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The impacts of the EU ETS on Norwegian plants' environmental and economic performance

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Abstract

This paper examines the impacts of the EU Emissions Trading System (ETS) on the environmental and economic performance of Norwegian plants. The ETS is regarded as the cornerstone climate policy in the EU and Norway, but there has been considerable debate regarding its effects due to low quota prices and substantial allocation of free allowances. The rich data allow us to investigate potential effects of the ETS on several important aspects of plant behavior. The results indicate a weak tendency of emissions reductions among Norwegian plants in the second phase of the ETS, but not in the other phases. We find no significant effects on emissions intensity in any of the phases, but positive effects on value added and productivity in the second phase. These positive effects may be due to the large amounts of free allowances, and that plants may have passed on additional marginal costs to consumers.

Keywords: Tradable emissions quotas, emissions intensity, productivity, propensity score matching, difference-in-differences

JEL codes: C23, C54, D22, Q54, Q58.

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1 Introduction

Since the establishment of the EU Emissions Trading System (ETS) in 2005, emissions trading has been the cornerstone policy instrument to reduce greenhouse gas (GHG) emissions in Europe. The aim of this paper is to investigate how the ETS has affected the environmental and economic performance of Norwegian manufacturing plants. In particular, we are interested in whether plants regulated by the ETS have reduced their emissions as a result of the regulation. Emissions reductions can take place by scaling down production and/or reducing emissions per output, and we also examine the effects on emissions per output, referred to as emissions intensity. A positive price on emissions allowances (quotas) should provide incentives to cut back on emissions. However, the price of allowances has periodically been low, and manufacturing plants have received most of the allowances they need for free.

We also estimate the effects of the ETS on plants' value added and productivity. Although environmental regulation puts constraints on plants, suggesting a negative impact, the Porter Hypothesis (Porter and Van der Linde, 1995) suggests that environmental regulation can increase plants' productivity and competitiveness as it provides incentives to innovate. When it comes to the ETS, the extent of free allocation also matters: If plants receive most of their allowances for free, and are able to pass on most of the marginal cost increase to consumers, they may be better off than without the ETS. The European Commission (2015) finds that a significant share of the emissions price is passed on to consumers for a number of products regulated by the ETS.

Martin et al. (2016) sum up the empirical evidence for the ETS so far, both regarding emissions and firms' performance. Some studies use aggregate data, and typically find emission reductions in the range 2-5 percent in phase I and/or II (see e.g. Ellerman and Buchner (2008), Egenhofer et al. (2011), Anderson and Di Maria (2011) and Bel and Joseph (2015)).

There are relatively few studies of the ETS using firm or plant level data. We are aware of only five such studies estimating the effects on emissions. Abrell et al. (2011) use plant-level data for firms in different EU/EEA countries for the years 2005-08, finding a significant reduction in emission growth when shifting from the first to the second phase (i.e., from 2007 to 2008). Wagner et al. (2014) use plant-level data for France to estimate the effects of the two first phases of the ETS. They find evidence of significant emissions reductions in phase II, as well as indications of emissions reductions in phase I. On average emissions were reduced by 15-20 percent. Similarly, Petrick and Wagner (2014) use plant-level data for German manufacturing firms for the years 2005-10, and find evidence of emissions reductions in the second phase: Emissions were reduced by on average one fifth. Jaraite and Di Maria (2016) also consider the years 2005-10, using plant-level data for Lithuania, finding no reductions in emissions, but a slight improvement in emissions intensity in 2006-7. Last but not least, Dechezleprêtre et al. (2018) use plant-level data for four EU/EEA countries (France, Netherlands, Norway and the UK), finding significant emission reductions in the order of 10 percent between 2005 and 2012. There also exist studies on other emissions trading systems using micro-data, such as Fowlie et al. (2012) who investigate effects of the Southern California's NO_x Trading Program (RECLAIM). The above mentioned studies exploit that only a subset of plants or firms were selected for program participation and identify the closest match among the plants or firms not selected for participation.[1]

When it comes to micro studies on economic performance, neither Abrell et al. (2011), Wagner et al. (2014), Jaraite and Di Maria (2016) nor Dechezleprêtre et al. (2018) find significant impacts of the ETS. The same applies to Anger and Oberndorfer (2008), who estimate the effects on revenues of German firms in 2005, and Chan et al. (2013) who inter alia examine impacts on material cost and revenue in the cement and iron&steel sectors in ten EU countries over the years 2001-09. Commins et al. (2011), however, find negative impacts on both value added and productivity of the first phase of the ETS on firms across the EU. On the other hand, Marin et al. (2018) find a positive impact on value added for manufacturing firms across the ETS in the second phase (2008-12). Löschel et al. (2019)

[1] Martin et al. (2014) use micro-data to analyze the impacts of the UK carbon tax, finding strong negative effects on energy intensity and use of electricity at manufacturing plants.

find positive impacts on economic performance, measured as the distance to the stochastic production frontier, for German manufacturing plants (especially during the first phase). There are also a number of studies using micro-data to examine the effects of the ETS on employment (e.g., Wagner et al. (2014) and Petrick and Wagner (2014)) or environmental innovations (e.g., Borghesi et al. (2015), Löfgren et al. (2013), Calel and Dechezlepretre (2016) and Calel (2018)).

We contribute to the existing literature in three ways. First, as already indicated there are few econometric studies of the ETS using micro-data, especially when it comes to effects on emissions. Decisions regarding emissions reductions take place at the plant level, and quotas have been allocated to individual plants based on their historic activity (emissions or output) or planned capacity. Thus, studies of the impacts of the ETS should ideally be carried out at the plant level, which we do using Norwegian data. Second, our specification allows us to compare the effects of the different phases. This is important as allocation rules and quota prices have differed much between phases. None of the cited studies have examined impacts beyond the second phase, and several have only examined the first and/or part of the second phase. Third, our rich data set allows us to control for plant heterogeneity through a number of control variables. For instance, we indirectly control for carbon taxes on fossil fuels combustion, using plant specific data on relative energy prices ("dirty" vs "clean").

Our paper also relates to the large theoretical literature on emissions trading and quota allocation (e.g., the seminal paper by Montgomery (1972)). Allocation of allowances in the ETS has to some degree been conditioned on plants' activity level, and hence may have influenced plants' decisions.[2] In the third phase beginning in 2013, allocation has shifted towards output-based allocation. As shown by Rosendahl and Storrøsten (2015), this gives firms more incentives to reduce emissions intensities than auctioning (or lump sum allocation). On the other hand, it is also possible that foresighted firms correctly anticipated

[2]This is to some degree intentional, as policy makers in Europe do not want firms to simply relocate to other jurisdictions with lax climate policies. See the substantial literature on carbon leakage, e.g., Martin et al. (2014), Böhringer et al. (2014).

that allocation of allowances would be based on their historic emissions a few years before the ETS was implemented, giving incentives to *increase* emissions before 2005.[3]

In order to identify the causal effects of the ETS, we exploit that only a subset of the plants were selected for participation. Other plants, at least in the manufacturing industries which we focus on, were mainly left unregulated with respect to GHG emissions, or have been paying a carbon tax (see Section 3.2). We use matching methods based on the program participation selection criteria to identify a comparable control group of plants that were not selected for program participation. Then we use difference-in-differences, and as an alternative, a fixed effects model, to investigate the effects of the ETS while controlling for a number of other important variables.

Our results indicate weak evidence of emissions reductions among Norwegian plants in the second phase of the ETS, but no significant effects of the two other phases. Moreover, we find no significant effects on emissions intensity of any of the three phases. Further, we identify positive effects of the second phase on both value added and productivity.

The rest of the paper is organized as follows. In Section 2 we present some background information on the ETS. Section 3 contains a description of the data and of the variables used in the empirical analysis. The econometric model and the results are presented in Section 4. Finally, Section 5 concludes and suggests some policy implications.

2 The Norwegian and the EU Emissions Trading System

The EU ETS regulates GHG emissions from energy production and some large manufacturing industries (see Ellerman et al. (2016) for a recent overview). Norway is not a member of the European Union, but has since 2008 participated in the EU ETS through the EEA (European Economic Area) Agreement between the EU and the EFTA countries. In this

[3] In the first two phases, allowances to Norwegian plants were grandfathered based on their emissions in 1998-2001. For EU countries, the base years differed somewhat. For several EU countries, the base years for allocation in the second phase included 2005, i.e., the first year of the first phase (Hintermann, 2010). The effects of different allocation rules have been studied analytically and numerically by e.g. Böhringer and Lange (2005), Rosendahl (2008) and Golombek et al. (2013).

section we give a brief description of the main elements of the three phases of the ETS, which are important when discussing the empirical results.

In the first "pilot" phase of the ETS (2005-07), around 40 percent of EU's CO_2 emissions were regulated (cf. EU's quota directive 2003/87/EF). Almost all allowances were allocated for free, mostly based on plants' historic emissions ("grandfathering"). The allowance price was initially high (see Figure 2), but plummeted towards zero in 2007 as it became clear that total allocation of allowances exceeded total emissions during this period.

In the first phase, Norway had an ETS that was not formally linked to the EU ETS. However, the Norwegian authorities accepted EUAs (i.e., EU ETS allowances) in its own ETS. Thus, Norwegian plants could buy allowances from EU plants, but not vice versa. Trade was very limited, however, accounting for only about 0.1 percent of total emissions by Norwegian ETS plants. As Norway introduced CO_2 taxes in many sectors of its economy in the 1990's, several industries were exempted from the ETS in the first phase although corresponding industries in the EU were regulated by the EU ETS. Merely 10 percent of Norwegian CO_2 emissions, mostly from the processing industries, were regulated by the ETS in this phase. Allocation of allowances was based on plants' emissions in the years 1998-2001. The very limited purchase of EUAs by Norwegian plants may suggest that the overall allocation was quite generous; this is confirmed by the fact that total allocation to Norwegian plants in the first phase exceeded total emissions by 8 percent. It is therefore relevant to ask whether Norwegian plants were facing a positive emissions price at all during phase I. At least the EUA price seems to have played a minor role for these plants, given the negligible trade in allowances between Norwegian and EU plants.[4]

In the second phase (2008-12) there were few changes in coverage and allocation rules in the EU ETS. Again the EUA price started high, but following the financial crisis and subsequent recession, the price dropped to moderate levels for the rest of phase II.

[4]According to the registry of the Norwegian Environment Agency, total trade in allowances between Norwegian plants during phase I amounted to around 2.5 percent of total regulated emissions. Almost 90 percent of this trade took place after the EU ETS price fell and then stayed below 1 Euro per ton in the spring of 2007.

From 2008 Norwegian plants were fully integrated into EU ETS, and Norway could no longer exempt industries from the ETS. Nitrous oxide (N_2O) emissions from production of nitric acid in Norway were opted in. Thus, the share of Norwegian GHG emissions regulated by the ETS increased to around 45 percent. The allocation was still based on emissions in the years 1998-2001, but plants with increased production and emissions since the base years received additional allowances for free.

In the third phase (2013-2020) additional industries and gases, such as perfluorocarbons ($PFCs$) from aluminium production, have been included. Around 50 (40) percent of the EU's CO_2 (GHG) emissions are now regulated by the ETS. The allocation rules have been harmonized across member states. Manufacturing industries still get large amounts of allowances, especially if they are categorized as significantly exposed to carbon leakage, with the allocation mainly based on plants' output in 2007-08. The EUA price was initially low (below 10 Euro per ton until 2018), partly because of the continued recession and partly because a large share of allowances was banked from the second to the third phase.

The EU harmonization in phase III also applies to Norway. For the Norwegian manufacturing indutries, the extent of free allowances has not changed much, but the allocation rule has shifted in line with changes in the EU ETS. Total CO_2 emissions from Norwegian manufacturing plants regulated by the EU ETS in 2013 have shown little variation during the estimation period, and were in 2013 2.6 percent below the level in 2004, but 1.8 percent above the level in 2001 (see Figure A.1 in the Appendix). The highest level was observed in 2010, shortly after the financial crisis. Emissions of N_2O, which were regulated by the ETS from the second phase, declined substantially from 2005 to 2009, whereas emissions of $PFCs$, which were regulated from the third phase, declined significantly from 2008 to 2010. As a consequence, total GHG emissions from the regulated plants have declined notably since the ETS was established in 2005, but at least for some plants the emissions reductions took place before they became regulated by the ETS.

Figure 1 illustrates the trend in yearly mean EUA prices and the annual mean emissions

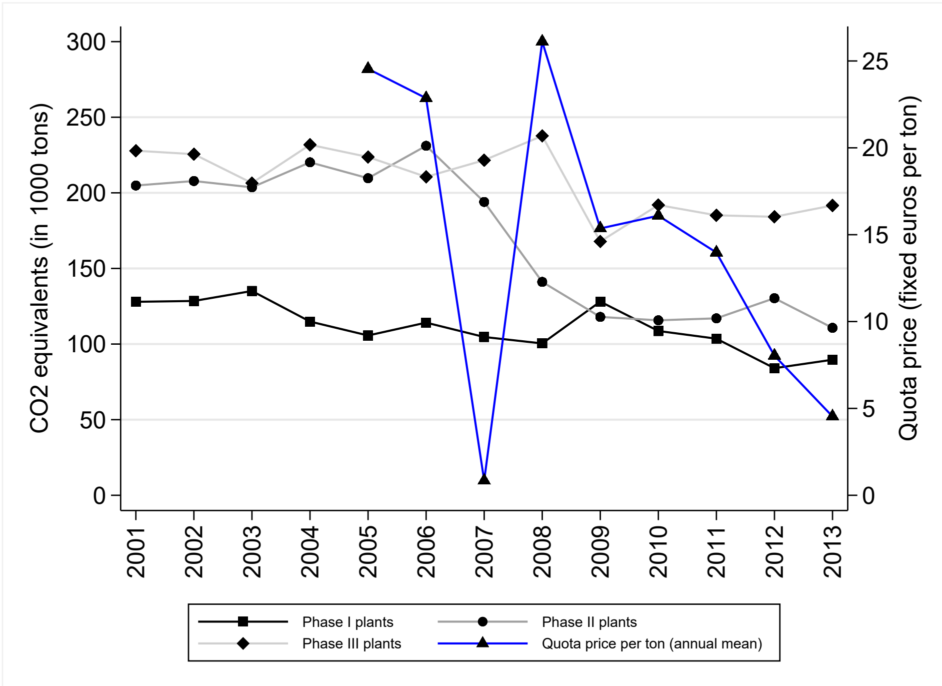

Figure 1: Annual mean emissions per plant of CO_2, N_2O, and $PFCs$ (in 1000 tons of CO_2-equivalents) of ETS regulated plants in the manufacturing industries (left hand axis) and real (deflated to 2013) ETS quota prices (right hand axis)

per plant. The emissions curves are phase specific, so that for instance the curve "Phase II plants" shows how emissions (on average) have developed over time for plants that were regulated from phase II and onwards. The figure indicates a small reduction in mean plant emissions for phase I plants from 2005 and for phase II plants from 2008, but emissions were on average declining also the year before phase I and phase II plants became regulated. In order to examine the effects of the regulation, we have to identify a relevant comparison group and also account for the variation in other variables than the ETS regulation.

3 Data sources and description of variables

We have constructed a plant-level panel data set that draws on several data sets from different sources. All data sets are merged using organizational number of the subsidiary as the plant identifier. The data span 13 years, from 2001 to 2013. A key data set comprises the data from the Norwegian Environment Agency (NEA) on annual emissions of all Norwegian plants regulated by the Norwegian ETS or the Norwegian Pollution Control Act, including emissions of CO_2, N_2O and $PFCs$ (measured in CO_2 equivalents).[5] This data set allows us to identify whether the plant is regulated by the ETS or not, and in which phase they enter.

The data are supplemented with annual plant level data containing information on number of employees, man hours, value added, energy use and prices, and industry affiliation. The data originate from different registers at Statistics Norway: Data on energy use and data on structural business statistics for manufacturing, mining and quarrying. The data set thus covers the industries B-C in the Standard Industrial Classification (SIC2007). A detailed description of the key variables is provided below, grouped into two main categories: Dependent variables and control variables, including other relevant GHG regulations.

3.1 Environmental and economic performance

We study the effects of the ETS on several dependent variables: Emissions, emissions intensity, labor productivity and value added. Our main measure of a emissions includes CO_2, N_2O and PFC emissions, but we also consider CO_2 emissions only.

Ideally, emissions intensity should be calculated as emissions relative to output produced (e.g., emissions per ton of steel or per ton of cement). However, as the type of output differs across plants and industries, it is challenging to compare output quantities across plants. Moreover, we do not have data for the quantities produced, only the value of production. Emissions intensity calculated as emissions relative to production value would be sensitive to changes in the output price. A common measure of emissions intensity is therefore emissions relative to the number of employees (see e.g. Wagner et al., 2014). As such a measure does not take into account that some employees have part-time positions, are on sick leave, work extra hours, etc., we instead calculate emissions intensity as emissions relative to man hours.

[5]According to the Norwegian Pollution Control Act, pollution is in general prohibited, but plants can apply for pollution permits. The emissions data are publicly available on the Norwegian Environment Agency's website.

As a plant could change its labor intensity during our estimation period, in Section 4.3 we also consider an alternative measure of emissions intensity, calculated as emissions relative to electricity use (measured in kWh per year). However, as the ETS should give incentives to switch between different energy goods, such as replacing coal or oil with electricity, our preferred measure is emissions relative to man hours.

Value added at factor prices is the plant's annual gross production value minus the cost of intermediates plus subsidies and minus taxes (except VAT). Production value is defined as turnover corrected for changes in stock of finished goods, work in progress and goods and services bought for resale. Cost of intermediates is the value of goods and services used as input in the production process, excluding fixed assets. Our measure of value added is an official measure taken from Statistics Norway.[6] The value added in NOK is deflated using the Producer Price Index (PPI) with 2013 as the base year.

Productivity should be measured as output produced relative to the use of input. Again, good measures of output is challenging to obtain as plants produce different types of goods, and we only have data on production value, not quantities produced. Despite this shortcoming, we use the value added at factor prices as a proxy for output. This measure has the advantage that it is comparable across plants. Further, we use man hours as a proxy for input, so that by plant productivity we mean labor productivity, i.e., value added at factor prices per man hour. Note that our measure of productivity should not be mixed with efficiency – since productivity is calculated as value added divided by man hours, the extent of free allocation to the firms will also affect productivity even if it does not have any influence on a firm's operation or efficiency.

3.2 Control variables

Contrary to studies at the industry level, we are able to take into account plant heterogeneity in our analysis, and thereby reduce the problem of omitted variable bias. This relates both

[6] A more detailed description of the measures is available at the homepage of Statistics Norway.

to plant characteristics, and to external factors for the plant such as prices and taxes.

If prices of fossil fuels increase relative to prices of carbon-free energy, firms may have incentives to reduce CO_2 emissions independently of the ETS. Thus, we control for such price changes. We derive plant-specific prices of petroleum, coal, gas and electricity as the plant's expenses (including tax payments) on the respective energy good (in NOK) relative to the corresponding energy content (in kWh). Electricity can be characterized as approximately carbon-free in Norway.[7] Hence, the relative energy price at the plant level is calculated as the price of "dirty" energy (weighted petroleum, coal and gas prices) relative to the price of "clean" energy (electricity).

Until the ETS was implemented, the cornerstone of Norwegian climate policy was a non-uniform carbon tax implemented in 1991, with exemptions for many energy-intensive manufacturing industries. As mentioned earlier, emissions regulated by the carbon tax were exempted from the ETS in the first phase but not from the second phase (e.g., pulp and paper). As the carbon tax has only been implemented on the use of fossil fuels, we indirectly control for this tax through the plant-specific relative energy prices.[8]

In addition, there have been arrangements between the Ministry of Climate and Environment and the processing industry in Norway to reduce aggregate GHG emissions not covered by the ETS or the tax. These voluntary agreements covered e.g. N_2O emissions from the production of nitric acid and PFC emissions from aluminium production, which were later regulated by the ETS. One arrangement had a target for the year 2007, while the follow-up arrangement had a target for 2008-12. According to the Norwegian Ministry of Climate and Environment (2014, p. 98), reductions in N_2O emissions from the production of nitric acid, due to the use of a new technology, was sufficient to fulfill the first arrangement. Thus, it is

[7]There is no emissions from electricity use, and renewable power (mainly hydro power) accounts for more than 95 percent of Norwegian (onshore) electricity production in the estimation period. Note that prices of electricity have varied quite little over time during our estimation period, and less than prices of fossil fuels, cf. e.g. Figure 3 in Bye and Klemetsen (2016).

[8]As changes in the carbon tax show up in changes in the relative energy price, this means e.g. that the estimated effects of the ETS for plants that were initially regulated by the tax, at least in principle apply to the effects of the ETS as such, and not to the net effects of replacing the carbon tax with the ETS.

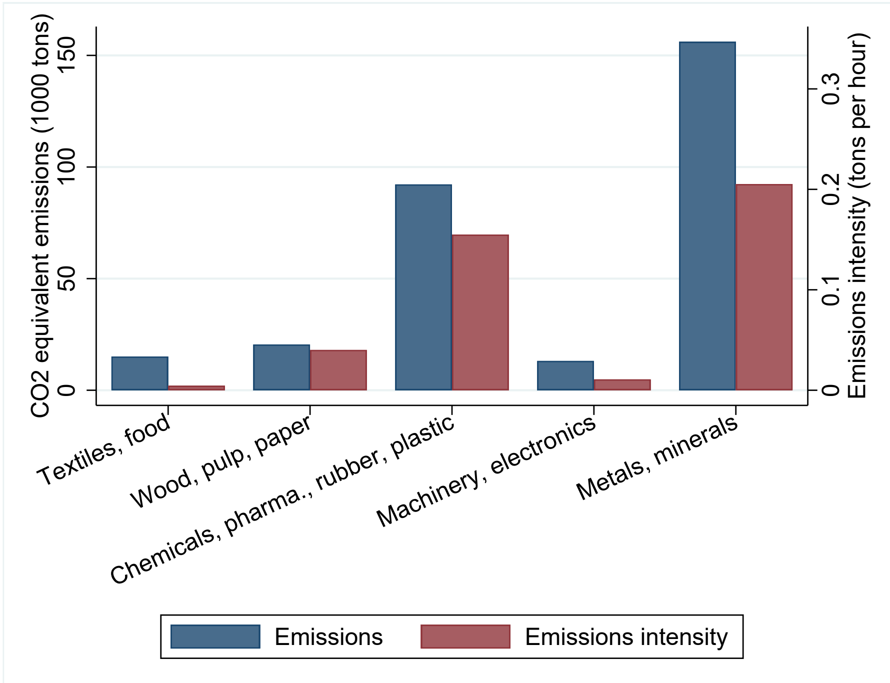

Figure 2: Mean plant emissions and emissions intensities (emissions per man hour) across aggregated manufacturing industries. CO_2, N_2O and $PFCs$ measured in CO_2 equivalents.

difficult to know whether these arrangements have had any influence on emissions, but since the arrangements applied to the whole industry, and not to specific firms, we do not have reason to believe that they affected the treatment group differently than the control group.

We seek to control for these arrangements, as well as other industry specific effects through the use of industry dummies. Figure 2 shows the plants' mean emissions and emissions intensity per aggregated manufacturing industry in the estimation period. We see that plants in Manufacturing of metals and minerals have the highest emissions and emissions intensities, followed by plants in Manufacturing of chemicals, pharmaceutics, rubber and plastic. When it comes to plant characteristics, we use the number of employees as a measure of plant size. Common trends in emissions are controlled for using phase dummies.

3.3 Sample summary statistics

Our initial sample of 665 Norwegian plants contains 4872 plant-year observations. Of these, 150 plants are regulated by the ETS at least one year. A small fraction are neither in manufacturing industries nor mining and extraction, and these are dropped (plant level data from Statistics Norway do not cover these industries). The control group is selected from the total population of plants emitting CO_2, N_2O or $PFCs$ (but not regulated by the ETS) using nearest neighbor propensity score matching (see Section 4). After matching, our data set consists of only manufacturing plants, i.e., there are no plants from the extraction industries such as oil and gas (Table A.2 in the Appendix shows share of plant-year observations across industries).[9] Our final unbalanced panel data set consists of 1,567 plant-year observations and 152 plants in the manufacturing industries, 72 of which are regulated by the ETS. In the initial data set, 100 of the regulated plants were manufacturing plants, which means that we keep slightly above 70 percent of these plants in our final data set.

Table 1 presents descriptive statistics for the matched sample, and demonstrates how the treatment and the control group differ with respect to observable variables. Table A.1 in the Appendix shows the corresponding figures before matching. The matching procedure reduces the differences between the treatment and control group substantially with respect to almost all variables (the exceptions are labor productivity and relative energy prices, where differences are quite small in any case), but the differences are still large, especially when it comes to emissions and emission intensities. The explanation is that only plants above a certain capacity limit are regulated by the ETS, cf. Section 4, combined with the fact that the Norwegian sample of similar plants acting as potential control plants is relatively small. Hence, there is a trade-off between heterogeneity and sample size. We use additional methods to reduce the selection issues further, such as taking into account the fixed effects through the Difference-in-differences model or a panel data model with plant

[9]The time paths of emissions and emissions intensities for oil and gas fields are highly influenced by the depletion of the fields' reservoir. See Gavenas et al. (2015) for a study of CO_2 emissions from Norwegian oil and gas fields.

Table 1: Summary statistics[1] after matching, 2001-2013

	Treatment group		Control group	
Variable	Mean	Median	Mean	Median
CO_2, N_2O and PFC emissions[1]	175,923	46,545	6,581	492
CO_2 emissions[1]	149,901	39,964	6,510	492
CO_2, N_2O and PFC emissions intensity[1]	.438	.205	.047	.002
CO_2 emissions intensity[1]	.399	.184	.046	.002
Labor productivity[2]	.57	.31	.42	.30
Number of employees	234.8	188	216.5	168
Relative energy prices ("dirty" over "clean")	1.06	.86	1.33	1.09
Value added[2]	228,832	112,443	105,149	66,663
Electricity use (kWh)	571,235	176,062	65,701	19,205
Man hours	387,927	293,730	301,543	231,761
Wages[2]	91,607	62,522	59,134	38,923
Operating profits[2]	103,752	46,253	68,069	41,252
Number of plant-year observations	743		824	
Numer of plants	72		80	

[1]All emissions are reported as tons of CO_2-equivalents
[2]All values in million NOK are deflated using the PPI with 2013 as base year.

fixed effects. The fixed effects model will pick up all observable and unobservable selection and heterogeneity issues to the degree that these are time-invariant. To the degree that the selection and heterogeneity varies over time, we rely on the plant level control variables.

Figure 3 illustrates the mean annual emissions intensities (index) for the matched sample of plants that operate during the entire estimation period, distinguishing between the three groups of treated plants and the control group. We see that plants included from phase I display increasing trends in emissions intensities until 2004, then decreasing in 2005, before increasing again in 2005-07, and then falling quite significantly from 2008. The decrease from 2007 to 2008 could be due to the high quota price in 2008, although we notice a decrease in emissions intensities for unregulated plants too. For plants included from phase II, emissions intensities appear to have decreased substantially from 2008 (when phase II started) and onwards. Plants included from phase III display a decreasing emissions intensity trend over most of the period, including 2013 (the year phase III was initiated), and this is also the case for plants which were never regulated by the ETS.

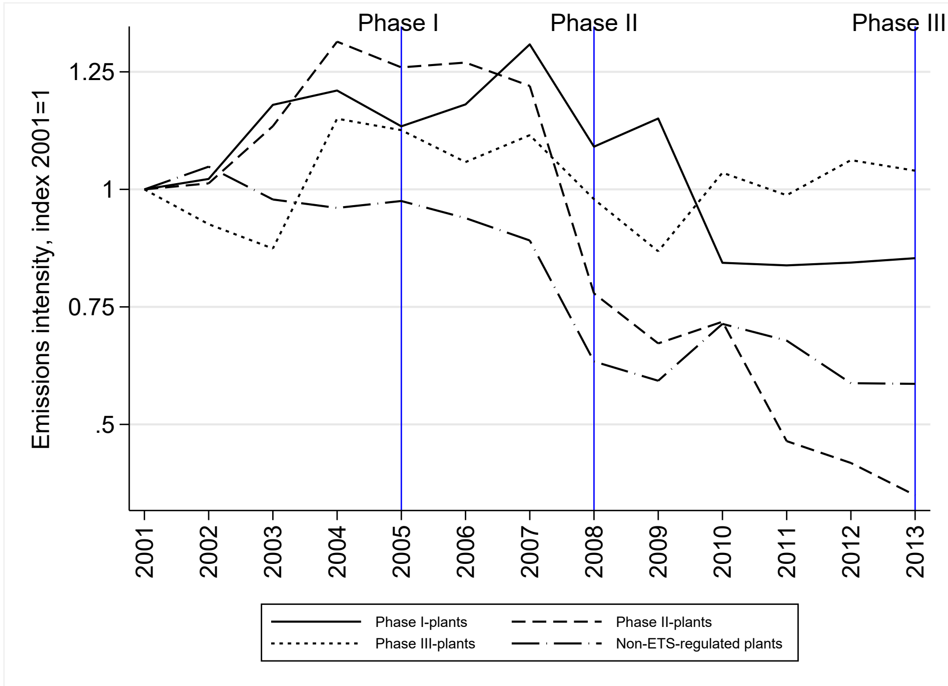

Figure 3: Mean annual emissions intensities for the matched sample of plants (CO_2 - equivalent emissions of CO_2, N_2O and $PFCs$ per man hour). Index: 2001=1

4 Empirical model and results

To investigate the effects of the ETS on Norwegian plants' environmental and economic performance, we exploit the fact that only a subset of the plants were selected for participation. The selection criteria is based on the type of pollutant, the plant activity (production of specific types of goods) and the capacity limit. We do not observe capacity limits for plants not regulated by the ETS, however.[10] For each plant regulated by the ETS we identify the closest matches among the plants not selected for participation in the ETS based on the propensity score.[11] The propensity score is the probability of receiving treatment conditional

[10]The capacity limit is specified as e.g. total thermal effect (typically 20 MW), or tons of products (steel, cement etc.) per hour or 24 hours. As the regulator selects plants for participation in the ETS based on the capacity limit, regression discontinuity constitutes as a suitable method for estimating the effects of the ETS (see e.g. Lee and Lemieux (2010)). However, the capacity limit varies with the main activity of the plant, and we do not have comparable data on the plants' activity in the control group. Also, there is a lot of missing values for the capacity measures. With an already small sample, it would thus not be manageable to use regression discontinuity methods based on the capacity limit. The participation requirements are found in Law on Greenhouse Gas Emissions ("Klimakvoteforskriften").

[11]The matching procedure used is the STATA routine *psmatch2* with 1-10 nearest neighbor matching. We perform a robustness test using 1-3 neighbors (see Section 4.3).

on some matching variables. The variables used are proxy measures of the participation requirements. In this way we identify a comparable control group of plants that were not selected for program participation. The probability of receiving treatment is conditional on the observed values in the year 2001 of the matching variables:[12] We require exact match on type of pollutant, and also on our proxy for plants' type of activity, i.e., the industry affiliation specified by standard industrial codes at the 2-digit level.[13] As continuous matching variables we include predetermined levels of emissions (as proxy for capacity limit) and number of employees (as a measure of plant size).

We calculate difference-in-differences, and as an alternative a fixed effects model, on the matched sample. The sample average treatment effect is estimated using dummy variables for each phase, indicating whether the plant participated in the ETS during this phase or not. We use the subscripts i, t, and p to denote plant, year and phase.

4.1 Basic difference-in-differences (DID)

For all four dependent variables, in general denoted Y, we estimate a basic DID. We define

$$E_{it} = \begin{cases} 1 & \text{if} \quad \text{plant } i \text{ is ETS-regulated in year } t \\ 0 & \text{if} \quad \text{plant } i \text{ is not ETS-regulated in year } t \end{cases}$$

Let T_i be the first year plant i is regulated by the ETS, and $\tau(p)$ the start-up year of phase p, respectively 2005, 2008, and 2013 for phase I, II and III. We specify our model in logarithmic form which means that we can interpret the estimates in terms of relative changes:

[12]The EU ETS was initiated in 2005, but was announced some years before (cf. Convery, 2009). In March 2000, a Green Paper on emissions trading was issued by the EU Commission, and hence the year 2000 can be seen as the announcement year of the EU ETS (cf. Wagner et al., 2014). In June 2001, the Norwegian government discussed through a White Paper a possible Norwegian ETS from 2005 (Norwegian Ministry of Environment, 2001). Nine months later, a new White Paper announced the start-up of the Norwegian ETS from 2005 (Norwegian Ministry of Environment, 2002). Hence, the plants' predetermined characteristics in 2001 are used as matching variables. An implication of this is that we do not allow entry of new plants after 2001 in our dataset.

[13]We perform a robustness test using the 3-digit level (see Section 4.3).

$$\log Y_{it} = \alpha_0 + \sum_{p\in\{1,2,3\}} \pi_p \, I\Big(\tau(p) \le t < \tau(p+1)\Big) + \sum_{p\in\{1,2,3\}} \gamma_p \, I\Big(\tau(p) \le T_i < \tau(p+1)\Big)$$
$$+\beta_p \sum_{p\in\{1,2,3\}} E_{it} \times I\Big(\tau(p) \le t < \tau(p+1)\Big) + \mathbf{X}_{it}' b + \epsilon_{it} \qquad (1)$$

In equation (1) α_0 is the constant term. The next terms are time dummies for each phase.[14] The parameters π_p thus pick up common trends during the phases not attributed to the ETS. The parameters γ_p are phase-group fixed effects that capture the mean difference before treatment between each phase-group (i.e. plants entering in phase I, II and III) and the control group. The phase-group fixed effects thus take into account heterogeneity between groups of plants that enter the ETS in different phases.

The parameters of main interest, β_p, capture the treatment effects of being regulated by the ETS in phase p (i.e., whether the plant is regulated in year t interacted with the phase dummies). The interaction term is thus equal to 1 if plant i is regulated by the ETS in year t and phase p includes year t. Note that we implicitly assume that the effect of phase p regulation is the same for all plants regardless of when they entered the ETS (as long as they were regulated in phase p). Our specification takes into account that the quota prices, the quota allocation rules and Norway's link with the EU ETS, have differed between phases. Hence, also the treatment effects may differ between phases. With respect to emissions and emissions intensities, we expect a negative estimate of β_p. Regarding the sign of the estimated effects on value added and productivity, we do not have any prior expectation.

The vector $\mathbf{X}_{it}$ contains the control variables described in Section 3.2, including dummies for industries (see Figure 2 for a list). The error term, ϵ_{it}, is assumed to be independent of the covariates in $\mathbf{X}_{it}$, the time dummies, the phase group fixed effect, and the treatment variable. Number of employees is lagged by one year $(t-1)$ to avoid the potential problem of reversed causality and to reduce potential problems of simultaneity. Before discussing the results in Section 4.3, we present an alternative specification.

[14] We include time dummies for each phase instead of year dummies because of the need for parsimony. This means that the time effects are constrained to be constant within each phase.

4.2 Panel data regressions with plant specific effects

It is possible that plant specific effects are not fully taken care of by the phase group fixed effects. The validity of equation (1) rests most critically on the assumption that the treatment variables are independent of the unobserved plant specific fixed effects. An endogeneity problem occurs if unobserved variables that affect the dependent variables, also affect the treatment variables. Thus, in an alternative model we specify plant specific effects as fixed effects, to allow correlation between unobserved plant specific fixed effects, ν_i, and the treatment variables.[15] This allows selection into treatment based on unobserved time invariant variables. A large part of the selection into treatment is likely to be time-invariant, as large and polluting firms usually tend to stay so (and vice versa for small and less polluting firms). Our basic identifying assumption is that conditional on a fixed firm effect and a vector of matching variables, the error term is independent of the treatment variables (unconfoundedness). A testable implication is that that the ETS-regulated plants and the plants in the control group should have a common pre-treatment trend, i.e. prior to the ETS regulation. If not, there will be systematic selection into treatment based on differenced variables, e.g. growth rates. Selection based on differenced variables is obviously is not picked up by the fixed effect, but is likely to be correlated with non-treated (counterfactual) outcomes of the treated firms, violating the unconfoundedness assumption.[16] The results of the specification test are reported in Table 2. As we cannot reject the null hypothesis of a common trend in the years before the plants are regulated, we have reason to believe that most of the selection and heterogeneity issues are time-invariant. The fixed effects model will pick up all

[15] An alternative could be to use instrumental variables. However, we are not aware of any variables that qualify as instruments, as the proxies for the ETS regulation selection criteria are all correlated with the dependent variables.

[16] We do this by adding the term $\mu I\left(t < T_i\right)$ to equation (2), testing the null hypothesis that $\mu = 0$ against the alternative that $\mu \neq 0$. The estimates are positive, but not significant and we cannot reject the null hypothesis of a common trend. There is thus no indication of significant time-variant heterogeneity between the ETS-regulated and the control plants. Based on this specification test the plant fixed effects and control variables are likely to capture the most evident selection effects and heterogeneity between the ETS-regulated and the control plants. In Appendix Table A.4 we report the results from a fixed effects panel data regression on a non-matched sample as a robustness analysis. However, as other selection issues, i.e. not picked up by the fixed effect, can still be present, and because matching is typically used in the ETS literature using micro data, we regard the estimations on the matched sample as our main results.

observable and unobservable selection and heterogeneity issues to the degree that these are time-invariant. However, to the degree that the selection and heterogeneity varies over time, we rely on the plant level control variables. Our fixed effects model is specified as follows:

$$\log Y_{it} = \sum_{p \in \{1,2,3\}} \pi_p \, I\Big(\tau(p) \leq t < \tau(p+1)\Big) + \sum_{p \in \{1,2,3\}} \beta_p \, E_{it} \times I\Big(\tau(p) \leq t < \tau(p+1)\Big) + \mathbf{X}_{it}' b + \nu_i + \epsilon_{it} \tag{2}$$

The specification in equation (2) is more appropriate for causal interpretations than the one in equation (1). However, the latter is much more parsimonious, which in particular can matter for such a small data set as we employ here. We thus argue that the specification in equation (1) is also a relevant measure of the treatment effects of the ETS.

4.3 Results

4.3.1 Environmental performance

The estimated effects of the ETS on emissions are presented in Table 2. Column (1) displays the results of the basic DID specification (1), whereas column (3) displays the results of the plant fixed effects specification (2). The estimated coefficients of main interest (β_p), i.e., the treatment effects, are displayed in the three first rows.

As seen in Table 2, we find no significant effect on emissions in phase I. The same applies to phase III, although the estimated effects are negative in both specifications. In phase II, on the other hand, the estimated effect on emissions is negative and significant at the 10 percent level in both specifications. This is in line with what we observed in Figure 1 above. The estimated coefficients suggest an emission reduction of around 30 per cent ($e^{-0.4} - 1 = -0.3$), which is quite substantial. If true, it would mean that annual GHG emissions from the treated plants (i.e., the treatment group after matching) would have been about 5 million tons higher in 2008-12 without the ETS. Norway's total GHG emissions in this period were on average 54 million tons per year. Given that we do not find significant impacts in phase III (2013), the estimated reduction in phase II may seem unrealistically high. Moreover, the standard error of the phase II coefficient is high, and hence the quantitative effect should be interpreted with caution.

A possible explanation for the lack of significant effect on emissions in phase I is that in this phase, Norway had an ETS that was not formally linked with the EU ETS, and that there may have been no binding cap on emissions from Norwegian plants in this phase (cf. Section 2). Another reason may be the fact that this was a pilot phase, and that the plants needed time to adjust to a new regulation. It is also possible that plants expected future allocation to be based on their emissions in phase I, in which case there could actually be some incentives to inflate emissions. Moreover, it may take time to adjust to a new regulatory regime, such as making investments in new equipment. Allocation was quite generous also in phase II and III,[17] but as Norwegian plants have been fully allowed to trade allowances with EU plants from phase II, the ETS price should have been of importance. The price of allowances was on average much higher in phase II than in phase III. This could possibly explain why we find weakly significant effect of phase II but not of phase III. Regarding the control variables, the signs of the coefficients are as expected.

Next, we investigate the effects on emissions intensity, see columns (2) and (4) in Table 2. We find no significant effects of any of the three phases. The estimated effects of phase II and III are negative in both specifications, but none are significant. The signs of the estimated β_2 coefficients are comparable with the ones for emissions. Yet, we cannot exclude the possibility that none of the phases have caused any emissions intensity reduction.

[17]In phase III, the allocation rules were changed more significantly, but most of the manufacturing industries still receive close to 100 percent of the allowances they need for free (cf. Section 2).

Table 2: Effects on emissions (CO_2 equivalent tons of CO_2, N_2O and PFCs) and emissions intensities (emissions per hour)

Response variable:	Coef.	(1) Log of emissions Est.	(2) Log of emissions int. Est.	(3) Log of emissions Est.	(4) Log of emissions int. Est.
Treatment Phase I	β_1	.03	.06	-.07	-.09
		(.21)	*(.19)*	*(.39)*	*(.15)*
Treatment Phase II	β_2	-.40*	-.20	-.38*	-.28
		(.22)	*(.20)*	*(.20)*	*(.19)*
Treatment Phase III	β_3	-.02	-.07	-.07	-.14
		(.41)	*(.33)*	*(.39)*	*(.27)*
Time dummy Phase I	π_1	-.17	-.09	-.19*	-.09
		(.16)	*(.13)*	*(.11)*	*(.09)*
Time dummy Phase II	π_2	-.11	-.21	-.25	-.26*
		(.19)	*(.15)*	*(.19)*	*(.15)*
Time dummy Phase III	π_3	-.24	-.17	-.40	-.26
		(.40)	*(.28)*	*(.41)*	*(.25)*
Group fixed effect Phase I	γ_1	3.46***	3.10***		
		(.46)	*(.41)*		
Group fixed effect Phase II	γ_2	3.67***	3.40***		
		(.52)	*(.51)*		
Group fixed effect Phase III	γ_3	3.37***	3.13***		
		(.52)	*(.48)*		
Log of relative energy prices		-.32***	-.30***	-.08	-.08
		(.09)	*(.09)*	*(.07)*	*(.06)*
Log of number of employees		.90***	-.21	.80***	-.18
		(.18)	*(.17)*	*(.25)*	*(.26)*
Plant fixed effects	ν_i	No	No	Yes	Yes
Plant specific control variables		Yes	Yes	Yes	Yes
Industry dummies		Yes	Yes	Yes	Yes
Number of plant-year obs.		1,454	1,449	1,454	1,449
Number of plants		144	144	144	144
Equation number		(1)	(1)	(2)	(2)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. Treatment plants are matched to control plants based on predetermined values of CO_2, N_2O and PFC emissions, number of employees, and exact matching on industries at the 2-digit level. Columns (1)-(2) are basic DID estimations with additional control variables. Columns (3)-(4) are panel data regression with plant fixed effects and additional control variables.

Specification test of common trend:	Est. *(Std.Er.)*	Est. *(Std.Er.)*	Est. *(Std.Er.)*	Est. *(Std.Er.)*
$H_o : \mu = 0$	-.11 *(.26)*	-.19 *(.28)*	.14 *(.20)*	.05 *(.21)*

4.3.2 Economic performance

We also investigate the effects of the ETS on real value added and labor productivity among Norwegian plants. The results are displayed in Table 3. Columns (1)-(2) display the results of the basic DID specification (1), whereas columns (3)-(4) display the results of the fixed effect specification (2).

For phase II, the estimated effects on value added and productivity are positive and significant. In both specifications, the estimated effect of phase II on value added is 0.24 (significant at the 5 percent level), which implies an estimated 27 percent increase in value added. The estimated effect of phase II on productivity is 0.25-0.26 (significant at the 1 percent level in column (2) and at the 5 percent level in column (4)). For phase I and III, the estimated effects on value added and productivity are positive but not significant.

The positive effects on value addded and productivity of phase II may at first seem a bit strange as the environmental regulation puts constraints on the plants. However, as discussed in the introduction, there are several possible reasons for such an effect. First, the manufacturing plants receive large amounts of free allowances. If they are able to reduce their emissions at relatively low costs, they can sell excess allowances and earn a profit that possibly exceeds their abatement costs. Moreover, if the marginal costs are (partly) passed on to consumers, their revenue could increase. The fact that we only find significant positive effects in phase II can be due to the relatively higher average quota price in this phase compared to phase III, and the fact that Norway had an ETS that was not formally linked with the EU ETS in phase I. Bushnell et al. (2013) show that stock prices for European carbon-intensive manufacturing industries declined when allowance prices were halved in April 2006, suggesting a positive relationship between quota prices and economic performance for the regulated plants. Further, the Porter Hypothesis (Porter and Van der Linde, 1995) points to the fact that environmental regulations give incentives to innovate, which may spur productivity and competitiveness. However, as this process is likely to take some time, the former explanation might be more plausible.

Still, even though a positive effect on economic performance can be explained, the estimated size of the effect seems quite high - 25-30 percent increase in value added and productivity. As a comparison, the average value of emission allowances across the treatment group corresponds to around 10 percent of the average value added in this group. Hence, although the estimated effects on value added and productivity in phase II seem robust across a number of specifications (see next subsection), the quantitative results should be interpreted with caution.

4.3.3 Leakage within firms

It could be argued that the emission reductions we find in phase II are at least partly due to reallocation of activity, and hence emissions, across plants within the same firm. That is, a firm can have incentives to reallocate some of its activity from regulated to unregulated installations to reduce the regulatory burden for the former one. To test this, we can perform the estimation at the firm level rather than the plant level, or we can add some variables to the plant level estimations above. We have tried both approaches.

Estimations at the firm level comes with some issues. First of all, since the ETS regulation is at the plant level, aggregating up to the firm level makes the estimation less precise (unless the reallocation issue is important). This is particularly so since many firms have a variety of different plants producing different goods. Moreover, ownership of plants sometimes change over time, making it difficult to generate a consistent data set with a sufficiently high number of firms (especially in the Norwegian case). In our sample, the number of units drops from 152 plants to 87, partly because of the aggregation and partly because we had to take out some of the plants with changing ownership. As a result, we no longer find significant effects on emissions in any of the phases (the sign of the estimate is still negative in phase II).

Table 3: Effects on value added and productivity

Response variable:	Coef.	(1) Log of value added Est.	(2) Log of productivity Est.	(3) Log of value added Est.	(4) Log of productivity Est.
Treatment Phase I	β_1	.01	.01	.02	.01
		(.11)	*(.11)*	*(.11)*	*(.11)*
Treatment Phase II	β_2	.24**	.26***	.24**	.25**
		(.10)	*(.10)*	*(.10)*	*(.10)*
Treatment Phase III	β_3	.05	.04	.05	.07
		(.17)	*(.17)*	*(.17)*	*(.17)*
Time dummy Phase I	π_1	.29***	.38***	.25***	.35***
		(.04)	*(.04)*	*(.05)*	*(.04)*
Time dummy Phase II	π_2	.47***	.52***	.44***	.52***
		(.05)	*(.05)*	*(.05)*	*(.05)*
Time dummy Phase III	π_3	.50***	.56***	.42***	.55***
		(.14)	*(.14)*	*(.14)*	*(.14)*
Group fixed effect Phase I	γ_1	.48***	.47***		
		(.09)	*(.09)*		
Group fixed effect Phase II	γ_2	.05	.10		
		(.15)	*(.14)*		
Group fixed effect Phase III	γ_3	.65***	.66***		
		(.11)	*(.11)*		
Log of relative energy prices		.06**	.06**	.004	.002
		(.03)	*(.03)*	*(.03)*	*(.03)*
Log of number of employees		.98***	.02	.71***	-.07
		(.05)	*(.05)*	*(.14)*	*(.08)*
Plant fixed effects	ν_i	No	Yes	No	Yes
Plant specific control variables		Yes	Yes	Yes	Yes
Industry dummies		Yes	Yes	Yes	Yes
Number of plant-year obs.		1,567	1,564	1,567	1,564
Number of plants		152	151	152	151
Equation number		(1)	(1)	(2)	(2)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. Treatment plants are matched to control plants based on predetermined values of CO_2, N_2O and PFC emissions, number of employees, and exact matching on industries at the 2-digit level. Columns (1)-(2) are basic DID estimations with additional control variables. Columns (3)-(4) are panel data regression with plant fixed effects and additional control variables.

Next, returning to the plant level data set, we introduce two new dummy variables to the estimations, to test the effects of leakage between plants within a firm. A dummy variable is set equal to 1 for ETS-regulated plants that belong to a firm which also owns one or more plants within the same 2-digit industry code not regulated by the ETS (but having GHG emissions), and 0 otherwise. The other dummy variable is set equal to 1 for plants in the control group that belong to a firm which also owns one or more ETS-regulated plants within the same 2-digit industry code, and 0 otherwise. If leakage takes place, we should expect the sign of the first dummy to be negative, while the second to be positive. The results are reported in Table A.3 in the Appendix, where δ_F and δ_T denote the first and second dummy, respectively. We find no significant effects for the first dummy, whereas the second dummy is positive and almost significant at 10 percent level in the fixed effect estimation (but not in the basic DID estimation). Moreover, the estimated coefficients for the treatment effect in phase II are also unaffected. Hence, on the one hand there is weak evidence suggesting that non-ETS plants may have increased their emissions, if they belong to a firm that also owns ETS-plants, while on the other hand there is no evidence that this is linked to the reduced emissions among the ETS-regulated plants in phase II.

4.3.4 Robustness tests

To investigate the robustness of our findings we perform several robustness tests. These are discussed in detail in the Appendix, and here we give a brief summary. First, we replicate an even simpler version of Table 2, a basic DID without plant level control variables, obtaining somewhat strengthened results for phase II, as expected. Second, seeing as the matching procedure reduces our sample size significantly, but still leaves a substantial amount of heterogeneity, a robustness test only relying on the panel data model with plant fixed effects and control variables seems reasonable. The "full" sample contains most Norwegian plants emitting the relevant greenhouse gases (CO_2, N_2O and PFCs), except for oil extraction plants. As expected, we find similar, but strengtened, results for phase II on emissions and emissions intensity.

Third, we replicate Table 2 with emissions of CO_2 only (i.e., excluding N_2O and PFCs). We find no significant effects on emissions and emissions intensities in any of the phases (for both specifications). The estimated effects of phase II are still negative across specifications, but not significant at conventional levels (although very close in some specifications). Fourth, we replace phase dummies with year dummies. The results are then very similar to the main results, both with respect to environmental and economic performance. Fifth, we test the effects of exluding the industry dummies. The results are again similar to the main results, except that the effect of phase II on emissions is no longer significant (although very close).

We also perform several robustness tests where we do not report detailed results. We use an alternative measure of emissions intensity – emissions relative to electricity use (cf. Section 3.1). The results in Tables 2-3 are largely confirmed. Further, we use a sample with 1:3 nearest neighbor matching rather than 1:10. Again, the estimated coefficients and the corresponding p-values are very similar to those reported in Tables 2-3. Moreover, we include the real quota price as a numerical variable. The estimates of the ETS treatment dummies (and their significance levels) are only marginally changed, whereas the estimate of the quota price is always insignificant. Finally, we replicate Tables 2-3 on a sample of treated and non-treated plants that are matched at the 3-digit industry level rather than at the 2-digit level. The estimated effects of phase II on emissions are no longer significant at conventional levels, which might be related to the drop in number of plant-year observations from 1,567 to 1,134. However, the estimated effects of phase II on value added and productivity are still significant and positive in both specifications.

5 Conclusions

In this paper we have examined impacts on Norwegian manufacturing plants of the ETS for the years 2005-2013, using plant level data. We have found somewhat mixed results.

Our estimation results suggest that the ETS may have led to emissions reductions in the second phase (2008-12). However, we do not find any significant effects in the first (2005-7) or the third phase (2013). Moreover, the results for phase II holds in some but not all robustness tests. Thus, the emission reduction found in phase II should be interpreted with caution, although other studies have come to similar conclusions (see the introduction). Further, the results based on 70-75 percent of the ETS-regulated manufacturing plants in Norway, may not carry over to other Norwegian manufacturing plants. Furthermore, the results can not be generalized to non-manufacturing industries regulated by the ETS, such as the oil and gas industry which is the biggest ETS-regulated sector in Norway. When it comes to emissions intensities, we find no significant effects in any of the phases.

The limited effects on emissions and emissions intensity in our estimations can possibly be explained by the fact that the manufacturing industries have received close to 100 percent of the quotas they need to cover their business-as-usual emissions. Surplus quotas could in principle have been sold to other plants, but low quota prices may have given limited incentives for emissions reductions. When it comes to phase I, Norway was not formally linked to the EU ETS, and it may be questioned whether there was any binding cap on emissions for Norwegian plants in this phase. Finally, the quota price was on average higher in the second phase than in the beginning of the third phase, which may explain why we find significant emissions reductions of phase II but not of phase III.

Our results further suggest that the ETS led to significantly higher value added and productivity in phase II. Again, we cannot be sure that this result carry over to the manufacturing plants that do not belong to our treatment group (even if they are regulated by the ETS), not to say non-manufacturing plants. This may be related to the large amounts of free allowances. If all allowances were instead auctioned by the government, the plants' costs would have been higher and thus value added and productivity lower. Furthermore, the plants may have been able to pass on (parts of) the increased marginal costs to the consumers, and hence increase their revenues through higher output prices. Finally, we notice that increased productivity due to environmental regulation is also consistent with the Porter Hypothesis.

We find no significant effects in the two other phases on neither productivity nor value added, although the estimates are consistently positive. The explanation for finding positive and significant impacts only in phase II could be the higher average quota prices.

In our study we control for phase time specific effects. However, it is possible that treated plants were differently affected by e.g. the financial crisis if they were more or less trade exposed than the control group. To our knowledge, empirical studies on the effects of the ETS on plants' or firms' emissions so far rely on matching methods in combination with difference-in-differences strategies. However, although we have required exact matching with respect to industry affiliation, differences between regulated and unregulated plants might not be fully accounted for. Even after matching, the treatment group in our analysis has much higher emissions and emissions intensities than the control group. As the regulator selects plants for participation in the ETS based on the capacity limit (e.g., total thermal effect or tons of products), regression discontinuity may constitute a suitable method for estimating the effects of the ETS if sufficient data are available, although this method would estimate a very local effect for the minority of installations being close to the capacity limit.

From a policy perspective, our results do not give clear conclusions with regard to whether emissions trading lead to lower emissions. As emissions trading is a quantity instrument, it should in theory lead to emissions reductions if the cap is set below the unregulated emissions level. However, in our study we have only looked at Norwegian plants, and not all European plants regulated by the EU ETS. Moreover, since plants regulated by the ETS are allowed to bank allowances to the next phase, and also buy offsets from the Clean Development Mechanism (CDM), total emissions by all European plants regulated by the EU ETS may well exceed the given emissions cap within a single year or phase.

In Norway, as well as in some other European countries, policy makers have been concerned about domestic GHG emissions, setting targets for their national emissions. Some countries have implemented climate policies in sectors already regulated by the ETS, such as the Carbon Price Floor in the UK electricity sector. In Norway, some of the ETS sectors also pay a CO_2 tax (oil and gas industry and aviation). If policy makers in Norway are concerned about the domestic emissions in the ETS sectors, also in the manufacturing industries, a natural suggestion would be to impose CO_2 taxes also for these industries (or a carbon price floor similar to the one in UK). However, higher CO_2 prices for Norwegian plants than for other European plants could lead to relocation of manufacturing industry from Norway to other European countries, with limited effects on global GHG emissions.

Given our findings that the Norwegian manufacturing plants have profitted from the ETS, at least in the second phase, one may argue that the economic performance of Norwegian plants on average would not be negatively affected by the ETS (compared to a situation with no ETS) if a smaller share of the allowances were given away for free to the plants. Free allocation of allowances is mainly motivated by the risk of carbon leakage. However, Martin et al. (2014) show that the current allocation in the EU ETS results in "substantial overcompensation for given carbon leakage risk". As allocation rules are determined at the EU level (also for the non-EU member Norway), the Norwegian authorities are not in a position to adjust the allocation. Nevertheless, our results should be relevant when considering the extent of allocation, both at the EU level and more generally.

Appendix

Data description and additional tables

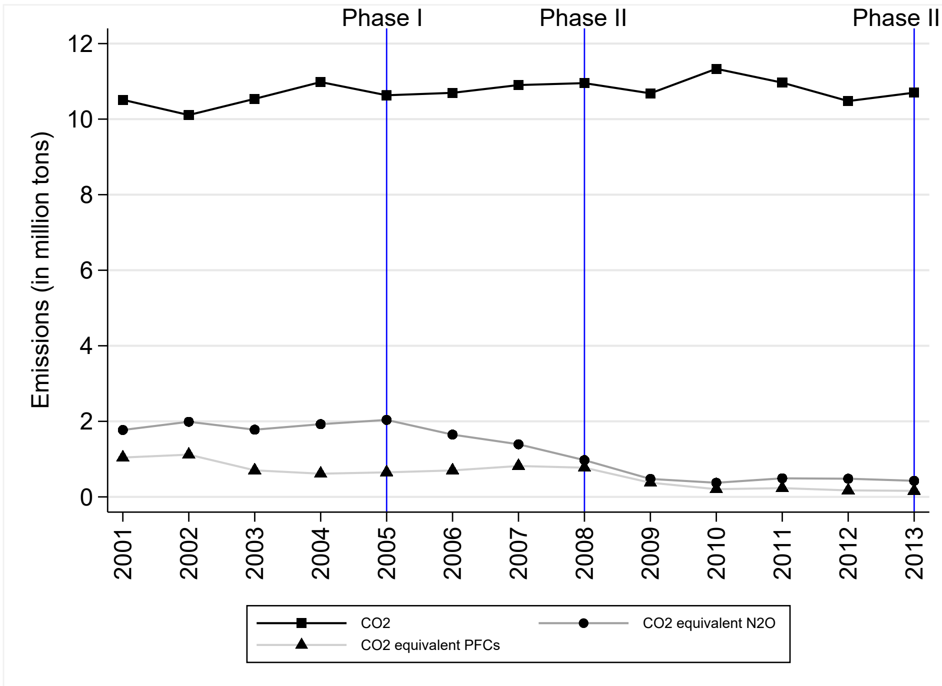

Figure A.1: Total annual emissions of CO_2, N_2O, and $PFCs$ (in million tons of CO_2-equivalents) from Norwegian manufacturing plants regulated by the ETS in 2013.

Table A.1: Share of plant-year observations across industries, 2001-2013

	Before matching		After matching	
	ETS plants	Non-ETS plants	Treatment	Control
Industry	Percent	Percent	Percent	Percent
Mining and extraction (excluding oil and gas)	0.6	6.3	0	0
Oil and gas extraction	33.4	0.7	0	0
Manuf. of textiles and food	6.5	38.9	8.2	36.5
Manuf. of wood, pulp and paper	14.8	3.8	22.2	8.3
Manuf. of chem., pharmac., rubber and plastic	14.2	19.6	23.2	19.8
Manuf. of metals and minerals	26.2	18.1	46.4	34.4
Manuf. of machinery and electronics	4.3	12.5	.03	1.0
Total	100	100	100	100

Table A.2: Summary statistics before matching, 2001-2013

	ETS plants		Non-ETS plants	
Variable	Mean	Median	Mean	Median
CO_2, N_2O and PFC emissions[2]	212,483	70,412	3,887	60.0
CO_2 emissions	175,715	43,241	3,835	47.0
CO_2, N_2O and PFC emissions intensity[2]	12.1	.185	.120	.0001
CO_2 emissions intensity	10.8	.131	.118	.00004
Labor productivity[3]	.541	.412	.625	.324
Number of employees	211	161	125	77
Relative energy prices (“dirty” over “clean”)	1.05	.86	1.16	.98
Value added[3]	213,260	89,385	74,707	38,736
Electricity use (kWh)	486,111	99,953	23,079	7,114
Man hours	381,436	263,336	203,462	122,597
Wages[3]	102,060	72,922	51,836	30,064
Operating profits[3]	119,672	45,223	51,801	20,583
Number of plant-year observations	1126		3746	
Numer of plants	150		515	

[1]All emissions are reported as tons of CO_2-equivalents

[2]All values in million NOK are deflated using the PPI with 2013 as base year.

Alternative estimations

In the main text, we gave a summary of the robustness tests. Here we provide more details about their results. First, we report a simpler version of Table 2, a basic DID without plant level control variables. The justification for such a model is the potential for “bad controls”, capturing parts of the effects that potentially are due to the regulation, or, on the other hand, potential endogeniety. The results are reported in Table A.4 in columns (1) and (2), and are quite similar to the main results, although the estimates for phase II are somewhat strengthened.

Table A.3: Robustness test including estimation of carbon leakage within firms

Response variable:	Coef.	(1) Log of emissions Est. *(Std.Er.)*	(2) Log of emissions Est. *(Std.Er.)*
Treatment Phase I	β_1	.04 *(.25)*	-.05 *(.19)*
Treatment Phase II	β_2	-.40* *(.23)*	-.36 *(.20)*
Treatment Phase III	β_3	-.02 *(.41)*	-.06 *(.39)*
Potential leakage from	δ_F	-.03 *(.80)*	.05 *(.33)*
Potential leakage to	δ_T	.05 *(.90)*	.47 *(.29)*
Plant fixed effects	ν_i	No	Yes
Plant specific control variables		Yes	Yes
Time dummies for each phase		Yes	Yes
Group fixed effect for each phase		Yes	No
Industry dummies		Yes	Yes
Number of plant-year obs.		1,454	1,454
Number of plants/firms		144	144
Equation number		(1)	(2)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. Treatment plants are matched to control plants based on predetermined values of CO_2, N_2O and PFC emissions, number of employees, and exact matching on industries at the 2-digit level. Column (1) is a basic DID estimation with control variables, whereas column (2) is a panel data regression with plant fixed effects.

Table A.4: Robustness tests: Basic DID without control variables; Estimation on full (non-matched) sample

Response variable:	Coef.	(1) Log of emissions Est. *(Std.Er.)*	(2) Log of emissions int. Est. *(Std.Er.)*	(3) Log of emissions Est. *(Std.Er.)*	(4) Log of emissions int. Est. *(Std.Er.)*
Treatment Phase I	β_1	-.02 *(.20)*	.15 *(.18)*	-.08 *(.13)*	-.02 *(.13)*
Treatment Phase II	β_2	-.64** *(.30)*	-.25 *(.22)*	-.43** *(.18)*	-.37** *(.18)*
Treatment Phase III	β_3	-.10 *(.42)*	-.15 *(.35)*	-.25 *(.21)*	-.20 *(.21)*
Plant fixed effects	ν_i	No	No	Yes	Yes
Plant specific control variables		No	No	Yes	Yes
Time dummies for each phase		Yes	Yes	Yes	Yes
Group fixed effect for each phase		Yes	Yes	No	No
Industry dummies		No	No	Yes	Yes
Number of plant-year obs.		1,454	1,449	2, 165	2,164
Number of plants/firms		144	144	281	281
Equation number		(1)	(1)	(2)	(2)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. Treatment plants in columns (1)-(2) are matched to control plants based on predetermined values of CO_2, N_2O and PFC emissions, number of employees, and exact matching on industries at the 2-digit level. Columns (1)-(2) are basic DID estimations without control variables. Columns (3)-(4) are panel data regressions with fixed effects on the full (non-matched) sample.

Specification test of common trend:				Est. *(Std.Er.)*	Est. *(Std.Er.)*
$H_o : \mu = 0$				.22 *(.23)*	.12 *(.23)*

The matching procedure reduces the sample size significantly, but still leaves a substantial amount of heterogeneity. Hence, we rely on the fixed effects model to take into account selection based on time-invariant variables, and on plant level control variables to take into account selection based on time-varying variables. We tested the implicit assumption of the fixed effects model of a common trend in the dependent variable prior to the regulation. The specification test statistic is reported in Table 2 (for the matched sample), and the test statistic and the results based on the "full" sample in columns (3) and (4) of Table A.4. The test statistic is never significant and we cannot reject the null hypothesis of a common trend ($H_0 : \mu = 0$). We thus have reason to believe that most of the selection and heterogeneity issues are in fact already taken into account through the fixed effects and the control variables. In the robustness test, there is no matching and the sample contains most Norwegian plants emitting the relevant greenhouse gases (CO_2, N_2O and PFCs), except for oil extraction plants.[18] The estimated effects on both emissions and emissions intensity in phase II are strengthened, as the significance is now well within the 5 percent level. The effects of the other phases are negative but not significant, as in other specifications.

[18]The oil extraction plants (Standard industrial code 6 at the 2-digit industry level) are excluded as they are likely highly different from the other plants. Moreover, some plants are automatically excluded because of missing values. Remaining plants are in Standard industrial codes B and C.

Table A.5: Effects on CO2 emissions and emissions intensity

Response variable:	Coef.	(1) Log of CO_2 Est.	*(Std.Er.)*	(2) Log of CO_2 int. Est.	*(Std.Er.)*	(3) Log of CO_2 Est.	*(Std.Er.)*	(4) Log of CO_2 int. Est.	*(Std.Er.)*
Treatment Phase I	β_1	.20	*(.14)*	.23	*(.14)*	.05	*(.10)*	.07	*(.10)*
Treatment Phase II	β_2	-.19	*(.18)*	-.14	*(.18)*	-.26	*(.14)*	-.22	*(.13)*
Treatment Phase III	β_3	-.08	*(.29)*	-.06	*(.30)*	-.01	*(.22)*	.02	*(.22)*
Time dummy Phase I	π_1	-.28**	*(.11)*	-.20*	*(.11)*	-.20*	*(.10)*	-.11	*(.10)*
Time dummy Phase II	π_2	-.36**	*(.13)*	-.24*	*(.13)*	-.14	*(.12)*	-.10	*(.13)*
Time dummy Phase III	π_3	-.09	*(.26)*	-.04	*(.27)*	-.20	*(.22)*	-.12	*(.22)*
Group fixed effect Phase I	γ_1	3.07***	*(.42)*	3.03***	*(.41)*				
Group fixed effect Phase II	γ_2	2.76***	*(.54)*	2.80***	*(.55)*				
Group fixed effect Phase III	γ_3	3.33***	*(.51)*	3.34***	*(.52)*				
Log of relative energy prices		-.30***	*(.09)*	-.30***	*(.09)*	-.02	*(.02)*	-.03*	*(.02)*
Log of number of employees		.62***	*(.17)*	-.39**	*(.17)*	.89***	*(.18)*	.01	*(.12)*
Plant fixed effects	ν_i	No		No		Yes		Yes	
Plant specific control variables		Yes		Yes		Yes		Yes	
Industry dummies		Yes		Yes		Yes		Yes	
Number of plant-year obs.		1,352		1,348		1,352		1,348	
Number of plants		143		143		143		143	
Equation number		(1)		(1)		(2)		(2)	

Notes: *** $p<0.01$, **$p<0.05$, * $p<0.1$. Robust standard errors in parentheses. Treatment plants are matched to control plants based on predetermined values of CO_2 emissions, number of employees, and exact matching on industries at the 2-digit level. Columns (1)-(2) are basic DID estimations with additional control variables. Columns (3)-(4) are panel data regression with plant fixed effects and additional control variables.

Third, we replicate Table 2 with CO_2 emissions only (i.e., excluding N_2O and PFCs). This is a relevant robustness test as relatively few plants emit N_2O or PFC emissions that are regulated by the ETS. The reason is partly that CO_2 emissions are much more widespread than other $GHGs$, but also because the ETS has mainly focused on CO_2. Obviously, this specification is more likely to accurately estimate the potential effects on CO_2 emissions. The results are displayed in Table A.5. In columns (1)-(2) we report the results of the basic difference-in-differences model with control variables, whereas in columns (3)-(4) plant fixed effects are included. We identify no significant effects of phase I or phase III in any specification. This is similar to the results when all three $GHGs$ are included. Further, the estimated effects of phase II are negative across all specifications, but not significant at conventional levels (the lowest p-value of 0.11 is obtained in columns (3)-(4) including plant fixed effects). The estimated effect on emissions (-0.26) is of the same order as the corresponding estimate in Table 2 (-0.38), i.e., when N_2O and PFC emissions are included.

Table A.6: Effects on emissions and emissions intensity. Robustness test with year dummies rather than time dummies for each phase

Response variable:	Coef.	(1) Log of emissions Est. *(Std.Er.)*	(2) Log of emissions int. Est. *(Std.Er.)*	(3) Log of emissions Est. *(Std.Er.)*	(4) Log of emissions int. Est. *(Std.Er.)*
Treatment Phase I	β_1	.04 *(.21)*	-.07 *(.19)*	-.08 *(.17)*	-.08 *(.15)*
Treatment Phase II	β_2	-*.40** *(.22)*	-.20 *(.20)*	-.39** *(.20)*	-.28 *(.18)*
Treatment Phase III	β_3	-.02 *(.41)*	-.06 *(.33)*	-.09 *(.39)*	-.14 *(.27)*
Plant fixed effects	ν_i	No	No	Yes	Yes
Plant specific control variables		Yes	Yes	Yes	Yes
Industry dummies		Yes	Yes	Yes	Yes
Year dummies		Yes	Yes	Yes	Yes
Number of plant-year obs.		1,454	1,449	1,454	1,449
Number of plants		144	144	144	144
Equation number		(1)	(1)	(2)	(2)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. Treatment plants are matched to control plants based on predetermined values of CO_2, N_2O and PFC emissions, number of employees, and exact matching on industries at the 2-digit level. Columns (1)-(2) are basic DID estimations. Columns (3)-(4) are panel data regression with plant fixed effects.

Table A.7: Effects on value added and productivity. Robustness test with year dummies rather than time dummies for each phase

Response variable:	Coef.	(1) Log of value added Est. *(Std.Er.)*	(2) Log of productivity Est. *(Std.Er.)*	(3) Log of value added Est. *(Std.Er.)*	(4) Log of productivity Est. *(Std.Er.)*
Treatment Phase I	β_1	-.01 *(.11)*	.001 *(.11)*	-.02 *(.10)*	-.01 *(.10)*
Treatment Phase II	β_2	.25** *(.10)*	.27*** *(.10)*	.22** *(.10)*	.26** *(.10)*
Treatment Phase III	β_3	.06 *(.17)*	.05 *(.17)*	.09 *(.17)*	.07 *(.17)*
Plant fixed effects	ν_i	No	No	Yes	Yes
Plant specific control variables		Yes	Yes	Yes	Yes
Industry dummies		Yes	Yes	Yes	Yes
Year dummies		Yes	Yes	Yes	Yes
Number of plant-year obs.		1,352	1,348	1,352	1,348
Number of plants		143	143	143	143
Equation number		(1)	(1)	(2)	(2)

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. Treatment plants are matched to control plants based on predetermined values of CO_2, N_2O and PFC emissions, number of employees, and exact matching on industries at the 2-digit level. Columns (1)-(2) are basic DID estimations. Columns (3)-(4) are panel data regression with plant fixed effects.

In our main analysis we assume for efficiency that time dummies (one for each phase) is sufficient to control for effects over time that are common for the ETS-regulated plants and control plants. In a robustness test we check whether this assumption holds by replacing the time dummies with year dummies. Years dummies are more likely to pick up on common trends in the entire period. The results are very similar to the main results, and are reported in the Appendix Tables A.6-A.7.

We use exact matching at the industry level as participation in the ETS is contingent on the plant activity. Moreover, industry dummies are included in the analysis as industry specific effects not related to the ETS could be present (e.g. related to technology development or the voluntary arrangements between the Ministry of Climate and Environment and the processing industry mentioned in Section 3.2). However, the industry dummies might also capture parts of the effect of the ETS. Hence we perform robustness analysis exluding industry dummies. The results are reported in the Appendix Tables A.8-A.9. The results are similar to the main results, except that the effect of phase II on emissions is no longer significant within conventional levels (although very close). It is thus unlikely that the industry dummies capture parts of the effect of the ETS.

We also perform a number of robustness tests for which we do not provide tables. We replicate the results of Tables 2 and A.5 using the alternative measure of emissions intensity mentioned in Section 3.1 – emissions relative to electricity use. The results are largely confirmed as the estimates and the corresponding p-values are similar to those in Tables 2 and A.5. Next, we replicate Tables 2-3 on a sample with 1:3 nearest neighbor matching rather than 1:10. Again, the estimates and the corresponding p-values are only marginally changed. Further, we perform an estimation including the real quota price as a numerical variable. The estimates of the ETS treatment dummies (and corresponding significance levels) for each phase are only marginally changed, whereas the estimate of the quota price is always insignificant. This may suggest that the annual quota price is less important than the average quota price over some years. Finally, we replicate Tables 2-3 on a sample of treated and non-treated plants matched at the 3-digit rather than the 2-digit industry level. The estimated effects of phase II on emissions are no longer significant at conventional levels, which might be related to the drop in number of plant-year observations from 1,567 to 1,134. However, the estimated effects of phase II on value added and productivity are still significant and positive in both specifications.

Table A.8: Effects on emissions and emissions intensity. Robustness test without industry dummies

		(1)		(2)		(3)		(4)	
Response variable:		Log of emissions		Log of emissions int.		Log of emissions		Log of emissions int.	
	Coef.	Est.	*(Std.Er.)*	Est.	*(Std.Er.)*	Est.	*(Std.Er.)*	Est.	*(Std.Er.)*
Treatment Phase I	β_1	.09	*(.22)*	.14	*(.20)*	-.07	*(.17)*	-.09	*(.15)*
Treatment Phase II	β_2	-.37	*(.23)*	-.16	*(.20)*	-.38*	*(.20)*	-.28	*(.18)*
Treatment Phase III	β_3	.08	*(.43)*	-.01	*(.34)*	-.07	*(.39)*	-.14	*(.27)*
Plant fixed effects	ν_i	No		No		Yes		Yes	
Plant specific control variables		Yes		Yes		Yes		Yes	
Time dummies for each phase		Yes		Yes		Yes		Yes	
Group fixed effect for each phase		Yes		Yes		No		No	
Industry dummies		No		No		No		No	
Number of plant-year obs.		1,454		1,449		1,454		1,449	
Number of plants		144		144		144		144	
Equation number		(1)		(1)		(2)		(2)	

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. Treatment plants are matched to control plants based on predetermined values of CO_2, N_2O and PFC emissions, number of employees, and exact matching on industries at the 2-digit level. Columns (1)-(2) are basic DID estimations. Columns (3)-(4) are panel data regression with plant fixed effects.

Table A.9: Effects on value added and productivity. Robustness test without industry dummies

		(1)		(2)		(3)		(4)	
Response variable:		Log of value added		Log of productivity		Log of value added		Log of productivity	
	Coef.	Est.	*(Std.Er.)*	Est.	*(Std.Er.)*	Est.	*(Std.Er.)*	Est.	*(Std.Er.)*
Treatment Phase I	β_1	.01	*(.11)*	.01	*(.11)*	-.02	*(.11)*	-.01	*(.11)*
Treatment Phase II	β_2	.27***	*(.10)*	.29***	*(.11)*	.22**	*(.10)*	.26**	*(.10)*
Treatment Phase III	β_3	.06	*(.17)*	.05	*(.17)*	.08	*(.17)*	.07	*(.17)*
Plant fixed effects	ν_i	No		No		Yes		Yes	
Plant specific control variables		Yes		Yes		Yes		Yes	
Time dummies for each phase		Yes		Yes		Yes		Yes	
Group fixed effect for each phase		Yes		Yes		No		No	
Industry dummies		No		No		No		No	
Number of plant-year obs.		1,352		1,348		1,352		1,348	
Number of plants		143		143		143		143	
Equation number		(1)		(1)		(2)		(2)	

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. Treatment plants are matched to control plants based on predetermined values of CO_2, N_2O and PFC emissions, number of employees, and exact matching on industries at the 2-digit level. Columns (1)-(2) are basic DID estimations. Columns (3)-(4) are panel data regression with plant fixed effects.

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