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Discussion Papers

Deutsches Institut für Wirtschaftsforschung 2019

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IMPRESSUM

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http://www.diw.de

ISSN electronic edition 1619-4535

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Previous version: February 2019

This version: August 2019

Newer version published as DP 1875 (2020). Please cite the new version

We thank conference participants at the Annual Congress of the European Accounting Association in Milan (2018), the World Congress of Environmental and Resource Economists in Gothenburg (2018), the VHB Congress in Magdeburg (2018), and seminar participants at the University of Erlangen-Nuremburg and Copenhagen Business School for valuable comments. We also thank Jan Abrell, Helene Naegele and Thorsten Sellhorn for comments on earlier drafts of this paper. All remaining errors are ours.

Abstract

We examine whether mandatory disclosure of greenhouse gas (GHG) emissions influences companies' GHG emission levels. We identify the disclosure effect by exploiting a mandate requiring UK-incorporated listed companies to disclose information on GHG emissions in their annual reports. Using a difference-in-differences design, we show that disclosing GHG emissions in annual reports reduces emission levels by about 18% over three years. We find that emission reductions primarily occur for first-time mandatory reporters compared to firms who already voluntarily reported GHG information prior to the mandate. Further, we find that the emission reductions are permanent rather than transitory and we obtain stronger results for firms with larger savings potentials. Our effects are robust to various sample specifications, i.e., analysis at the installation- and firm-level, alternative control groups, and propensity score matching.

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

JEL: Q28, Q40, M41, M48

1. Introduction

We show that mandatory disclosure of greenhouse gas (GHG) emissions in annual reports influences companies' GHG emission levels. Prior research provides evidence that GHG emissions influence climate change, which is associated with rising sea levels and extreme weather phenomena like hurricanes, floods, heatwaves and droughts (IPCC, 2018). The Global Risk Report 2019, a global survey of 1,000 experts from business, government, academia and non-governmental organizations released by the World Economic Forum, ranks extreme weather and failure to limit the magnitude of climate change as top risks (World Economic Forum, 2019). Thus, under the Paris Agreement,[1] many countries have pledged to reduce their emissions of carbon dioxide and other greenhouse gases in order to limit the global temperature increase and have committed themselves to do so by enacting appropriate climate regulation. More than 40 countries have implemented a cap-and-trade program, which grants companies tradable allowances for how much carbon they can emit each year. In addition to the traditional measures of carbon pricing or emission standards, policymakers are increasingly requiring companies to disclose information on emissions.[2] However, surprisingly little is known about whether this measure can contribute to a reduction in GHG emissions. To fill this void, we examine this topic for a GHG emission disclosure mandate in the UK.

Our empirical strategy exploits the UK Companies Act (Strategic Report and Directors' Report) Regulations 2013, which implemented a mandate requiring UK-incorporated listed companies to report GHG emissions in their annual reports. Prior to the mandate, all (listed and non-listed) companies had to gather and report the emissions of their individual installations

[1] For further information on the Paris Agreement, cf. https://unfccc.int/process-and-meetings/the-paris-agreement/the-paris-agreement.

[2] A recent EU directive requires large public-interest companies to publish information on their handling of social and environmental challenges. Approximately 6,000 companies and groups across the European Union are covered by the directive. Affected entities must disclose information on the impacts of their operations on the environment, such as greenhouse gas emissions and local air pollution, and the use of renewable and/or non-renewable energy (European Parliament, 2014). Since 2015, also the TCFD, an industry-led task force on climate-related financial disclosures, establishes principles for companies to disclose their climate-related risk, cf. https://www.fsb-tcfd.org/.

(e.g., power plants or cement plants) regulated under the European Union Emissions Trading System (EU ETS) to a publicly available register.[3] However, complex corporate structures impeded the mapping of installations in this register to firms they belong to. Hence, the UK disclosure policy, by requiring the disclosure of aggregated emission data at the company level and thereby reducing costs for obtaining this information, aimed at increasing transparency concerning a company's GHG emissions for all interested parties.

We use this setting with installation-level data on CO_2 emissions ex-ante of the mandate to add to the growing 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 the return on assets and 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. 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.

Our paper differs from the aforementioned studies in several ways. First, 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 is a potential and long-term threat on a *global* scale. In case of *local* hazards, local stakeholders can be expected to aggressively use disclosure to pressure companies into reducing direct risks. In contrast, with global pollution, the intensity of stakeholder action is less clear because the costs from GHG pollution and therefore the benefits of reducing it, are distributed globally and often do not affect *local* stakeholders.[4] Second, Chen et al. (2018) use a broad disclosure regulation regarding CSR reports and measure the effect indirectly by a reduction in SO_2

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

[4] 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 feasible through end-of-pipe abatement, like the installation of filters. Economically viable end-of-pipe abatement technologies for GHG emissions are still experimental. Thus, the real effect of GHG emissions disclosure requirements is less clear.

emissions in cities, where at least one factory of a company is located. I.e., emissions are measured at the (city-level) destination and do not capture all firm emissions. However, destination-based pollution data can differ from pollution at the firm-level for instance in case of emission reallocation across regions or as a result of weather conditions and unobserved local emissions. Instead, we measure emissions at the source, i.e., emissions of installations (located in multiple countries) owned by a firm.

Third, the results of Chen et al. (2018) capture the effect of both disclosure of new information and of internal acquisition and measurement of the relevant information. Following Christensen (2017), we isolate the effect of disclosure regulation because we examine the effects of an additional disclosure channel for pre-existing information on GHG emissions, which are more readily accessible to investors through the disclosure mandate.

We argue that the GHG disclosure mandate should reduce emissions via real effects resulting from targeted transparency. Companies that are required to disclose GHG emissions for the first time provide decision-useful information to stakeholders who consider these in their expectations and actions. To avoid negative consequences of relatively high emissions, companies change their emission behavior either in reaction or in anticipation to shareholders' actions. We also argue that even companies that voluntarily disclosed GHG emissions prior to the mandate could (further) reduce their emissions due to comparability benefits.

To examine whether the UK GHG emission disclosure mandate contributes to a reduction of emissions, we use a difference-in-differences approach (DiD) surrounding the implementation of the Companies Act. We compare the difference between pre-mandate and post-mandate emission data for affected firms with pre-mandate and post-mandate emission data for a sample of control firms. This comparison is feasible because we are able to retrieve the aforementioned emission data as firms had to report emissions on an installation basis to a central registry already before the disclosure mandate. The treatment group in our DiD design consists of all installations, located in the UK or another European country, ultimately owned

by UK-incorporated companies affected by the Act.[5] The control group consists of installations ultimately owned by companies not affected by the Act, i.e., unlisted UK-incorporated firms as well as listed firms from EU15 countries. In addition, we differentiate between first-time mandatory reporters and already voluntary reporters.

Our results provide evidence of a significant reduction in GHG emissions after the Act for treatment group firms relative to control group firms. The effect is sizable in its magnitude – depending on the model– between 17% and 19.5% over three years. We observe the emission reduction for both first-time mandatory adopters and already voluntary adopters. However, the effect is more pronounced for first-time mandatory reporters.[6]

In additional tests, we find that the emission reductions occur over several years and obtain stronger results for larger emitters with larger savings potential. We also provide evidence that the aforementioned effects are robust to various sample specifications, i.e., installation- and firm-level analysis, alternative control groups, and propensity score matching. Lastly, we find that the effect is permanent rather than transitory.

This paper contributes to the literature in several ways. First, we add to the literature on the real effects of disclosure regulation. Prior studies document real effects of disclosure regulation on investment (Biddle et al., 2009; Biddle and Hilary, 2006; Cheng et al., 2013; Graham et al., 2011; Shroff, 2017; Shroff et al., 2014), mine safety (Christensen et al., 2017), managerial short-termism (Ernstberger et al., 2017; Granja, 2018; Kraft et al., 2018), and wastewater as well as sulfur dioxide emissions (Chen et al., 2018). We contribute to this literature by documenting the direct influence of mandating GHG disclosure on the level of emissions, which ultimately relate to climate change, one of the most pressing problems of mankind.

[5] For our study, we focus 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, and Dechezleprêtre et al. (2014) find no substitution effect across countries, our results should not be severely biased.
[6] We find virtually unchanged results if we eliminate already voluntary reporting firms from the sample.

Second, we contribute to the literature on GHG disclosure. The literature so far primarily focuses on economic consequences in terms of changes in stock prices or cost of capital (Kleimeier and Viehs, 2016; Krueger, 2015; Matsumura et al., 2014). Prior literature on voluntary disclosure of GHG information is plagued with endogeneity problems (Broadstock et al., 2018). We show that mandatory disclosure of GHG information on average is associated with lower GHG emissions for mandatory as well as voluntary adopters of GHG emission disclosure.

Third, we contribute to the literature examining different regulatory options for mitigating greenhouse gas emissions. Several studies analyze the abatement effect of cap-and-trade systems on emissions in the industry and electricity generation sectors (Anderson and Di Maria, 2011; Bel and Joseph, 2015; Ellerman et al., 2010; Martin et al., 2016; Murray and Maniloff, 2015). We contribute to this literature by providing evidence on the effect of mandatory GHG emission disclosure on emission levels, thus, offering insights on the gains of such mandates that help regulators in assessing different regulatory options.

The remainder of this paper is structured as follows. Section 2 introduces the regulatory background and develops the hypotheses. Section 3 presents the research design. Section 4 presents and discusses the results. Section 5 concludes.

2. Regulatory Background and Hypothesis Development

2.1. Regulatory Background

Our study focuses on The Companies Act 2006 (Strategic Report and Directors' Report) Regulations 2013. Under the Act, listed companies are required to disclose information on their 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.[7] The Act applies to all fiscal years ending on or after the 30th of September 2013 and requires affected companies to report their direct and indirect GHG emissions during the last 12 months.[8] Affected companies must report in the directors' report within the annual report direct emissions caused by the combustion of fuel and the operation of any facility, as well as the indirect emissions resulting from the purchase of electricity, heat, steam or cooling in metric tonnes of carbon dioxide equivalent (tCO_2eq).[9] 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 Financial Reporting Council.[10] Directors may face fines if they were reckless or failed to take reasonable steps to ensure compliance (paragraph 415, (4) and (5) of the Act).

Because emission data due to the Act are only available following the implementation of the Act in 2013, we use emission data obtained from the EU ETS, which covers direct emissions of affected installations from combustion and other industrial processes. Examples for such installations are power plants, crude oil refineries or chemical factories. Since its introduction in 2005, the EU ETS has represented the world's largest multi-national emission trading system.[11] The EU ETS covers about 45% of the European Union's GHG emissions (Ellerman et al., 2016).[12] The remaining emissions not covered by the EU ETS primarily come from

[7] Companies entitled to prepare their financial accounts under the small company exemption of The Act are not required to disclose their emissions.
[8] Companies may also report emissions for a period different from their financial year. For our study, we use data provided by the EU ETS. EU ETS data are measured on a calendar year base.
[9] Emissions of the following greenhouse gases have to be reported: 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 give individual figures for emissions of each of the greenhouse gases listed. However, they must state the annual quantity of GHG emissions in tCO_2eq.
[10] The Financial Reporting Council is a regulatory body responsible for (among others) conducting enforcement investigations to promote transparency and integrity in business.
[11] Emission trading systems similar to the EU ETS have also been introduced elsewhere. In the US, the Californian 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).
[12] In our sample country UK, the EU ETS covers a similar share of total emissions. E.g., 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).

transport, residential, and agricultural sectors (DECC, 2016). Installations are covered by the EU ETS if they contain a combustion plant with a rated thermal input of at least 20 MW – enough to supply about 15,000 households with electricity – or if they perform one (or more) of the following activities: 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 energy supply, business, and industrial process sectors, where it covered approximately 79% of UK emissions in 2014.

The data gathered in the framework of the EU ETS is publicly available in the European Union Transaction Log (EUTL) register, which provides installation-level information such as an installation's name, address information, its operator, the sector it is assigned to in the EU ETS (i.e., its main activity type, e.g., production of bulk chemicals or combustion of fuels). It also provides information on the volume of *verified emissions*. Verified emissions are emissions of an installation that have been verified by an independent third party accredited by the relevant administrative bodies (European Parliament, 2003), which are typically accounting and engineering companies.

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.[13]

2.2. Hypothesis Development

We examine the real effects of introducing mandatory GHG emission disclosure. Besides the overall effects we in particular investigate the effects for the following two subsamples: (1) *first-time mandatory adopters*, i.e. firms that did not disclose GHG emissions and are forced to

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

disclose them under the Act, and (2) *already voluntary adopters*, i.e. firms that already disclosed GHG emissions voluntarily prior to the Act and mandatorily continue to do so after it.

To derive a hypothesis on the impact of GHG emission disclosure for *first-time mandatory adopters*, we draw upon the theory of the targeted disclosure cycle (Fung et al., 2007). The theory, in general, assumes that disclosure influences the behavior of its receivers, i.e., share- and stakeholders, leading to real effects. Information providers, i.e., companies, directly react to the different behavior of information receivers and/or preemptively react to the anticipated behavior by changing their relevant decisions.

In a recent literature review, (Hombach and Sellhorn, 2019) apply this theory to corporate disclosure. They define five criteria that have to be met so that corporate reporting has real effects. First, the corporate disclosure regulation needs to change the actual disclosure implying efficient enforcement of new disclosure requirements. We already discussed in the background section that the Act mandates disclosure of GHG emissions for all listed UK-incorporated firms as part of the directors' report. In addition, the directors' report is approved by the board of directors and reviewed by the statutory auditor, and detailed application guidance assures the comparability of GHG disclosure across firms (DEFRA and DBEIS, 2013).[14]

Second, the new disclosure needs to enter users' information sets used for decision-making. As mentioned in the background section, companies already had to collect and disclose GHG emissions on an installation basis. Only after the mandate disclosure is available on a company basis, allowing investors and other stakeholders to assess a company's GHG emissions and to compare them to those of peers. Thus, the Act reduces stakeholders' information processing costs with respect to GHG emissions. Moreover, the disclosure of GHG emissions in annual

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

reports has raised awareness of companies' GHG emissions.[15] This effect might have been reinforced by traditional and social media, which pick up on the emission data and report on it.

Third, the new information for stakeholders needs to be considered in their expectations and actions. GHG emission data are important to them because they influence a company's climate risks, including (i) regulatory risks (ii) reputational risks, and (iii) litigation risks (DEFRA, 2010). Regulatory risks are future changes in regulations that may increasingly internalize the cost of carbon (e.g., through trading schemes or taxes) or may prescribe technical requirements and hence increase companies' cost or even endanger their business model. Reputational risks result from changing the market or consumer behavior. Consumers or other pressures may take action on climate change (e.g., boycotts) which affects firms with high emission levels. E.g., Kölbel et al. (2017) show that media coverage of corporate social irresponsibility increases their financial risk by providing conditions that increase the likelihood of stakeholder sanctions. Litigation risks results from a growing probability of environmental or climate litigation (Erion, 2009). Prior literature shows that investors consider environmental risks in their investment decisions as indicated by a positive association of environmental risks with cost of capital (Sharfman and Fernando, 2008) and negative association with a firm's market value. In addition, shareholders and other stakeholders have social preferences for low emissions beyond their financial or risk implications, i.e., shareholders and stakeholders do not only care about payoffs but also about an ethical behavior of the firm (Kim et al., 2019). Friedman and Heinle (2016) show in an analytical model how investor preferences for non-cash flow-based activities like CSR can influence market prices and in turn, induce managers to undertake these activities. In line with this model, Amel-Zadeh and Serafeim (2018) show in a global survey of institutional investors that ethical considerations play an important role for investment decisions besides financial aspects.

[15] According to a survey of PricewaterhouseCoopers (PwC) in partnership with the Carbon Disclosure Project (CDP) voluntary reporting of GHG emissions raised awareness of companies' boards, employees and the public (PWC and CDP, 2010).

Fourth, managers need to change their decisions concerning emissions as a direct response to the change in stakeholders' behavior or preemptively by anticipating the change in stakeholders' behavior. We argue that the Act could impose market-value penalties for high emission companies (Griffin et al., 2017; Matsumura et al., 2014). The market value-penalties of high emissions constitute a feedback effect of GHG disclosure, which can reinforce managers' efforts to reduce emissions because changes in a company's market value are an important determinant of managers' variable compensation. Lower market values or even higher emissions themselves can harm companies' reputation (DEFRA, 2010) and thus may tarnish their managers' reputation. Moreover, managers want to avoid other stakeholders' reactions, such as consumer boycotts, and want to appeal to investors with social preferences. Thus, managers are aware of stakeholders' actions and responses to high emissions and might reinforce emission abatement efforts.

There are also arguments suggesting any effects of mandatory GHG emission disclosure could be small or even negligible. UK firms were already subject to the EU's cap-and-trade scheme when the Act came into effect which provides incentives for GHG reductions. Thus, GHG disclosure might have no incremental effect. In addition, the EU ETS scheme or inferences of GHG emissions from voluntarily disclosing peers might generate sufficient information on GHG emissions. Griffin et al. (2017) show that investors price GHG emissions negatively for both disclosing *and* non-disclosing firms. Thus, additional costs of GHG disclosure might outweigh the potential benefits of disclosure because investors may rely on other information channels.

However, given the lower processing costs for stakeholders and the increased awareness of GHG emissions, we expect that the GHG disclosure mandate spurs managerial efforts to reduce emissions and hypothesize:

H1: The UK mandate for disclosure of GHG emissions leads to lower emission levels for first-time adopting entities.

Our second hypothesis focuses on the effects of the Act on *Already voluntary adopters*. These firms already disclosed GHG emissions before the Act came into force. Thus, at first sight, the Act did not change stakeholders' information set concerning the GHG emissions of these companies. As one important link of the aforementioned theory is missing, the Act might not have an additional impact on abatement efforts for *Already voluntary adopters*.

However, we argue that the Act can change stakeholders' information set also for *Already voluntary adopters*. First, stakeholders of these firms might benefit from comparability benefits when other firms in the UK are forced to adopt GHG emission disclosure allowing for more efficient benchmarking with peers (Cao et al., 2019; DEFRA, 2010). Besides the reliability and relevance of information, its comparability is also important for decision making. Comparisons allow stakeholders to better assess the relative magnitude of emissions and, thus, to draw better conclusions for their actions (Wang, 2014). In addition, the stakeholders might benefit from firms' reporting improvements around the mandate, as many *Already voluntary adopters* had no clear guidelines and, thus, have not provided comparable and consistent data on GHG emissions prior to the Act. 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 from them to use voluntary GHG emission data (Trucost, 2009). Based on these two arguments, we predict that the aforementioned targeted disclosure cycle for corporate disclosure – albeit possibly to a lesser extent – also holds for *Already voluntary adopters*. We predict:

H2: The UK mandate for disclosure of GHG emissions leads to lower emission levels for already voluntary reporting entities.

3. Research Design and Data

3.1. Empirical Model

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

$$Ln\_Emission = \beta_0 + \beta_1 \times Post + \beta_2 \times (Post \times First\text{-}time\ mandatory) + \beta_3 \times (Post \times Already\ voluntary) + \sum Control\ variables + Fixed\ effects\ for\ installation, year, industry\text{-}year, and\ country\text{-}year + \varepsilon \quad (1),$$

where all variables are defined in Table 1. *Ln_Emission* are the direct GHG emissions of affected installations.[16] Because emission data is not available in annual reports of (all) affected firms prior to the mandate, we use highly granular administrative emission data—at the installation level— obtained from the EU ETS.[17]

As the treatment group, we use installations ultimately held by UK listed and incorporated firms because these firms are affected by the mandate for GHG emission disclosure. Because several UK firms already voluntarily disclose GHG emission information prior to the mandate, we separately examine the effect of the mandate for GHG emission disclosure for the following two groups of firms: *First-time mandatory* reporting firms, which did not publish any GHG emission information prior to the mandate,[18] and *already voluntary* reporting firms, which published GHG emission information prior to the mandate, based on CDP data.

As the control group, we use installations ultimately held by firms not affected by the mandate for GHG emission disclosure. In detail, the control group consists of non-listed British firms as well as firms listed in other EU15 countries. This approach enables us to estimate the following effects of the mandate: (1) for first-time mandatory reporting firms relative to non-

[16] We discuss the common trend of GHG emissions for treated and non-treated firms in our results section.
[17] For robustness, we also aggregate installation-level emissions to the firm level and find virtually unchanged results.
[18] We find virtually unchanged results if we exclude all voluntary reporting firms.

reporting firms not affected by the mandate, (2) for already voluntary reporting firms relative to other already voluntary reporting firms not affected by the mandate.

Post is a binary variable indicating periods after the mandate for GHG emission disclosure. For our study, we focus on the period 2008 to 2016[19]. The mandate for GHG emission disclosure applies to all fiscal years ending on or after the 30th of September 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.

Our variables of interests are β_2 (*Post* × *First-time mandatory*) and β_3 (*Post* × *Already voluntary*). A negative coefficient estimate on β_2 indicates that the disclosure mandate for first-time mandatory reporters is associated with a reduction in emissions (H1). A negative coefficient estimate on β_3 indicates that the disclosure mandate for already voluntary reporters is associated with a reduction in emissions (H2).

To control for differences in characteristics of firms who 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.[20] Year fixed effects capture annual effects affecting all installations symmetrically, for instance, changes in gasoline prices.[21] Industry-year fixed

[19] We find virtually unchanged results if we use a shorter period, i.e., the period from 2010 to 2015.
[20] Because we are using installation fixed effects, we do not include a binary variable indicating treatment group installations for all tests.
[21] 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*).

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. Because not all installations of a firm are located in the same country as the owning firm itself[22], divergent national regulations or social pressure by certain interest groups might have an influence on emission reductions, too.[23]

To reduce the influence of outliers, we winsorize all continuous variables at the 1st and 99th percentiles.[24] In addition, we draw our inferences based on standard errors clustered by firm because all installation of a UK listed and incorporated firm are affected by the mandate independent of the actual geographic location or industry of an installation.

--- Insert Table 1 here ---

3.2. Data

Our starting sample comprises all European (EU27 plus Norway) installations covered by the EU ETS with available emission data over the period 2008 to 2016 (66,573 installation-years, 7,397 unique installations). To derive our final sample, we first exclude installations without a known ultimate owner. This step is required, as the assignment of an installation to either the treatment or control group depends on its ultimate owner being affected by the mandate to disclose GHG emissions or not. The link between the installation owner and the ultimate owner is established by the Ownership Links and Enhanced EUTL Dataset Project (OLP).[25] The OLP provides the name and Bureau-van-Dijk identification number (BvD-ID) of an installation's parent company. This step leads to the omission of 13,770 installation-years

[22] As an example, the French firm Compagnie de Saint-Gobain owns installations in 15 different European countries.
[23] We find virtually unchanged results if we use firm fixed effects instead of installation fixed effects. We note that using firm fixed effects assumes a similar emission level for all installations of one 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.
[24] We find virtually unchanged results using no outlier treatment and using trimming at the 1st and 99th percentiles.
[25] Data and a technical note describing the mapping between installations and ultimate owners is available from http://fsr.eui.eu/climate/ownership-links-enhanced-eutl-dataset-project/.

(1,530 unique installations). Next, we exclude installations ultimately owned by firms from non-EU15 countries to ensure a comparable regulatory and economic environment, which helps create a valid control group. This step leads to the omission of 14,589 installation-years (1,621 unique installations). Further, we exclude firms from countries with other emission-related regulations to reduce the influence of confounding events. During our sample period, only Ireland implemented an emission-related regulation (InsideIreland.ie, 2009). Excluding installations owned by Irish firms leads to an omission of 486 installation-years (54 unique installations).[26]

Next, we limit the sample to installations owned by UK firms (listed and unlisted) as well as installations owned by listed firms from other EU15 countries. We use all UK firms, listed and unlisted, to examine the effect of the mandate for GHG emission disclosure without potential effects inherent in a multi-country setting. In the UK sample, we thus compare the decisions of listed UK firms with respect to their level of emissions with that of unlisted UK firms. In our international sample, we only use listed firms from other EU15 countries as the control group, because only listed UK firms are affected by the mandate. Thus, listed firms from EU15 countries are more comparable to our treatment group firms.[27]

Further, we exclude firms which changed the ultimate owner in the years surrounding the mandate to disclose GHG emissions because these installations are likely affected by divergent firm policies. These requirements lead to an omission of 27,333 installation-years (3,037 unique installations). Next, we exclude installations from the energy sector based on the NACE Rev.2 sector code "35". We exclude energy sector installations because the UK government implemented a policy to support the EU ETS allowance price in 2013, too (so-called Carbon Price Floor [CPF]). The CPF taxes fossil fuels to generate electricity (Hirst, 2018). This policy

[26] We find robust results if we exclude installations owned by French firms. During the sample period, France mandated disclosure of-among others-broad environmental information. We keep these installations for our main tests, because France did not explicitly mandate disclosure of GHG emissions.
[27] We find virtually unchanged results if we only use either UK unlisted or EU listed firms as control group. If we additionally limit our sample to installations located in EU-15 countries, we again find unchanged results.

fosters incentives to reduce emissions to avoid additional tax payments. Thus, including these installations would likely lead to overstated results.[28] 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.[29] In detail, we exclude installations if their minimum emission level over the sample period is less than 1 percent of their maximal emission level.[30] This step leads to an omission of 90 installation-years (10 unique installations). Finally, we exclude installations with missing data on firm-level control variables. Firm-level data are taken from the BvD Amadeus database.

Overall, we end up with a balanced panel of 4,797 installation-years (533 unique installations) over the period 2008 to 2016. Table 2 presents the composition of our sample. In line with the scope of the EU ETS, most installations are from the manufacturing sector (see Panel A). With regard to the distribution of treatment and control group firms (see Panel B), we find that a similar number of installations is owned by first-time mandatory and already voluntary reporting firms. At the firm level, in line with (Sustainalytics, 2016), we find that 48 percent of the affected firms already disclosed GHG information prior to the mandate. Thus, our installations are approximately equally distributed across first-time mandatory and already voluntary reporting firms. Our control group is considerably larger because our treatment group is limited to UK-incorporated listed firms, whereas our treatment group consists of UK firms as well as firms listed in other EU15 countries. With regard to geographic distribution (see Panel C), most installations are located in the UK, Germany, and France.

--- Insert Table 2 here ---

[28] We exclude all energy sector installations (electricity, gas, and heat) and not only electricity because these three types are closely related and are often produced in the same installation. Our results are robust to not excluding energy sector installations or only excluding electricity installations.

[29] 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.

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

4. Results

4.1. Descriptive statistics

Table 3 reports descriptive statistics (Panel A) and correlations (Panel B) for our final sample. The average (median) installation in our sample produces yearly emissions of 199,655 (36,966) tCO_2eq. Due to skewed distribution, we use log-transformed emissions for our multivariate analysis. With regard to characteristics of firms who ultimately hold installations, we find that treatment group firms are larger, more highly leveraged, and have a higher return on assets compared to control group firms.[31] With regard to the Pearson and Spearman correlations, we do not observe high correlations between the continuous variables.

--- Insert Table 3 here ---

4.2. Implications of the Act on GHG emissions

Table 4 reports results for testing the influence of the GHG disclosure mandate on GHG emissions for first-time mandatory (H1) and already voluntary reporting (H2) firms. If GHG disclosure leads to lower GHG emissions, we expect to observe negative and significant estimates.

Columns (I) and (II) in Table 4 present results obtained when assigning a common treatment to both first-time mandatory and already voluntary reporting firms. Columns (III) and (IV) present results obtained when using separate difference-in-differences coefficients for first-time mandatory and already voluntary reporting firms. We present both specifications without (columns (I) and (III)) and with (columns (II) and (IV)) firm-level control variables. In all specifications, we use installation, year, industry-year, and country-year fixed effects, to account for heterogeneity of installations and firms who ultimately own installations.

In column (I) the estimate of the coefficient related to the common interaction term *Post * Treatment* is significantly negative (p: <0.01). In economic terms, the point estimate suggests

[31] We find virtually unchanged results if we use a matched sample to account for differences in firm characteristics.

that, during the years after the implementation of the Act, affected firms on average decreased their GHG emissions by about 21% as compared to companies not affected by the Act. For interpreting estimated magnitudes, note that throughout this section we report the approximate estimates as reported in the regression tables. Precise transformations differ by a margin, e.g., the coefficient of -0.21 implies an effect of 100*(exp(-0.21)-1) = -0.189, i.e. 18.9%. Controlling for differences in firm characteristics (column (II)), we obtain an estimate of -0.19. Hence, differences in firm characteristics do not alter our main estimates. Lastly, note the high adjusted R^2, which is due to our stringent fixed effects specification that absorbs installation-specific and year-specific unobservable heterogeneity, as well as industry trends and country trends.

To differentiate the effect of the regulation on first-time mandatory and already voluntary reporting firms, we include distinct *Post* × *Treatment* variables for first-time mandatory and already voluntary reporting firms, as shown in columns (III) and (IV). Column (III) reports the estimates without further firm-level controls. We find a negative and significant coefficient estimate on *Post* × *First-time mandatory* (p: <0.01) indicating a reduction in GHG emissions by about 22% for firms which did not publish GHG information prior to the disclosure mandate. For *Post* × *Already voluntary* we find a negative and significant coefficient of comparable magnitude (p: <0.05). This result indicates that both groups of firms affected by the disclosure mandate to reduce emissions. Column (IV) again confirms the magnitudes of reduction when controlling for firm size, return on assets, and leverage. Overall, Table 4 confirms hypothesis 1 and presents evidence for hypothesis 2, yet only at lower levels of significance. We conjecture that the decline in significance is due to the concurrence of the treatment with already voluntary disclosure in this group and that this concurrence renders the treatment effect to be less precise.

--- Insert Table 4 here ---

4.3. The treatment effect as compared to listed and unlisted firms

Our results above rely on a control group that consists of installations owned by both non-listed UK firms and listed European firms. Next, we investigate whether the treatment effect

entails variation with respect to these different firm types in our control group. Specifically, we split the sample and re-run model 1 when isolating the two control groups. Column (I) of Table 5 presents results using only installations from listed European firms as control group. Because the GHG mandate applies to UK listed firms only, an advantage of using installations from European listed firms as control group is that we obtain a more homogeneous sample composition in terms of firm characteristics. By contrast, using listed firms only may have the disadvantage that treatment and control group installations are located in different countries and, thus, may be influenced by different regulatory environments.

Using a sample of listed firms only, we find reductions of about 16% for first-time mandatory reporters (p: <0.01). In contrast, already voluntarily reporting firms exhibit no significant treatment effect. Hence, when comparing the treatment effect to installations from listed firms abroad, our results only confirm hypothesis 1, on first-time mandatory reporters. The effect for already voluntary reporters again shows little or no significance at conventional levels (p: <0.15), further indicating that hypothesis 2 is neither strictly rejected nor supported.

Column (II) of Table 5 presents results using only installations from UK incorporated firms as control group. Because the GHG mandate applies to UK listed firms only, using UK incorporated firms not affected by the Act as control group enables us to examine within-country effects because all installations are ultimately held by firms located in the same country. However, because UK firms not affected by the Act are not publicly listed, these firms may differ from listed firms.[32] Using a sample of UK incorporated firms only, we find a significant reduction in GHG emissions for first-time mandatory reporters of about 29% (p: <0.01) and for already voluntary reporters of 33% (p: <0.01). These high magnitudes require further elaboration. Below, we illustrate that both the treatment and the control group show negative parallel trends for their emissions. British non-listed firms, however, do not share this negative

[32] As an example, Burgstahler et al. (2006) provide evidence that European private and public firms respond differentially to institutional factors, such as outside investor protection and capital market structure.

trend. As such, our estimates are larger in absolute value when using British non-listed firms as the control group.

While we therefore suggest taking results when isolating the sample for the British non-listed control group with care, this sub-sample, however, allows for an investigation of geographic variation in the treatment effect. In detail, we split the sample into emissions of UK firms incurred by installations in foreign countries (column (III)) and emissions of UK firms incurred by installations within the UK (column (IV)).

This split reveals a substantial difference in the reduction of emissions. For first-time mandatory reporters, we only find a significant reduction for emissions incurred in the UK. This finding points to a home bias of the treatment effect. For already voluntary reporters, we find a significant reduction in emissions in both sub-samples. Note, however, that the sample size is strongly reduced compared to the baseline estimation, as the EU listed firms constitute the by far larger control group of our full sample. Hence, our findings on the home bias should be considered bearing the smaller sample size in mind.

--- Insert Table 5 here ---

4.4. Savings potential for firms with few and many installations

Next, we investigate a possible channel for emission reductions: firms' savings potential as a function of their size and number of installations. To explore this channel, we run our main model 1 separately for firms with high and low savings potential. I.e., we split the sample and report estimates for firms with a number of installations above the sample median, and with firms who control a number of installations equal or less than the sample median.

Table 6 presents the results for the savings potential analysis. We only find significant reductions in GHG emissions for firms (first-time mandatory and already voluntary reporters) with a high savings potential and no effect for firms with a low savings potential. We conclude that larger firms, i.e., firms with many installations are driving the overall emission reductions. We conjecture that this effect may be attributed to specialized departments within larger firms

that are able to manage the firms' emission portfolios more efficiently and react to the treatment more strongly. Furthermore, as hypothesized in our theory section, stakeholders actively take the disclosure of GHG emissions and resulting regulatory and market risks into consideration. Our finding that the effect of mandatory disclosure is more pronounced for larger firms may therefore also result from well-organized stakeholders for large firms.

--- Insert Table 6 here ---

4.5. Assessing the validity of the common trend assumption

The main assumption underlying the difference-in-differences approach is a common trend between control and treatment group firms prior to treatment (Roberts and Whited, 2013). Albeit an implementation over two fiscal years reduces the likelihood of divergent trends, we control for a potential violation of a common trend assumption. To examine whether a common pre-treatment trend exists, we follow the literature (Bertrand and Mullainathan, 2003; Serfling, 2016). In detail, we replicate our main model 1 and replace the interaction terms *Post* × *First-time mandatory* and *Post* × *Already voluntary* 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.

Figure 1 depicts the results of this estimation. Values on the y-axis show point estimates and 90 percent confidence intervals for first-time mandatory reporters (black circle, dashed line) and for already voluntary reporters (white circle, solid line). 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 >= 2 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 >= 2 value is the average of the estimates for the interactions between *Treat* and year dummies for periods two or more years after treatment.

We observe a clear difference in pre-treatment and post-treatment coefficients, both for first-time mandatory reporters and already voluntary reporters. While both treatment groups have somewhat higher pre-treatment emissions than the control group, this difference is fairly stable during the pre-treatment period and not significantly different from zero in each year from t <= -4 to t = -1. Based on these results, we do not find a violation of the common trends assumption.

The estimates for the period t = 0 to t >= 2 show the evolution of the treatment effect over time. We observe that emissions in the treatment group decrease significantly for both groups of reporters. Moreover, installations of both first-time mandatory reporters and already voluntary reporters reduce their emissions in every post-treatment year, instead of yearly reductions converging back to zero. This suggests that the effect of the Act on emissions is permanent rather than transitory.

--- Insert Figure 1 here ---

4.6. Alternative sample compositions

Next, we re-estimate our main model utilizing a number of alternative sample compositions (Table 7). In detail, we use samples at the firm instead of at the installation level to account for a potential substitution across installations of a firm who ultimately owns installations. Further, we use matched as well as stratified samples to account for heterogeneity in the industry composition of installations as well as firms.

First, we test whether our installation-level results also hold at the firm level (Table 7 column (I)).[33] As described in Section 3.2, we use the mapping of installation to the firm level from the Enhanced EUTL Project to connect installations to their ultimate parent companies.

[33] For the firm-level analyses, we additionally include firm and year fixed effects instead of our fixed effects used for estimating model 1.

Using aggregated firm level emissions, we find that first-time mandatory GHG emission disclosure leads to significantly lower emissions (p: <0.01), by about 26% compared to the control group. The estimated effect is similar to our main results at the installation level. This result confirms that the Act leads to a decrease in emissions at the firm level, instead of effecting a mere shift of emissions between installations belonging to the same firm. In contrast to our baseline specification at the installation level, we do not find a significant emission reduction effect for already voluntary reporting firms. While the point estimate is quantitatively in line with our baseline results at the installation level, it is not significant at conventional levels.

Second, we use a matched sample at the firm level to account for differences in firm characteristics (Table 7 column (II)). In detail, we match firms using one-to-one nearest neighbor propensity score matching without replacement, based on pre-treatment data from 2008 for size, performance, and leverage, as well as the industry affiliation according to the Fama and French 5-industries classification (Fama and French, 2016). We match using pre-treatment data to reduce a potential influence of the treatment on our matching variables. We find a significant reduction in GHG emissions for first-time mandatory reporters (p: <0.01) of about 29%. The effect is in line with our unmatched sample. However, in contrast to the unmatched sample, we also find a significant decrease in emissions by 28.5% for already voluntary reporters (p: <0.10).

Third, we disaggregate the firm-level matched sample (Table 7 column (III). I.e., we use all installations of the firms identified in our firm-level matched sample. Results confirm the finding from the firm-level regression that both first-time mandatory reporters and already voluntary reporters significantly decrease their emissions in the periods following the Act. However, the decrease is found to be less strong – 17.5% for first-time mandatory reporters and 18.3% for already voluntary reporters – compared to the firm-level analysis. This effect is attributable to our strict fixed effects design at the installation level, which is not feasible at the firm level.

Fourth, we use an industry stratified sample at the installation level (Table 7 column (IV)). We stratify the sample at the NACE Rev.2 sector level so that our sample only consists of installations belonging to industries available in both the treatment and control group. We find a significant reduction in GHG emissions for first-time mandatory and already voluntary reporters. We conclude that differences in the industry composition of installations do not drive our results.

Fifth, we match installations using one-to-one nearest neighbor propensity score matching without replacement within the stratified sample based on 2008 data of size, performance, and leverage of their parent firm (Table 7 column (V)). Again, we find a significant reduction in GHG emissions for first-time mandatory reporters (p: <0.05) of about 25% and no significant reduction in GHG emissions for already voluntary reporters.

Overall, the different samples corroborate our main findings with regard to H1 that a mandate to disclose GHG information is associated with a reduction in GHG emissions for firms which did not publish GHG information prior to the mandate. Findings with regard to H2 are weaker and sensitive to the sample composition.

--- Insert Table 7 ---

5. Conclusion

In this paper, we examine whether mandatory greenhouse gas (GHG) emissions disclosure in annual reports reduces companies' GHG emission levels. We apply a difference-in-differences design on a set of European installations and exploit the introduction of The Companies Act 2006 (Strategic Report and Directors' Report) Regulations 2013 as a source of exogenous variation in the disclosure regime. We find that installations of UK companies affected by the Act exhibit significant reductions in GHG emissions compared to control installations, controlling for various time-variant and time-invariant fixed effects as well as control variables at the firm level. We present separate estimates for first-time mandatory adopters, i.e., firms that did not disclose GHG emissions previously and are forced to disclose

them under the Act, and already voluntary adopters, i.e., firms that already disclosed GHG emissions voluntarily prior to the Act and mandatorily continue to do so afterward. We find that, on average, installations owned by first-time mandatory adopters exhibit additional emission reductions between 17% and 19.5% over three post-treatment years, relative to pre-treatment emission levels. For the already voluntary reporters, we find less pronounced effects.

In additional tests, we observe that the emission reductions are permanent rather than transitory and are more pronounced for larger emitters with a larger savings potential. The aforementioned effects are robust to various sample specifications, i.e., installation and firm-level analysis, alternative control groups, and propensity score matching.

The emissions data has been publicly available already before the UK mandate, albeit at installation-level and with high costs of matching emissions to respective firms. Our analysis in this paper shows that requiring companies to disclose their emissions in a manner easily accessible to shareholders has real effects on company emissions. We conjecture that disclosure provides decision-useful information to stakeholders. In turn, GHG disclosure provides managers with incentives to search for additional abatement opportunities. Therefore, increasing transparency and decreasing the costs of information with respect to emissions is an effective policy to achieve emission reductions. Based on our analysis, further emission abatement may be achieved by extending the disclosure requirement to other countries.

Our analysis is subject to limitations. First, we only investigate the effects of a mandate for disclosing GHG emissions in the UK. However, given the large market and the high quality data on verified emissions we believe that the results might be also applicable to other settings. Second, we provide evidence on the emission reduction effect of GHG emission disclosure but do not investigate to what extent the reduction of GHG can contribute to mitigating climate change. Overall, we view this paper on the real effects of mandatory disclosure of greenhouse gas emissions as a starting point for examining the underlying mechanisms behind the observed emission reductions.

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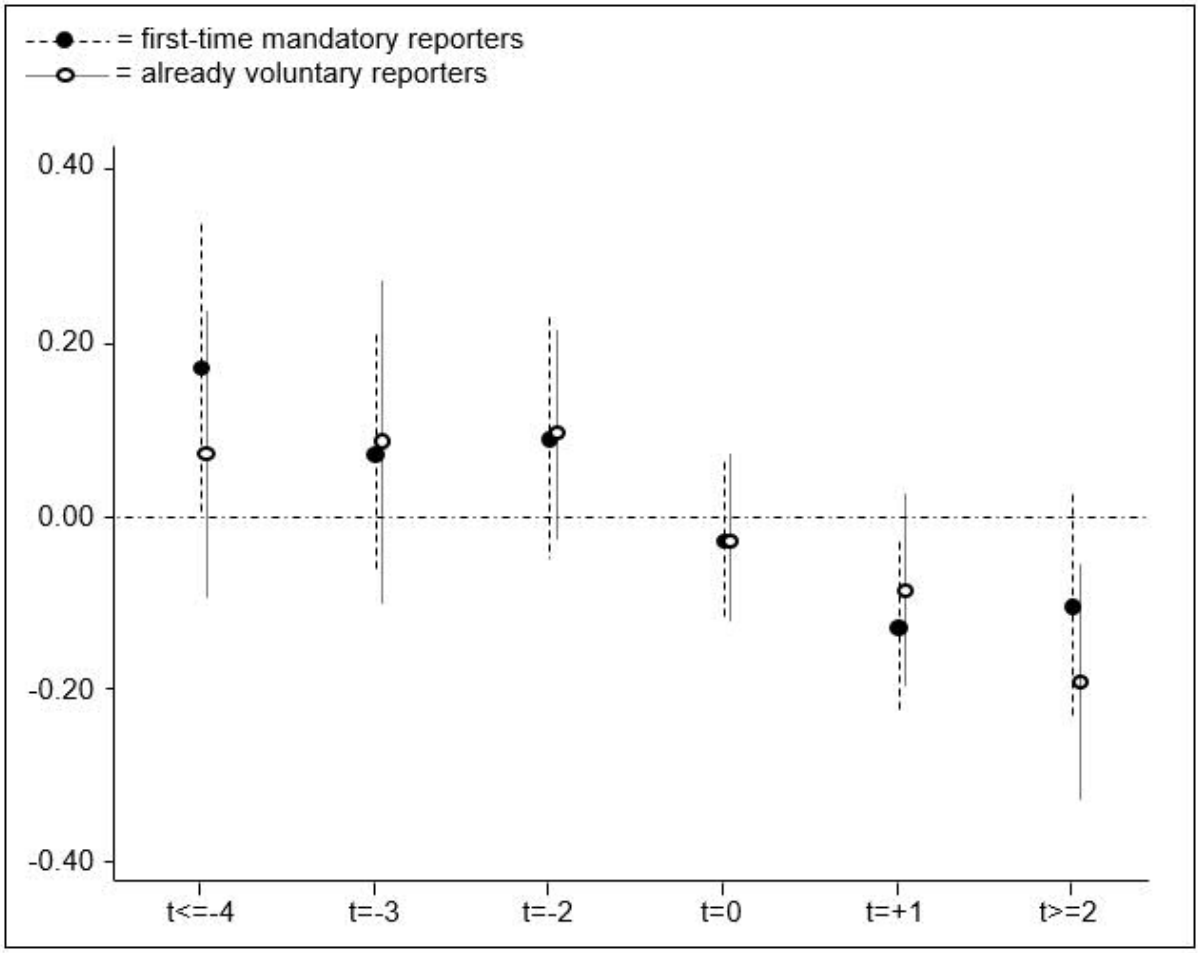
Figure 1: Pre-treatment trend analysis and annual decomposition of the counterfactual treatment effect



This figure shows the change in verified emissions surrounding the mandate to disclose GHG information to examine differences in pre-treatment emission trends for treatment and control group firms. For this test, we replicate model 1 but replace *Post* × *First-time mandatory* and *Post* × *Already voluntary* with separate interaction terms for each year surrounding the mandate to disclose GHG information. 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 mandatory 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 mandatory application of the Act. We use the last year prior to the first-time application (t=-1) as the benchmark period. Values on the y-axis indicate coefficient estimates and 90 percent confidence intervals.

Table 1: Definition of variables

Variable	Definition	Data Source
Dependent variable		
Ln(Emission)	Natural logarithm of yearly verified emissions in metric tons of CO_2eq.	EU ETS
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: installation ultimately held by a UK listed and incorporated firm affected by the mandate for GHG emission disclosure. I.e., 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, 0:otherwise.	EU ETS
First-time mandatory	Indicator variable, 1: installation ultimately held by a firm which did not publish GHG emissions prior to the mandate, 0: otherwise. Information is taken from the Carbon Disclosure Project, CDP (https://www.cdp.net/en).	CDP
Already voluntary	Indicator variable, 1: installation ultimately held by a firm which voluntarily published GHG emissions prior to the mandate, 0: otherwise. Information is taken from CDP (https://www.cdp.net/en).	CDP
Control variables		
Size	Natural logarithm of total assets of the respective global ultimate owner.	BvD Amadeus
Performance	Return on assets of the respective global ultimate owner.	BvD Amadeus
Leverage	Long term liabilities plus short-term liabilities divided by total assets.	BvD Amadeus

This table defines all variables used in this study.

Table 2: Sample composition

Panel A - Installation Sector Composition

NACE Rev. 2 Sector	Frequency	Percentage
Information and Communication	36	0.75%
Manufacturing	4,221	87.99%
Mining	387	8.07%
Public Administration and Defence	18	0.38%
Scientific and Technical Activities	36	0.75%
Transportation	45	0.94%
Water and Waster	54	1.13%
Total	4,797	100.00%

Panel B - Control and Treatment Group Composition

	Control Group	Treatment Group
First-time mandatory reporter	2,754	423
Already voluntary reporter	1,188	432

Panel C - Geographic Distribution of Installations

Installation Country	Frequency	Country
Austria	36	0.75%
Belgium	270	5.63%
Czech Republic	36	0.75%
Germany	792	16.51%
Denmark	63	1.31%
Spain	594	12.38%
Finland	207	4.32%
France	828	17.26%
UK	1,071	22.33%
Hungary	18	0.38%
Ireland	27	0.56%
Italy	180	3.75%
The Netherlands	117	2.44%
Norway	54	1.13%
Poland	117	2.44%
Portugal	45	0.94%
Romania	63	1.31%
Sweden	279	5.82%
Total	4,797	100.00%

This table presents the sample composition by industry sector (Panel A), treatment and control group (Panel B), and country (Panel C).

Table 3: Descriptive statistics and correlations

Panel A: Descriptive statistics

	Observations	Mean	Lower quartile	Median	Upper quartile	Standard deviation
Ln(Emission)	4,797	10.65	9.59	10.52	11.67	1.70
Size	4,797	16.74	15.78	16.85	17.72	1.66
Performance	4,797	0.05	0.02	0.05	0.08	0.06
Leverage	4,797	0.60	0.53	0.59	0.67	0.14

Panel B: Correlations

		(1)	(2)	(3)	(4)
Ln(Emission)	(1)		**0.148**	0.016	**-0.254**
Size	(2)	**0.154**		**0.128**	**0.165**
Performance	(3)	**0.028**	**0.181**		**-0.222**
Leverage	(4)	**-0.262**	**0.156**	**-0.182**	

This table presents descriptive statistics (Panel A) as well as Pearson (below) and Spearman (above the diagonal) correlations for all continuous variables. Bold figures in Panel B indicate statistically significant correlations that are at least at the 10 percent level. Variable definitions are presented in Table 1. All continuous variables are winsorized at the 1st and 99th percentiles.

Table 4: Effect of the Act on GHG emissions

	Full Sample			
Dependent variable:	(I)	(II)	(III)	(IV)
Ln(Emission)	Coef./t-stat.	Coef./t-stat.	Coef./t-stat.	Coef./t-stat.
Experimental variables				
Post	-0.073	-0.103	-0.073	-0.103
	(-0.762)	(-0.901)	(-0.760)	(-0.899)
Post* × *Treatment	**-0.210*** **	**-0.191*** **		
	(-3.063)	**(-3.061)**		
Post* × *First-time Mandatory			**-0.217*** **	**-0.194*** **
			(-3.362)	**(-3.273)**
Post* × *Already Voluntary			**-0.202** **	**-0.187** **
			(-2.191)	**(-2.186)**
Control variables				
Size		-0.057		-0.057
		(-1.052)		(-1.056)
Performance		0.017		0.017
		(0.058)		(0.057)
Leverage		0.242		0.242
		(1.498)		(1.488)
Installation fixed effects	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Installation sector x year fixed effects	Included	Included	Included	Included
Installation country x year fixed effects	Included	Included	Included	Included
Observations	4,797	4,797	4,797	4,797
Adjusted R^2	0.956	0.957	0.956	0.957

This table presents the regression results for the effect of a mandate to disclose GHG information on GHG emissions. The dependent variable is the natural logarithm of yearly emissions in metric tons of CO_2eq. All models are estimated at the installation level. Columns (I) and (II) present the estimates of a difference-in-differences model obtained when assigning a common treatment to both first-time mandatory and already voluntary reporting firms. Column (III) and (IV) presents the estimates of our main specification separately for first-time mandatory and already voluntary reporting firms. We do not include a treatment group main effect because we estimate all models including installation fixed effects. Although we include time fixed effects in all specifications, *Post* is not omitted, as the treatment in 2013 is staggered (depending on the end date of the financial year in 2013 of the company holding an installation. All continuous variables are winsorized at the 1st and 99th percentiles. ***, **, and * indicate significance at 0.01, 0.05, and 0.1 levels, respectively, using a two-tailed test. All standard errors are clustered by firms who ultimately own installations.

Table 5: Effect of the Act on GHG emissions using EU and UK samples

	Listed Firms Only	UK Firms only		
	All Emissions	All Emissions	Foreign Emissions	National Emissions
Dependent variable:	(I)	(II)	(III)	(IV)
Ln(Emission)	Coef./t-stat.	Coef./t-stat.	Coef./t-stat.	Coef./t-stat.
Experimental variables				
Post	0.071	0.101	0.105	0.100
	(0.695)	(0.653)	(1.310)	(0.562)
Post* × *First-time mandatory	**-0.164*** **	**-0.291*** **	**-0.140**	**-0.342*** **
	(-2.916)	**(-3.315)**	**(-1.097)**	**(-3.223)**
Post* × *Already voluntary	**-0.131**	**-0.333*** **	**-0.275** **	**-0.350*** **
	(-1.551)	**(-3.433)**	**(-2.608)**	**(-3.212)**
Control variables				
Size	-0.036	0.029	0.005	0.028
	(-0.578)	(0.344)	(0.041)	(0.291)
Performance	0.053	-0.149	0.135	-0.227
	(0.179)	(-0.504)	(0.389)	(-0.671)
Leverage	0.118	0.445**	0.053	0.522***
	(0.638)	(2.608)	(0.269)	(2.851)
Installation fixed effects	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Installation sector x year fixed effects	Included	Included	Included	Included
Installation country x year fixed effects	Included	Included	Included	-
Observations	4,581	1,035	252	783
Adjusted R^2	0.963	0.969	0.983	0.964

This table presents the regression results for the effect of a mandate to disclose GHG information on GHG emissions accounting for differences in control group composition. For column (I), we only use listed firms, i.e., all installations of treated firms and all installations held by firms listed in other EU15 countries. For column (II), we only use UK incorporated firms, i.e., all installations of treated firms and all installations held by other UK incorporated firms not affected by the disclosure mandate (private firms). For column (III), we use the same subsample as for column (II) but only installations located outside of the UK. For column (IV), we use the same subsample as for column (II) but only installations located in the UK. The dependent variable is the natural logarithm of yearly emissions in metric tons of CO_2eq. All models are estimated at the installation level. We do not include a treatment group main effect because we estimate all models including installation fixed effects. Although we include time fixed effects in all specifications, *Post* is not omitted, as the treatment in 2013 is staggered (depending on the end date of the financial year in 2013 of the company holding an installation. All continuous variables are winsorized at the 1st and 99th percentiles. ***, **, and * indicate significance at 0.01, 0.05, and 0.1 levels, respectively, using a two-tailed test. All standard errors are clustered by firms who ultimately own installations.

Table 6: Effect of the Act on GHG emissions depending on firms' savings potential

	Number of installations per firm	
	Low	HIGH
Dependent variable:	(I)	(II)
Ln(Emission)	Coef./t-stat.	Coef./t-stat.
Experimental variables		
Post	-0.007	-0.246*
	(-0.045)	(-1.888)
Post* × *First-time mandatory	**-0.182**	**-0.173*** **
	(-1.164)	**(-2.686)**
Post* × *Already voluntary	**0.097**	**-0.228** **
	(0.434)	**(-2.474)**
Control variables		
Size	-0.022	-0.066
	(-0.163)	(-1.076)
Performance	0.347	-0.072
	(0.946)	(-0.191)
Leverage	0.201	0.243
	(1.339)	(1.181)
Installation fixed effects	Included	Included
Year fixed effects	Included	Included
Installation sector x year fixed effects	Included	Included
Installation country x year fixed effects	Included	Included
Observations	585	4,167
Adjusted R²	0.974	0.962

This table presents the results of regression analyses examining the effect of a mandate to disclose GHG information on GHG emissions depending on firms' savings potential. For this test, we split the sample at the median depending on the number of installations. I.e., for column (I) we use all installations if the firm which ultimately holds an installation, owns less or just as many installations than the median firm in our sample. For column (II), we use all installations if the firm, which ultimately holds an installation, owns more installations than the median firm in our sample. In our sample, the median firm owns two installations. In both specifications, the dependent variable is the natural logarithm of yearly emissions in metric tons of CO_2eq. All models are estimated at the installation level. We do not include a treatment group main effect because we estimate all models including installation fixed effects. Although we include time fixed effects in all specifications, *Post* is not omitted, as the treatment in 2013 is staggered (depending on the end date of the financial year in 2013 of the company holding an installation. Variable definitions are presented 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, using a two-tailed test. All standard errors are clustered by firms who ultimately own installations.

Table 7: Effect of the Act on GHG emissions for alternative sample compositions

	Firm-level analysis		**Installation level analysis**		
	Full Sample	Matched Sample	Based on GUO Matching	Industry Stratification	Installation Matching
Dependent variable:	(I)	(II)	(III)	(IV)	(V)
Ln(Emission)	Coef./t-stat.	Coef./t-stat.	Coef./t-stat.	Coef./t-stat.	Coef./t-stat.
Experimental variables					
Post	0.080	0.212	0.130	-0.135	0.276*
	(0.770)	(1.422)	(0.960)	(-1.183)	(1.865)
Post* × *First-time mandatory	**-0.263*** **	**-0.292*** **	**-0.175** **	**-0.178*** **	**-0.254** **
	(-3.867)	**(-3.014)**	**(-2.252)**	**(-3.067)**	**(-2.051)**
Post* × *Already voluntary	**-0.162**	**-0.285***	**-0.183***	**-0.174** **	**-0.136**
	(-1.109)	**(-1.978)**	**(-1.879)**	**(-2.071)**	**(-0.896)**
Control variables					
Size	-0.036	-0.041	-0.163	-0.062	-0.025
	(-0.649)	(-0.384)	(-1.078)	(-1.145)	(-0.132)
Performance	0.191	0.148	0.305	0.003	0.975**
	(0.839)	(0.459)	(0.733)	(0.011)	(2.173)
Leverage	0.037	0.271	0.212	0.242	-0.020
	(0.318)	(1.420)	(1.102)	(1.409)	(-0.072)
Firm fixed effects	Included	Included			
Year fixed effects	Included	Included	Included	Included	Included
Installation fixed effect			Included	Included	Included
Installation sector x year fixed effects			Included	Included	Included
Installation country x year fixed effects			Included	Included	Included
Observations	963	432	2,124	4,608	927
Adjusted R^2	0.986	0.986	0.962	0.953	0.954

This table presents the results of regression analyses examining the effect of a mandate to disclose GHG information on GHG emissions using alternative sample compositions. For column (I), we aggregate installation level data to the firm level and estimate a firm level regression. I.e. the dependent variable is defined as the natural logarithm of yearly verified emissions of all firms' installations. For column (II), we conduct a 1:1 propensity score matching at the firm level. I.e. for each treatment group firm, we identify the nearest neighbour in terms of firm size, financial performance, and leverage. To avoid an indirect effect of the Act on our matching variables, we matching using 2008 values. We conduct the matching separately for first-time mandatory and already voluntary reporters. For column (III), we use the firms identified using the propensity score matching in column (II) and estimate an installation level analysis. I.e. we use all installations ultimately held by firms of our firm-level matched sample. For column (IV), we conduct an industry stratified sample. I.e. we only use a treatment group installation, if there is at least one control group installation within the same NACE rev.2 industry sector. We conduct the stratification separately for first-time mandatory and voluntary reporters. For column (V) we conduct a 1:1 propensity score matching at the installation level. I.e., we use the same matching approach as for column (II), but directly match at

the installation level. In all specifications, the dependent variable is the natural logarithm of yearly verified emissions in metric tons of CO_2eq. All models are estimated at the installation level. We do not include a treatment group main effect because we estimate all models including installation fixed effects. Although we include time fixed effects in all specifications, *Post* is not omitted, as the treatment in 2013 is staggered (depending on the end date of the financial year in 2013 of the company holding an installation. Variable definitions are presented in Table 1. All continuous variables are winsorized at the 1^{st} and 99^{th} percentiles. ***, **, and * indicate significance at 0.01, 0.05, and 0.1 levels, respectively, using a two-tailed test. All standard errors are clustered by firms who ultimately own installations.