and sales may appear surprising for two related reasons. First, as mentioned above, raw information about Scope 1 emissions at the level of individual installations was already available prior to the 2013 mandate. As such, the mandate arguably only "pilloried" the treated firms to the extent that firm-level emissions became more accessible and transparent. Second, more than two-thirds of the treated firms in our sample participated in the Carbon Disclosure Project (CDP) which provides a global forum for firms, and/or their subsidiaries, to supply voluntary information about their GHG emissions and ongoing efforts to reduce those emissions.[11] There are no precise standards or requirements for the information firms furnish to the CDP. A reflection of this lack of formal requirements is that the CDP rates the participating firms with a score regarding the credibility and comprehensiveness of their voluntary disclosures. One way to interpret the UK GHG reporting mandate is that the obligation to report total corporate Scope 1 and 2 CO_2 emissions annually forced the

[11] Broadstock, Collins, Hunt and Vegos (2018) point to the endogeneity issues that have afflicted empirical studies on voluntary corporate disclosures of GHG emissions.

"unraveling" of the voluntary and partial disclosure "equilibrium" that was in effect prior to 2013.

Several other contemporaneous studies have recently examined the effects of legal mandates to report GHG emissions. Closest to our study are the papers by Jouvenot and Krueger (2020), Grewal (2020) and Tomar (2019), with the first two of these also focusing on the UK disclosure mandate for GHG emissions beginning in 2013. To quantify the impact of the mandate on subsequent emission levels, Jouvenot and Krueger (2020) rely on voluntarily disclosed information in the CDP database and on emission estimates issued by non-governmental organizations that track corporate GHG emissions. In contrast, our analysis relies on the granular, time-consistent and verified data provided to the European Union Transaction Log (EUTL) register at the level of individual production sites, both prior to and after the mandate. Beyond the impact of the mandate on actual emissions, our study examines the impact on financial operating performance, while Jouvenot and Krueger (2020) study the consequences of the mandate on investments by large institutional investors and on media coverage.

In another independent study, Grewal (2019) considers a population of firms that were voluntarily reporting information about their GHG emissions and establishes an incremental reduction effect for those in the population that also became subject to the 2013 UK mandate. Grewal's findings are consistent with our view expressed above that the reporting mandate had an additional "pillory effect" beyond the information that firms already disclosed through channels like the Carbon Disclosure Project.

Tomar (2019) studies the effect of a regulation that obligates large US production facilities to report their emissions to the US Environmental Protection Agency. In contrast to the UK regulation, however, there is no requirement for US firms to disclose their emissions in the aggregate as part of the annual report. In that sense the post-mandate disclosure requirement in Tomar's study is similar to the pre-mandate setting UK firm faced on account of the EU ETS regulations prior to the 2013 Act. Tomar (2019) develops an estimation technique for assessing emission levels in the pre-treatment period for both the treatment and the control group. At the same time, Tomar's main findings, like the other two studies discussed above, are broadly consistent with our first result that the disclosure mandate resulted in a significant subsequent GHG emission reduction.

Beyond the issue of GHG emissions, our study adds to the broader literature on real effects of mandatory disclosure of environmental and social firm performance. In a recent study, Chen et al. (2018) show that mandatory CSR reporting decreases local pollution levels, i.e., wastewater and sulfur dioxide (SO_2) emissions in Chinese cities in which firms affected by the

CSR mandate are located. Chen et al. also argue that the CSR mandate was associated with a decrease in firms' accounting rates of return. Christensen et al. (2018) provide evidence that disclosure required by the Dodd-Frank Act on mine safety in financial reports improves worker safety in the respective mines.

While these papers study the impact of disclosure on a *local* and direct hazard in terms of city-level wastewater and SO_2 emissions and mine injuries, we focus on the impact of disclosure on GHG emissions which are considered a long-term hazard to global climate stability. In case of *local* hazards, it appears immediately plausible that local stakeholders can exert pressure on companies to take remedial actions. In contrast, with global pollution, the intensity of stakeholder pressure is less clear because the costs from GHG pollution and therefore the benefits of reducing it, are distributed globally and often do not tangibly affect *local* stakeholders.[12]

Chen et al. (2018) examine a comparatively broad disclosure regulation regarding CSR reports and measure the effect indirectly by a reduction in SO_2 emissions in cities, where at least one factory of a company is located, i.e., emissions are measured at the (city-level) destination. However, destination-based pollution data can differ from pollution at the firm-level for instance due to a shifting of pollution activities across regions or as a result of weather conditions and unobserved local emissions. In contrast, we rely on emissions data at the source, i.e., emissions of installations (located in multiple countries), and trace these emissions back to firm. Finally, the results of Chen et al. (2018) reflect two distinct effects: a new disclosure requirement and the measurement and collection of pertinent data. Similar to the setting in Christensen et al. (2017), we are in a position to isolate the effect of the disclosure mandate which effectively becomes an additional communication channel for information on GHG emissions that was collected in the same manner before and after the mandate.

The remainder of this paper is structured as follows. Section 2 introduces the regulatory background and develops the hypotheses to be tested. Section 3 describes the research design. Results are presented and discussed in Section 4. We conclude in Section 5.

[12] Prior literature has established that SO_2 is a local hazard affecting human health outcomes and agricultural yields (EPA 2019; Muller, Mendelsohn, and Nordhaus 2011), while CO_2 constitutes a non-toxic, yet global long-term threat to the stability of the world's climate. In that sense, SO_2 effects are significantly different from those of GHG emissions like CO_2. In addition, abatement of GHG emissions must materialize through costly changes to production processes or reductions in production volume, while abatement of SO_2 emissions is relatively straightforward with the installation of filters.

2. REGULATORY BACKGROUND AND HYPOTHESIS DEVELOPMENT

2.1. Regulatory Background

The 2013 Regulations of the Companies Act 2006 (Strategic Report and Directors' Report) were presented to the UK Parliament by the Department for Business, Innovation and Skills (BIS) on June 11, 2013. The regulations were published in August of 2013 and came into effect on October 1, 2013.[13] Accordingly, "listed companies" are required to disclose their direct and indirect GHG emissions in their annual reports. Section 385 (2) of the Act defines a listed company as a UK-incorporated company whose equity share capital is either listed on the Main Market of the London Stock Exchange, an exchange in a European Economic Area state, the New York Stock Exchange or Nasdaq. The Act applies to all fiscal years ending on or after the 30th of September 2013.

Under the Act, affected companies must report in the directors' report their direct (Scope 1) and indirect (Scope 2) GHG emissions during the last 12 months.[14] Direct emissions are those caused by the combustion of fuels and the operation of production facilities. Indirect emissions are those resulting from the purchase of electricity, heat, steam or cooling. The following greenhouse gases have to be included in the report: carbon dioxide (CO_2), methane (CH_4), nitrous oxide (N_2O), hydrofluorocarbons (HFCs), perfluorocarbons (PFCs) and sulphur hexafluoride (SF_6). Companies are not required to provide individual emission figures for each of the greenhouse gases listed. Instead they must report the aggregate annual quantity of GHG emissions in tCO_2eq, with the weights determined according to the weights set by the IPCC.[15]

To study the effect of the 2013 carbon disclosure mandate on the emissions of the affected firms in Britain, we examine data collected through the Emission Trading (ETS) scheme of the European Union (EU). The data gathered under the auspices of the EU ETS is publicly available in the European Union Transaction Log (EUTL) register, which provides installation-level information including an installation's name, its operator and the sector it is assigned to in the EU ETS (e.g., production of bulk chemicals or combustion of fuels). The EUTL also provides

[13] Leuz and Wysocki (2016) argue that new regulations are often a response to major events like scandals or crises and that financial markets react to the major events triggering a change in firm behavior. In our setting, the mandate for disclosing GHG emissions was not a reaction to a major event but was part of a broader amendment aimed at simplifying and restructuring companies' narrative reports. In addition, a search of all major events in 2012 and 2013 on the Wikipedia sites "2012 and 2013 in the United Kingdom" for "climate" or "scandal" yields no climate change related events or relevant company scandals (the only scandal was the manipulation of LIBOR rates).

[14] Companies may also report emissions for a period different from their financial year. For our study, we use data provided by the EU ETS, measured on a calendar year base.

[15] The UK Government publishes the conversion factors for GHG emissions to tCO_2 equivalents on an annual base. These factors are consistent with international carbon reporting practice and follow IPPC standards. The report must be approved by the Board of Directors and reviewed by the statutory auditor, while compliance with the act is enforced by the UK Financial Reporting Council. Directors of the affected firms face fines for being reckless or failing to take reasonable steps to ensure compliance (paragraph 415, (4) and (5) of the Act).

information on the volume of *verified emissions*, as reported by an independent third party (e.g., accounting and engineering companies) that is accredited by the relevant administrative bodies (European Parliament 2003).

Since its introduction in 2005, the EU ETS has become the world's largest multi-national emission trading system.[16] This cap-and-trade mechanism covers industries that account for about 45% of the European Union's GHG emissions (Ellerman, Marcantonini, and Zaklan 2016).[17] The remaining emissions that are not covered primarily come from the transportation, residential, and agricultural sectors (DECC 2016). Installations are covered by the EU ETS if they include a facility with a rated thermal input of at least 20 MW – enough to supply about 15,000 households with electricity at a given point in time – or if they fall into one or several of the following manufacturing segments: oil refineries, coke ovens, iron and steel, cement clinker, glass, lime, bricks, ceramics, pulp, paper and board, aluminum, petrochemicals, ammonia, nitric, adipic, and glyoxylic acid production (European Commission 2015). Due to these rules, EU ETS coverage is concentrated in the sectors of energy supply and industrial manufacturing, covering approximately 79% of all UK emissions in 2014.

Annual emissions by installations vary widely, ranging from a few thousand tons per year at small installations to several million tons at large coal power plants. The average (mean) UK installation covered by the EU ETS had emissions of about 100,000 tons of CO_2 equivalents during our sample period.[18]

From an inference perspective, there are several benefits to our approach of tracking direct (Scope 1) CO_2 emissions from individual installations according to the EU ETS data. Most importantly, the reported figures were measured and selectively verified in a time consistent manner both before and after the UK disclosure mandate went into effect. Unlike other studies, for instance, Tomar (2019), we therefore do not face the task of having to estimate a firm's emissions prior to the reporting mandate.

To examine the impact of the disclosure mandate on subsequent emissions, we aggregate the emissions data at the installation level and assign them to the firm owning the installations. As

[16] Emission trading systems similar to the EU ETS have also been introduced in a few other jurisdictions. In the US, the California cap-and-trade scheme covers GHG emissions from power generation and industrial activities of facilities based in California (Air Resources Board 2016) and the Regional Greenhouse Gas Initiative covers emissions from electricity production in nine Northeastern and Mid-Atlantic states (RGGI 2013).

[17] For our sample country, the UK, the EU ETS covers a similar share of total emissions. For instance, in 2014, the first full year after the introduction of the disclosure rule the EU ETS covered about 41% of total UK emissions. In 2014, total UK emissions were 514 million tons of CO_2eq, while the sum of verified emissions of all UK installations in the UK was 208 million tons of CO_2eq (DECC 2016).

[18] For our study, we focus on installations with data over the complete sample period. As a consequence, our sample is skewed towards larger installations.

illustrated in Figure 1, the mapping from installations to firms (mathematically, a function) is carried out for both the treated UK-headquartered firms and the control group of other firms, not subject to the mandate.

---Insert Figure 1 here---

We emphasize that in the context of our study the disclosure mandate did not entail additional public information, because the emissions of the treated firms were already publicly accessible, albeit in a rather fragmented form, through the European Union transaction log (EUTL) register. In essence, the informational effect of the disclosure mandate was to increase *transparency* in so far as the interested public no longer had to implement the mapping shown in Figure 1 in order to calculate a firm's overall Scope 1 emissions.

2.2. Hypothesis Development

The hypotheses tested in this paper pertain both to changes in CO_2 eq. emissions and to changes in the financial operating performance of the firms subject to the UK disclosure mandate. Broadly, our approach is consistent with the so-called targeted disclosure cycle theory (Fung, Weil, and Graham 2007). Accordingly, disclosures influence the behavior of receivers (stakeholders) leading to real effects in the variables to be disclosed in the future. The information providers anticipate these behavioral changes and take actions that enable more favorable disclosures.

In a recent survey article, Hombach and Sellhorn (2019) lay out a set of criteria in order for corporate reporting mandates to have real effects. Aside from sufficiently precise reporting standards for which compliance is enforceable, the reporting variables in question must generate additional public information, or at least reduce information gathering costs to the public. Hombach and Sellhorn (2019) also posit that the disclosed information must be broadly value relevant, possibly by illuminating potential future risks to the business, in order for real effects to emerge from a reporting mandate. Finally, management must have both the discretion and the motivation to subsequently improve outcomes with regard to the disclosure variable in question.

The above criteria all appear to be met in the context of the UK 2013 carbon reporting mandate, beginning with a precise reporting standard and the prospect of enforcement.[19] In

[19] In case of non-compliance, the UK Financial Reporting Council is in charge of bringing claims to court.

addition, it is plausible that for many firms their stakeholders, including customers, employees and capital providers, view the firm's carbon footprint as a negative. Because of the externality that the firm's emissions have on world's climate, these constituents would intrinsically prefer a lower carbon footprint, yet they understand that certain emissions levels are unavoidable if the firm is to manufacture its products in a cost competitive manner.

In order to reduce their carbon footprint, firms will almost certainly incur higher production costs (otherwise they would have had every reason to take the appropriate action prior to the 2013 regulation). The magnitude and shape of the individual abatement cost curves, however, appears difficult to gauge. Among the emission reducing measures that would be available to most publicly listed firms in the short and medium run is the procurement of more electricity from renewable sources (and thereby reduce Scope 2 emissions). For manufacturing firms, additional reduction measures include a possible change in the output mix as well as input substitutions that phase-out the use of carbon-intensive materials, like coal.[20] Finally, holding output and production processes constant, many businesses will be in a position to reduce their fossil-fuel consumption through additional maintenance expenditures and investments in energy efficient equipment.

If the treatment group in our sample indeed viewed the 2013 carbon disclosure mandate as a "pillory" that created stakeholder pressure for subsequent improvements in GHG emissions, the immediate question becomes what cost-benefit tradeoffs a firm faces in reducing its carbon footprint. On the benefit side, firms may anticipate that corresponding production cost increases can at least be partially passed on to consumers. While the possibility of pass-through generally depends on a firm's cost structure and pricing power, a tangible reduction in publicly reported carbon emissions will arguably improve the firm's public image. That, in turn, may lead to greater customer loyalty, and thereby a lower price elasticity of demand for customers who regard the firm's carbon footprint as a negative attribute.[21] In that context, the decision to reduce emissions in the interest of "stakeholder capitalism" may be partially aligned with the interests of shareholders.

Another stakeholder group that may react favorably to improvements in reported carbon emissions are the firm's own employees. For the same reasons as articulated in connection with customers, some employees will be intrinsically concerned about the climate damaging effects

[20] In production processes requiring high operating temperatures, for instance, some industries can switch from using coal or oil as a heating agent and instead rely either on electricity or less-carbon intensive agents like natural gas or hydrogen.

[21] Hoyt and Reichelstein (2011a and 2011b) argue that this linkage was important for the management of the equipment retailer REI in setting sustainability targets, including a substantial carbon footprint reduction.

of the firm's emissions. This group will tend to identify more strongly with an organization that can publicly and credibly claim to be innovative and progressive on the issue of climate change. To the innovating firms, the potential benefit from such an improvement in public perception is that ceteris paribus less compensation may be required to retain valued employees.

Finally, management of the firms in our treatment group may anticipate penalties by the capital markets unless improvements in carbon emissions can be claimed in time periods following a precise disclosure mandate. In recent years, institutional equity investors have become increasingly vocal that meaningful sustainability practices are becoming a major criterion for the inclusion of firms in their portfolios.[22] A potential penalty for firms that are viewed as stagnant in their emission levels, or other corporate sustainability activities, is that they may be "shunned" by some investor groups, effectively leading to an increase in their cost of equity capital.[23] The models of Kraus et al. (2010) and Friedman and Heinle (2016) characterize formal equilibria where equity investors can effectively exert pressure on the firm to improve its sustainability practices.[24] The preference structure of these institutional investors may directly reflect the intrinsic preferences of their capital providers. Some institutional investors have also been explicit that they view sustainability practices as an effective mitigation strategy with regard to future financial risks, specifically the risk of carbon intensive assets becoming economically impaired.[25]

There are also arguments to suggest that the real effect of the UK mandate could be minor. These arguments include the possibility that the added transparency resulting from the disclosure requirement was inconsequential in so far as concerned parties could already access the EU transaction log prior to the mandate and, with some additional information gathering effort, trace the emissions from individual installations back to the parent companies owning the installations (Figure 1).

In addition, a substantial share of the treatment firms in our sample did already report information related to their GHG emissions to the Carbon Disclosure Project (CDP) prior to 2013. We note, however, that these disclosures are neither subject to binding standards, nor are

[22] In a global survey of institutional investors, Amel-Zadeh and Serafeim (2018) document that ethical considerations play an increasingly important role for investment decisions besides financial aspects.

[23] A recent illustration of such "penalties" was provided in the annual letter to shareholders by the CEO of BlackRock, L.Fink, when he threatened: "*we will be increasingly disposed to vote against management and board directors when companies are not making sufficient progress on sustainability-related disclosures and the business practices and plans underlying them*" (Timmer, 2020).

[24] Disclosure is not an issue in these models as the CSR variable of interest is assumed to be publicly available information.

[25] The linkage between environmental sustainability and long-term financial sustainability is emphasized, for instance, by investment funds like Generation Investment Capital (Bebb and Reichelstein, 2016).

they generally verified by a third party.[26] Participation in the CDP has always been voluntary and therefore reflects self-selection by the participating firms.[27] Nonetheless, these disclosures may already have conveyed material firm-level information about emissions ahead of the 2013 regulation.[28] Finally, while the treated firms may indeed feel a "pillory effect" for their emissions, the actual reduction potential available to them in the short and medium run, i.e., over a period of four years in our observation window, may have been limited. This may be especially plausible because many of the treated firms were already subject to carbon pricing under the EU ETS system and therefore already had an incentive to adjust their production processes in the direction of more energy- and carbon-efficiency.

Mindful of the preceding countervailing considerations, we nonetheless state the following hypothesis.

H1*: Firms subject to the UK mandate for annual disclosure of GHG emissions attain a larger reduction in their Scope 1 emissions than firms not subject to the mandate.*

We recall that our overall hypothesis is that a carbon disclosure mandate will pressure firms to reduce their emissions because doing so improves the firm's standing with key stakeholder groups. This line of reasoning also makes it plausible that, among the treated firms, those who had relatively high emissions prior to the mandate will feel additional pressure, and thus reduce their emissions by a larger percentage in the post-mandate time period. We test a second variant of H1 for treated firms which in the pre-treatment period received relatively intense analyst coverage, as measured by the number of analysts following the firm. Our reasoning here is that the transparency effect of the mandatory disclosure in annual reports will be magnified for those firms receiving more intense coverage by analysts.

Our next two hypotheses pertain to the operating performance of the treated firms. Specifically, we examine changes in their reported cost of goods sold and their sales. If firms

[26] A survey of UK equity fund managers shows that the lack of detailed requirements concerning reporting methodologies and organizational boundaries as well as the lack of a third-party audit or review was an important barrier for their reliance on voluntary GHG emission data (Trucost 2009).

[27] Examining voluntary carbon disclosure in the UK retail sector, Sullivan and Gouldson (2012) indicate that voluntary disclosure limits investors' ability to assess corporate climate performance due to different scopes of reporting and inconsistencies in the information being reported. In addition, DEFRA (2010) states that "even though CDP data is widely regarded as the most complete and comprehensive dataset on climate disclosure, many question quality of data as an issue which is inevitable with any voluntary disclosure scheme not requiring third party verification".

[28] According to a survey of PricewaterhouseCoopers (PwC) in partnership with the Carbon Disclosure Project (CDP), voluntary reporting of GHG emissions by a company raised the awareness of emissions for the board of directors, employees and the general public (PwC and CDP 2010).

subject to the mandate did indeed reduce their emissions differentially more than the control group, as postulated in H1, the treated firms should also experience a corresponding increase in their production costs in the subsequent post-mandate period. In order to reduce emissions, the treated firms presumably had to make a costly switch in production inputs. In addition, they may have invested new fixed assets that enabled a smaller carbon footprint. The corresponding depreciation charges would then increase Cost of Goods Sold (COGS) in subsequent years. This consideration motivates the following second hypothesis. [29]

H2a: *Firms subject to the UK mandate for annual disclosure of GHG emissions exhibit an increase in their reported COGS in comparison to firms not subject to the mandate.*

Standard microeconomic theory suggests that the impact of an increase in production costs on revenue will depend on several factors including the price elasticity of demand and whether the cost increase pertains to the fixed or variable costs of production. Importantly, in the context of the disclosure mandate examined here, the increase in production costs is endogenous to the extent that firms take deliberate actions in order to lower their carbon footprint. In so doing, they will generally anticipate a partial offset of the higher production costs through a corresponding increase in sales. In addition to the usual possibilities of a cost pass-through, firms may anticipate a "reward effect" in the form of greater customer loyalty, and a corresponding lower price elasticity of demand, for those firms that can demonstrate improvements in their carbon footprint in the post mandate period.

H2b: *Firms subject to the UK mandate for annual disclosure of GHG emissions exhibit an increase in their sales in comparison to firms not subject to the mandate.*

3. RESEARCH DESIGN AND DATA

3.1.Empirical Test: Hypothesis H1

To estimate the effect of the GHG emission disclosure mandate on actual emissions following the UK Companies Act, we use the following staggered difference-in-differences approach:

[29] It is, of course, possible that firms took costly actions to reduce emissions yet do not experience any offset in sales, simply because they anticipated an even steeper sales decline absent any reduction in emissions.

$$E = \alpha_0 + \alpha_1 \cdot Post + \alpha_2 \cdot Post \cdot Treat + \sum Control\ variables$$
$$+ Fixed\ effects\ for\ installation, year, industry\text{-}year, and\ country\text{-}year + \varepsilon, \quad (1)$$

where all variables are defined in Table 1. We estimate multiple variants of the regression equation in (1) which specify the dependent variable "emissions" *E* as either i) the natural logarithm of CO_2 equivalent emissions at the level of individual installations, or ii) the natural logarithm of CO_2 equivalent emissions at the aggregate level of the firm owning the installation (as illustrated in Figure 1), or iii) the natural logarithm of carbon intensity, defined as the ratio of CO_2 emissions to COGS at the firm level.[30] By construction, the carbon intensity variable should reveal if a reduction in emissions should be be attributed to a mere reduction in production volume.

Post is a binary variable indicating periods after the mandate for GHG emission disclosure. Our study is based on observations from the years 2008 to 2016. The mandate for GHG emission disclosure applies to all fiscal years ending on or after September 30th, 2013. If a firm's fiscal year ends before the 30th of September 2013, we define the installation-level emission data observed for the calendar year 2013 to be a pre-treatment observation. If a firm's financial year ends on or after the 30th of September 2013, we define the installation-level emission data observed for the calendar year 2013 to be a post-treatment observation. Consequently, the mandate to disclose GHG emissions applies to firms as of 2013 or 2014 (staggered implementation), depending on the respective fiscal year-end date.

Treat is another indicator variable which assumes the value 1 only if a particular firm was subject to the mandate. For our tests at the level of individual installations, *Treat* assumes the value 1 if and only if the installation belongs to a firm that was obligated to report its emissions following the 2103 Act. As the control group, we use installations ultimately held by firms not subject to the mandate for GHG emission disclosure. Thus, the control group consists of non-listed British firms as well as firms listed in other EU15 countries.

Our primary variable of interest is $\alpha_2 \cdot Post \cdot Treat$, with a negative coefficient α_2 indicating that in the aggregate the treated firms achieved an incremental reduction in emissions following the disclosure mandate. In order to detect an additional incremental effect attributable to firms that either had large emissions prior to the mandate, or had relatively intense analyst coverage, we introduce additional triple indicator variables in testing the variants of H1.

[30] We discuss the common trend of GHG emissions for treated and non-treated firms in our results section.

To control for differences in characteristics of firms that ultimately own installations, we include firm-level variables to account for differences in firm size, profitability, and leverage. To control for time-variant and time-invariant installation heterogeneity, we include installation, year, industry-year, and country-year fixed effects for all tests. Installation fixed effects control for any time-invariant installation-specific unobserved heterogeneity such as installation-specific abatement cost.[31] Year fixed effects capture annual effects that pertain to all installations, for instance, changes in gasoline prices.[32] Industry-year fixed effects capture the annual effects of all installations within the same industry. We define installation industries based on the NACE Rev.2 sector classification. Country-year fixed effects capture the annual effects of all installations within the same country. Since not all installations of a firm are located in the same country as the parent company itself,[33] divergent national regulations may also have an influence on emission reductions.[34]

To reduce the influence of outliers, we winsorize all continuous variables at the 1st and 99th percentiles.[35] We draw our inferences based on standard errors clustered by firm reflecting that all installations of a UK listed and incorporated firm subject to the mandate, independent of the actual geographic location or industry classification of an installation.

--- Insert Table 1 here ---

3.2.Empirical Test: Hypothesis H2a and H2b

To estimate the effects of the GHG emission disclosure mandate on the subsequent financial operating performance of the treated firms, we separately gauge the impact on production costs and sales. Specifically, we test the impact on each variable through a regression approach that parallels equation (1) in connection with H1.

$$C = \beta_0 + \beta_1 \cdot Post + \beta_2 \cdot Post \cdot Treat + \sum Control\ variables + Fixed\ effects\ for\ installation,\ year,\ industry\text{-}year,\ and\ country\text{-}year + \varepsilon, \quad (2)$$

[31] Because we are using installation fixed effects, we do not include a binary variable indicating treatment group installations for all tests.

[32] Because of the staggered implementation, we are able to estimate a model including year fixed effects as well as a variable indicating periods after the mandate for GHG emission disclosure (*Post*).

[33] As an example, the French firm Compagnie de Saint-Gobain owns installations in 15 different European countries.

[34] We find virtually unchanged results if we use firm fixed effects instead of installation fixed effects. We note that using firm fixed effects assumes similar emission levels for all installations of the same firm. However, most firms own several installations of different sizes, in different countries, and in different industries. Thus, we use installation instead of firm fixed effects for our main tests. Our results are robust to excluding industry-year or country-year fixed effects.

[35] Virtually identical results obtain with no outlier treatment and trimming at the 1st and 99th percentiles.

where all variables are defined in Table 1. As before, we employ a staggered difference-in-differences approach with the same regressors as in equation (1). Our dependent variable in equation (2) is the natural logarithm of COGS for the respective firms.

To test the impact of the disclosure mandate on sales, according to H2, we conduct the following regression:

$$S = \gamma_0 + \gamma_1 \cdot Post + \gamma_2 \cdot Post \cdot Treat + \sum Control\ variables + Fixed\ effects\ for\ installation, year, industry\text{-}year, and\ country\text{-}year + \varepsilon. \quad (3)$$

Here, *S* represents the natural logarithm of sales. As before, we employ a staggered difference-in-differences approach. We estimate our regressions for H2a and H2b only at the firm level, reflecting that costs and sales data is not available at the installation level data. *Post* and *Treat* are defined as before. The variable of interest is *Post · Treat*. Similar to H1, we expect a positive coefficient on *Post · Treat*, i.e., β_2 and γ_2, which would indicate higher COGS (sales) subsequent to the UK Companies Act. All control variables and fixed effect variables are as explained above.

3.3.Additional Interaction Effects

In additional tests, we examine variations in observed emissions, costs, and sales effects for different sub-samples of firms. Specifically, we compare firms with high versus low emission levels prior to treatment. We also compare firms with high versus low analyst coverage prior to treatment. The benchmarks for these sample splits are the emission levels and analyst coverage prior to treatment in order to avoid any effect of the treatment on the sub-sample composition.

For the subsample of relatively large emitters, we again introduce an indicator variable. It takes the value of one if a firm's emission level during the pre-treatment periods is bigger or equal to the sample median, and zero otherwise. For the subsample of high versus low analyst coverage, we use a binary variable indicating high analyst coverage. This variable takes the value of one if a firms' analyst coverage during the pre-treatment period is larger than or equal to the sample median, and zero otherwise. Analyst coverage is taken from I/B/E/S, where we impute a value of zero if analyst coverage is missing.

To examine cross-sectional variation in the treatment effect, we use our prior regression model with an additional interaction term *Treat · Post · {High Emitter, High Coverage}.*

3.4.Data

Our calculations are based on the verified emission data of the EU ETS, which covers individual installations, but not the firms owning the installations. Thus, our sample selection proceeds at the installation level before we aggregate data at the firm level. Figure 1 illustrates the assignment of installations to firms. Our starting sample comprises all European (EU27 plus Norway) installations with emissions for the period 2009-2017 (14,000 installations).

We first exclude installations when we could not determine the ultimate corporation owning the installation. This step is required, as the assignment to either the treatment or control group as well as pre- and post-treatment periods depend on a firm being subject to the mandate. The link between the installation and the ultimate corporate owner is established by the Ownership Links and Enhanced EUTL Dataset Project (OLP).[36] The OLP provides the name and Bureau-van-Dijk identification number (BvD-ID) of an installation's parent company. Fiscal year ends are taken from BvD Orbis database. This step leads to a sample of 6,326 installations owned by 1,993 unique firms.

Next, we limit our sample to installations owned by firms from EU15 countries. The EU ETS comprises all stationary installations in all European countries. Because firms from non-EU countries like the US or Japan operate installations in Europe too, the inclusion of these installations might lead to biased estimates of firms' overall emission levels. While European firms operate most of their installations in other EU countries, non-European firms also tend to operate most of their installations in non-European countries. As we only observe emissions of European installations, we focus on European firms for all tests. This step results in a sample of 5,195 installations owned by 1,485 unique firms.

We also limit the sample to listed firms because of the scope of the UK Companies Act. In addition, disclosure and emission reduction incentives of listed and unlisted firm difficult to compare. By restricting attention to listed and, thus, larger firms for our control, we ensure the comparability of firms in the sample. This step leads to a sample of 2,274 installations owned by 197 unique firms.

We further exclude firms from countries with other confounding regulations. During our sample period, only Ireland implemented an emission-related regulation (InsideIreland.ie 2009). Next, we exclude installations if the owning firm changed during the sample period because, otherwise, the assignment of the treatment group might be biased. These two restrictions lead to a sample of 2,212 installations owned by 193 unique firms.

[36] Data and a technical note describing the mapping from installations to ultimate owners is available at http://fsr.eui.eu/climate/ownership-links-enhanced-eutl-dataset-project/.

Our sample also excludes installations from the energy sector based on the NACE Rev.2 sector code "35". This exclusion reflects that for this sector the UK government implemented an additional policy to support the EU ETS allowance price in 2013 (so-called Carbon Price Floor (CPF)). The CPF applies to electricity producers (Hirst 2018) and provides an additional emission charge beyond the EU ETS. Thus, the inclusion of these installations would likely lead to overstated results.

Next, we exclude installations in case of maintenance or shut down because these emission reductions are likely unrelated to the mandate to disclose GHG emissions.[37] In detail, we exclude installations if their minimum emission level over the sample period is less than 1% of their maximal emission level.[38] This step leads to a sample of 1,101 installations owned by 166 unique firms.

Finally, we exclude installations with missing emission data over the sample period. We use a balanced sample approach to avoid measurement error of our difference-in-differences and fixed effects design. This step leads to a sample of 769 installations owned by 139 unique firms. Finally, we exclude observations in case of missing data for firm-level dependent and control variables. This step leads to a sample of 676 installations owned by 126 unique firms.[39]

Overall, we arrive at a balanced sample of 6,066 installation-years (676 unique installations) and 1,134 firm-year (126 unique firms for the period 2009-2017. Table 2 delineates the sample selection and Table 3 presents the sample composition by country. An average sample firm operates 5.32 unique installations. In our final sample, 22 firms belong to the treatment group and 104 firms to the control group. Regarding the geographic distributions, all our treatment group firms are incorporated in the UK whereas most of our control group firms are incorporated in France (24 firms), Germany (21 firms), Spain (15 firms), and Sweden (13 firms).

--- Insert Table 2 here ---

--- Insert Table 3 here ---

Table 4 reports descriptive statistics (Panel A) and correlations (Panel B) for our final sample. The average treatment (control) group firm in our sample produces yearly emissions of 827,069 (1,213,081) tCO_2eq. With respect to our outcome variables, we observe that treated firms emit less CO_2 on average, have a lower emission intensity, a higher cost of goods sold

[37] As an example Lynemouth Power Station reported 2,717,964 tCO_2eq in 2014, 1,287,305 tCO_2eq in 2015, and 1,059 tCO_2eq in 2016.

[38] We find virtually unchanged results if we use a lower threshold (0.5%) or a higher threshold (5%).

[39] Allowing for an unbalanced sample would again not result in a tangible change of our findings.

and lower sales, as well as a higher gross margin than control firms. Due to a skewed distribution, we use log-transformed emissions, emission intensity, sales and cost in our regression analysis. With regard to characteristics of firms, we find that firms in the treatment group are larger, more highly leveraged, and have a higher share of fixed to total assets compared to control group firms. With regard to the Pearson and Spearman correlations, we do not observe high correlations between the continuous control variables.

--- Insert Table 4 here ---

4. RESULTS

4.1. Impact of the 2013 Act on GHG emissions and emission intensity

Table 5 reports our findings regarding the impact of the GHG disclosure mandate on subsequent GHG emissions at the installation- and firm level. We also test the impact of the mandate on the variable carbon intensity, defined as the ratio of emissions to COGS. By taking logarithms, we can interpret a transformed value of the corresponding coefficients as additive percentage changes.

Columns (I) and (II) in Table 5 present results of our estimations using installation-level emission data. The estimations at the installation level provide results using the most disaggregated data possible. While our main interest is at the firm level, the installation-level estimates establish a baseline reading that allows us to assess possible effects of aggregating installation-level data at the firm level. Columns I and II present the findings without and with firm-level control variables, respectively. In all specifications, we use installation, year, industry-year, and country-year fixed effects, to account for unobserved heterogeneity.

In column (I) of Table 5 the estimate of the coefficient related to the policy variable *Post* · *Treatment* is significantly negative (p: <0.05). In economic terms, the point estimate suggests that, during the years after the implementation of the Act, affected firms on average decreased their GHG emissions by about 14% compared to companies not affected by the Act, because 1 - exp(-0.155) = 0.14. Controlling for differences in firm characteristics (column (II)) essentially does not alter our main result as we obtain an estimate of -0.158, corresponding to an average emissions decrease of 15%. We note that the high adjusted R^2 is due to the stringent fixed effects specification that absorbs installation-specific and year-specific unobservable

heterogeneity, as well as industry trends and country trends.[40] The installation-level results support H1, i.e., the Act causes an emission reduction among treated installations, compared to installations owned by firms in the control group.

Columns (III) and (IV) in Table 5 present results analogous to columns (I) and (II) at the firm level after aggregating the emissions of installations at the level of the ultimate entities owning the installation. These and all remaining firm-level regressions control for unobserved heterogeneity by including year and owner-specific fixed effects. Column (III) reports the estimates without further firm-level controls. We again find a negative and significant coefficient estimate on *Post · Treatment* (p: <0.01) indicating a reduction in GHG emissions for treated firms by about 18%. This reduction is comparable to the emission reduction of about 15% measured using installation-level emission data (column (I)), suggesting that the reduction in emissions occurs fairly evenly across installations owned by each firm. In the richer specification including further covariates (column IV), we control for the same firm-level covariates as in column (II). Column (IV) corroborates the magnitude of the emission reduction obtained in the simpler model, again with an estimated effect of the Act on treated firms of about 18% (p: <0.01) when controlling for firm size, the ratio of fixed assets to total assets, and leverage. The analysis of firm level emissions thus also supports hypothesis H1.

In the final set of regressions shown in Table 5, we estimate the effect of the Act on firm-level carbon intensity. This dependent variable, defined as emissions divided by COGS, ensures that any observed emission reductions are not a merely a consequence of reductions in production volume. Column (V) reports the estimates without further firm-level controls, while column (VI) shows the results when including covariates. The findings obtained here are very similar for both specifications, with an estimated reduction in carbon intensity of about 26% (p: <0.01). This shows that the emission reduction is not mainly driven by a decline in output or capacity utilization.

--- Insert Table 5 here ---

4.1.1. Assessing the validity of the common trend assumption

The main assumption underlying the difference-in-differences approach is a common trend between treatment and control group firms (Roberts and Whited 2013). To examine whether a

[40] Controlling for installation and year fixed effects, which would make the fixed effects structure analogous to the firm-level regressions, affects the results only insignificantly. In this case we obtain coefficient estimates of -0.165 and -0.171, respectively.

common pre-treatment trend exists, we follow the literature (Serfling 2016; Bertrand and Mullainathan 2003). Specifically, we replace the interaction terms *Post · Treatment* with separate interaction terms for each period relative to the implementation of the GHG mandate in 2013. We adjust time periods to reflect the staggered nature of the treatment. When estimating the model, insignificant pre-treatment coefficients close to zero indicate the validity of a common pre-treatment trend.

In our tests of parallel trends we focus on firm-level emissions (Figure 2) and emission-intensity (Figure 3).[41] Values on the y-axis show point estimates and 90 percent confidence intervals. Values on the x-axis refer to the number of years removed from the start of the post-treatment period. The benchmark period is t = -1, i.e., the last pre-treatment year. Due to the staggered nature of the treatment for many installations t = -1 is 2012, while for some it is 2013. The pre-treatment period consists of the periods t <= -4 to t = -1, while periods t = 0 to t >= 4 constitute the post-treatment period. We aggregate the coefficients for the first and last periods depicted, also due to the staggered treatment. Accordingly, the t <= -4 value represents the average of the coefficient estimates for the interactions between each *Treat* variable and year dummies for periods four or more years prior to treatment. Analogously, the t >= 4 value is the average of the estimates for the interactions between *Treat* and year dummies for periods four or more years after treatment.

Regarding firm-level emissions, we observe a clear difference in pre-treatment and post-treatment coefficients. While treated firms have somewhat higher pre-treatment emissions than the control group four years prior the mandate, this difference is fairly stable during the three-year pre-treatment period and very close to zero. Point estimates are not significantly different from zero in any year from t > -4 to t = -1. Based on these results, we do not find any violation of the common trends assumption for firm level emissions. The estimates for the period t = 0 to t >= 4 show the evolution of the treatment effect over time. We observe that emissions in the treatment group decrease significantly immediately after the start of the post-treatment period. Moreover, firms reduce their emissions in every post-treatment year, except for an insignificant reduction in the final post-treatment. This suggests that the effect of the Act on emissions is permanent rather than transitory.

--- Insert Figure 2 here ---

[41] The parallel trends assumption also holds for installation-level emissions.

A similar picture emerges when we test for parallel common trends with regard to carbon intensity,. None of the estimates of the pre-treatment coefficients are statistically different from zero, thus supporting the common trend assumption. Again, we observe a clear treatment effect, which manifests itself at the beginning of the post-treatment period. Post-treatment coefficients are negative and significant, indicating that the Act led to a permanent rather than transitory decline in emission intensity.

--- Insert Figure 3 here ---

4.1.2. Effects for large emitters and firms with high analyst coverage

This subsection explores differential effects on emissions and carbon intensity for firms likely to face stronger visibility and stakeholder pressure. Specifically, we examine whether large emitters and firms facing more intense analyst coverage feel stronger pressure to pursue emission reductions. Large emitters are defined as those firms that had above-median emissions during the pre-treatment period. To identify firms with intense analyst coverage, we collect data on the number of earnings forecasts available for each firm. A firm is then said to have high analyst coverage if its number of earnings forecasts available during the pre-treatment period is above the median. We introduce an additional interaction term into the regression analysis to capture the effect of each differentiating variable: *Post · Treatment · Large Emitter* for large emitters, and *Post · Treatment · High Analyst* for firms subject to high analyst coverage. Table 6 reports the results of this analysis. Columns (I)-(IV) of Table 6 contain the results for large emitters, while columns (V)-(VIII) present the results of the analogous regressions for firms exposed to relatively many analysts.

We find that large emitters reduce their emissions significantly more than the average firm (p: <0.01), with an estimated additional reduction of approximately 6.5% in the specification including firm-level covariates (column (II)), compared to the average treatment effect of the Act. In contrast, we find that the carbon intensity of large emitters declined by approximately 6% (p: <0.01) less than the average (column (IV)). The dampened effect on carbon intensity for large emitters may reflect that their cost of goods sold decreased or increased less as compared to the average (see also our results H2). Overall, the results in Table 6 suggest that large emitters had additional abatement potential which may have been realized at a relatively low cost.

Somewhat different findings emerge for firms exposed to high analyst coverage. These firms also achieved an additional emission reduction of about 2.5% (p: <0.01) in excess of the emission reduction of the average firm, lending support to the idea that higher visibility increase

abatement efforts. Considering the carbon intensity of firms with high analyst coverage, our estimates show that emission intensity decreased by an additional 4.7% (p: <0.01), as shown on column (VIII) of Table 6. Thus, when compared to the average, firms with high analyst coverage did achieve relatively larger improvements in carbon intensity after the regulation went into effect. Taken together, our findings suggest that firms with higher stakeholder pressure (due to high initial emissions) and firms with greater visibility both responded more actively to the reporting regulation.

--- Insert Table 6 here ---

4.2. Effects on financial operating performance

Our next set of tests addresses the effects of the mandate on the financial operating performance of the disclosing firms. As argued above, firms will face cost-benefit tradeoffs in reducing their carbon footprint. To explore this trade-off, we separately estimate the effects of the mandate on production costs (measured as COGS) and sales. As before, we introduce fixed effects indicator variables for installations, year, industry-year, and country-year in all specifications in order to account for unobserved heterogeneity.

4.2.1. Effects on production costs

To estimate the changes in production costs, we rerun our difference-in-differences estimation and use the logarithm of costs of goods sold as the dependent variable. Column (I) in Table 7 shows that firms subject to the mandate incur, on average, a statistically significant increase in production costs of about 8% (p: <0.05). This finding is robust in magnitude and significance also after controlling for a range of firm characteristics in column (II) of Table 7. Overall, these results lend support to hypothesis H2a that the reduction in emissions by the disclosing firms was accompanied by higher production costs.

Increases in production costs required to achieve emission reductions may depend on whether firms have low or high emission levels prior to the disclosure mandate. We investigate potential heterogeneity in cost reductions further below and first present our findings on hypothesis H2b on sales effects.

--- Insert Table 7 here ---

4.2.2. The effects on sales

To test hypothesis H2b, we examine the implications of the reporting mandate on the sales of disclosing firms. Table 7 presents the results. The average effect is positive and significant, indicating an increase in sales of about 7% (p: <0.05), as shown in column (III). When adding firm controls in column (IV), the magnitude of this effect remains robust with an estimated sales increase of about 6% (p: <0.05), again supporting H2b. While we lack data to introduce an instrument for the underlying mechanism, e.g., by controlling for a firm's pricing power, the estimates in Table 7 show that emission reductions are accompanied by an increase in gross revenue that is directly comparable in magnitude to our estimate of the incremental production costs. To further illustrate this point, our estimates for the respective increases in COGS and sales indicate the following: On average, if a firm in the treatment group earned a sales revenue that entailed a markup of at least 1.7% on its COGS prior to the mandate, then its operating profits would increase during the post mandate years, assuming all SG&A expenses remain unchanged[42].

We also explore the net effects on financial operating performance by looking at gross margins. Accordingly, we compute the gross margin percentage as *(Sales - Costs of Goods Sold)/Sales*. Columns (V) and (VI) of Table 7 yield a small negative, yet statistically insignificant, point estimate. Thus, the gross margins of disclosing firms following the 2013 mandate is not statistically different from the margins of our control group of non-disclosing firms. This result holds with and without controlling for firm characteristics.

Taken together, our results in this subsection show that disclosing firms on average experience both significant cost and sales increases, which roughly cancel each other in their net effect on gross margins. As such, our findings provide no evidence that the mandate to disclose greenhouse gas emissions had, on average, a deteriorating effect on the financial operating performance of the treatment group.

5. SENSITIVITY AND ADDITIONAL ANALYSES

As a first sensitivity test, we construct a matched sample of treatment and control firms. Accordingly, we match each treatment firm with a control group firm within the same Fama and French 5 industry based on pre-treatment emission levels, i.e. 1-to-1 matching without replacement. We use the same dependent variables as in our prior tests.

[42] The 1.7% benchmark is obtained as the ratio *exp(0.079)/exp(0.062*), referring back to the DiD coefficients in Table 7.

Table 8 presents the results and shows that our estimates are qualitatively similar to the ones obtained before.

We acknowledge that during our sample period France mandated broad corporate disclosures regarding environmental stewardship. While we kept French installations for our main tests, we find virtually unchanged results for emission reductions if we exclude installations owned by French firms. Our results are also robust to not excluding energy sector installations or only excluding electricity installations from our sample, which, as discussed in our data section, have been subject to additional climate regulations during our observation period.

We also explore potential heterogeneity in our estimated effects for COGS, sales, and gross margins. In view of our findings that larger emitters tend to achieve greater abatements, it seems plausible that firms with large emissions prior the mandate also achieve emission reductions at relatively low cost.

To identify the cost effects for relatively large emitters (identified by emission levels before the mandate), we interact the treatment effect with our indicator variable for large emitters (Untabulated). The point estimate for this interaction is negative and significant (p: <0.01), suggesting that large emitters reduce emissions at significantly lower costs. We also interacted the treatment effect on COGS with our indicator for low/high analyst coverage, but did not identify any incremental effects on production costs for firms with intense analyst coverage.

Somewhat surprising, large emitters also exhibit no significant effects with regard to their gross margin, despite their ability to achieve emission reductions at lower cost. We attribute this finding to large emitters also facing a less pronounced effect on their sales and therefore their gross margins remain unchanged on average. The missing sales effect of large emitters may be linked to their lower incremental abatement costs that would need to be passed on. Also, large emitters tend to operate in industries with a relatively high exposure to global trade. Firms with more global operations are likely to experience an attenuated effect related to their public image in response to a national disclosure regulation such as the UK mandate.

--- Insert Table 8 here ---

6. CONCLUSION

In the current debate about alternative remedies for the impending climate crisis, economists generally point to pricing carbon emissions or subsidizing low-carbon energy systems. This

paper examines whether the mere requirement to disclose carbon emissions in annual reports will induce manufacturing firms to lower their corporate emissions over time. Our basic hypothesis is that, even without any regulatory obligation to improve emissions, a formal and structured CO_2 reporting requirement will have a "pillory effect" because a firm's stakeholders regard the firm's emissions to be a negative attribute.

Our study applies a difference-in-differences design to a set of European installations and exploits the introduction of the 2013 UK Companies Act 2006 (Strategic Report and Directors' Report) as a source of exogenous variation in the disclosure regime. We find that installations of UK companies subject to the reporting regulation exhibit significant reductions in GHG emissions compared to control installations, after controlling for various time-variant and time-invariant fixed effects as well as firm specific variables. We find that, on average, firms reduce emission between 14 and 18% over three post-treatment years, relative to pre-treatment emission levels. These results are obtained at the installation level and at the firm level. In additional tests, we observe that the emission reductions are permanent rather than transitory and are more pronounced for larger emitters with larger savings potential. Also, firms subject to high analyst coverage achieve above-average emission reductions.

With regard to financial outcomes, we document that emission reductions come at a significant increase in production costs, as measured by the firms' COGS. Firms incurred that cost voluntarily, yet they also attained an almost compensating increase in sales, possibly reflecting an improved standing with customers due to their efforts in connection with a central CSR variable. Taken together, we obtain no evidence that the disclosure mandate had a deteriorating effect on the gross margins of the firms that were subject to the reporting regulation.

The emissions data for the firms in our treatment group had already been publicly available prior to the UK mandate, albeit in distributed and fragmented form so that outside observers had to incur material search costs in order to assign installation-level emissions to publicly listed firms. Our analysis indicates that the disclosure requirement made firm-specific carbon footprint information more accessible to stakeholders and thereby resulted in real emission reduction effects. In conclusion, the increased transparency associated with the reporting mandate in the UK appears to have been a meaningful policy tool in reducing carbon emissions.

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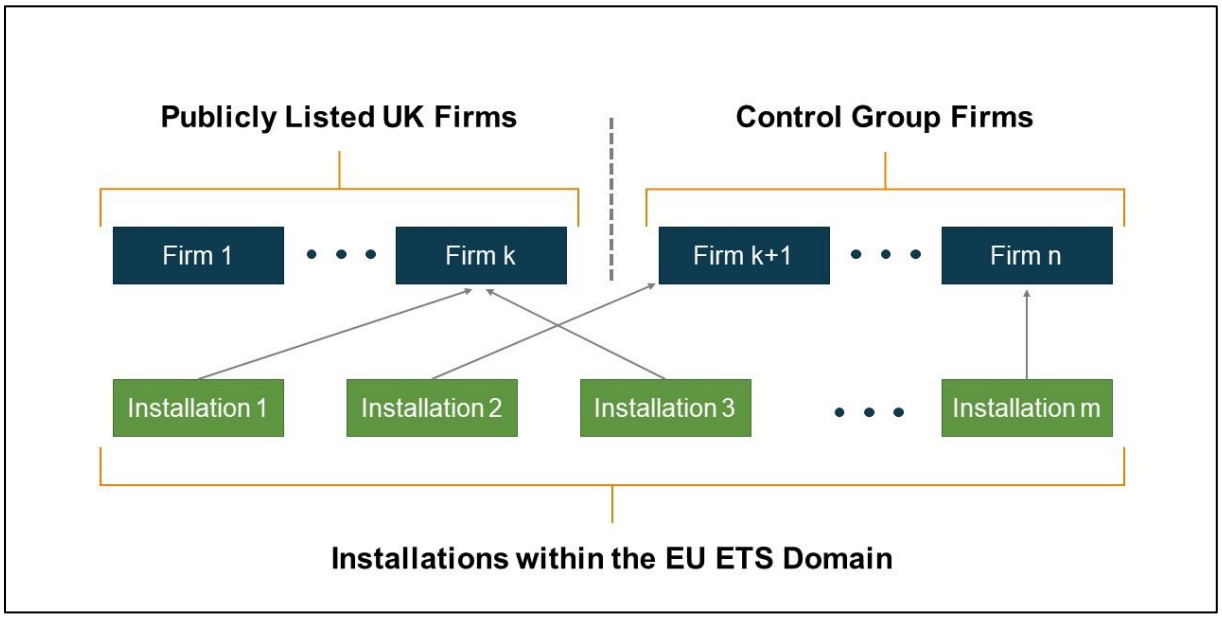
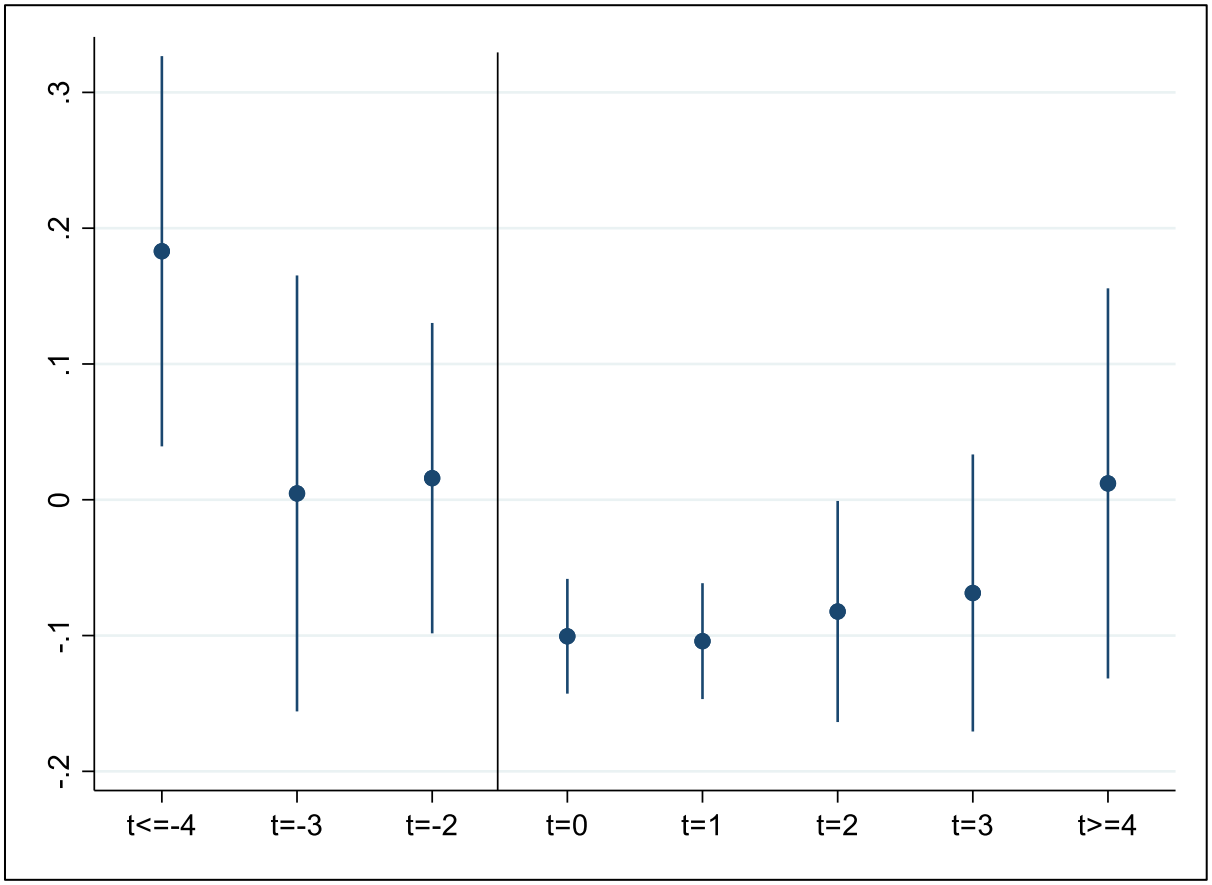
Figure 1: Mapping of installations to firms



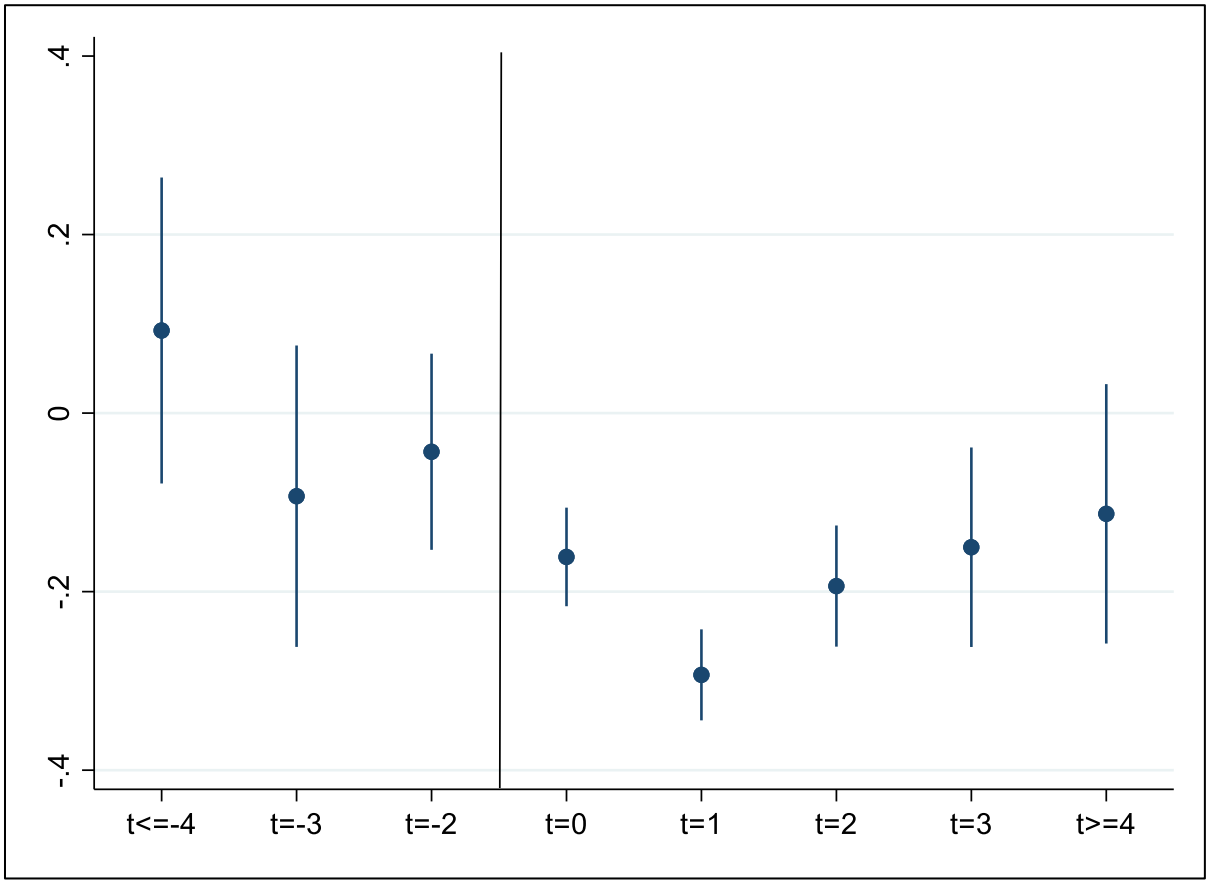
Notes: This figure shows the mapping of data at the installation level as reported in the EU ETS to treatment and control group firms. Using information on the owner of each installation, we map one installation to either a treatment or a control group firm. The link between the installation owner and the ultimate owner (i.e., firm in our sample) is established by the Ownership Links and Enhanced EUTL Dataset Project (OLP), which provides the name and Bureau-van-Dijk identification number of an installation's parent company.

Figure 2: Change in emissions surrounding the disclosure mandate



Notes: This figure shows the change in verified emissions surrounding the mandate to disclose GHG information. Values on the y-axis show differences in the natural logarithm of emissions of treated and control group firms and 90 percent confidence intervals. To obtain these estimates, we replicate model (1) but replace *Post · Treatment* with separate interaction terms for each individual year surrounding the mandate. To account for the staggered implementation of the Act, we adjust time periods using relative time periods. Values on the x-axis indicate periods relative to the first-time application of the Act (t=0). For firms with fiscal year end after September 30th 2013, t=0 refers to the year 2013. For firms with fiscal year end before September 30th 2013, t=0 refers to the year 2014. Periods t=-2, t=-3, and t<=-4 indicate periods prior to the first-time application of the Act. Periods t=0, t=+1, and t>=2 indicate periods on or after the first-time application of the Act. We use the last year prior to the first-time application (t=-1) as the benchmark period.

Figure 3: Change in carbon intensity surrounding the disclosure mandate



Notes: This figure shows the change in carbon intensity surrounding the mandate to disclose GHG information. Values on the y-axis show differences in the natural logarithm of emissions of treated and control group firms and 90 percent confidence intervals. To obtain these estimates, we replicate model (1) but replace *Post · Treatment* with separate interaction terms for each individual year surrounding the mandate. To account for the staggered implementation of the Act, we adjust time periods using relative time periods. Values on the x-axis indicate periods relative to the first-time application of the Act (t=0). For firms with fiscal year end after September 30th 2013, t=0 refers to the year 2013. For firms with fiscal year end before September 30th 2013, t=0 refers to the year 2014. Periods t=-2, t=-3, and t<=-4 indicate periods prior to the first-time application of the Act. Periods t=0, t=+1, and t>=2 indicate periods on or after the first-time application of the Act. We use the last year prior to the first-time application (t=-1) as the benchmark period.

Table 1: Definition of variables

Variable	Definition	Data Source
Dependent variables		
ln(Emissions)	Natural logarithm of yearly verified emissions in metric tons of CO_2eq.	EU ETS
ln(COGS)	Natural logarithm of COGS.	BvD Orbis
ln(Sales)	Natural logarithm of sales.	BvD Orbis
ln(CI)	Natural logarithm of verified emissions scaled by COGS (in '000 USD).	EU ETS, BvD Orbis
Gross Margin	Sales minus COGS scaled by sales.	BvD Orbis
Moderator variables		
High Emitter	Indicator variable, 1: a firms' emission level during the pre-treatment periods is above or equal to the sample median, 0: otherwise.	EU ETS
High Coverage	Indicator variable, 1: a firms' analyst coverage during the pre-treatment periods is above or equal to the sample median, 0: otherwise.	I/B/E/S
Experimental variables		
Post	Indicator variable, 1: financial years ending on or after the 30th of September 2013, 0: otherwise.	EU ETS
Treatment	Indicator variable, 1: UK listed and incorporated firm affected by the mandate for GHG emission disclosure. I.e., UK-incorporated company whose equity capital is either listed on the Main Market of the London Stock Exchange, an exchange in a European Economic Area state, the New York Stock Exchange or Nasdaq, 0: otherwise.	EU ETS
Control variables		
Size	Natural logarithm of a firms' market value. Market value is defined as enterprise value minus long-term debt + cash – loans.	BvD Amadeus
Leverage	Long term liabilities plus short-term liabilities divided by total assets.	BvD Amadeus
Asset Intensity	Fixed assets scaled by total assets	BvD Orbis

Notes: This table defines all variables used in this study.

Tabe 2: Sample selection

	Installation level			Firm level
Initial sample:	**Exclusion**	**Installation-years**	**Unique Installations**	**Unique firms**
All stationary installations listed in the EU ETS with non-missing emission data for the period 2009-2017		98,836	14,000	
Exclusion of…				
… installations without known ultimate owner and date of fiscal year end	-52,868	45,968	6,326	1,993
… installations owned by non-EU firms	-8,104	37,864	5,195	1,485
… installations owned by unlisted firms	-20,752	17,112	2,274	197
… installations affected by confounding regulations or ownership changes	-320	16,792	2,212	193
… energy sector installations	-7,677	9,115	1,224	175
… virtually closed installations (i.e. maintenance or temporary shutdown)	-696	8,419	1,101	166
… unbalanced emission data	-1,498	6,921	769	139
… missing firm-level data	-855	6,066	674	126
Final sample for regression analyses		6,066	674	**126**
Firm-year observations for the period 2009-2017				**1,134**

Notes: This table describes the sample selection for estimating the influence of a GHG disclosure mandate on firms' GHG emission levels and operating performance.

Table 3: Sample composition

	Control group	Treatment group	Total
Austria	45	0	45
Belgium	54	0	54
Germany	189	0	189
Denmark	27	0	27
Spain	135	0	135
Finland	72	0	72
France	216	0	216
United Kingdom	0	198	198
Luxembourg	9	0	9
The Netherlands	45	0	45
Portugal	27	0	27
Sweden	117	0	117
Total	936	198	1,134

Notes: This table presents the geographic distribution of sample firms. Treatment group firms are listed and incorporated in the United Kingdom. Control group firms are listed and incorporated in other EU15 states.

Table 4: Summary statistics

	N	Mean	Lower quartile	Median	Upper quartile	Std. deviation
Panel A: Treatment Group						
Emissions (t CO2eq)	198	827,069	27,539	51,004	202,332	2,621,052
ln(CI)	198	-3.851	-5.195	-3.892	-2.728	2.071
COGS ('000.000 USD)	198	25,536	1,071	4,842	11,353	59,108
Sales ('000.000 USD)	198	38,954	1,855	15,987	34,829	72,870
Gross Margin	198	0.484	0.231	0.515	0.723	0.249
Leverage	198	0.215	0.137	0.218	0.281	0.113
Size	198	23.01	21.982	23.685	24.965	2.309
Asset Intensity	198	0.666	0.605	0.683	0.767	0.133
Panel B: Control group						
Emissions (t CO2eq)	945	1,213,081	29,078	152,327	463,316	3,326,045
ln(CI)	945	-3.353	-4.860	-3.246	-1.521	2.363
COGS ('000.000 USD)	945	15,620	1,161	4,973	14,345	28,263
Sales ('000.000 USD)	945	23,734	2,513	8,121	26,933	39,176
Gross Margin	945	0.403	0.268	0.368	0.521	0.18
Leverage	945	0.183	0.115	0.176	0.241	0.101
Size	945	22.379	21.255	22.570	23.591	1.777
Asset Intensity	945	0.610	0.516	0.617	0.704	0.128

Panel C: Correlations

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ln(Emissions)	(1)		**0.607**	**0.284**	**0.266**	**-0.175**	**0.076**	**0.158**	**0.124**
ln(CI)	(2)	**0.628**		**-0.523**	**-0.531**	**0.105**	**-0.056**	**-0.516**	**0.136**
ln(COGS)	(3)	**0.325**	**-0.529**		**0.969**	**-0.374**	**0.110**	**0.787**	-0.049
ln(Sales)	(4)	**0.308**	**-0.523**	**0.973**		**-0.184**	**0.148**	**0.876**	**0.055**
Gross Margin	(5)	**-0.200**	**0.151**	**-0.401**	**-0.191**		**0.098**	**0.087**	**0.389**
Leverage	(6)	0.046	-0.039	**0.091**	**0.141**	**0.151**		**0.162**	**0.330**
Size	(7)	**0.193**	**-0.485**	**0.799**	**0.881**	**0.094**	**0.186**		**0.228**
Asset Intensity	(8)	**0.120**	**0.134**	-0.031	**0.070**	**0.405**	**0.354**	**0.253**	

Notes: This table shows the descriptive statistics for all variables used in the regression separately for treatment (Panel A) and control firms (Panel B). In Panel A and B, values for emissions, costs, and sales are in non-log form. Panel C presents Spearman (above) and Pearson (below the diagonal)

correlations for all variables used for empirical analyses. Bold figures in Panel C indicate statistically significant correlations that are at least at the 10 percent level. Variable are defined in Table 1. All continuous variables are winsorized at the 1st and 99th percentiles.

Table 5: The effect of a GHG disclosure mandate on emissions

	Installation Level		Firm level			
	(I)	(II)	(III)	(IV)	(V)	(VI)
Dependent variable:	**ln(Emissions)**	**ln(Emissions)**	**ln(Emissions)**	**ln(Emissions)**	**ln(CI)**	**ln(CI)**
Experimental variables						
Post	0.020	0.016	0.181**	0.180**	0.146*	0.131
	(0.759)	(0.797)	(0.026)	(0.038)	(0.090)	(0.112)
Post · Treatment	**-0.155****	**-0.157****	**-0.180*****	**-0.182*****	**-0.251*****	**-0.253*****
	(0.011)	**(0.012)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**
Control variables						
Leverage		-0.335		-0.073		-0.534
		(0.123)		(0.842)		(0.274)
Size		-0.014		-0.012		-0.212***
		(0.674)		(0.688)		(0.004)
Asset Intensity		0.229		0.236		0.178
		(0.490)		(0.183)		(0.629)
Intercept	10.528***	10.772***	11.699***	11.839***	-3.497***	1.278
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.426)
Firm fixed effects			Yes	Yes	Yes	Yes
Installation fixed effects	Yes	Yes				
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Installation sector · year fixed effects	Yes	Yes				
Installation country · year fixed effects	Yes	Yes				
Observations	6,066	6,066	1,134	1,134	1,134	1,134
Adjusted R^2	0.955	0.955	0.982	0.982	0.973	0.974

Notes: This table presents the results of estimating the effect of a GHG disclosure mandate on GHG emissions (equation (1)). The dependent variable in columns (I) to (IV) is the natural logarithm of yearly emissions in metric tons of CO_2eq. The dependent variable in columns (V) and (VI) is the natural logarithm of yearly emissions in metric tons of CO_2eq scaled by cost of goods sold, i.e. carbon intensity. Reported values are coefficients and p-values in parentheses. Variables are defined in Table 1. All continuous variables are winsorized at the 1st and 99th percentiles. ***, **, and * indicate

significance at 0.01, 0.05, and 0.1 levels, respectively. In columns (I) and (II) standard errors are clustered by the firm that owns the installation. In columns (III) to (VI) standard errors are clustered by country.

Table 6: The effect of high ex-ante emissions levels and analyst coverage

	Moderating effect of high ex-ante emission levels				Moderating effect of high ex-ante analyst coverage			
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Dependent variable:	**ln(Emissions)**	**ln(Emissions)**	**ln(CI)**	**ln(CI)**	**ln(Emissions)**	**ln(Emissions)**	**ln(CI)**	**ln(CI)**
Experimental variables								
Post	0.179**	0.178**	0.150*	0.133	0.181**	0.180**	0.145*	0.129
	(0.026)	(0.039)	(0.088)	(0.112)	(0.026)	(0.039)	(0.091)	(0.114)
Post · Treatment	**-0.162*** **	**-0.161*** **	**-0.288*** **	**-0.272*** **	**-0.170*** **	**-0.167*** **	**-0.234*** **	**-0.221*** **
	(0.000)	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**
Post · Treatment · Large Emitter	**-0.057*** **	**-0.065*** **	**0.115*** **	**0.060*** **				
	(0.000)	**(0.000)**	**(0.000)**	**(0.003)**				
Post · Treatment · High Coverage					**-0.015*** **	**-0.024*** **	**-0.026*** **	**-0.050*** **
					(0.000)	**(0.008)**	**(0.000)**	**(0.000)**
Control variables								
Leverage		-0.081		-0.527		-0.074		-0.537
		(0.823)		(0.287)		(0.839)		(0.270)
Size		-0.013		-0.211***		-0.012		-0.213***
		(0.668)		(0.005)		(0.685)		(0.004)
Asset Intensity		0.240		0.175		0.240		0.186
		(0.179)		(0.633)		(0.181)		(0.620)
Intercept	11.700***	11.861***	-3.499***	1.257	11.699***	11.841***	-3.496***	1.282
	(0.000)	(0.000)	(0.000)	(0.437)	(0.000)	(0.000)	(0.000)	(0.424)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,134	1,134	1,134	1,134	1,134	1,134	1,134	1,134
Adjusted R^2	0.982	0.982	0.973	0.974	0.982	0.982	0.917	0.973

Notes: This table presents the results of estimating the effect of a GHG disclosure mandate on GHG emissions. The dependent variable in columns (I) to (IV) is the natural logarithm of yearly emissions in metric tons of CO_2eq. Reported values are coefficients and p-values in parentheses The dependent variable in columns (V) and (VI) is the natural logarithm of yearly emissions in metric tons of CO_2eq scaled by cost of goods sold. Variables are defined in Table 1. All continuous variables are winsorized at the 1st and 99th percentiles. ***, **, and * indicate significance at 0.01, 0.05, and 0.1 levels, respectively. Standard errors are clustered by country.

Table 7: The effect of a GHG disclosure mandate on financial operating performance

	(I)	(II)	(I)	(II)	(III)	(IV)
Dependent variable:	**ln(COGS)**	**ln(COGS)**	**ln(Sales)**	**ln(Sales)**	**Gross Margin**	**Gross Margin**
Experimental variables						
Post	0.025	0.041*	0.014	0.037*	0.001	0.004
	(0.362)	(0.054)	(0.584)	(0.064)	(0.861)	(0.648)
Post · Treatment	**0.085****	**0.079****	**0.074****	**0.062****	**-0.009**	**-0.011**
	(0.031)	**(0.017)**	**(0.036)**	**(0.010)**	**(0.213)**	**(0.116)**
Control variables						
Leverage		0.512*		0.733**		0.070
		(0.079)		(0.018)		(0.250)
Size		0.181***		0.215***		0.027**
		(0.004)		(0.000)		(0.022)
Asset Intensity		0.183		0.392		0.099**
		(0.570)		(0.246)		(0.037)
Intercept	22.118***	17.818***	22.741***	17.503***	0.417***	-0.261
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.283)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,134	1,134	1,134	1,134	1,134	1,134
Adjusted R^2	0.982	0.983	0.987	0.99	0.913	0.917

Notes: This table presents the results of estimating the effect of a GHG disclosure mandate on operating performance (equations (2) and (3)). The dependent variable in Columns (I) to (II) is the natural logarithm of yearly COGS. The dependent variable in columns (III) and (IV) is the natural logarithm of yearly sales. The dependent variable in columns (V) and (VI) is the yearly gross margin. Reported values are coefficients and p-values in parentheses Variables are defined in Table 1. All continuous variables are winsorized at the 1st and 99th percentiles. ***, **, and * indicate significance at 0.01, 0.05, and 0.1 levels, respectively. Standard errors are clustered by country.

Table 8: Results for matched sample

	(I)	(II)	(III)	(IV)	(IV)
Dependent variable:	**ln(Emissions)**	**ln(CI)**	**ln(COGS)**	**ln(Sales)**	**Gross Margin**
Experimental variables					
Post	0.266**	0.308**	-0.046	-0.036	0.014
	(0.020)	(0.047)	(0.550)	(0.489)	(0.262)
Post · Treatment	**-0.206****	**-0.404****	**0.178***	**0.159****	**-0.017**
	(0.050)	**(0.016)**	**(0.055)**	**(0.042)**	**(0.220)**
Control variables					
Leverage	0.221	-0.906	1.242*	1.257*	0.044
	(0.672)	(0.326)	(0.051)	(0.081)	(0.561)
Size	0.010	-0.271**	0.237*	0.302***	0.046***
	(0.707)	(0.034)	(0.070)	(0.007)	(0.010)
Asset Intensity	0.182	0.842**	-0.397*	-0.311*	0.108
	(0.403)	(0.025)	(0.053)	(0.065)	(0.165)
Intercept	10.553***	1.990	16.313***	15.547***	-0.626*
	(0.000)	(0.442)	(0.000)	(0.000)	(0.054)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	378	378	378	378	378
Adjusted R^2	0.957	0.964	0.987	0.990	0.940

Notes: This table presents the results of estimating the effect of a GHG disclosure mandate on GHG emissions and operating performance. We use a one-to-one matching without replacement on pre-treatment emissions for firms within the same Fama and French 5 industry. Reported values are coefficients and p-values in parentheses. Variables are defined in Table 1. All continuous variables are winsorized at the 1st and 99th percentiles. ***, **, and * indicate significance at 0.01, 0.05, and 0.1 levels, respectively. Standard errors are clustered by country.

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