Under Pressure: The Link between Mandatory Climate Reporting and Firms' Carbon Performance

1. Introduction

Firms experience increasing pressure to report on their carbon risks, strategies, and emissions (e.g., Reid & Toffel, 2009). In this context, voluntary climate reporting initiatives such as the Carbon Disclosure Project (CDP) have emerged and national mandatory climate reporting regimes have been established (e.g., the GHGRP). While there is convincing evidence that climate data is used by investors for firm valuation (e.g., Chapple et al., 2013; Clarkson et al., 2015; Matsumura et al., 2014; Schiemann & Sakhel, 2019; Solomon et al., 2011), whether the reporting of such data leads to improvements in the carbon performance of reporting firms is still being discussed (e.g., Belkhir et al., 2017; Downar et al., 2019; Haque & Ntim, 2018; Qian & Schaltegger, 2017; Tomar, 2019). We contribute to this discussion by assessing the effect of a mandatory climate reporting regime on the carbon performance of affected firms.

A growing number of political measures to limit climate change stresses the relevance of this research. During the United Nations Framework Conference on Climate Change (UNFCCC) in Paris in November and December 2015, 197 parties agreed to the goal of limiting the rise in global temperature to significantly below 2°C (preferably 1.5°C) compared to the preindustrial level (UNFCCC, 2015). This goal requires tremendous efforts to reduce carbon emissions in the public as well as the private sector. According to a special report by the Intergovernmental Panel on Climate Change (IPCC), the remaining carbon budget for a 66% likelihood of limiting global warming to 1.5°C amounts to 420 $GtCO_2$, and about 580 $GtCO_2$ for a 50% likelihood (Rogelj et al., 2018). In other words, carbon neutrality must be reached in about 20 years (66% chance) or 30 years (50% chance), respectively. Consequently, political decision makers around the globe have discussed and implemented several measures to decrease carbon emissions in the private sector, such as carbon taxes (e.g., Norway and

Switzerland), climate-friendly mobility concepts (e.g., lowering the VAT rate on train tickets in Germany) and especially Emissions Trading Systems (ETS) (e.g., the EU ETS). The measurement and reporting of carbon emissions is a requirement for the application and assessments of these measures.

Drawing on institutional and legitimacy theory, we suggest that a mandatory climate reporting regime leads to firm carbon performance improvements. If firms are mandated to report their carbon emissions, they might face increasing institutional pressures to limit their contribution to climate change (e.g., DiMaggio & Powell, 1983; Meyer & Rowan, 1977). According to legitimacy theory, firms respond to institutional pressures in order to ensure their "license to operate" (Newson & Deegan, 2002). We argue that mandatory climate reporting could trigger substantive firm carbon performance measures due to a combination of legislative (reporting) and societal (disclosing) pressure.

The GHGRP from the EPA provides a unique setting to test this assumption. This mandatory climate reporting regime obliges firms to report emissions from sources which emit 25,000 metric tons or more of carbon dioxide equivalent per year in the United States (US) (EPA, 2020). It covers direct carbon emitters, suppliers of fossil fuel and industrial gas as well as facilities that store carbon dioxide underground for sequestration or other reasons. Certain industries such as the agricultural industry are exempt from the GHGRP. Reporting is, in general, at the facility level and is conducted annually, covering emissions from the previous year. The EPA verifies the collected data by itself and subsequently publishes the data on its website. The aim of the GHGRP is to better understand the sources of carbon emissions in the US and to support the development of further regulations to reduce emissions. The EPA estimates that the facilities covered by the GHGRP are responsible for approximately 85-90% of the total US carbon emissions (EPA, 2020). In contrast to the EU ETS, the GHGRP is not connected to a carbon emission reduction program and pricing mechanism. Consequently, improvements in carbon performance subsequent to the introduction of the GHGRP are directly attributable to the mandatory reporting effect, and results are not distorted by carbon pricing effects.

Our methodological framework is based on panel data. We apply propensity score matching and a difference-in-differences approach. For the period from 2007-2016, we estimate the effect of the GHGRP on the carbon performance of affected US firms. Our results show that while there were no significant differences in the development of carbon performances between affected and unaffected firms prior to the introduction of the GHGRP, affected firms show a better carbon performance development subsequent to its introduction. This observation is robust to changes in the difference-in-differences design and the sample composition. However, an additional analysis also indicates limitations of the GHGRP regarding direct net emission reductions.

The contributions of our study are threefold. First, we add to the strand of literature on the effects of climate reporting on the carbon performance of reporting firms. Existing studies mainly focus on the effects of voluntary climate reporting (Belkhir et al., 2017; Haque & Ntim, 2018; Qian & Schaltegger, 2017) or of mandatory climate reporting on facility emissions (Downar et al., 2019; Matisoff, 2013; Tomar, 2019). We extend this scope by analyzing the impact of the introduction of a mandatory climate reporting regime on the carbon performance of US firms on firm level, meaning global scope 1 emissions. This extension is important because facility level analysis cannot capture whether firms shift carbon emissions from facilities falling under the GHGRP to other facilities, which would result in no overall improvements in carbon performance. Second, we provide insights into the effect mechanisms of mandatory climate reporting regimes by differentiating between the effects of the introduction of GHGRP (2010) and the first release of the carbon emission data (2012). Third, knowledge about the influence of different climate reporting designs on firms' carbon performance is essential for the future design of policies towards a low carbon economy (e.g., Bel & Joseph, 2015; Haites, 2018; Murray & Maniloff, 2015). Our results indicate that a mandatory climate reporting regime might be a suitable political instrument to improve firm carbon performance.

The remainder of this paper is structured as follows. In Section 2, we present the theoretical background, related literature and derive our hypothesis. Section 3 describes our research design. Section 4 discusses the results of our empirical tests and Section 5 concludes our study.

2. Theoretical background, related literature and hypothesis development

We embed our study in institutional and legitimacy theory. Institutional theory describes the influence of institutional pressures on organizations (DiMaggio & Powell, 1983; Meyer & Rowan, 1977). In this context, DiMaggio & Powell (1983) differentiate between three institutional pressures: coercive, mimetic and normative pressure. Coercive pressure can be exerted directly via regulations or laws or indirectly via the expectations of the environment in which the firms operate. Mimetic pressure is a consequence of uncertainty. Due to uncertainty about the consequences of different actions, firms copy the business behavior of other firms. Normative pressure results from the expected application of normative rules about professional business behavior, developed and promulgated by, among others, universities, manager networks, and trade associations (DiMaggio & Powell, 1983).

The introduction of the GHGRP is primarily related to two different sources of coercive pressure. First, the program obliges firms to report the carbon emissions of carbon intensive facilities to the EPA since 2010. This stresses the need to monitor and manage carbon emissions, creating the impression that more restrictive regulations might follow and carbon performance improvements are inevitable (legislative pressure). Second, the public disclosure of the collected data since 2012 and the rising societal awareness on the consequences of climate change pressure firms to limit their contribution to climate change (societal pressure).[1]

Firms' reactions to coercive pressure are connected to issues of legitimacy (DiMaggio & Powell, 1983; Meyer & Rowan, 1977). Suchman (1995) defines legitimacy as "a generalized perception or assumption that the actions of an entity are desirable, proper, or appropriate within some socially constructed system of norms, values, beliefs, and definitions" (p. 574). In other words, firms are considered legitimate if their activities do not violate the rules and values of their environment (Dowling & Pfeffer, 1975). According to Deegan (2002), legitimacy is necessary for two main reasons. First, legitimate firms have better access to critical resources. Second, illegitimate firms could be confronted with various forms of retribution which threatens their ability to survive (e.g., loss of sales or an insufficient workforce). Consequently, firms respond to coercive pressure to ensure their legitimacy, which is seen as a "license to operate" in a society (Newson & Deegan, 2002).

Climate change is an important political and societal issue. The growing concern about the speed and consequences of global warming as documented by the IPCC (Rogelj et al., 2018) has led to regulatory measures (e.g., the EU ETS) and rising public attention. The contribution of private firms to climate change is an important aspect in this debate (e.g., CDP, 2017). According to Randers (2012), firms must cut their "GHG emissions per unit of value added'' by 5% per year to keep global warming below 2°C compared to the preindustrial level, while simultaneously ensuring economic growth. Consequently, climate change and carbon emission management have become a legitimacy issue for many firms. Legitimacy-seeking management measures can either be substantive or symbolic (Ashforth & Gibbs, 1990). While substantive

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[1] An improvement in carbon performance could also be the consequence of mimetic or normative pressure. For example, firms might be motivated to manage carbon emissions if their peers do or carbon risk management has become a part of a professional business behavior. Yet, considering the increasing number of political measures and social movements to limit climate change in recent years, we argue that coercive pressure is the main influence on firms' carbon performance efforts.

management involves real changes in a firm's goals, structures, processes or practices, symbolic management implies that a firm simply tries to portray such a change to appear consistent with social values and expectations without fundamentally implementing this change (Ashforth & Gibbs, 1990). Therefore, it is necessary to assess the real effects of legitimacy-seeking management measures related to climate change, to avoid capturing symbolic changes.

We analyze whether a mandatory climate reporting regime is a suitable mediator to coercive pressure, triggering substantive carbon performance management measures. This question is of critical importance, since research has shown that voluntary social and environmental reporting is often used by firms as a tool to communicate their legitimacy to external parties (Archel et al., 2009; Deegan, 2002; O'Donovan, 2002) without improving their actual environmental performance (e.g., Haque & Ntim, 2018). In line with this concern, some empirical studies show that poorer environmental performers are more likely to disclose non-financial information voluntarily to legitimize their poor performance (e.g., Cho et al., 2012; Cho & Patten, 2007; Patten, 2002). Specifically focusing on climate reporting, Haque & Ntim (2018) report that firms adopting voluntary reporting guidelines, such as the guidelines from the Global Reporting Initiative (GRI), are more likely to implement carbon reduction measures. However, these carbon reduction measures are often just symbolic and do not improve carbon performance. Consistently, Belkhir et al. (2017) and Bernard et al. (2015) find no correlation between GRI reporting and firms' carbon emissions. However, Qian & Schaltegger (2017) report that increases in voluntary carbon disclosure levels (via CDP) are related to improvements in carbon performance.

While several mandatory climate reporting regimes were implemented in recent years, investigating their effect on firms' carbon performance is a rather new research area. Downar et al. (2019) show, for a sample of UK facilities, that the introduction of the UK Companies Act Regulations 2013 leads to a higher reduction of carbon emissions for facilities falling under the regulation than for other facilities. However, Downar et al. (2019) only focus on absolute carbon emissions as a dependent variable instead of using an additional emission intensity measure to capture actual activity levels. For the US, Matisoff (2013) finds no improvements in carbon emission intensity or absolute carbon emission levels for facilities regulated by different state level mandatory climate reporting regimes. Contrary to this, Tomar (2019) indicates that facilities reporting under the GHGRP reduce absolute carbon emissions by 7% after the disclosure of this data in 2012. Our study differs from these previous studies by assessing the effect of the introduction of the GHGRP on firms' carbon emission intensity based on firms' global scope 1 emissions. Firms' global scope 1 emissions are a good basis for our analysis because they are not impacted if firms shift production (and, thereby, carbon emissions) from facilities falling under the GHGRP to other facilities (these can be facilities within the US, which do not reach the threshold value of the GHGRP or facilities outside the US). The use of a carbon emission intensity variable seems advised to capture the activity levels of firms.

We argue that a mandatory climate reporting regime, which is not connected to a carbon emission reduction program, will lead to improvements in carbon performance over time for the following reasons. First, the need for regulatory intervention in climate reporting stresses the importance of monitoring and potentially managing carbon emissions and highlights the possibility of the introduction of further climate change regulations. Second, the collection and disclosure of carbon emission data through public institutions as well as easy and free access to the data might improve the trustworthiness of the data and enables society to surveil the development of firms' carbon performance. This leads to enhanced societal pressure on firms to improve carbon performance. Third, since firms are obliged to report, there is no self-selection bias of reporting firms, where firms can use disclosure as a symbolic action to legitimize their action. Instead only substantive action leading to subsequent improvements in carbon performance are positively acknowledged by society. Fourth, the success of the Toxic Release Inventory (TRI) in the US highlights how mandatory disclosure can improve substantive environmental performance. Since its implementation in 1988, the TRI tracks the management of certain toxic chemicals that may pose a threat to the environment (EPA, 2019). US facilities in different industries must annually report how much of each chemical they release into the environment or manage through recycling, energy recovery and treatment. The reporting is mandatory when the use of these chemicals exceeds established levels. Currently, the TRI covers more than 650 different toxic chemicals. In 1995, seven years after its implementation, the release of covered chemicals declined by 45% (Fung & O'rourke, 2000). In their assessment of the TRI, Fung & O'rourke (2000) attribute its success, among other things, to the possibility for citizens to inform themselves about toxic chemical releases and the resulting public pressure on polluting firms. Both, the TRI and the GHGRP, are pure mandatory reporting regimes, in the sense that they are not explicitly linked to any reduction programs. The introduction of the GHGRP therefore provides a suitable setting to assess whether a mandatory climate reporting regime can contribute to substantive firm carbon performance improvements. Based on the arguments above, we formulate the following hypothesis:

H1: Firms affected by the GHGRP will improve their carbon performance subsequent to the introduction of the reporting regime to a greater extent than unaffected firms.

3. Research design

3.1. Sample

We collect data from different sources for our analysis. Our main sample comprises US firms with scope 1 emissions data in the Trucost database from 2007-2016. Trucost is a specialized provider of firm level environmental information, which employs different estimation models to fill data gaps for non-reporting firms. Theoretically, in a voluntary carbon reporting environment, firms could make the choice to disclose their carbon emissions dependent on how well they perform. That means, firms which already have a good carbon performance or are on their way to achieve it, will be more likely to disclose their emissions voluntarily. Firms with lackluster performance, on the other hand, are more likely not to disclose. This would introduce a bias towards good carbon performers in our dataset and would make it more difficult to identify improvements in carbon performance. By using Trucost carbon emission data and including carbon emission estimations for non-reporting firms, we can mitigate this bias. Busch et al. (2020) show that reliability and consistency of Trucost estimated scope 1 emissions is similar to actual reported scope 1 emissions. In addition, the inclusion of these estimations allows us to investigate carbon emissions before the respective firms started reporting or the mandatory regulation (i.e., the GHGRP) was introduced.

We exclude firms from the financial industry in our analysis, since these firms typically do not face issues in dealing with scope 1 emissions and the unique characteristics of financial firms can skew results (Delmas et al., 2015). This leads to an initial sample of 1,458 US firms with 8,017 firm-year observations. We then identify firms that own and operate facilities falling under the GHGRP by collecting facility information through the EPA Facility Level Information on Greenhouse Gases Tool (FLIGHT) and hand-collecting the International Securities Identification Number (ISIN) for each firm that is listed in the FLIGHT as a facility owner. In cases where a facility is not 100% owned by a single firm, we identify the firm owning the largest share as the principal owner. Using the ISIN as an identifier, we can then match the data to our dataset allowing us to categorize firms as being directly affected by the GHGRP, because facilities they own are mandated to report (263 firms with 1,910 firm-year observations) and firms that are not directly affected (1,195 firms with 6,107 firm-year observations. We combine these carbon emission data with financial information from Thomson Reuters Datastream. Due to data availability, we lose additional observations and are left with a sample of 1,454 firms and 7,961 firm-year observations. We also winsorize all continuous variables in our analysis at the highest and lowest 1%-level to curb the effect of potential outliers in the data.

3.2. Variables

In our base analysis, our dependent variable is a process-based measure for carbon performance. We operationalize this variable as carbon emission intensity (*Scope 1 Intensity*), calculated as scope 1 emissions divided by total assets. *Scope 1 Intensity* captures firms' efforts to reduce carbon emissions in their operations, while controlling whether firms are expanding/reducing their operations. In contrast, outcome-based measures like total carbon emissions are often not able to fully capture managerial efforts aimed at reducing carbon emissions, since they disregard changes in the amount of firms' operations (Busch & Hoffmann, 2011). Nevertheless, the overarching goal of climate regulation is the reduction of the total anthropogenic carbon emissions. Carbon performance is starkly connected to this narrative. If carbon performance becomes a legitimacy-threatening factor, firms with a good carbon performance could conceivably benefit, for example, by increasing their market share at the cost of poor performers. Thus, an individual firm with increasing emissions but an improving carbon performance (i.e., an improving carbon emission intensity) can reduce its industry's absolute emissions by crowding out inferior performers.

Scope 1 Intensity is a proxy that captures the direct emissions targeted by the GHGRP. The actual emissions that fall under the GHGRP are direct emissions of individual facilities exceeding 25,000t of carbon emissions annually. On the one hand, no data is available for this level before the introduction of the regulation and direct facility level emissions are the main driver of a firm's total direct emissions. On the other hand, a focus only on the facility level emissions cannot control for potential shifts of direct emissions. For example, after the introduction of the GHGRP, a firm could shift its production (and thereby direct carbon emissions) to a facility not covered by the GHGRP (i.e., a facility inside the US, but with direct emissions below the threshold value or a facility outside of the US). While a facility level analysis would report improvements in such a scenario, a firm level analysis would not.

Therefore, *Scope 1 Intensity* is closely related to the emissions falling under the GHGRP but captures the behavior of the complete firm. This is also reflected in the rate of scope 1 emissions to facility level emissions in our sample. On average, Trucost emissions of sample firms regulated by the GHGRP consist of 80% facility level emissions which are covered by the GHGRP and 20% other emissions (i.e., outside the GHGRP).

In our base analysis, we investigate the effect of the GHGRP introduction on US firms' carbon performance through a difference-in-differences estimation. The EPA introduced the GHGRP in 2010. However, there was a delay between facilities submitting data to the EPA and the EPA making the information publicly available. Data collected under the GHGRP was released for the first time in January 2012 (Tomar, 2019). Coercive pressure through government regulation should, thus, be exerted in 2010. Pressure from the firms' environment can be a factor both in 2010 and 2012. With the introduction of the GHGRP in 2010, it became evident to firms that the data collected by the EPA would eventually be made public. Once the publication started in 2012, the public got access to the actual carbon emission data. Accordingly, we analyze effects following both events separately by creating two dummy variables *Post1* and *Post2*. The variables take the value of 0 for years before the treatment and the value of 1 starting with the respective treatment year, meaning 2010 for *Post1* and 2012 for *Post2*. We use a dummy variable *Treat* to capture firms affected by the GHGRP. *Treat* takes the value of 0 for firms that are not directly affected by the GHGRP because they don't own regulated facilities and the value of 1 for firms which own regulated facilities. The interaction of *Post1* and *Post2* with the *Treat* variable allows us to measure the difference in the carbon performance development between affected and unaffected firms after the respective treatment.

We also control for several other factors that could influence firms' carbon performance. The amount of property, plant and equipment (PPE) indicates the extent to which a firm relies on physical production assets in its business. Thereby, PPE capture a firm's tangible assets with an expected useful life of over one year which are used to produce goods or for the distribution of services. We measure *PPE Intensity* as the ratio of the net value of PPE to total assets. A high *PPE Intensity* indicates that a firm relies more heavily on physical production processes which typically causes more carbon emissions compared to a firm, for example, from the service industry, which requires less PPE in its production processes. Therefore, we expect *PPE Intensity* to be negatively correlated with carbon performance. We use a firm's total assets as an indicator of firm size (variable *Total Assets*, denominated in 1,000 US dollars*)*. We expect *Total Assets* to be positively related to carbon performance, because larger firms can have efficiency gains due to economies of scale (Clarkson et al., 2008). Larger firms may also have more resources to build know-how in carbon emission accounting and to improve carbon performance. Furthermore, large firms are more visible to lawmakers and the public and are thus more exposed to political and societal coercive pressure. We control for risk, operationalized as *Leverage* and calculated as the total long-term debt to common equity ratio. The reporting of carbon emissions is an essential indicator for a firm's sustainability performance and the legitimacy of its ongoing operations in which debt-holders are particularly interested (Dhaliwal et al., 2014). The introduction of a mandatory reporting regime that forces firms to report their carbon emissions therefore provides an additional incentive to reduce carbon emissions for highly leveraged firms. Thus, we expect a positive correlation between leverage and carbon performance. Lastly, we control for firms' profitability (*ROA*) measured as the ratio of income before extraordinary items and total assets (Barth et al., 2017), since profitable firms have more opportunities and resources available to implement measures to improve their carbon performance.

Table 1 shows the industry distribution in percent in our different samples. As expected, we see a substantial difference in the sample composition for the oil & gas, basic materials and utilities industries, where the majority of firms are affected by the GHGRP. Conversely in the health care, consumer services and technology industries, a substantial proportion of firms are not affected by the regulation.

Insert Table 1 about here.

3.3. Model and measurement

We test H1 through a difference-in-differences analysis, where *Post* is either *Post1* (GHGRP introduction) or *Post2* (first publication of carbon emission data) as described above and *Treat* captures whether a firm is affected by the GHGRP. Therefore, our regression model has the following form:

$$Scope\ 1\ intensity_{i,t} = \beta_0 + \beta_1 Post_t + \beta_2 Treat_i + \beta_3 Post_t * Treat_i + \beta_n \begin{pmatrix} PPE\ Intensity_{i,t} \\ Total\ Assets_{i,t} \\ Leverage_{i,t} \\ ROA_{i,t} \end{pmatrix} + \varepsilon_{i,t}$$

We employ an OLS regression with firm-clustered standard errors. The clustered standard errors help us account for within-cluster correlation and heteroscedasticity (Gow et al., 2010; Petersen, 2009). We also test the parallel trend assumption that is underlying the difference-in-differences estimation method. We follow Chen et al. (2018) and apply a timing approach where we substitute our interaction term (*Post* * *Treat*) with timing variables, *year-1* and *year+1,* which we individually interact with our treatment indicator to track the effect of the regulation over time compared to the benchmark years 2010 and 2012, respectively. If the introduction of the regulation and the publication of carbon emission data have a causal effect on carbon performance, we should not see significant effects before *year-1* or after *year+1*. We test the parallel trend immediately around the introduction of the regulation, because firms operate in a complex environment of mimetic, normative and coercive pressures. Over a longer period of time, various events, such as the 2008 oil price drop or the release of the new IPCC Assessment Report in 2014, can influence firms and industries to different degrees and direct more attention towards the issue of climate change. Such events could potentially introduce confounding factors. We perform the parallel trend test with different model specifications that are robust to heteroscedasticity, including GLS models with and without firm fixed effects and OLS models with clustered standard errors and Huber-White sandwich estimators.

3.4. Propensity score matching

When there are concerns that the treatment group is not randomly selected, which typically is the case in real world settings, propensity score matching can be used to make the treatment and control groups more comparable regarding the observable variables (Chen et al., 2018). The propensity score match is based on the likelihood estimation of a control group firm having received the treatment. Using the respective pre-treatment period, we estimate the probability of a given firm being affected by the GHGRP. We identified a set of variables that capture firms' unique characteristics linked to their carbon performance. Since our dependent variable is based on absolute scope 1 emissions and *Total Assets*, we use these variables in our matching and add *Leverage*, *PPE Intensity*, and *ROA*. By matching treatment with control firms using these variables, we generate a sample that allows us to compare firms, which are similar in their emissions and firm characteristics in the pre-treatment periods (2009 and 2011) but differ in whether they are affected by the GHGRP.

We use these variables to find a nearest neighbor matched control firm for each treated firm in a one-to-one matching with replacement. Additionally, we define a maximum caliper distance of 0.2 for our matching, suggested to be the most effective distance when estimating differences in means in observational studies (Austin, 2011). The nearest neighbor method finds the closest match to the treated firm and allowing replacement improves the matching quality. Furthermore, when combined with a caliper, the likelihood of bad matches is further decreased (Caliendo & Kopeinig, 2008; Shipman et al., 2016). The effectiveness of the propensity score matching can be evaluated by looking at the standardized bias (Rosenbaum & Rubin, 1985), and by comparing the difference in means before and after the matching (Caliendo & Kopeinig, 2008). For our propensity score match, we find that the mean difference between the treatment and control group is substantially reduced, meaning that on all relevant dimensions, both treatment and control group firms exhibit similar characteristics before the respective treatment. Additionally, while the mean difference between treatment and control groups is significant before the matching, it is insignificant afterwards. A reduction to around 5% bias is generally considered as acceptable in most empirical studies (Caliendo & Kopeinig, 2008) which, importantly, is fulfilled for our dependent variable in both samples for the treatment years 2010 and 2012.

We then conduct a difference-in-differences estimation on the propensity score-matched samples with the treatment years 2010 and 2012, respectively. Following our previous approach, we employ an OLS regression.

4. Empirical Results

4.1. Descriptive statistics

Panel 1 of Table 2 shows the descriptive statistics of all the variables in our model after winsorizing at the highest and lowest 1% level. In the base sample, firms release 2,072,579t of carbon emissions on average per year, or 0.139t of carbon emissions per 1,000 US dollars in total assets and have 21.9 billion US dollars in total assets. They also own, on average, 0.295 US dollars in PPE per US dollar of total assets. In the sample comparison between regulated and unregulated firms, we find that carbon emissions (*log Scope 1*) are higher in the regulated sample than in the unregulated sample. In absolute terms, the difference in carbon emissions between the regulated and unregulated sample is about 5.7 million tons and unregulated firms release about 12% of the carbon emissions of regulated firms on average. *Scope 1 Intensity* is also higher in the regulated firm sample (an average of 0.361t of carbon emissions per 1,000 US dollars in total assets) compared to the unregulated firm sample (an average of 0.070t of carbon emissions per 1,000 US dollars in total assets). Furthermore, more than 5% of firm-year observations have a leverage of 0.

Panel 2 of Table 2 also shows the Pearson correlation coefficients of our variables. While all coefficients are significant at $p < 0.01$, the correlation coefficients are typically low. Even the highest coefficient values (correlation of *log Scope 1* and *Scope 1 Intensity*; *log Scope 1* and *PPE intensity*) do not exceed 0.6. Therefore, we do not expect problems with multicollinearity.

Insert Table 2 about here.

The descriptive statistics for our propensity score matching samples (PSM samples) are shown in Panel 3 of Table 2. The sample means for the regulated and unregulated firms lie close together for both treatment years. This indicates that the matching process produced two samples in which the regulated and unregulated firms are similar regarding the relevant dimensions affecting carbon emissions. As further support for the efficiency of our matching process, the differences between treatment and control groups were significant at $p<0.01$ before matching, while after the matching no significant differences occurred.

4.2. Base analysis

The results of our base analysis, presented in Table 3, are in line with H1. Column (1) shows a significantly negative coefficient for the interaction term *Post**Treat* (coefficient: -0.079; $p < 0.01$). That means, after the introduction of the GHGRP in 2010, firms in the treatment group improve their carbon performance significantly more than firms in the control group. In addition, *Post* is also significantly negative (-0.017; $p < 0.1$) indicating that all firms improved their carbon performance to some degree. This result also means that firms in the treatment group saw a total change of *Scope 1 Intensity* of -0.096 after the GHGRP introduction (-0.017 – 0.079). More specifically, based on the average carbon emission intensity for treatment firms in the pre-treatment period (0.427), the total reduction of carbon emission intensity translates into an improvement in carbon performance of around 22.5% (0.096/0.427), while the improvement attributable to being affected by the GHGRP still amounts to around 18.5% (0.079 / 0.427).

These results are confirmed by the propensity score matched sample presented in column (5), albeit with a smaller carbon performance improvement. The average carbon emission intensity of 0.429 for treated firms in the pre-treatment period in the PSM sample, and the coefficient of -0.050 for the interaction term *Post*Treat* suggest an improvement attributable to being affected by the GHGRP of around 11.7%. The coefficient of *Post* is -0.042, indicating a total improvement effect of -0.092 (-0.042 – 0.050) or of around 21.4% of pre-GHGRP *Scope 1 Intensity* (0.092/0.429).

Insert Table 3 about here.

The treatment year 2012 captures whether the publication of reported carbon emission data leads to additional societal pressures on firms to improve their carbon performance. While both political and societal coercive pressure play a role in firms' reaction for the year 2010, only additional societal pressure through public awareness of emission levels should influence firms' decisions in 2012. Column (3) of Table 3 shows a significant coefficient of *Post*Treat* (-0.080, $p < 0.01$). However, this result is not robust, because the PSM sample (column (7)) has an insignificant coefficient for *Post*Treat* (-0.007, $p > 0.1$). While Tomar (2019) finds carbon emission improvements on the facility level after the public disclosure event in 2012, we cannot unequivocally confirm this result.

Note that the control variables included in the base regressions in columns (1) to (4) have very similar coefficients in 2010 and 2012. This similarity is because we run the regressions on the same sample and vary between the models only in regards to the treatment variable. All of our base analyses were also tested for parallel trends. Results are presented in columns (2), (4), (6) and (8), showing no significant effects before or after the respective treatment year (variables: *Year-1*Treat* and *Year+1*Treat*). The results of the additional variations of the parallel trend test confirm our initial findings. We therefore conclude that our results indeed measure the hypothesized events and their effects on carbon performance.

Overall, we interpret the results as a strong indication that firms forced to disclose their facility level emissions to the EPA perceive this requirement as a form of regulatory and societal coercive pressure, to which they respond with substantive emission reduction efforts. Firms are not only reducing facility level emissions as reported by Tomar (2019), but are also improving their overall carbon performance. Tomar (2019) argues that firms' operating facilities, which are closely clustered to other reporting facilities, can profit from this proximity through benchmark learning, adapting similar techniques to surrounding reporting entities. Our results extend this idea to the level of the individual firm. The benchmark learning and adaptation of similar techniques are not just materializing in surrounding reporting entities, but also seem to be permeating the same organization, thus improving the carbon performance of the entire firm.

4.3. Additional analyses and robustness tests

We perform a number of additional analyses to address potential limitations in our base analysis. First, in the US setting, we compare firms directly affected by the GHGRP with firms that are not affected. The GHGRP focuses on firms in high carbon emitting industries, and despite our efforts to create a comparable sample via propensity score matching, the results could also be impacted by other events in the same years, which affected the carbon performance of the treatment firms more than the carbon performance of the control firms. For example, from 2009 to 2010, the oil price increased considerably. Therefore, our first additional analysis focuses on a comparison of the treatment firms within the US with firms outside the US, which have similar characteristics to the treatment firms. We use firms from the EU, which are affected by the EU ETS, for comparison, because the EU ETS focuses on similar firms (i.e., high emitting firms). Indeed, both regulations are similar in their focus on facilities with emissions above an annual threshold of 25,000 metric tons. However, the EU ETS is connected to a price on carbon and it was introduced in 2005. That means, comparable EU firms do not undergo significant changes in EU ETS regulation between 2009 and 2010 because they have already undergone these changes.

We gather information about firms affected by the EU ETS similarly to those affected by the GHGRP. We collect the facility level carbon emission data from the database Carbon Market Data. We then use the database's matching of facilities to firms and additionally add ISIN identifiers manually, where they are missing. We then use the same model as in our previous analysis to calculate the difference-in-differences between US and EU firms that are subject to a mandatory reporting regime. We also collect monthly carbon price information from the European Energy Exchange (EEX) and calculate annual average EU ETS allowance prices, which we use as an additional control variable (*ETS Allowance Price*).

Table 4 shows results of the US-EU difference in difference model. Column (1) presents a significant coefficient for *Post*Treat* (-0.0141; $p < 0.01$), indicating that US firms improve their carbon performance significantly more than their European counterparts after the GHGRP was introduced. This might seem to be a surprising result at first, because the EU regulation affects the same industries and has an additional carbon price attached to it. However, we do not see a significant effect from the allowance price in our regression analysis, which is consistent with the common assessment that the early phases of the EU ETS failed to incentivize investments in emission-reducing measures, due to an oversupply of emission allowances and thus too low allowance prices (Edenhofer et al., 2017). In fact, during our sample period, the average allowance price fluctuated around 10 euros per metric ton of carbon emissions.[2] If the market motivates firms to reduce carbon emissions, the extremely low allowance price may overwrite signals of regulatory coercive pressure through mandatory reporting. The number of

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[2] The average allowance price for 2009 and 2010 was 12.65 euros and 16.28 euros respectively.

allowances issued to firms (the main determinant of the allowance price) is determined through the same regulatory processes as the reporting regulation. Therefore, a low allowance price might signal low regulatory pressure on carbon performance improvements. Although the EU and US firms affected by their respective regulation can be expected to be very similar, the setting is not ideal for a difference in difference analysis because our control firms (EU firms) are not free of any treatment (they just received the treatment much earlier). Therefore, we add a second analysis, and compare US firms affected by GHGRP to high carbon emitting firms located in the rest of the world which are not affected by the GHGRP or the EU ETS. We first identify industries characterized by high carbon emissions. The Industry Classification Benchmark (ICB) classifies listed firms according to their main source of revenue. Table 1 highlights the industry composition and the predominant industries of our US treatment sample (regulated firms sample). Based on this classification, we take the top 50% of emitters in terms of absolute carbon emissions in each of the following industries: Oil & Gas (ICB 0001), Basic Materials (ICB 1000), Industrials (ICB 2000), Consumer Goods (ICB 3000) and Utilities (ICB 7000). Thus, we identify firms that should operate similar facilities to their US counterparts in the most emission intensive industries. We then proceed, as previously, by conducting a difference-in-differences analysis, including a test for parallel trends. Results in Table 4 column (3) contain a significantly negative coefficient for *Post*Treat* (-0.062; $p < 0.01$), meaning that US firms improved their carbon performance significantly more than similar firms outside the US and EU. This finding further confirms our base analysis results and supports the notion that mandatory climate reporting is an effective tool to improve firm carbon performance.

Insert Table 4 about here.

In further analysis, we investigate whether the introduction of the GHGRP also had an effect on firms' absolute carbon emissions. The idea behind introducing legislation on firm carbon emissions is to counteract and prevent climate change. Focusing on reductions of absolute carbon emissions seems to be a straightforward approach to evaluate the effectiveness of such a policy. We, therefore, perform another analysis, where we use the natural logarithm of absolute scope 1 emissions as the dependent variable in our model.

Table 5 presents our analysis of absolute carbon emissions for the treatment years 2010 and 2012. In columns (1) and (3) of Table 5 we present the results of the difference-in-differences analysis using our base sample and our PSM sample for the treatment year 2010. Columns (5) and (7) provide the same analysis using the treatment year 2012. The even-numbered columns in Table 5 show the respective parallel trend tests for each analysis.

In Table 5 column (1), *Post* has a significantly negative coefficient (-0.567, $p < 0.01$), which means that untreated firms have 56.7% less emissions in the post-treatment period on average than in the pre-treatment period. *Post*Treat* is significantly positive (0.252, $p < 0.01$) indicating that treatment firms reduced their emissions significantly less than control firms. More specifically, treatment firms decreased their absolute emissions following the GHGRP introduction by only 31.5% (56.7% - 25.2%) compared to the 56.7%-reduction of control firms. We find a similar effect for the public release event in 2012. Note, while the reductions seem large at first, they represent changes during a decade (our sample ranges from 2007-2016). Therefore, our results are in line with Downar et al. (2019) who report a facility-based carbon emission reduction of 18% over three years for UK firms around the introduction of a mandatory reporting regime. While Tomar (2019) shows that reporting facilities reduce their absolute emissions in response to the GHGRP introduction, our results suggest that these absolute reductions do not directly translate into a reduction of the entire firms' carbon emissions. This could mean that firms' operations, which were directly affected, continued to expand outside the regulatory boundaries of the GHGRP.

Results for absolute carbon emissions do not seem to match results from our base analysis, which focuses on carbon emission intensity. A potential reason is that the period from 2010 to 2016 saw a recovery after the financial crisis. Therefore, firms expanded their operations. For high emitting firms, this expansion would have led to a massive increase in direct carbon emissions. However, carbon reduction initiatives were still effective in lowering carbon emission intensity. Indeed, firms affected by the GHGRP saw an average increase in their total assets as well as PPE net worth of about 13% and 11% respectively between 2010 and 2016.

We also perform a number of additional robustness tests. First, we apply a fixed effects regression model with firm-clustered standard errors, which helps control for omitted variable bias. Firm fixed effects allow us to control for unobserved firm-specific characteristics, while year fixed effects control for overall time series effects like additional confounding events. The fixed effects models confirm our initial hypothesis tests. Second, we test a specification of our base model without control variables to see if our results hold. Third, we test our base model using the Huber-White sandwich estimators, which are robust to heteroscedasticity (White, 1980). Fourth, we also use both, metric tons of scope 1 emissions and scope 1 intensity, calculated as the ratio of scope 1 emissions to PPE, as alternative dependent variables for our base model. Lastly, we limit our sample period to the years directly surrounding the introduction of the GHGRP in 2010 (sample limited to 2008-2012) and the publication of the GHGRP data in 2012 (sample limited to 2010-2014). All of these tests confirm our initial results.

5. Conclusion

We analyzed whether the introduction of a mandatory climate reporting regime leads to improvements in firms' carbon performance, measured by carbon emission intensity. Our setting is the US GHGRP, which was introduced in 2010 and started publishing collected carbon emission data in 2012.

We find that the carbon performance of US firms regulated by the GHGRP improved significantly more than the carbon performance of unregulated US firms. This observation holds for both considered events, the introduction of the GHGRP and the first-time publication of carbon emission data. However, at the same time, regulated US firms reduced their absolute carbon emissions less than unregulated US firms. This indicates that regulated US firms improved the effectiveness of their operations (as captures by carbon emission intensity), while at the same time increasing (decreasing) the amount of operations more (less) than unregulated US firms. Overall, our results suggest that a mandatory climate reporting regime positively influences firms' carbon management, but further regulatory efforts to achieve meaningful carbon emission reductions are necessary.

Our study and its results contribute to literature on climate reporting. First, we add to research focusing mainly on voluntary climate reporting, which identifies either no or limited effects on firms' carbon performance (Belkhir et al., 2017; Haque & Ntim, 2018; Qian & Schaltegger, 2017). We show that a move towards mandatory climate reporting does not only matter on a facility level (Downar et al., 2019; Tomar, 2019) but also on firm level, represented by a firm's global scope 1 emissions. Second and contrary to Tomar (2019), we show that both events, the introduction of GHGRP in 2010 and the public release of this information in 2012, impact firm level carbon performance. Additionally, we add to the literature about policy designs targeting a low carbon economy (e.g., Bel & Joseph, 2015; Haites, 2018; Murray & Maniloff, 2015). We show that mandatory climate reporting contributes directly to increased carbon performance and, therefore, deserves political consideration. Of course, the importance of a mandatory climate reporting regime can be further underscored by the argument that such a regime also enables researchers to thoroughly analyze the effectiveness of other firm-related carbon reduction policies.

Some limitations of our research design are worth mentioning because they emphasize potential for further research and they emphasize careful interpretation of our results. The nature of a mandatory climate reporting regime is to target high emitting facilities. Thereby, our sample is not based on a random assignment of firms to the treatment and control groups. That means, as in many real-world cases, we cannot create a perfect experimental setting. However, we address this issue through a number of control variables, the propensity score matching, and additional analyses to the best extent possible. With a general tendency towards more climate reporting, for example as part of Canada's COVID-19 Economic Response Plan (Government of Canada, 2020), there are more opportunities to analyze the effectiveness of mandatory climate reporting regimes in different institutional contexts. In addition, due to the nature of our setting, we do not have information about firms which do not voluntarily report carbon emissions previous to the GHGRP. We rely on estimates from Trucost for these firms. Therefore, if Trucost's estimation process is systematically flawed, this would impact our results. Additionally, our study does not analyze the concrete mechanism behind the carbon reduction of firms. For example, capital market pressures might play a role, because previous literature reports a negative firm value effect of carbon emissions (Matsumura et al. 2014; Griffin et al. 2017). Further research can analyze whether mandatory climate reporting regimes impact the negative firm value effect of carbon emissions.

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Table 1. Industry composition of samples

	Number of unique firms	Full sample	Regulated firms sample	Unregulated firms sample
Oil & Gas	100	8.33 (633)	23.63 (447)	3.56 (216)
Basic Materials	88	6.38 (508)	16.81 (318)	3.13 (190)
Industrials	360	25.10 (1,998)	16.97 (321)	27.63 (1,677)
Consumer Goods	175	11.96 (952)	13.85 (262)	11.37 (690)
Health Care	213	11.37 (905)	3.49 (66)	13.82 (839)
Consumer Services	240	17.55 (1,397)	2.06 (39)	22.38 (1,358)
Telecommunications	18	1.32 (105)	0.53 (10)	1.57 (95)
Utilities	61	5.53 (440)	16.81 (318)	2.01 (122)
Technology	199	12.47 (993)	5.87 (111)	14.53 (822)
Sum	1,454	100 (7,961)	100 (1,892)	100 (6,069)

Table 1 presents the industry composition as a percent of the full sample, the regulated firms sample and the unregulated firms sample. The number of firm-year observations are in parentheses.

Table 2. Descriptive statistics and correlations.

Panel 1

Full sample

Variable	N	Mean	Std. Dev.	p5	p50	p95
log Scope 1	7,961	11.437	2.491	7.651	11.212	16.224
Scope 1 Intensity	7,961	0.139	0.384	0.001	0.020	0.821
PPE Intensity	7,961	0.295	0.249	0.028	0.208	0.801
Total Assets	7,961	13,735,131	32,869,891	370,735	4,091,400	52,985,400
Leverage	7,961	0.791	1.703	0.000	0.467	3.390
ROA	7,961	0.042	0.113	-0.127	0.051	0.169

Regulated firms sample

Variable	N	Mean	Std. Dev.	p5	p50	p95
log Scope 1	1,892	13.875	2.118	10.600	13.783	17.557
Scope 1 Intensity	1,892	0.361	0.575	0.006	0.106	1.594
PPE Intensity	1,892	0.487	0.249	0.108	0.484	0.877
Total Assets	1,892	26,675,197	52,545,566	1,060,400	9,945,246	109,892,000
Leverage	1,892	1.037	1.642	0.000	0.699	3.455
ROA	1,892	0.030	0.112	-0.124	0.038	0.143

Unregulated firms sample

Variable	N	Mean	Std. Dev.	p5	p50	p95
log Scope 1	6,069	10.677	2.076	7.386	10.600	14.220
Scope 1 Intensity	6,069	0.070	0.265	0.001	0.015	0.220
PPE Intensity	6,069	0.235	0.217	0.022	0.156	0.739
Total Assets	6,069	9,701,089	22,099,473	307,821	3,117,600	40,865,000
Leverage	6,069	0.714	1.715	0.000	0.397	3.352
ROA	6,069	0.046	0.113	-0.129	0.055	0.174

Panel 2

Correlation coefficients

Variable	*log Scope 1*	*Scope 1 Intensity*	*PPE Intensity*	*Total Assets*	*Leverage*	*ROA*
log Scope 1	1					
Scope 1 Intensity	0.574*	1				
PPE Intensity	0.532*	0.357*	1			
Total Assets	0.354*	0.048*	0.064*	1		
Leverage	0.112*	0.045*	0.123*	0.057*	1	
ROA	0.102*	-0.036*	-0.072*	0.050*	-0.076*	1

Panel 3

Year 2010

PSM sample of regulated firms

Variable	N	Mean	Std. Dev.	p5	p50	p95
log Scope 1	1,544	14.052	1.983	10.811	13.956	17.383
Scope 1 Intensity	1,544	0.364	0.589	0.006	0.108	1.603
PPE Intensity	1,544	0.479	0.251	0.106	0.470	0.878
Total Assets	1,544	26,963,177	49,475,745	1,581,386	12,592,202	86,814,000
Leverage	1,544	1.019	1.506	0.002	0.716	3.208
ROA	1,544	0.032	0.108	-0.115	0.040	0.140

PSM sample of unregulated firms

Variable	N	Mean	Std. Dev.	p5	p50	p95
log Scope 1	767	13.163	2.064	9.895	12.999	16.530
Scope 1 Intensity	767	0.259	0.557	0.002	0.043	1.513
PPE Intensity	767	0.409	0.267	0.038	0.352	0.869
Total Assets	767	23,994,116	39,074,517	1,862,746	9,328,500	89,724,000
Leverage	767	0.846	1.611	0.000	0.552	3.813
ROA	767	0.050	0.073	-0.050	0.053	0.140

Year 2012

PSM sample of regulated firms

Variable	N	Mean	Std. Dev.	p5	p50	p95
log Scope 1	1,551	14.042	1.957	10.822	13.951	17.373
Scope 1 Intensity	1,551	0.334	0.534	0.006	0.105	1.488
PPE Intensity	1,551	0.476	0.253	0.104	0.463	0.881
Total Assets	1,551	27,793,156	50,106,108	1,671,408	12,787,000	93,837,000
Leverage	1,551	1.029	1.572	0.001	0.697	3.267
ROA	1,551	0.034	0.108	-0.115	0.041	0.142

PSM sample of unregulated firms

Variable	N	Mean	Std. Dev.	p5	p50	p95
log Scope 1	823	13.182	1.837	10.615	12.879	16.569
Scope 1 Intensity	823	0.241	0.546	0.003	0.037	1.489
PPE Intensity	823	0.411	0.263	0.072	0.356	0.869
Total Assets	823	24,559,308	37,389,438	19,48,675	9,558,300	89,507,000
Leverage	823	1.072	2.102	0.000	0.596	5.571
ROA	823	0.058	0.066	-0.036	0.056	0.156

Panel 1 of Table 2 contains the descriptive statistics of the base sample as well as the sub-samples of only regulated firms and only unregulated firms. Note that *Total Assets* is denominated in 1,000 US dollars.
Panel 2 of Table 2 presents the Pearson correlation coefficients for the full sample (N=7,961) with * denoting a significance level of $p<0.05$.
Panel 3 contains the descriptive statistics of the PSM samples, split into regulated and unregulated firms, for the treatment years 2010 and 2012. Note that *Total Assets* is denominated in 1,000 US dollars.

Table 3. Regression results of the base analysis using the full sample and the PSM sample.

Treatment year	Full sample				PSM sample			
	2010		**2012**		**2010**		**2012**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post*Treat	**-0.079*****		**-0.080*****		**-0.050***		**-0.007**	
	(0.028)		**(0.029)**		**(0.027)**		**(0.035)**	
Year-1*Treat		**0.050**		**0.009**		**0.0505**		**-0.010**
		(0.034)		**(0.034)**		**(0.085)**		**(0.085)**
Year+1*Treat		**0.018**		**-0.032**		**0.016**		**-0.004**
		(0.034)		**(0.028)**		**(0.086)**		**(0.085)**
Post1	-0.017*				-0.042**			
	(0.009)				(0.020)			
Post2			-0.015				-0.091***	
			(0.009)				(0.029)	
Treat	0.248***	0.185***	0.235***	0.194***	0.139	0.098***	0.067	0.065**
	(0.054)	(0.011)	(0.051)	(0.012)	(0.089)	(0.029)	(0.089)	(0.029)
Year-1		0.004		0.003		-0.003		0.016
		(0.018)		(0.018)		(0.070)		(0.069)
Year+1		0.004		-0.003		-0.006		-0.036
		(0.018)		(0.013)		(0.070)		(0.069)
PPE Intensity	0.411***	0.411***	0.411***	0.411***				
	(0.060)	(0.018)	(0.060)	(0.018)				
Total Assets	-0.000	-0.000	-0.000	-0.000				
	(0.000)	(0.000)	(0.000)	(0.000)				
Leverage	-0.001	-0.001	-0.001	-0.001				
	(0.005)	(0.002)	(0.005)	(0.002)				
ROA	-0.019	-0.014	-0.031	-0.012				
	(0.046)	(0.035)	(0.046)	(0.035)				
Constant	-0.010	-0.024***	-0.013	-0.023***	0.289***	0.260***	0.347***	0.303***
	(0.013)	(0.007)	(0.013)	(0.007)	(0.070)	(0.023)	(0.073)	(0.023)
Observations	7,961	7,961	7,961	7,961	2,311	2,311	2,386	2,386
R-squared	0.166	0.165	0.166	0.163	0.011	0.008	0.009	0.003
Firms	1,454	1,454	1,454	1,454	235	235	244	244
Cluster	Firm	-	Firm	-	Firm	-	Firm	-

Table 3 presents the regression results of our base analysis with *Scope 1 Intensity* as the dependent variable in all the regression models. Columns (1)-(4) contain regression models based on the full sample and columns (5)-(8) contain regression models based on the PSM samples.
Standard errors are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table 4. Regression results of the comparison between the US and the EU and the US and the rest of the world.

	US/EU		**US/Rest of the world**	
	(1)	(2)	(3)	(4)
Post1*Treat	**-0.141*** **		**-0.062***	
	(0.041)		**(0.033)**	
Year-1*Treat		**0.118*** **		**0.069*** **
		(0.030)		**(0.027)**
Year+1*Treat		**-0.037**		**0.015**
		(0.040)		**(0.025)**
Post1	0.047		-0.041**	
	(0.031)		(0.019)	
Treat	-0.022	-0.167**	-0.202***	-0.256***
	(0.080)	(0.077)	(0.060)	(0.047)
Year-1		-0.062***		0.000
		(0.022)		(0.016)
Year+1		0.047		-0.018
		(0.036)		(0.015)
ETS Allowance Price	0.001	-0.002		
	(0.002)	(0.002)		
PPE Intensity	0.853***	0.850***	1.222***	1.221***
	(0.138)	(0.137)	(0.106)	(0.106)
Total Assets	-0.000*	-0.000*	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Leverage	-0.010	-0.011	0.024*	0.023*
	(0.015)	(0.015)	(0.013)	(0.013)
ROA	0.479***	0.490***	0.913***	0.935***
	(0.175)	(0.177)	(0.215)	(0.217)
Constant	0.048	0.118*	0.012	-0.016
	(0.068)	(0.066)	(0.041)	(0.039)
Observations	2,908	2,908	7,710	7,710
R-squared	0.111	0.108	0.144	0.143
Firms	371	371	1,096	1,096
Cluster	Firm	Firm	Firm	Firm

Table 4 presents the regression results of the comparison US/EU and US/Rest of the world with *Scope 1 Intensity* as the dependent variable and the treatment year 2010 (*Post1*) in all regression models. In the US-EU comparison, *Treat* identifies firms that fall under the GHGRP or the EU ETS regulation. In the US-Rest of the World comparison, *Treat* identifies US firms that fall under the GHGRP and the worst emitters in certain high-emission industries in the rest of the world. Column (1) shows the results of the comparison between US and EU firms and column (3) shows the results of the comparison between US firms and firms in the rest of the world, excluding the EU. Columns (2) and (4) contain the respective parallel trend tests.
Standard errors are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table 5. Regression results for absolute emissions.

Treatment year	**2010**		**2012**	
	(1)	(2)	(3)	(4)
Post*Treat	**0.252*****		**0.270****	
	(0.097)		**(0.107)**	
Year-1*Treat		**-0.179**		**-0.147**
		(0.173)		**(0.172)**
Year+1*Treat		**-0.150**		**-0.054**
		(0.171)		**(0.142)**
Post1	-0.567***			
	(0.054)			
Post2			-0.613***	
			(0.056)	
Treat	1.753***	1.982***	1.768***	1.978***
	(0.174)	(0.057)	(0.173)	(0.058)
Year-1		0.426***		0.270***
		(0.091)		(0.090)
Year+1		0.338***		-0.247***
		(0.090)		(0.067)
PPE Intensity	3.733***	3.752***	3.715***	3.763***
	(0.262)	(0.089)	(0.261)	(0.089)
Total Assets	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Leverage	0.054**	0.053***	0.056**	0.051***
	(0.022)	(0.012)	(0.022)	(0.012)
ROA	3.026***	3.058***	2.863***	3.074***
	(0.299)	(0.178)	(0.298)	(0.179)
Constant	9.896***	9.386***	9.859***	9.451***
	(0.096)	(0.035)	(0.093)	(0.035)
Observations	7,961	7,961	7,961	7,961
R-squared	0.496	0.496	0.501	0.491
Firms	1,454	1,454	1,454	1,454
Cluster	Firm	-	Firm	-

Table 5 presents the regression results of our base model with *log Scope 1* as the dependent variable. Column (1) contains the results for 2010 as the treatment year and column (3) contains the results for 2012. Columns (2) and (4) present the respective parallel trend tests.
Standard errors are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.