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# Fighting Climate Change with Disclosure? The Real Effects of Mandatory Greenhouse Gas Emission Disclosure

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# Fighting Climate Change with Disclosure?
# The Real Effects of Mandatory
# Greenhouse Gas Emission Disclosure

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**Abstract**

We examine how mandatory disclosure of greenhouse gas (GHG) emissions influences companies' emission levels. We identify the effect of full transparency by exploiting a mandate requiring UK-incorporated listed companies to disclose information on GHG emissions in their annual reports. Comparing the emissions of installations (e.g. power plants, or oil refineries) owned by listed companies and installations owned by firms not subject to the mandate, we document that disclosing GHG emissions in annual reports reduces emission levels by up to 18%. Emission reductions occur across all industries but are largest for installations from the energy supply industry. Our results are robust to various specifications and document the incremental effect of disclosing emission data in annual reports, as firms had to report emission data to a central register already before the disclosure mandate.

**Keywords:**

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

## 1. Introduction[1]

We examine how mandatory disclosure of greenhouse gas (GHG) emissions in financial reports influences companies' emission levels. Mitigating environmental, social and economic effects of climate change are central challenges policymakers and industry are facing today. About half of the worldwide GHG emissions result from industrial activities as well as electricity and heat production (IPCC, 2014). In the past decades, numerous attempts to reduce firms' GHG emissions have been made at different levels and with different regulatory approaches. In addition to the classical instruments of intervention, such as setting standards or pricing emissions, policy makers are increasingly requiring companies to disclose information on emissions.[2] This study investigates whether such disclosure induces companies to reduce their emissions. Specifically, we exploit emission data at installation-level and study the real effects of a mandate to disclose GHG emissions for listed companies based in the United Kingdom (UK).

In a recent study, Chen et al. (2017) show that mandatory CSR reporting decreases return on assets and pollution levels (wastewater and sulfur emissions) in cities in which firms affected by the CSR mandate are located. Our paper complements this study in serval ways. First, while Chen et al. (2017) provide evidence of the *social externalities* of a *broad* Chinese regulation which requires to disclose information on "environmental

[1] We thank conference participants at the Annual Congress of the European Accounting Association in Milan, the World Congress of Environmental and Resource Economists in Gothenburg, the VHB Congress in Magdeburg 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.

[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. E.g., 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).

protection and sustainable development" (p. 4), we examine the *real effects* of a *specific* UK regulation on disclosing CHG emissions. We define real effects as situations in which the disclosing entity changes its behavior (e.g. its resource allocation) because of a disclosure mandate (Kanodia, 2006). "[R]eal-effects studies are of particular relevance because the notion that mandating disclosure induces desirable and/or discourages undesirable behavior by the disclosing party is an important motivation for transparency regulation in many areas" (Leuz and Wysocki, 2016, p. 578). Second, while Chen et al. (2017) focus on the *local* impact of CSR reporting in terms of city-level wastewater and $SO_2$ emissions, our paper focusses on the *global* impact in terms of GHG emissions. Third, while Chen et al. (2017) use a Chinese setting in which many state-owned enterprises operate and in which the regulation was accompanied by a number of governmental CSR actions and *political pressure*, we investigate a pure disclosure requirement in the UK and the economic considerations of firms that led to a decrease in emissions without any political influence on firms.

To investigate the impact of an emission disclosure mandate on the level of emissions, we need to compare the difference between pre-mandate and post-mandate emission data for affected firms with pre-mandate and post-mandate emission data for benchmark firms. This comparison is usually impossible because the pre-mandate emission data for treatment firms as well as pre- and post-mandate emission data for benchmark firms are not available. Chen et al. (2017) also face this problem and admit that they "are not aware of any time-series firm-level pollution databases for firms in China" (Footnote 3). Thus, they have to rely on the assumption that the overall emissions in a region where a firm is headquartered is influenced by the firm's emissions. In our specific setting we are able to retrieve the aforementioned emission data because firms had to report these data to a central register already before the disclosure mandate. This feature of our setting allows us to isolate the impact that disclosing GHG emissions has from the measuring of emissions which might also drive emission reductions. Moreover, we are able to exploit the staggered implementation of the disclosure mandate. Overall, our setting allows us a tight identification strategy in a difference-in-differences analysis in order to document a direct link between mandatory GHG emission disclosure and the magnitudes of emission reductions resulting from the disclosure mandate.

Specifically, we examine a UK mandate which came into effect in 2013 and requires all listed companies to report GHG emissions in their annual reports. Previously, all (listed and non-listed) companies had to gather and report the emissions of their individual installations regulated under the European Union Emissions Trading System (EU ETS) to a publicly available register.[3] However, complex corporate structures impede the mapping of installations in this register to companies they belong to. By requiring the disclosure of aggregated emission data on company-level and thereby reducing costs for obtaining this information for all interested parties, transparency concerning a company's GHG emissions increases.

The EU ETS also requires companies to pay for each emitted metric ton of carbon dioxide equivalent ($tCO_2eq$) covered by the EU ETS, so that each metric ton is cash-flow-relevant. Therefore, increased transparency concerning a company's GHG emissions could affect its equity valuation (e.g. Matsumura et al., 2014; Krueger, 2015). This effect or the anticipation of this effect may alter managers' behavior and provide an incentive to increase a company's environmental performance. Furthermore, disclosure lowers hurdles for benchmarking the environmental performance of companies. As a consequence, increased transparency of GHG emissions may influence the investment decision of investors with a focus on socially responsible investments.

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

To examine whether the inclusion of GHG emission disclosure (“GED”) in financial reports contributes to a reduction of GHG emissions of affected companies, we use a difference-in-differences approach (DiD) and exploit an exogenous variation introduced by a UK disclosure mandate. This mandate was part of the UK Companies Act 2006 (Strategic Report and Directors' Report) Regulations 2013 (“the Act”). The treatment group in our DiD design consists of installations ultimately owned by companies affected by the Act. The control group consists of UK-based installations ultimately owned by companies not affected by the Act. As both groups are UK-based, they are subject to the same concurrent economic shocks.

Controlling for time trends and installation-specific differences, we document a significant effect of the Act on GED for the treatment group of installations--depending on the model--between -14.8% and -18.0% relative to the control group. This result is robust to a number of specifications. In additional tests, we find that emission reductions are largest for installations from the electricity, gas, steam and air conditioning supply industry, while also being sizable for manufacturing sectors.

This paper contributes to the literature in several ways. First, we add to the growing literature on real effects of disclosure regulation. Most existing studies in finance and accounting focus on real effects of disclosure regulation on investment (e.g. Biddle and Hilary, 2006; Biddle et al., 2009; Graham et al., 2011; Cheng et al., 2013; Shroff et al., 2014; Shroff, 2017). In contrast, contributions investigating real effects on firm choices other than investment are relatively scarce (e.g. Dyreng et al., 2015; Granja, 2016; Christensen et al., 2017; Ernstberger et al., 2017). We contribute to this literature by documenting the direct impact of introducing mandatory GHG disclosure regulation on emission abatements. on an installation basis. Moreover, we document the magnitude of the GHG emission reductions of a disclosure mandate.

Second, we contribute to the already existing literature on GHG disclosure. The literature so far primarily focuses on *expected* regulatory effects instead of *real* effects, that is they examine the effect of GHG disclosure on, e.g., market prices or cost of capital (e.g. Matsumura et al., 2014; Krueger, 2015; Kleimeier and Viehs, 2016). We add to this literature by focusing on real effects induced by internal managerial behavior, as opposed to effects that primarily relate to an altered market evaluation of affected companies.

Third, we contribute to the literature examining different regulatory options for mitigating greenhouse gas emissions. In the industry and electricity generation sectors the preferred regulatory approach is a mix of command-and-control policies, subsidies to specific GHG abatement technologies, e.g. government support for renewable energy, and carbon pricing through cap-and-trade.[4] Various studies have attempted to estimate the abatement effect of cap-and-trade systems on emissions in the industry and electricity generation sectors (e.g. Ellerman et al., 2010; Anderson and Di Maria, 2011; Bel and Joseph, 2015; Murray and Maniloff, 2015; Martin et al., 2016). In contrast to the diverse literature available on the effect of cap-and-trade systems on emissions, to our knowledge, there are no studies which examine the effect of mandatory GHG emission disclosure on emission levels, as this is a relatively recent regulatory development. Our analysis which also provides effect magnitudes can help regulators in weighing different regulatory measures against each other to make an informed decision.

The remainder of this paper is structured as follows. Section 2 introduces the regulatory background relevant to our empirical setup and develops the hypothesis.

[4] The concept of a cap-and-trade system is that participants obtain emission allowances as certificates in accordance with individual targets. Certificates can be sold to and purchased from the market. A liquid market for emission allowances provides a transparent price signal and contributes to an efficient allocation of emission reductions. The flexibility of tradable emission allowances allows companies to choose the most cost-effective option when dealing with their emissions. Companies short of allowances face a trade-off between purchasing extra allowances on the market and investing in more efficient technologies or moving to less carbon-intensive energy sources.

Section 3 presents the research design. Section 4 discusses the results of the empirical analyses. In Section 5 we perform robustness checks. Section 6 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 an European Economic Area state, the New York Stock Exchange or Nasdaq.[5] The Act applies to all financial years ending on or after the 30th of September 2013 and requires affected companies to report their GHG emissions during the current and previous financial year.[6] 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 $tCO_2eq$. [7]

The UK Department for Environment Food & Rural Affairs Companies provides detailed application guidance on how to implement the disclosure mandate. The company's board must approve the directors' report which includes the disclosure on GHG emissions. The report must then be signed on the board's behalf by a director or the

[5] Companies entitled to prepare their financial accounts under the small company exemption of The Act are not required to disclose their emissions.

[6] Companies may also report emissions for a period different from their financial year.

[7] Emissions of the following greenhouse gases have to be reported: carbon dioxide (CO2), methane (CH4), nitrous oxide (N2O), hydrofluorocarbons (HFCs), perfluorocarbons (PFCs) and sulphur hexafluoride (SF6). 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 tonnes of carbon dioxide equivalent ($CO_2eq$).

company secretary. Moreover, the statutory auditor is required to review the report, i.e. has to consider whether the information is consistent with the financial statements and whether the information is materially incorrect or materially inconsistent with the financial statements based on the knowledge acquired by the auditor in the course of performing the audit. Finally, the Enforcement Division of the UK Financial Reporting Council conducts enforcement investigations and can bring prosecutions against auditors or accountants where there appears to be misconduct or a breach of relevant professional standards.

The emission data we use in this paper are 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. The EU ETS comprises more than 13,500 stationary installations and covers about 45% of the European Union's GHG emissions (Ellerman et al., 2016).[8] Installations are covered by the EU ETS if they contain a combustion plant with a rated thermal input of at least 20 MW 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 remaining emissions not covered by the EU ETS primarily come from the transport, residential and agricultural sectors (DECC, 2016).

[8] 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 CO2 equivalent, while the sum of verified emissions of all UK installations in the UK was 208 million tons of CO2 equivalent (DECC, 2016).

Emission trading systems similar to the EU ETS have also been introduced elsewhere.[9] In the US, the Californian cap-and-trade scheme covers GHG emissions from power generation and industrial activities of facilities based in California (ARB, 2016) and the Regional Greenhouse Gas Initiative covers emissions from electricity production in nine Northeastern and Mid-Atlantic states (RGGI, 2013).

Under the EU ETS, affected installations must submit *emission allowances* for every metric ton of carbon dioxide ($CO_2$) or the equivalent amount of Nitrous oxide ($N_2O$) or Perfluorocarbons (PFCs) covered by the EU ETS that they emitted in the previous year. The overall number of initially allocated allowances is constrained by design so that each allowance holds value for polluting companies.

The data gathered in the framework of the EU ETS is publicly available online in the 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 number of allocated emission allowances as well as historical *verified emissions*.[10] 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.

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[9] See e.g. https://icapcarbonaction.com/en/ for an overview of global emissions trading schemes.

[10] Allocated emission allowances are reported for all previous trading phases, and planned allocations are provided for the entire current trading phase of the EU ETS. Therefore, as the current phase of the EU ETS comprises the period from 2013-2020, the planned number of freely allocated emission certificates for several years in the future is also available.

## ***2.2.*** *Hypothesis Development*

Studying the 'real effects' of accounting information is based on the theory of the targeted disclosure cycle (Fung et al. 2007) which requires the following links between disclosure regulation and their real effects: (1) the disclosure regulation, (2) their implementation and enforcement, (3) the use/awareness/processing of the new disclosures by users, (4) the users' response, i.e. the change in users' behavior as a result of the new information, (5) the disclosing firms' observing (or anticipating) of users' response, and, finally, (6) the disclosing firms' change in its 'real' actions. We already discussed the mandatory regulation for disclosing GHG emissions (1) and its enforcement (2) in the background section. Thus we focus on the other links in the following.

With regard to link (3) we assume that users of the new disclosures are primarily investors requiring information on GHG emissions to evaluate a company's risks or opportunities from climate change which influence the future return on investment. Climate risks include (i) regulatory risks (ii) reputational risks, and (iii) litigation risks. Regulatory risks form high emissions result from the uncertainty over climate change policies. Future climate change regulations may increasingly internalize the cost of carbon (e.g. through trading schemes or taxes or) or may prescribe technical requirements and hence increase companies' cost or even endanger their business model. Reputational risks result from changing market or consumer behavior. Consumers or other pressures may take action on climate change (e.g. boycotts) which affects firms with high emission levels. Litigation risks result from a growing probability of environmental or climate litigation. Prior literature shows 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.

Climate opportunities include the potential for (i) cost savings, (ii) new technologies or products, and (iii) reputational benefits. Benefits from cost savings result from a lower consumption of energy or resources. Striving to reduce emissions may also result in developing new products and services in the area of energy-efficient technologies or renewable energy. Reputational benefits result from taking a proactive approach to climate change to improve their reputation and/or their brand.

In addition, investors have preferences for low emissions beyond their expected influence on future return on investment. 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 (2017) show in a global survey of institutional investors that ethical considerations play an important role for investment decisions besides financial aspects.

An important requirement for investors using GHG emissions in their decision making is that GED made emissions more transparent. Before the UK mandate companies already had to collect and disclose GHG emissions on an installation basis. However, only after the mandate disclosure is available on a company basis and not only on an installation basis, allowing investors or other stakeholders to assess a company's GHG emissions and to compare them to those of peers.

Disclosing GHG emission levels can increase the transparency about a company's emission levels and thus lead to market value-penalties (Matsumura et al., 2014), which in turn may induce managers to increase their efforts to abate emissions.

Link (4) assumes that investors respond to the new information. We argue that the increased transparency about emissions leads to market-value penalties for at least three reasons. First, cash flow effects arise through payments for emission allowances and/or high costs for abatement measures. Every year installations must surrender emission allowances to cover their GHG emissions in the previous year. A high level of GHG emissions means that–in addition to its compliance cost in the current period[11]–the company may be exposed to higher compliance costs in the future, as the price of allowances will increase once the cap-and-trade scheme becomes more stringent and free allocation of allowances may expire.[12] All actual and expected future cash flows for high emissions decrease a company's market value. Second, high emissions could increase investors' perceptions of firm risk because the compliance with future climate change regulations could increase companies' cost or even endanger their business model. Higher company risk is associated with a lower market value. Third, investors having preferences for low emissions beyond their cash flow or risk effects could impose market-value penalties for high emission companies.

The market value-penalties of high emissions constitute a feedback effect of GHG disclosure which can reinforce managers' efforts to reduce emissions. Changes in a company's market value are an important determinant of managers' variable compensation and emission levels are increasingly directly included as a key performance indicator for determining managerial bonuses.[13] Moreover, lower market values or even higher emissions themselves can harm managers' reputation within and outside the company. Thus, in line with link (5), managers are aware of investors' responses. Finally, both the relevance for compensation and the potential impact on managerial reputation induce managers to reinforce emission abatement efforts establishing link (6)

Thus, we predict that the UK mandate spurs managerial efforts to reduce emissions and hypothesize:

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[11] Especially in manufacturing sectors some or all of the compliance cost is defrayed through the provision of allowances free of charge ("free allocation"). However, even under free allocation emissions entail an opportunity cost at the level of the EUA price.

[12] Current negotiations on EU ETS reform at the EU level indicate a substantial increase in stringency of the EU ETS, so that that allowance prices are likely to increase in the future (Thomson Reuters, 2017).

[13] E.g. Shell included "[n]ew metrics for greenhouse gas (GHG) management – these now form 10% of the annual bonus scorecard" (Shell 2016, p. 82).

*H1: The UK mandate for disclosure of GHG emissions leads to lower emission levels for affected installations.*

## 3. Research Design and Data

### *3.1. Empirical Model*

To estimate the effect of the inclusion of GED in financial reports on GHG emissions we apply a difference-in-differences approach and use the introduction of the Act as exogenous variation in the disclosure regime. Specifically, we compare differences in pre-treatment (i.e. prior to mandatory GED) and post-treatment (i.e. after mandatory GED) emissions. We compare installations ultimately held by firms affected by GED (i.e. treatment installations) with installations ultimately held by firms not affected by GED (i.e. control installations).[14] Given standard assumptions, which we investigate further below, the difference in these differences can be attributed to GED.

As mentioned in Section 2.1., the Act applies to all financial years ending on or after the 30th of September 2013. The data we use in this paper is not based on financial years but on calendar years which allows us to examine a staggered adoption of the Act. If a GUO's financial year ends before the 30th of September 2013 (i.e. between the 1st of January 2013 and the 29th of September 2013), we define the installation-level emission data observed on the 31st of December 2013 for the calendar year 2013 to be a pre-treatment observation. If a GUO's financial year ends after the 30th of September 2013 (i.e. between the 1st of October 2013 and the 31st of December 2013) we define the installation-level emission data observed on the 31st of December to be a post-treatment observation. For 342 of the 437 installations in our sample we define the emission

[14] Our sample entails 186 firms, of which 24 are treated. We therefore pursue our analysis at the installation level, controlling for, amongst other, firm-level fixed effects.

observation in 2013 as first post-treatment observation. Figure 1 depicts our allocation rule graphically.

In our first specification, we include time and industry fixed effects and estimate the following regression:

$$Ln(Em_{i,t}) = \alpha_t + \beta_1 * Treat_i + \beta_2 * Post_{i,t} + \lambda * Treat_i * Post_{i,t} + \gamma_j + e_{i,t} \quad (1)$$

where $Ln(Em_{i,t})$ is the natural logarithm of installation $i$'s verified emissions in year $t$, and $\alpha_t$ and $\gamma_j$ are time and industry fixed effects, respectively.[15] . $Post_{i,t}$ is a dummy variable marking all post-treatment observations and $Treat_i$ is a dummy variable marking observations from installations assigned to the treatment group. Yearly time fixed effects capture annual effects affecting all installations, for instance, regulatory amendments to the EU ETS or changes in gasoline prices during our period of observation. Even though we include time fixed effects, $Post_{i,t}$ is not omitted because we exploit a staggered treatment effect for each installation *i* in our sample. The Act applies to all financial years ending on or after the 30th of September 2013. We utilize calendar-year emission data in our analyses. Depending on the end date of the financial year in 2013 of the firm ultimately holding an installation, we classify an installation's 2013 calendar-year emission observation as either its last pre-treatment or first post-treatment observation.

In our second specification, we add firm fixed effects to capture time-constant unobserved heterogeneity at the firm level, e.g. differences in emissions abatement costs across companies. We estimate the following regression:

$$Ln(Em_{i,t}) = \alpha_t + \beta_1 * Treat_i + \beta_2 * Post_{i,t} + \lambda * Treat_i * Post_{i,t} + \eta_j + e_{i,t} \quad (2)$$

where $\eta_j$ denote firm-level fixed effects. In our main specification, we estimate the following regression:

[15] We use the NACE industry standard classification system. The NACE system is the statistical classification system of economic activities used in the European Union. It utilizes four hierarchical levels: 21 Sections, 88 Divisions, 272 Groups and 615 Classes.

$$Ln(Em_{i,t}) = \alpha_t + \beta_2 * Post_{i,t} + \lambda * Treat_i * Post_{i,t} + \delta_i + e_{i,t} \quad (3)$$

where $\delta_i$ are installation fixed effects included to account for any static installation-specific unobserved heterogeneity.[16]

The coefficient of interest across all specifications is $\lambda$ as it captures the difference in pre- and post-treatment differences of the emission levels of treatment and control group installations.

*3.2. Data*

To construct the sample, we start with yearly data on verified emissions from 2010 to 2016 of 1,301 UK-based installations. The data is publicly available online in the EUTL register. However, the information available in the EUTL is not sufficient to directly link operator holding accounts (OHAs) and installations to parent companies. Thus, for our setting it is necessary to link all installations to parent companies, as the assignment of an installation to either the treatment or the control group depends on its ultimate owner being affected by the Act or not. This link between OHAs and parent companies is established by the Ownership Links and Enhanced EUTL Dataset Project (OLP). The OLP provides the name and Bureau-van-Dijk identification number (BvD-Id) of an installation's parent company.

For 237 of the 1,301 installations, the OLP does not provide information on their parent companies and for additional 166 installations, the mapped BvD-Id of the respective parent company is not valid in Bureau-van-Dijk's database Amadeus.[17] We exclude these installations as we use information provided in Amadeus to examine corporate structures and assign installations to either the treatment or the control group. The remaining sample is further reduced to 509 installations as there is no emission information for 389

[16] As installation fixed effects are included, $Treat_i$ is omitted in this specification.
[17] Amadeus provides information on approximately 21 million companies across Europe.

installations in either the front or the back years of our period of observation. This is due to the opening or closing of installations across our sample period.[18]

We then assign the parent companies (and with them the installations in the sample) to either the treatment or the control group. For each company, we examine its corporate structure and determine its Global Ultimate Owner (GUO). We are able to identify the GUOs of 460 of the 509 installations.[19] 49 installations linked to 34 parent companies are omitted, as their GUOs and therefore their group affiliation cannot be unequivocally determined.[20] If GUOs fulfil the criteria which make them subject to The Companies Act 2006 (Strategic Report and Directors Report) Regulations 2013, we assign them and the respective installations to the treatment group. If not, we assign the GUOs and the respective installations to the control group.

To control for the undue influence of outliers, we trim the top and bottom 1% of emission observations. We exclude installations with trimmed observations for the entire sample period to keep a balanced panel. This final step leads to a balanced panel of 437 installations with continuous yearly emission data from 2008 to 2016 (i.e. 9 years) with in total 3,933 installation-year observations. We present a table summarizing our sample selection in Appendix B.

Table 1 depicts the distribution of treatment and control installations across industries. 119 of the 437 installations are assigned to the treatment group. Half of the installations in our sample operate in NACE-Section C (Manufacturing), about 20% of the

[18] We exclude these installations because we opt for a balanced panel. Furthermore, most of these installations exhibit only emission observations during the pre-treatment period.

[19] Complex corporate structures complicate this group assignment. Appendix A provides details on the process of assigning installations to either the treatment or the control group using the Amadeus database.

[20] This is either due to missing ownership data or ambiguities in the ownership data on different corporate levels or changes in the ownership structure during the time of observation leading to varying group affiliation.

installations operate in NACE-Section B (Mining and quarrying) and again 20% operate in NACE-Section D (Electricity, gas, steam and air conditioning supply).

Table 2 provides descriptive statistics of the verified emissions for pre- and post-treatment period per group. We either classify an installation's emission observation in 2013 as the last pre-treatment observation (i.e. $Post_{i,t} = 0$) or first post-treatment observation (i.e. $Post_{i,t} = 1$) depending on the end of its GUO's financial year in 2013.[21] An installation's post-treatment period therefore either comprises verified emissions from 2012 to 2016 or from 2013 to 2016. The first 3 columns report mean, median and standard deviation of yearly emissions of treatment installations over different periods. Emissions are presented in 1,000 $tCO_2eq$. Columns 4 to 6 report the same information, but for control installations. The table shows that there is a considerable difference in the pre-treatment emission level of both groups. Additionally, the development of emission levels per group within our sample period is depicted in Figure 2.

## 4. Results

### *4.1. Implications of the Act on GHG emissions*

Table 3 reports the results of estimating the three specifications introduced in Section 3.1 . Column 1 presents the estimates obtained using the first specification including time and industry fixed effects. As already shown in Table 2, the coefficient on *Treat* reveals a considerable difference in the pre-treatment emission level between treatment and control group. Since we include time fixed effects in all specifications and *Post* is only included due to the staggered treatment, *Post* cannot classically be interpreted as the post-to-pre-difference of emissions within the control group. The estimate of the coefficient

[21] Appendix C provides details on the process of classifying an installation's emission observation in 2013 as either pre- or post-treatment observation.

related to the interaction term of *Treat**Post* (i.e. $\lambda$) is negative across all three specifications, These results suggest that, after implementing the regulation, companies affected by the Act decreased their GHG emissions more strongly than companies not affected by the introduction of mandatory GHG disclosure. In economic terms, the estimates obtained for $\lambda$ indicate an average treatment effect of installations affected by the Act of -14.8% (in column 1) and -18.0% (in column 2 and 3)[22]

*4.2. Industry variation in the treatment effect*

The regression estimates presented so far support the hypothesis of a positive effect of mandatory GED on emission abatement. In the following, we test whether this effect varies in magnitude by industry. As the energy industry, and in particular the electricity industry, covers the majority of emissions in our data, we re-run our baseline specification excluding both latter.

Therefore, we further separate installations operating in NACE-Section D across sub-classes. Of the 95 installations in NACE-Section D, 40 operate in NACE-Class 35.11 (Production of electricity), 20 operate in NACE-Class 35.22 (Distribution of gaseous fuels through mains) and 35 installations operate in NACE-Class 35.30 (Steam and air conditioning supply). We then re-run the analyses and exclude installations from NACE-Section D and NACE-Class 35.11, respectively.

Panel A of Table 4 reports the results obtained by estimating the three specifications on a sample excluding installations from NACE-Section D. In the main specification including installation and time fixed effects, we find that the coefficient estimate for $\lambda$ is negative and statistically significant. The same can be observed for both specifications including industry and firm-level fixed effects. Compared to the estimates presented in Section 4.1, the magnitude of the estimated treatment effect is smaller. In economic terms,

[22]Note that observations with zero emissions have been excluded from our sample. As these could represent market exits due to the mandate, our estimates present a lower bound.

the estimated treatment effect induced by the Act is in the range of -11.3% (columns 2 and 3) to -12.3% (column 1).

Table 4 Panel B reports the results of excluding all installations from NACE-Class 35.11. The results are similar to those presented in Panel A. The estimated treatment effect is again significant in all specifications. In economic terms, the estimated treatment effect is in the range of -14.4% to -15.0%.

Importantly, our findings imply that emission reductions occur across a range of covered industries. In particular, the UK electricity sector is subject to a carbon price floor as of April 2013. To the degree that this yearly changing price floor is not captured by yearly fixed effects, differences in the distribution of power plant technologies across the treatment and control group may affect the estimated treatment effect. As a consequence, our baseline results in Table 3 may be compromised.[23] Furthermore, renewable energy capacity, especially wind and photovoltaic assets, strongly increase during our period of observation. This increase reduces emissions in the power sector, and we might falsely attribute measured effects to emission disclosure.

However, first, the examination of installations operating in NACE-Class 35.11 shows that the power plant types and installed generation capacities are practically equally distributed across the treatment and the control group. Second, our results in Table 4 illustrate that the disclosure effect is not solely driven by the power sector where emission reductions also occurred due to increased renewable power generation.

[23] There are considerable differences in the specific $CO_2$ emissions per megawatt-hour produced ($MWh_{el}$) depending on the power plant type and its technological state. For example, average specific $CO_2$ emissions for different power plant types are in the range of 1.03 $tCO_2/MWh_{el}$ for lignite, about 0.91 $tCO_2/MWh_{el}$ for hard coal and 0.41 $tCO_2/MWh_{el}$ for natural gas (IEA, 2016).

## 5. Robustness Checks

### *5.1. Assessing the validity of the parallel trends assumption*

The main assumption underlying the difference-in-differences approach is that in the absence of the treatment, the development of emissions of the treatment and the control group would have been the same (i.e. parallel trends assumption). We check for a possible violation by replacing the single interaction term of *Treat***Post* with separate interaction terms of *Treat* and year-dummies. When estimating the model, insignificant pre-treatment coefficients close to zero indicate validity of the parallel trends assumption. Post-treatment coefficients significantly different from zero indicate differences in the post-treatment trend (i.e. the treatment effect).

Figure 3 depicts the results of estimating our main specification. 2012 is chosen as base (benchmark) period, as for most installations, it is the last pre-treatment year. The significant non-zero estimates during the post-treatment period depict the treatment effect. Based on these results we do not find a violation of the common trends assumption, as none of the counterfactual treatment effects in our 2010 to 2016 sample is significantly different from zero prior to 2012.

Figure 3 also plots estimates for the counterfactual treatment effect in a sample including years of financial crises 2008 and 2009. Here, the results indicate that there could be deviations in the trends of emission development of the treatment and the control group in the first years of the sample period. We conjecture that the differences during these years may be driven by the economic crisis that may have differently impacted listed and non-listed firms. As there are indications of a possible violation of the parallel trends assumption, we report estimates (on a significant treatment effect) for a prolonged sample period from 2008 to 2016 in Table 5. The results in Table 5 indicate statistically stronger treatment effects. However, we consider our main findings on the disclosure effect to be those based on the 2010 to 2016 sample presented in Table 3.

### 5.2. Matching of Installations

As a further robustness check we re-estimate our econometric specifications utilizing a matched sample. Installations are matched within NACE-Divisions. Our starting point is the full sample of 437 installations. The installations are then divided by treatment and control group and NACE-Division. Within NACE divisions, we randomly match installations. If the number of installations per group is the same in a division, we keep all installations. If, within a division, the number of installations assigned to the two groups differs, we keep all installations in the smaller group. We then randomly match installations from the other group with these installations so that within each division an equal number of control and treatment installations remains. This process leads to a sample of 198 installations.[24]

Panel A of Table 6 reports the results of applying our econometric specifications on the matched sample. In specifications 2 and 3, the estimates of the treatment effect are significant and in line with the results of the main analysis. In these specifications, we document, on average, an additional emission reduction by -16.4% to -21.0% for installations affected by the regulation, compared to control installations.[25] In specification 1, which only includes time fixed effects, the results are in line in terms of size, but are insignificant.

### 5.3. Pseudo Treatment

As a third robustness check, we randomly assign installations to a treatment and a control group. In our sample, 119 of the 437 installations are assigned to the treatment group. We construct a pseudo treatment dummy leading to the same distribution of pseudo treatment and control installations and re-run the analyses described in Section

[24] The reduction from 437 to 198 installations is mainly a consequence of the substantially smaller number of treatment installations compared to the number of control installations. Furthermore, the distribution of installations is not equal across NACE-Divisions and groups.

[25] Similar results are obtained when installations are not randomly matched within NACE-Divisions but are additionally matched based on their pre-treatment emission levels.

4.1. Panel B of Table 6 reports the results. We find that the average pseudo treatment effect is zero and insignificant across all specifications.

## 6. Conclusion

In this paper, we examine whether mandatory greenhouse gas emissions disclosure in annual reports reduces companies' greenhouse gas emission levels. We apply a difference-in-differences framework on a set of UK 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 companies affected by the Act exhibit significant reductions in GHG emissions compared to control installations, controlling for time fixed effects and installation fixed effects. We find that, on average, installations of affected companies exhibit additional emission reductions of -14.8% to -18.0% relative to pre-treatment emission levels.

We then examine industry variations in this effect. We find that the observed effect is primarily driven by installations from the electricity, gas, steam and air conditioning supply sector. We exclude installations from this industry group and again observe a negative and significant effect. In economic terms, the estimated treatment effect is in the range of -11.3% to -12.3% of additional emission reductions induced by the Act compared to control installations. Furthermore, we re-run the analyses but only exclude installations from production of electricity. Again, the results are consistent with our main findings. The observed negative effect is robust to estimating our models on a reduced sample based on matching within sectors.

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 therefore 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 unlocks the financial markets channel to emission abatement. It 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 the limitation that we do not explicitly model (industry-specific) mechanisms behind the observed emission reductions. We therefore 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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**Table 1: Distribution of installations across NACE-Sections**

| | Number of installations | NACE-Section B | NACE-Section C | NACE-Section D | Other Sections |
|---|---|---|---|---|---|
| Treatment group | 119 | 34 | 38 | 34 | 13 |
| Control group | 318 | 58 | 182 | 61 | 17 |
| Total | 437 | 92 | 220 | 95 | 30 |

This table shows the distribution of installations across NACE-Sections. NACE-Section B: Mining and quarrying; NACE-Section C: Manufacturing; NACE-Section D: Electricity, gas, steam and air conditioning supply; Other Section includes installations from NACE-Section A: Agriculture, forestry and fishing (4 installations), NACE-Section E: Water supply, sewerage, waste management and remediation management (8 installations), NACE-Section H: Transportation and storage (5 installations), NACE-Section J: Information and communication (5 installations), NACE-Section M: Professional, scientific and technical activities (6 installations) and NACE-Section O: Public Administration and defence, compulsory social security (2 installations).

**Table 2: Univariate analysis of verified emissions**

| | Treatment group | | | Control group | | |
|---|---|---|---|---|---|---|
| | Mean | Median | SD | Mean | Median | SD |
| Verified emissions (2010-2016) | 103.88 | 21.93 | 288.09 | 187.15 | 35.07 | 497.34 |
| Pre-treatment verified emissions (2010-Treatment) | 114.87 | 21.78 | 344.56 | 189.55 | 32.52 | 514.23 |
| Post-treatment verified emissions | 93.62 | 21.93 | 222.84 | 185.17 | 36.40 | 483.23 |

This table shows summary statistics of installation-level emission data. Verified Emissions are presented in 1,000 $tCO_2eq$.

**Table 3: Effect of the Act on GHG emissions**

| Specification | (1) | (2) | (3) |
|---|---|---|---|
| Treat | -0.604**<br>(0.193) | 3.187***<br>(1.0183) | -<br>- |
| Post | 0.616***<br>(0.178) | -0.0787<br>(0.0733) | -0.079<br>(0.071) |
| Treat*Post | -0.148*<br>(0.086) | -0.180**<br>(0.0788) | -0.180**<br>(0.076) |
| Time fixed effects | Yes | Yes | Yes |
| Industry fixed effects | Yes | No | No |
| Firm fixed effects | No | Yes | No |
| Installation fixed effects | No | No | Yes |
| Observations | 3,059 | 3,059 | 3,059 |
| Adj. $R^2$ | 0.228 | 0.636 | 0.918 |

This table reports regression estimates of three econometric specifications differing in included fixed effects. The dependent variable is the natural logarithm of yearly verified emissions in 1,000 metric tons of $CO_2$eq. Treat is a dummy variable indicating treatment installations (dummy is equal to 1) and control installations (dummy is equal to 0). Post is a dummy variable indicating pre-treatment observations (dummy is equal to 0) and post-treatment observations (dummy is equal to 1). The interaction term Treat*Post is 1 for post-treatment observations of installations in the treatment group and zero otherwise. In the third specification, Treat is omitted, as we include installation fixed effects in this specification. 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, installation-year observations in 2013 are either already classified as post-treatment observations or as pre-treatment observations). The numbers in brackets are robust standard errors, clustered at installation-level.

* Statistical significance at 10% level (two-tailed).

** Statistical significance at 5% level (two-tailed).

*** Statistical significance at 1% level (two-tailed).

**Table 4: Effect of the Act on GHG emissions using a reduced sample**

**Panel A: Excluding installations from NACE-Section D**

| Specification | (1) | (2) | (3) |
|---|---|---|---|
| Treat | -0.231<br>(0.175) | -6.200***<br>(0.0307) | -<br>- |
| Post | 0.221<br>(0.172) | -0.0425<br>(0.0618) | -0.043<br>(0.059) |
| Treat*Post | -0.123*<br>(0.068) | -0.113*<br>(0.0618) | -0.113*<br>(0.059) |
| Time fixed effects | Yes | Yes | Yes |
| Industry fixed effects | Yes | No | No |
| Firm fixed effects | No | Yes | No |
| Installation fixed effects | No | No | Yes |
| Observations | 2,394 | 2,394 | 2,394 |
| Adj. $R^2$ | 0.355 | 0.782 | 0.929 |

**Panel B: Excluding installations from NACE-Class 35.11**

| Specification | (1) | (2) | (3) |
|---|---|---|---|
| Treat | -0.380**<br>(0.185) | -6.189**<br>(0.0389) | -<br>- |
| Post | 0.321**<br>(0.162) | -0.0767<br>(0.0762) | -0.077<br>(0.073) |
| Treat*Post | -0.144*<br>(0.085) | -0.150*<br>(0.0779) | -0.150**<br>(0.075) |
| Time fixed effects | Yes | Yes | Yes |
| Industry fixed effects | No | Yes | No |
| Firm fixed effects | No | Yes | No |
| Installation fixed effects | No | No | Yes |
| Observations | 2,779 | 2,779 | 2,779 |
| Adj. $R^2$ | 0.307 | 0.702 | 0.916 |

This table reports regression estimates of three econometric specifications differing in included fixed effects. In Panel A, installations from NACE-Section D (Electricity, gas steam and air conditioning supply) are excluded. In Panel B, installations from NACE-Class 35.11 (Electricity Production), which is a subcategory of NACE-Section D are excluded. The dependent variable in all specifications is the natural logarithm of yearly verified emissions in 1,000 metric tons of $CO_2$eq. Treat is a dummy variable indicating treatment installations (dummy is equal to 1) and control installations (dummy is equal to 0). Post is a dummy variable indicating pre-treatment observations (dummy is equal to 0) and post-treatment observations (dummy is equal to 1). The interaction term Treat*Post is 1 for post-treatment observations for installations in the treatment group and zero otherwise. In the third specification, Treat is omitted, as we include installation fixed effects in this specification. 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, installation-year observations in 2013 are either already classified as post-treatment observations or still as pre-treatment observation). The numbers in brackets are robust standard errors, clustered at installation-level.

* Statistical significance at 10% level (two-tailed).

** Statistical significance at 5% level (two-tailed).

*** Statistical significance at 1% level (two-tailed).

**Table 5: Effect of the Act on GHG emissions including years of the financial crisis**

| Specification | (1) | (2) | (3) |
|---|---|---|---|
| Treat | -0.571***<br>(0.190) | 3.328***<br>(1.110) | -<br>- |
| Post | 0.640***<br>(0.178) | 0.0544<br>(0.763) | -0.054<br>(0.074) |
| Treat*Post | -0.195**<br>(0.086) | -0.229***<br>(0.0832) | -0.229***<br>(0.081) |
| Time fixed effects | Yes | Yes | Yes |
| Industry fixed effects | Yes | No | No |
| Firm fixed effects | No | Yes | No |
| Installation fixed effects | No | No | Yes |
| Observations | 3,933 | 3,933 | 3,933 |
| Adj. $R^2$ | 0.228 | 0.228 | 0.909 |

This table reports regression estimates of three econometric specifications differing in included fixed effects applied on an extended sample period from 2008 to 2016. The dependent variable is the natural logarithm of yearly verified emissions in 1,000 metric tons of $CO_2$eq. Treat is a dummy variable indicating treatment installations (dummy is equal to 1) and control installations (dummy is equal to 0). Post is a dummy variable indicating pre-treatment observations (dummy is equal to 0) and post-treatment observations (dummy is equal to 1). The interaction term Treat*Post is 1 for post-treatment observations of installations in the treatment group and zero otherwise. In the third specification, Treat is omitted, as we include installation fixed effects in this specification. 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, installation-year observations in 2013 are either already classified as post-treatment observations or as pre-treatment observations). The numbers in brackets are robust standard errors, clustered at installation-level.

* Statistical significance at 10% level (two-tailed).

** Statistical significance at 5% level (two-tailed).

*** Statistical significance at 1% level (two-tailed).

**Table 6: Robustness Checks**

**Panel A: Matching based on NACE-Divisions**

| Specification | (1) | (2) | (3) |
|---|---|---|---|
| Treat | -0.472*<br>(0.259) | -0.465*<br>(0.243) | -<br>- |
| Post | 1.507***<br>(0.302) | 0.927***<br>(0.280) | -0.083<br>(0.129) |
| Treat*Post | -0.154<br>(0.116) | -0.210*<br>(0.114) | -0.164*<br>(0.098) |
| Time fixed effects | Yes | Yes | Yes |
| Industry fixed effects | No | Yes | No |
| Installation fixed effects | No | No | Yes |
| Observations | 1,386 | 1,386 | 1,386 |
| Adj. $R^2$ | 0.040 | 0.181 | 0.892 |

**Panel B: Random assignment of treatment dummy**

| Specification | (1) | (2) | (3) |
|---|---|---|---|
| Starting year | 2010 | 2010 | 2010 |
| Treat | -0.042<br>(0.194) | -0.020<br>(0.174) | -<br>- |
| Post | 1.452***<br>(0.223) | 0.744***<br>(0.190) | -0.139**<br>(0.064) |
| Treat*Post | -0.028<br>(0.088) | 0.008<br>(0.085) | -0.007<br>(0.081) |
| Time fixed effects | Yes | Yes | Yes |
| Industry fixed effects | No | Yes | No |
| Installation fixed effects | No | No | Yes |
| Observations | 3,059 | 3,059 | 3,059 |
| Adj. $R^2$ | 0.013 | 0.204 | 0.918 |

This table shows two robustness checks. In Panel A, we re-estimate the econometric specifications utilizing a reduced matched sample. We match installations within NACE-Divisions. Because of the uneven distribution of treatment and control installations over NACE-Divisions, the matched sample is reduced to 198 installations (in contrast to 437 installations in the full sample). In Panel B, we use a pseudo treatment dummy. In the full sample, 119 of the 437 installations are treatment installations. We construct the pseudo treatment dummy in a way that 119 of the 437 installations in the sample are randomly assigned to a pseudo treatment group. We then re-estimate the econometric specifications. The dependent variable in all specifications is the natural logarithm of yearly verified emissions in 1,000 $tCO_2eq$. Treat is a dummy variable indicating treatment installations (dummy is equal to 1) and control installations (dummy is equal to 0). Post is a dummy variable indicating pre-treatment observations (dummy is equal to 0) and post-treatment observations (dummy is equal to 1). The interaction term Treat*Post is 1 for post-treatment observations for installations in the treatment group and zero otherwise. In the third specification, Treat is omitted, as we include installation fixed effects in this specification. 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 ultimately holding an installation, installation-year observations in 2013 are either classified as post-treatment observations or as pre-treatment observations). The numbers in brackets are robust standard errors, clustered at installation-level.

* Statistical significance at 10% level (two-tailed).

** Statistical significance at 5% level (two-tailed).

*** Statistical significance at 1% level (two-tailed).

**Figure 1. Classification emission data in 2013 as pre- or post-treatment observations**

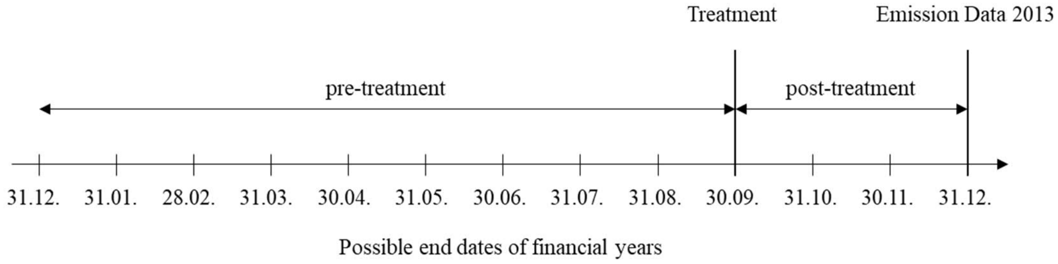
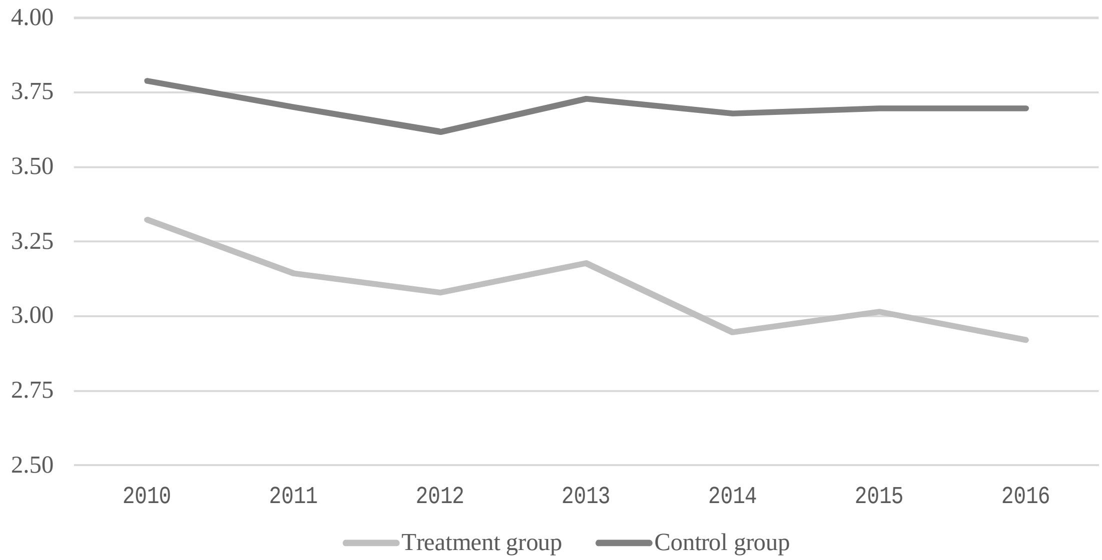
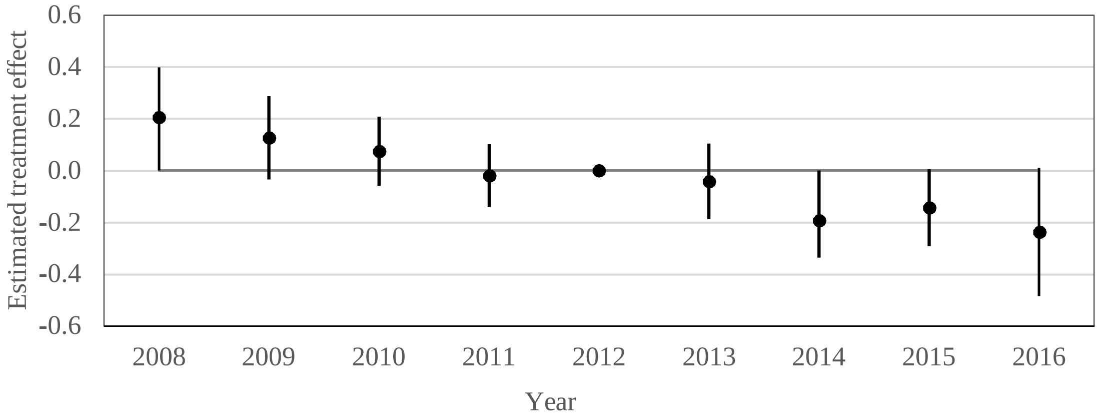


**Figure 2: Development of emissions per group**




This figure shows the development of emission levels per group across our sample period. Emissions per group are depicted as the mean of the natural logarithms of yearly verified emissions in 1,000 metric tons of $CO_2$eq.

**Figure 3: Parallel trends assumption – estimates of the counterfactual treatment effect**



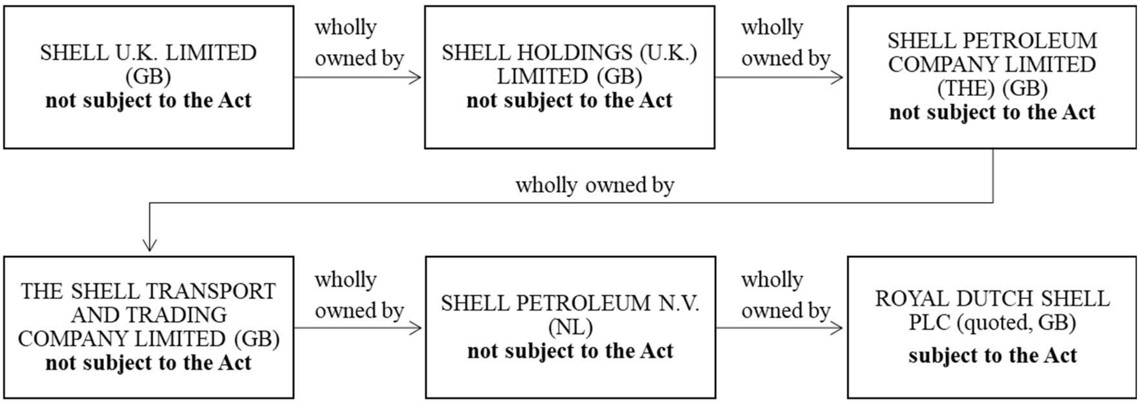

This figure shows regression coefficient estimates of the counterfactual treatment effect and 95% confidence intervals based on standard errors bootstrapped at installation level estimated on an extended sample covering 2008 to 2016. By replacing the single interaction term of Treat * Post with interaction terms of Treat with year-dummies we investigate indications of possible violations of the parallel trends assumption. 2012 is the benchmark period as it is the last pre-treatment year for most of the installations in the sample (i.e. the coefficient for 2012 is equal to zero). The econometric specification includes time and installation fixed effects (specification 3).

## Appendix A. Assignment of installations to either the treatment or the control group

In this Appendix, we expand on the process of assigning installations in our sample to either the treatment or the control group. As explained in Section 3.2, the OLP links installations from the EUTL to their parent companies by providing their names as well as BvD-Ids. We start the assignment process with a reduced sample of 509 installations. Using information from the OLP we link these installations with BvD-Ids (i.e. parent companies). We then use Bureau-van-Dijk's database Amadeus to determine whether the parent company provided by the OLP is an installation's Global Ultimate Owner (GUO) or whether the parent company itself is ultimately owned by another entity (i.e. we determine the GUO of the installation). Following the OLP, we define a GUO as an entity that controls at least 50.01% of all corporate levels below it. Furthermore, the GUO does not have any shareholders holding more than 50.01% of its equity. If the GUO is a government, we define the GUO to be the (non-governmental) entity one level below.

Corporate structures of the parent companies in the sample seem to be very different. While there are parent companies of installations that can directly be identified as GUOs, for other companies more than ten ownership levels need to be examined. Furthermore, changes over time need to be considered. Besides classical acquisitions of companies on certain corporate levels by other companies, the transfer of companies from one holding company to another (e.g. from Hold-Co One to Hold-Co Two to Hold-Co Three of the same GUO) complicates the determination of GUOs. Moreover, corporate structures mostly include entities of different legal types (i.e. private or public) as well as from various countries (e.g. the UK, the Netherlands, Germany, and Luxembourg). Especially the latter fact is a main reason why we are not able to assign 34 parent companies of installations to one of our two groups. Data availability on corporate structures reaches its limits in Amadeus when companies are located in countries such as Jersey, the Bahamas or Cayman Islands. At the end of this process, we are able to determine the

GUOs of 261 companies of installations in the sample (some parent companies are simultaneously also the GUO). For these 261 GUOs, we then determine whether they are affected by the mandatory emission disclosure provisions of The Companies Act 2006 (Strategic Report and Directors' Report) Regulations 2013. As explained in Section 2.1, the Act requires listed companies to disclose information on their greenhouse gas emissions in their annual reports. Section 385 (2) of the Companies Act 2006 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 or on an exchange in a European Economic Area state, or admitted to trading at the New York Stock Exchange or Nasdaq. Companies not meeting certain thresholds (i.e. sales or balance sheet total) are explicitly exempt from the regulation. If GUOs fulfil these criteria, we assign them and the respective installations to the treatment group. If not, the GUOs and the respective installations are assigned to the control group.




The figure depicts the process of linking Shell U.K. Limited, which, as a private UK-based company is not subject to the Act to Royal Dutch Shell PLC, a public UK-incorporated company listed on the Main Market of the London Stock Exchange, which is subject to the Act.

### Appendix B. Sample Selection

This Appendix summarizes our sample selection and lost observations when linking installations-level emission data to parent companies, presented in table B.1.

**Table B.1: Linking Installation-level Emissions to Parent Companies**

| | No | Percent | Yes | Percent | Total |
|---|---|---|---|---|---|
| Valid BvD-Id | 403 | 31.0 | 898 | 69.0 | 1,301 |
| Balanced panel: No observations in front or back years during observation period | 389 | 43.3 | 509 | 56.7 | 898 |
| Parent company not unequivocally determined | 49 | 9.6 | 460 | 90.4 | 509 |
| Trimming top and bottom 1% | 23 | 5.0 | 437 | 95 | 460 |

This table shows the number of observations lost when linking emissions data of individual installations to parent companies. The full sample of emissions data consists of 1,301 installations. In 2014, emissions from these installations cover about 41% of total UK $CO_2$ emissions, or about 79% of all emissions in the energy supply, business and industrial process sectors. Our final sample consists of 437 installations that cover about 13% of all or 27% of all emissions in the energy supply, business and industrial process sectors, respectively.