Carbon Taxation, Firm Performance, and Labor Demand

Jimmy Karlsson*

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Abstract

This paper investigates the environmental and economic effects of carbon taxation, including the impacts on labor demand for different workers. Using matched employer-employee data from the Swedish registers from 2004 to 2018, I estimate the effects of a reform that increased the stringency of the tax for a subset of firms in the manufacturing sector. In a Difference-in-Differences framework, I find that the reform significantly reduced emissions, primarily through a switch to biofuels. However, it also reduced revenue and employment among emission-intensive firms. The negative employment effects are more pronounced for low-educated workers, suggesting a skill-biased effect of carbon taxation, although high-educated workers are also negatively affected at the most exposed firms. On average, the result corresponds to semi-elasticities of -0.58% per euro tax increase for emissions, and -0.20% for low-educated employment. The paper concludes with a discussion on the implications for the green transition and labor market inequality.

Keywords: Carbon taxation, Climate change, Firm performance, Inequality, Employment

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1 Introduction

The threat of climate change calls for a rapid decrease in carbon emissions. Carbon taxation is one of the main climate policy instruments that achieve cost-effective emission reductions, and the number of countries with a carbon tax in place is growing (World Bank, 2024). If sufficiently high, a carbon tax corrects the externality of climate change damages, and creates an incentive to move away from fossil fuels (Timilsina, 2022). However, despite its appealing properties, carbon taxation has faced considerable political resistance due to potentially negative impacts on jobs and inequality (Vona, 2019). This has especially been the case for the fossil-dependent manufacturing sector, which in many western countries already has experienced a declining share of (low-skill) employment due to, for example, changes in trade (Autor et al., 2013) and technological change (Graetz & Michaels, 2018). As a consequence, politicians are faced by a perceived trade-off between the green transition and protecting industrial firms and their workers.

However, despite the urgent need to reduce emissions, there is still limited empirical evidence on the impacts of carbon taxation, and especially its effects on labor market inequality. One reason is data limitation. Studying impacts on labor market inequality, i.e. heterogeneous effects on labor demand for different types of workers, requires access to data on individuals and their characteristics. Identifying changes in labor demand within firms also requires matching workers to employers that are more or less affected by carbon taxation. Another reason is a lack of quasi-experimental settings to estimate causal effects (Vrolijk & Sato, 2023). Since environmental regulations are shaped by economic and political factors, there tend to be systematic differences in regulatory stringency across firms, for example through tax exemptions to emission-intensive firms (Ekins & Speck, 1999). These selection effects often lead to empirical challenges, such as limited samples of treated firms and endogeneity concerns.

This paper studies the causal effects of carbon taxation on firms' environmental and economic performance and labor demand, exploiting a reform in the Swedish carbon tax that led to a differential increase in firm-level tax rates between 2011-2018. During this time period, Sweden had the highest carbon tax in the world (World Bank, 2025), and by far the closest to recent estimates of the social cost of carbon (Moore et al., 2024).[1] However, prior to 2011, Swedish manufacturing firms paid a subsidized rate of the carbon tax. The subsidy reduced the effective tax rate by up to 79%, and was given to firms through a system of tax refunds. Applications of tax refunds were granted conditional on fulfilling certain criteria related to firm-, fuel-, and usage-specific factors (Skatteverket,

[1] Moore et al. (2024) provides an average of \$132/ton CO_2 in a meta-analysis of the literature on Social Cost of Carbon (SCC) estimates, with a very wide distribution. In the same paper, using a different approach, the authors estimate an average SCC of \$283/ton CO_2. The Swedish tax was \$141/ton CO_2 in 2018.

2007). Refund eligibility rules thus created variation in firm-level effective carbon tax rates in the manufacturing sector. Between 2011-2018, a reform gradually removed the possibility to apply for refunds, which lead to a substantial increase in the stringency of the effective carbon tax for previously subsidized firms.

In this paper, I develop a Difference-in-Differences framework around the reform that isolates exogenous variation in firm-level carbon tax rates, by comparing firms with and without tax refunds *before* the phase-out was announced, before and after the reform. Combined with matched employer-employee data, this paper provides the first evidence of the causal effects of carbon taxation on labor market inequality using firm-level variation. The paper also presents new evidence on the effectiveness of carbon taxation in reducing emissions, the mechanisms behind the estimated effects, and its impacts on overall economic performance. The policy-relevance likely extends to other jurisdictions implementing carbon taxation and similar policies on manufacturing, like the EU's upcoming carbon market ETS2 (European Commission, 2024).

The result shows a significant reduction in emissions due to the removal of the carbon tax rebates, which is mainly explained by a switch from fossil fuels to biofuels. I also find significant negative effects on revenue and employment. The negative employment effects are more pronounced for workers without a high school degree, which indicates a skill-biased effect of carbon taxation against low-educated workers. The effects are similar for men and women, but stronger for workers above 40 years old. Scaled by the average effective tax rate increase of around 66 EUR/ton CO_2, the results correspond to significantly estimated semi-elasticities of carbon taxation of -0.58% per EUR/ton for emissions, and -0.20% for employment of low-educated workers (-0.31% in emission-intensive firms).

The substantial negative effect on emissions (and positive effect on biofuel consumption) suggests a clear substitution towards non-fossil fuels. Whether the decrease in employment is due to fuel substitution, for example if labor is used less intensively after the shift, or due to a contraction of output, is difficult to determine. Observing the same effect for revenue does, however, point to a negative effect on output explaining the reduction in employment. This point is made stronger by the fact that the negative economic impacts are concentrated among emission-intensive firms, who are more exposed to changes in the carbon tax. Distinguishing between these mechanisms is important, as it determines whether the findings of this paper are general features of the green transition, or specific to a cost-increasing carbon tax. For example, the existence of a dominating output effect implies different effects on workers under green subsidies (Popp et al., 2021).

The study contributes to an increasing empirical literature estimating the effects of carbon taxation and emissions trading, at various levels of aggregation. At the country-level using a synthetic control approach, prior studies have found significant effects of carbon taxation on emission reductions in transportation in Sweden (Andersson, 2019) and Finland (Mideksa, 2024), and in electricity generation in the UK (Leroutier, 2022). Significant emission reductions or improvements in emission intensity have also been estimated for the manufacturing sector in different settings using firm-level data (Brännlund et al., 2014; Jaraitė & Maria, 2016; Martinsson et al., 2024). Martinsson et al. (2024) analyzes the effect on emission intensity of variations in the Swedish carbon tax during an overlapping time period as in this paper. However, the authors do not observe tax refunds, and thus estimate the effects by assigning marginal tax rates to firms mainly through industry codes, in a panel data regression with fixed effects. Hence, their identification relies on stronger assumptions regarding exogeneity in year-to-year changes in exemptions given to specific industries.

Studying similar tax exemptions as this paper, Martin et al. (2014) evaluates the effects of a combined energy and carbon tax in the UK, using variation in eligibility for a tax rebate among manufacturing firms. The authors find a negative indirect effect on emissions (through a reduction in electricity use), without any significant effects on employment. Gerster and Lamp (2024) studies the effects of an electricity tax in Germany, exploiting exemption rules in a Regression Discontinuity Design. The paper finds a positive effect of tax exemptions on manufacturing electricity consumption, but no effect on employment. I contribute to these papers by extending the analysis to the impacts on labor market inequality. Combining firm- and individual-level data allows me to estimate heterogeneous effects across different types of workers, beyond what is masked in average effects.

A number of papers estimate the impacts of the European Union Emissions Trading System (EU ETS) on emissions and economic outcomes, using firm- or installation-level data together with matching estimators to obtain suitable control groups among non-ETS firms (Colmer et al., 2024; Dechezleprêtre et al., 2023; Marin et al., 2018). Despite significant reductions in emissions, they find no effect on employment. The studies on emissions trading provide interesting results for one of the most important climate policies in place, but do not necessarily extrapolate to the case of carbon taxation. Under a number of assumptions, emissions trading and carbon taxation lead to the same substitution effects away from fossil fuels. However, depending on the design (such as the initial allocation of emissions trading permits), the two policies can lead to different economic impacts (Timilsina, 2022).

Several papers have studied the effects of a revenue-neutral carbon tax in British Columbia, Canada (Azevedo et al., 2023; Yamazaki, 2017; Yip, 2018). For example, Yip (2018) uses individual-level data in a Difference-in-Differences framework with other Canadian provinces as control groups, and finds that implementation led to increasing unemployment rates for low-educated workers. The studies differ from mine for two reasons. First, the fact that the tax was revenue-neutral, through lower taxes and lump-sum transfers to households, means that negative output effects due to the tax might have been mitigated. Second, the regional implementation implies that the estimated effects capture the net effects after general equilibrium adjustments, which might be substantial given the complementing policies. Analyzing the effects of carbon taxation where treatment varies at the firm-level enables the identification of within-firm changes in labor demand, and therefore leads to a more structural understanding of the impacts on labor market inequality.

The remainder of this paper proceeds as follows. Section 2 describes the details of the Swedish carbon tax and the reform studied in this paper. Section 3 describes the dataset and presents descriptive statistics. Section 4 presents the empirical framework, and Section 5 presents the results. Section 6 contains a robustness analysis of the main result. Section 7 discusses the implications of the result in this paper, and concludes.

2 Institutional Background

The Swedish carbon tax was implemented in 1991, and established a price on emissions from fossil fuels consumed for heating or engine operation (SFS 1994:1776). The tax was added to an already existing energy tax on fuels, but differs in that the tax rate is proportional to the carbon content of the fuel. For most fuel consumers the tax is included in the price set by the seller, who is tax liable and hence the one paying the tax in practice (Hammar & Åkerfeldt, 2011). The tax burden faced by firms and households purchasing fuels thus depends on the extent to which retailers increase fuel prices when faced by a higher tax. The pass-through of fuel taxation to prices is generally found to be high (Dovern et al., 2023; Lade & Bushnell, 2019), and has been estimated to close to 100% in the case of EU ETS prices and electricity prices (Fabra & Reguant, 2014).[2]

In 2004, the level of the tax was set to 910 SEK/ton CO_2, and gradually increased to 1,150 in 2018 (which corresponds to a rate of 122 EUR/ton CO_2).[3] However, many manufacturing firms have experienced substantially lower effective tax rates due to generous exemptions from both carbon and energy taxation over time. In this paper, I will focus on the main exemption for the manufacturing sector, which is the reduced tax rates for fuels used for heating and stationary engines ('heating fuels'). Below I outline the relevant details of the policy, and how various reforms led to the variation in firm-level stringency used in the analysis.

[2]Energy-intensive firms can also choose to be registered taxpayers, and pay the tax themselves in their declaration. Technically, the tax is levied when a fuel is sold by a registered taxpayer to a consumer. Between registered taxpayers, taxation is suspended (Hammar & Åkerfeldt, 2011).

[3]The average exchange rate over the period was 9.39 SEK/EUR, which is used for all currency conversions in this paper.

Between 2004-2010, the government offered a 79% refund of the carbon tax for eligible firms (Hammar & Åkerfeldt, 2011). Eligibility was based on three main criteria. First, tax refunds were only granted for fuels consumed in a *manufacturing process*. Second, refunds were only granted for uses *other than motorized vehicles*, excluding gasoline, crude tall oil and some heavy heating oils (like diesel).[4] Third, the manufacturing process in which the fuel was used had to be the *main activity* of the firm. The first condition excludes fuels used after the manufactured goods have been fully processed, such as for storage in certain temperatures before the goods are sold. The second condition practically allows the use of fuels for heating and stationary engines, except for the listed fuels. The third condition required an assessment by the tax authority of what constitutes the main economic activity of the firm, where the SNI industry classification (the Swedish version of NACE) is a guidance, but not a perfect predictor. In the end, this means that tax refund eligibility was a combination of firm-, fuel-, and usage-specific factors (Skatteverket, 2007).

Since most firms did not pay the tax directly, they received the refunds by submitting an application to the tax authority. In the application, a firm would state the quantity of fuels that would entitle them a refund, as long as the fuel had been purchased within the last three years. Under the assumption that the firm bore the entire burden of the carbon tax through a higher fuel price (i.e. a 100% pass-through rate), the firm could claim a refund corresponding to 79% of the indirect tax payment. Hence, between 2004-2010, eligible manufacturing firms paid between 21-100% of the general carbon tax on their total emissions, depending on the share of fossil fuels that they used for exempted usages. The lower bound corresponds to eligible firms that only consumed fossil fuels that were covered by the exemption.[5]

Figure 1 shows the share of the general carbon tax paid by manufacturing firms by eligibility status, and how reforms increased the stringency of the tax over time. Starting in 2011, the manufacturing rebates were gradually reduced, and in 2018 they were completely removed. The phase-out of the rebates was communicated in two steps, which are outlined in Figure 2. In 2009, the government released a new plan to achieve its medium-term climate targets. The plan included an increase in the share of the carbon tax paid by industry from 21% to 30% in 2011, with an additional increase to 60% in 2015. An assessment was planned to be made in 2015 to evaluate the effectiveness and socioeconomic costs of the policy (Government Bill 2008/09:162). After elections in 2014,

[4]This condition also excludes fuels used for transportation of goods on roads.

[5]For firms regulated by the EU ETS the tax share was lower, and has been 0% since 2011 to avoid double carbon pricing (Ryner, 2022). Before 2011, energy-intensive firms with carbon tax payments exceeding 0.8% of sales could also apply for a lower rate for the excess amount (24% of the tax otherwise paid), affecting approximately 20 firms in the manufacturing sector. The threshold was increased to 1.2% in 2011, and in 2015 this option was removed (Government Bill 2009/10:41).

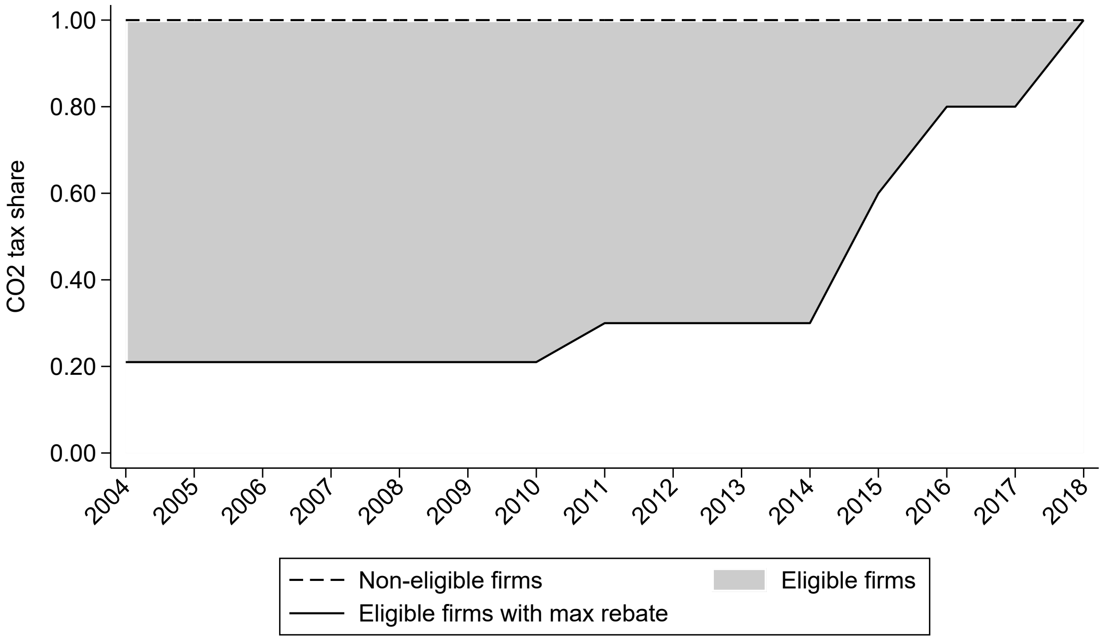


Figure 1: Possible CO_2 tax shares for firms that are either eligible (shaded area) or not eligible (dashed line) for the refund on heating fuels. Solid line represents eligible firms that only consume fossil fuels covered by the exemption, and therefore reach the lowest possible CO_2 tax as a share of the general level.

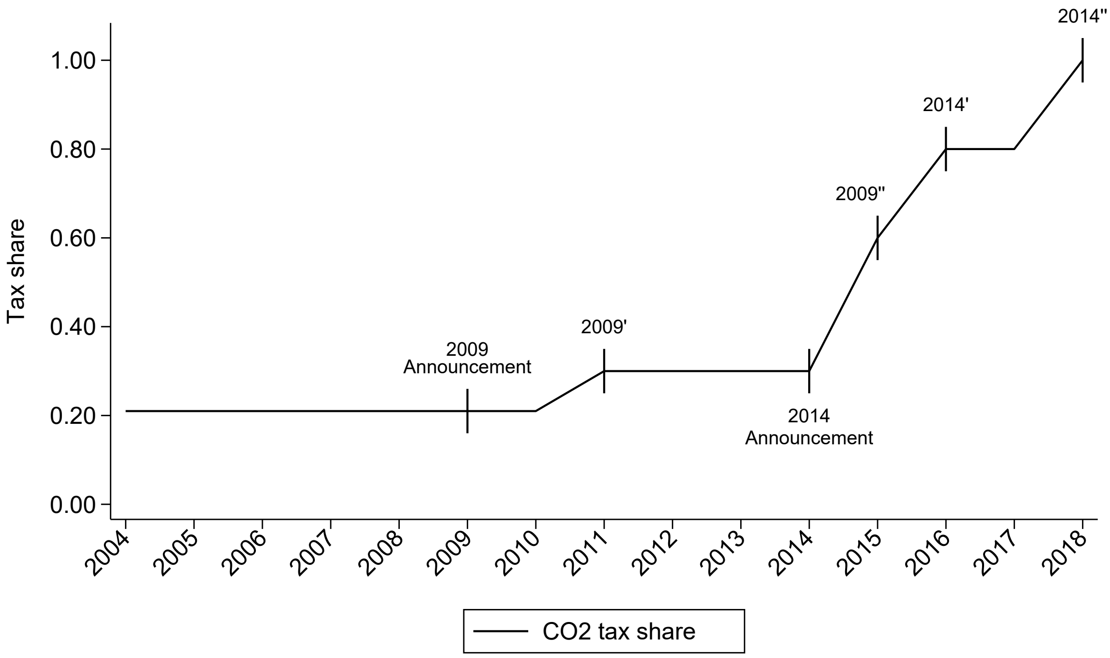


Figure 2: Timing of policy implementation. Figure 2 shows the timing of announcements of the two policy changes (in 2009 and 2014), and their corresponding increases in tax shares for eligible heating fuels.

the new government already presented an updated climate plan which mandated further emission reductions domestically (Government Bill 2014/15:1). The new plan featured an increase in the industrial tax share in 2016, and a complete phase-out of the rebates in 2018. From this year, firms in the manufacturing sector pay 100% of the carbon tax.[6]

Before 2011, manufacturing firms were also similarly exempt from energy taxation by 100%.[7] In 2011, the energy tax refund was decreased to 70%, and the level of the general tax rates were changed to better reflect the energy content of the fuels (Government Bill 2009/10:41). This change was communicated at the same time as the carbon tax reform in 2009, and the manufacturing share of the energy tax remained at the higher level (i.e. 30% of the general rate) over the time period (Skatteverket, 2018). While the energy tax reform coincided with the initial change in the carbon tax, and therefore increasing the overall stringency of the fuel taxes, Section 4 will show that the carbon tax reform had a much larger impact on firms' total tax burden.

The removal of carbon tax rebates constitutes a suitable setting to study the environmental and economic impacts of climate policy, for three reasons. First, cross-sectional variation in uptake of tax rebates before announcement means that firms were differentially exposed to the reform. Second, the fact that rebates were not only based on industry classification, but rather fuel usage, allows for a comparison of firms within the same industries, reducing the risk of confounding factors. Third, the effective tax rate increased by up to five times over the reform period for the most affected firms, meaning that the reform substantially raised the incentives for emission reduction. Hence, the removal of rebates induced meaningful and plausibly (conditionally) exogenous variation in carbon taxation stringency across firms over time.

3 Data

Data sources The sample is constructed from the Energy Use in Manufacturing survey (ISEN). It is a mandatory annual survey for all manufacturing firms with more than 9 employees, and collects information on the cost and quantity of energy consumption by fuel type. The dataset used in this paper covers the years 2004-2018, and is linked to administrative tax records, which includes information about firms' accounting. By combining fuel consumption with fuel-specific emission factors from Swedish Environmental Protection Agency (2021), I obtain firm-level annual emissions. The dataset is further

[6]Additional rebates have been directed to specific sectors. Fuels used in the production of energy products and in some metallurgical and mineralogical processes are completely exempt from carbon taxation, and fuels used for special vehicles in manufacturing in the mining industry have been subject to a lower rate (Skatteverket, 2018).

[7]This does not include the tax on electricity, which has been constant for manufacturing firms at a subsidized rate of 0.005 SEK/KWh over the time period (SFS 1994:1776).

linked to the population of Swedish individuals in working age (16-64) through their November employment (primary earnings source) in a given year, which provides information about worker characteristics. The dataset does not measure individuals' working hours at a firm. In order to get to a measure that is closer to full-time employment at a firm, I follow Graetz (2020) and categorize individuals as working in a given year only if their annual earnings from their primary employer exceed the annual price base amount.[8] To some extent, this removes a potential channel of firms' employment adjustments, if they reduce the number of low-wage part-time workers as a result of the policy change.

Importantly, the dataset is linked to a register containing information about firms' excise duty refunds, which covers the energy and carbon taxes on fuels. This dataset is available from 2008, which, combined with fuel consumption data, allows me to calculate the implied net carbon tax rate for each firm in a given year.[9] Since the majority of these firms do not pay the tax directly to the authority, I calculate indirect gross tax payments based on fuel consumption and official tax rates (in SEK per volume). From this I obtain firm-level net carbon tax rates by subtracting any deduction or refund observed in the tax register. In some cases, the resulting tax rates are negative. One potential reason for this is measurement error in the fuel consumption survey or when matching fuels to tax rates in the regulatory text, which categorizes fuels differently. A second potential reason is the possibility for firms to apply for refunds retrospectively up to three years after purchasing a fuel, which could result in an accumulation of refunds exceeding gross tax payments in some years. Related details about outliers and sample exclusion rules are described in Appendix A. [10]

Sample restriction Below I present criteria for sample restriction and descriptive statistics by treatment status, which motivates an introduction of the treatment definition. The foundation of the analysis in this paper is a comparison of firms that experienced a substantial increase in the effective carbon tax rates, with firms whose tax rates were nearly constant over the time period. The changes induced by the removal of the tax rebates thus make it possible to compare firms that had a tax rebate before the announcement of the phase-out in 2009, with firms that already paid the general tax rate. Hence, treatment is a binary indicator variable equal to 1 for firms that had a tax rebate in 2008.

In the following analysis I make use of two samples with different selection criteria. Regardless of sample, however, I remove firms that were ever regulated by the EU ETS.

[8] The price base amount ('prisbasbelopp') was 41,000 SEK in 2008, and is set by the statistical authority for various administrative purposes (SCB, 2024).

[9] The dataset misses observations for 2013.

[10] Since the main empirical analysis in the paper is based on a binary definition of treatment (having a tax rebate or not), the exact level of net tax rates will be less important for the result.

(a) CO_2 **(b)** Employment

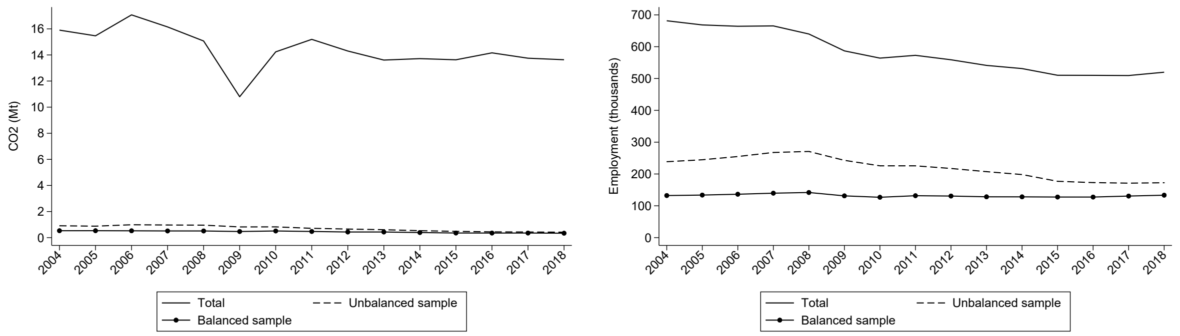


Figure 3: Aggregate manufacturing emissions and employment. Aggregates are calculated from firms in the Energy Use in Manufacturing survey (ISEN).

This is done to avoid endogenous selection in and out of regulation of the domestic carbon tax, since EU ETS firms have been subject to different rebates (and a complete exemption from the tax since 2011).[11] In the main analysis, I restrict the sample to a balanced panel of firms with observations in all years between 2004-2018. This makes it possible to evaluate differential trends between treated and control firms in the relevant outcomes before the implementation of the reform, and removes any compositional effects over time. It also allows me to further restrict the sample to firms with positive emissions in all pre-reform years 2004-2008, which increases the comparability between firms and therefore internal validity. The secondary sample is characterized by a less restrictive selection criteria. This sample consists of firms that are observed, with positive emissions, at least in 2007 and 2008.[12] The unbalanced panel is used to evaluate the sensitivity of the result to sample restriction, and to investigate compositional changes (i.e. firm exit).

Figure 3 shows the coverage of the two samples in terms of emissions and employment, in relation to all manufacturing in ISEN. Both samples constitute a small share of manufacturing emissions. This is due to the selection criteria excluding firms which were *ever* regulated by the EU ETS. Manufacturing emissions in Sweden are characterized by a heavily skewed distribution, with a strong selection of energy- and emission-intensive firms falling under the EU ETS. However, this is not the case for number of workers, where the analyzed firms make up a substantial share of manufacturing employment.

Descriptive statistics The above sample restrictions result in a dataset of 1,078 unique firms (3,026 in the unbalanced sample), of which 58% are treated. Table 1 presents

[11] This condition excludes 260 manufacturing firms, out of which 173 would have fulfilled the additional sample restrictions.

[12] Observations in 2007 are used to construct control variables related to business cycle exposure before the impacts of the financial crisis in the sensitivity analysis.

summary statistics for the main sample over 2004-2018, and tests differences in pre-reform means between the treatment and control group. All monetary variables are measured in million Swedish Krona (mSEK), except workers' annual income measured in SEK. Treated firms are on average larger in terms of value added, revenue, capital (fixed assets), and employment. The difference is, however, most pronounced when comparing CO_2 emissions. Treated firms use more fossil fuels in relation to their total energy consumption, and emit significantly more CO_2. Workers at treated firms earn significantly less, and are less likely to have obtained a high school degree. Figure 4 shows the distribution of treatment across manufacturing industries, confirming that treated firms are not clustered in specific sectors. Figure 5 shows the distribution of treated firms' carbon tax share in 2008. It reveals that the majority of firms with tax refunds received the maximum amount, resulting in an effective tax rate corresponding to 21% of the general rate. Figure A.1 in the Appendix shows that there is considerable overlap in outcomes between the control and treatment group when log-transformed.

Figure 6 plots the raw trends in average outcomes for the treatment and control group in relation to the announcement of the reform (2009) and the year of implementation (2011). A salient feature is the impacts of the financial crisis in 2009, which caused a sudden fall in firm performance. Despite differences in levels, average outcomes for the two groups run parallel over all pre-reform years, with the exception of capital. Firms in the control group seem to have a steeper increase in the leading years, which warrants extra caution when analyzing this outcome. A general sensitivity analysis with respect to influence from business cycles is carried out in Section 6.

	Mean	SD	Min	Max	Pre-reform means Treated	Pre-reform means Control	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(5) - (6)
#Firms	1,078.00	0.00	1,078.00	1,078.00	624.00	454.00	170.00
Treatment	0.58	0.49	0.00	1.00	1.00	0.00	1.00
Value added (mSEK)	101.84	427.88	-90.01	15,423.59	104.59	76.31	28.28**
Revenue (mSEK)	352.39	1,894.34	0.00	78,894.10	385.34	221.65	163.69**
Fixed assets (mSEK)	112.94	999.01	-0.01	33,733.99	101.45	59.52	41.93**
Exporter	0.86	0.35	0.00	1.00	0.86	0.83	0.03**
CO_2 emissions (ton)	416.34	1,226.05	0.00	20,617.01	732.87	144.56	588.31***
CO_2 intensity (ton/mSEK)	11.09	244.86	-2,587.34	19,847.65	33.01	4.13	28.89*
Fossil energy share	0.25	0.24	0.00	1.00	0.38	0.20	0.17***
Average income	308,794.36	59,681.30	131,600.00	631,781.86	261,534.51	269,112.04	-7,577.53***
Employment	122.49	288.21	1.00	7,398.00	143.40	104.39	39.01***
Employment: No high school	0.20	0.11	0.00	0.76	0.25	0.22	0.03***
Employment: High school	0.63	0.12	0.10	1.00	0.61	0.62	-0.02***
Employment: Above high school	0.17	0.12	0.00	0.80	0.14	0.16	-0.02***
Employment: STEM	0.11	0.10	0.00	0.77	0.09	0.10	-0.02***
Employment: Female	0.21	0.15	0.00	0.94	0.22	0.20	0.02***
Employment: Age 16-29	0.17	0.10	0.00	0.72	0.19	0.18	0.01**
Employment: Age 30-39	0.21	0.09	0.00	0.79	0.24	0.26	-0.02***
Employment: Age 40-49	0.28	0.09	0.00	0.81	0.26	0.26	-0.00
Employment: Age 50-64	0.34	0.13	0.00	1.00	0.30	0.29	0.01**
Observations	16,170						

Table 1: Descriptive statistics for the main (balanced) sample. Monetary variables are measured in million Swedish Krona (mSEK), except income, which measures workers' annual income in SEK. The average exchange rate over the period was 9.39 SEK/EUR. CO_2 intensity is measured as ton CO_2 divided by value added (in mSEK). Fossil energy share shows the firms' share of fossil fuels out of total energy consumption. Employment disaggregations represent shares of total employment at a firm in a given year. STEM shows the average share of employed workers with a higher education (above high school) in Science, Technology, Engineering and Mathematics. Column '(5) - (6)' shows differences in means in pre-reform years (2004-2008) between the treatment and control group. *** p<0.01, ** p<0.05, * p<0.1

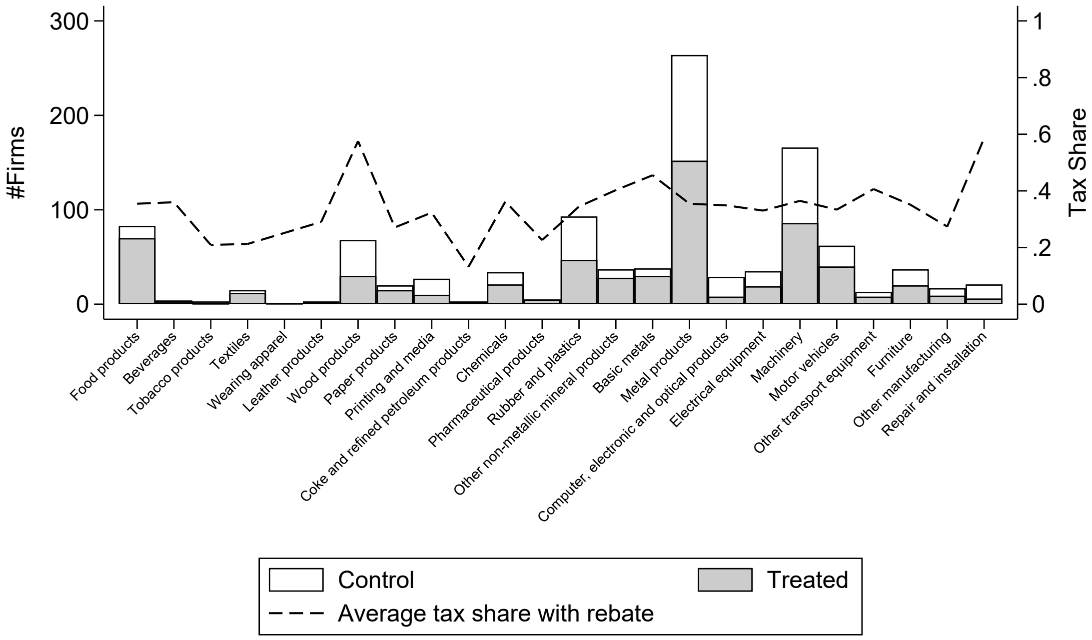


Figure 4: Treatment by industry as defined by the pre-reform uptake of CO_2 tax refunds. Dashed line represents the average tax share among treated firms within the industry.

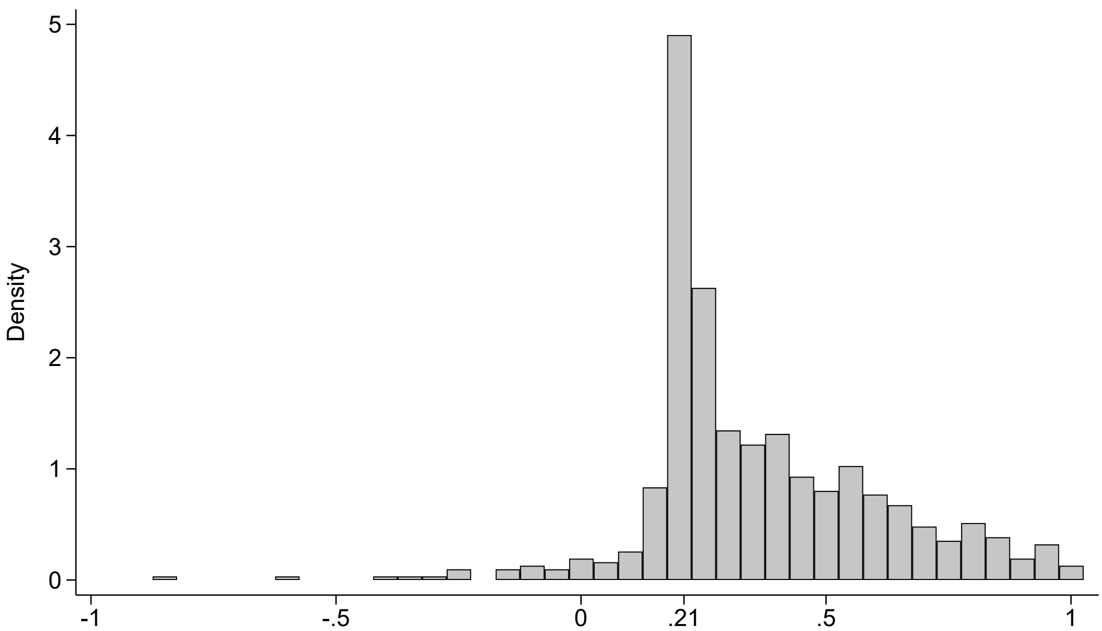


Figure 5: Distribution of pre-reform carbon tax shares among treated firms.

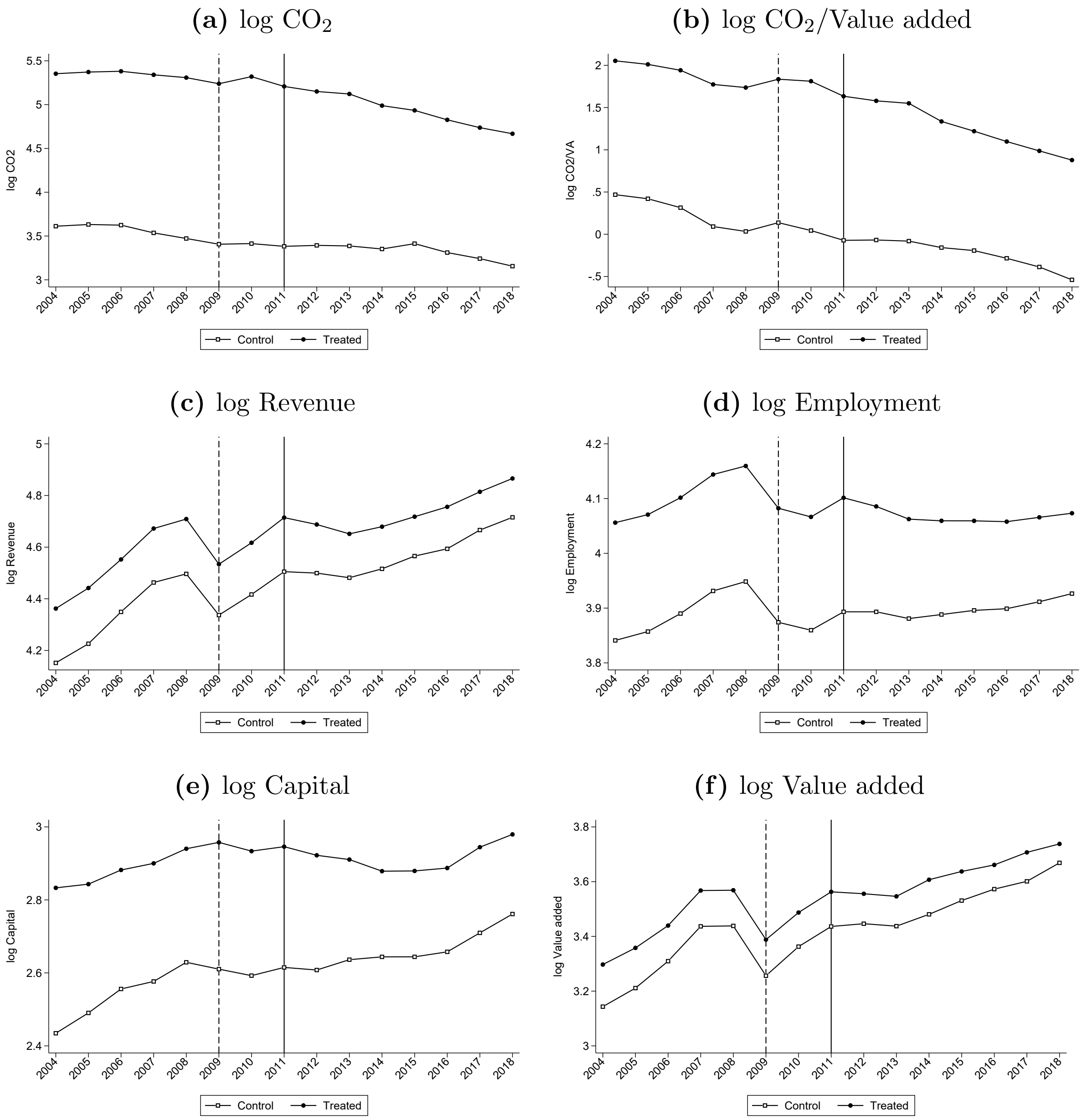


Figure 6: Raw trends in outcomes by treatment status.

4 Empirical Framework

The cross-sectional variation in tax rebate uptake creates firm-level variation in exposure to the reform. Treated firms are those whose carbon tax rebates were removed over the treatment period. I define the first year of treatment as the year of the first announcement to lower the tax rebates for manufacturing firms, which happened in 2009. The control group consists of firms that already paid the full carbon tax rate in 2008, before the rebates were phased out. These firms are arguably unaffected by the policy change.

Although Fig 5 shows that there is a distribution of pre-reform tax shares among treated firms, using the continuous variation might be problematic since it implicitly requires assuming homogeneous treatment effects across treatment dosages (Callaway et al., 2024). Hence, due to the likely selection into a specific level of pre-reform tax shares, an estimate from a continuous specification would represent a combination of the treatment effect of a unit increase in treatment dosage and heterogeneity in treatment effects across firms. Allowing for heterogeneous effects is especially important in this setting, since firms with different characteristics are expected to respond differently when subject to the same environmental regulation (Steigerwald et al., 2021). For this reason, a binary definition of treatment is preferred.

The empirical set-up is therefore a case with unequal baseline treatment status, in which treatment status differ across groups pre-reform, and (in this case) converge to the same treatment post-reform (Tazhitdinova & Vazquez-Bare, 2025). Hence, I identify treatment effects by comparing outcome trends of firms going from low to high carbon tax rates ('switchers') with outcome trends of firms that always experience high carbon tax rates ('stayers').

The analysis is based on three different Two-Way Fixed Effects (TWFE) models. The two first models employ a binary definition of treatment, which is adopted simultaneously for all treated firms. The first approach is an event-study capturing the dynamics of the estimated treatment effect between 2004-2018. It is represented by the following equation

$$\log Y_{jt} = \sum_{k=2004, k \neq 2008}^{2018} \beta^k \times \mathbf{1}(t = k) \times D_j + \eta_j + \alpha_{It} + \epsilon_{jt} \tag{1}$$

where 2008 is the omitted year of reference.[13] Y_{jt} is the outcome of firm j in year t. I control for firm fixed effects η_j and year-by-industry fixed effects α_{It} to accommodate shocks specific to industry I. Treatment D_j equals one if firm j had a carbon tax rebate in 2008. β^k captures the marginal effect of higher carbon tax stringency (through lower rebates) in year k.

[13]This means that treatment effects will be compared to differences in the year before the observed impacts of the financial crisis (see Figure 6).

The second specification is a long difference that estimates the following two-period equation

$$\log Y_{jt} = \eta_j + \Gamma_I \times Post_t + \beta^D \times D_j \times Post_t + \epsilon_{jt} \tag{2}$$

where t is either 2008 or 2018, and $Post_t = \mathbf{1}(t = 2018)$ is an indicator for the final year of the reform. $\Gamma_I \times Post_t$ is an interaction of industry indicators and the year indicator to control for industry-specific trends. β^D represents the long-difference estimate of the complete phase-out of the tax rebates. ϵ_{jt} is an error term allowed to correlate over time within firms regardless of specification.

The third specification is a version of Eq. (2), which also allows for variation in treatment *intensity*:

$$\log Y_{jt} = \eta_j + \Gamma_I \times Post_t + \sum_{p=1}^{6} \beta^p \times d_j^p \times Post_t + \epsilon_{jt} \tag{3}$$

Here, D_j is replaced by dosage d_j, which is a measure of the firm's increase in net carbon tax rate between 2008-2018. In order to avoid imposing a linear relationship between outcome and treatment, and the issues with continuous treatment mentioned above, I define d_j as a multi-valued, discrete variable, containing the ranking of the firms' treatment intensity. Hence, I divide d_j into 6 equally sized groups, with values ranging from 1 to 6, where d_j^p is an indicator variable for firm j's membership in group p. Interacted with $Post_t$, the group indicators capture the effect at different levels of treatment intensity in a non-parametric way. They are estimated using the same excluded control group as the previous specifications, and are therefore compared to firms that did not experience a change in their carbon tax rate ($p = 0$).[14] Firms in group $p = 6$ have the highest treatment intensity, although one should keep in mind the slight abuse of notation when defining intensity based on the size of *change* in treatment status from low to high.

Both approaches require a number of assumptions for the treatment effects to be identified, some of which have been outlined in the recent econometrics literature. Perhaps the most standard are the assumption of 'No Announcement' and the Stable Unit Treatment Value Assumption (SUTVA) (de Chaisemartin & D'Haultfœuille, 2025). The first one assumes that firms did not anticipate the reform, which is why the starting year of treatment is defined as the year the reform was first *announced*. The second assumption rules out spillover effects between treatment and control firms. Since treatment is fixed at the level of the firm based on their 2008 status, control firms could not 'become treated'

[14] Hence, there are 454 firms in $p = 0$, and 104 firms in each treated group $p \in \{1, 2, 3, 4, 5, 6\}$.

over time, thus ruling out one channel of potential spillovers. However, this condition also assumes that there are no spillovers from general equilibrium effects.

Tazhitdinova and Vazquez-Bare (2025) formalizes the parallel-trends assumptions needed when treatment and control groups have unequal baseline treatment status. Using the terminology of stayers and switchers, where stayers are always-treated by a higher tax, identification requires two set of assumptions. The first is the 'canonical' (conditional) parallel trends assumption of Difference-in-Differences (DiD), in which the trends in outcomes between any two time periods are parallel between stayers and switchers in the counterfactual scenario where *neither group is treated by a high tax rate*. The second set of assumptions requires 'time-invariant and non-cumulative effects' for stayers.

In practice, the additional assumptions outlined by Tazhitdinova and Vazquez-Bare (2025) put restrictions on the dynamics of the effect of the carbon tax for the control group. The time-invariance of the effect is needed to make the stayers a suitable control group in any year.[15] There is no clear feature of the regulatory context surrounding the carbon tax suggesting that this assumption would not hold, especially considering that the general tax rate only saw a moderate increase over the time period. Assuming non-cumulative effects is in turn needed to avoid a bias from a change in the treatment effect on the stayers in the post-reform years. It is expected that long-term exposure to high carbon taxation incentivized firms to invest in cleaner production (e.g. by switching fuels), and that this effect would be characterized by an adjustment period. However, given that control firms experienced the high carbon tax rate for several years before the reform, it is likely that they had sufficient time to adjust to their long-run state within the pre-reform years. For these reasons, it is expected that any violation to this assumption would be observed in a test of pre-reform trends (Tazhitdinova & Vazquez-Bare, 2025).

The assumptions relate to the recent literature on treatment effect heterogeneity and 'forbidden comparisons' in DiD settings (Baker et al., 2025). While the estimation procedure compares just-treated with always-treated, the simultaneous adoption ensures that switchers are only compared to stayers, avoiding issues of negative weights (Tazhitdinova & Vazquez-Bare, 2025). Hence, under the assumptions above, the TWFE models estimate the average treatment effect on the treated (ATT) of a higher carbon tax on the switchers, without having to restrict heterogeneity in treatment effects.

[15] The reason for this is that the observed outcome for stayers used in the regression is conceptually considered as the sum of the potential outcome of the stayers under a low carbon tax and the corresponding ATT of a high carbon tax for stayers in a given year.

5 Result

5.1 Was the carbon tax reform salient?

Before going to the main result, I present some 'first stages' on the economic burden of firms induced by the reform. Figure 7a plots the average calculated net carbon tax shares for the treatment and control group, respectively, over 2008-2018. The average trend for treated firms mirrors the policy change in Figure 2, while the average share for control firms is constantly close to 1, thus validating the treatment assignment. Figure 7b shows the corresponding trends for the energy tax, which exhibits an expected jump in 2011 for the treatment group.

However, Figure 7c and 7d show that the change in the carbon tax put a much larger economic pressure on affected firms in relation to their total costs. The figures show the calculation of firms' respective net tax payments divided by total costs, keeping quantities (i.e. emissions or MWh) and total costs fixed at 2008 levels.[16] The graphs indicate that the cost share of treated firms' indirect carbon tax payments would have increased from around 0.1% to 0.4%, without adjusting to the new tax rate. Figure 7c also reveals that the tax rebates were effective in leveling out the carbon tax burden between more or less emission-intensive firms in the sample before the reform, as average costs shares are similar for treatment and control firms in 2008-2009. In addition, the relatively flat lines for control firms in Figure 7a and 7c support the assumption that 'stayers' experienced a constant treatment of the carbon tax over the time period in the data.

While the change in the energy tax share for treated firms led to a small increase in the economic burden of energy consumption (in relation to how much they consumed before the reform), they were substantially more exposed to the change in the carbon tax. Hence, any estimated effects of the reform is likely to be driven by the change in carbon taxation, due to its larger impact on overall costs (output effect), and the stronger incentives to switch to low-carbon production (substitution effect).

5.2 Average effects of carbon taxation

This section presents the results on the average effects of carbon taxation on emissions, firm performance, and labor demand, using the event-study and the long-difference specification. Table 2 contains the result from estimating the long-difference model of Eq. (2). The result in column (1) shows a significantly negative treatment effect on emissions by 30.3% in 2018 (-0.361 log points). As expected, raising the effective carbon tax rates for firms seems to be an effective tool to incentivize emission abatement. Column (2) also

[16] Specifically, the (counterfactual, in absence of response to treatment) costs shares are obtained from Cost share_{jt} = Net tax $\text{rate}_{jt} \times Q_j^{2008}/\text{Total cost}_j^{2008}$, with Q_j^{2008} being the firm's emissions or energy consumption in 2008.

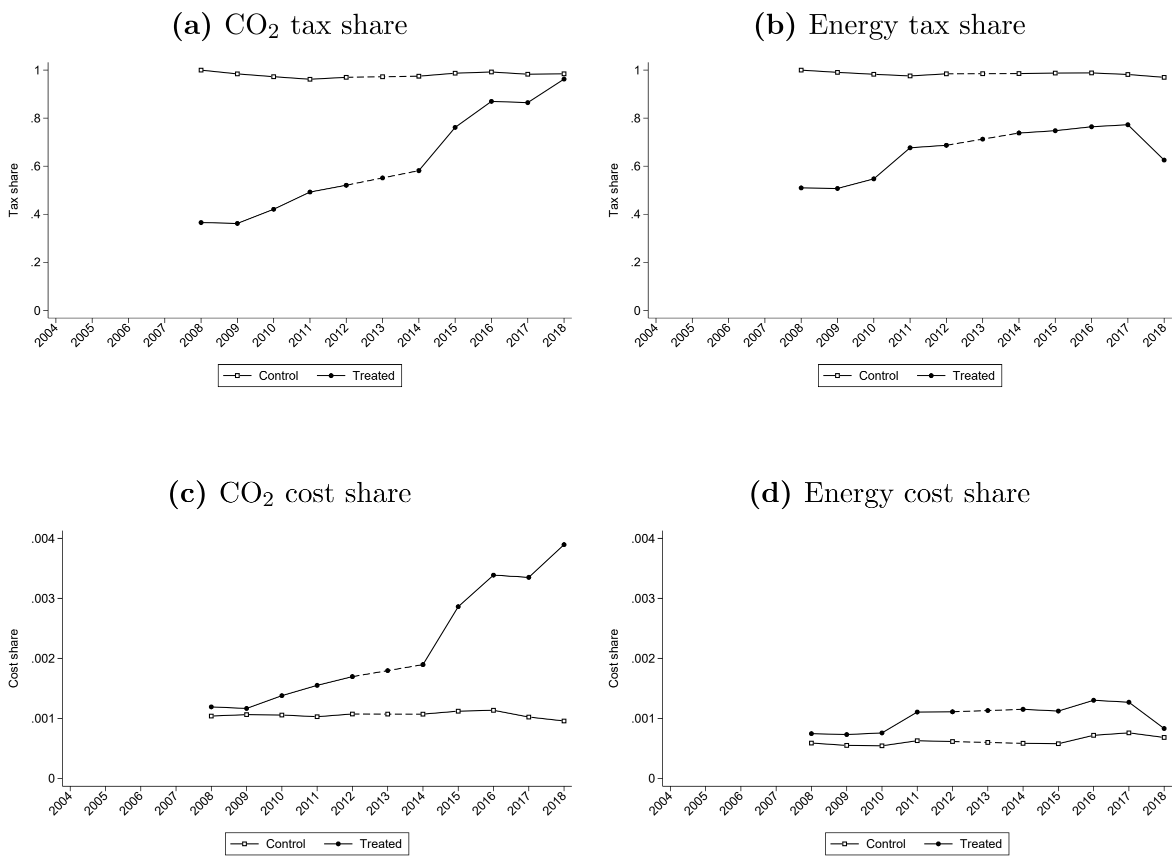


Figure 7: Firm-level fuel tax and cost shares by treatment over time. Treatment is based on CO_2 tax refund uptake in 2008. CO_2 tax shares are calculated by combining each fuel's gross CO_2 tax rate with firms' fossil fuel consumption and CO_2 tax refunds. Energy tax shares are calculated by combining each fuel's gross *energy* tax rate with firms' energy consumption and energy tax refunds. Figure 7c and 7d show the economic exposure to the respective tax change, by multiplying the time-varying net tax rate with a fixed ratio of quantity (emissions or MWh energy) to total cost, based on 2008 levels. Data for 2013 is missing and is therefore imputed.

	log CO_2	log CO_2/VA	log Revenue	log Employment	log Capital	log VA
	(1)	(2)	(3)	(4)	(5)	(6)
$D \times Post$	-0.361***	-0.323***	-0.059*	-0.070***	-0.094	-0.053
	(0.100)	(0.107)	(0.032)	(0.026)	(0.069)	(0.036)
$e^{\beta} - 1$	-0.303	-0.276	-0.057	-0.067	-0.090	-0.052
Observations	1,840	1,824	2,148	2,154	2,138	2,134

Table 2: Average effects from the long difference specification of Eq (2) using a balanced panel of firms. Standard errors in parenthesis are clustered by firm. *** p<0.01, ** p<0.05, * p<0.1

shows a similar, significant effect on emission *intensity*. This suggests that the emission reductions are not mainly driven by a contraction of output, in line with previous research (Colmer et al., 2024; Dechezleprêtre et al., 2023; Martin et al., 2014). In contrast to the majority of previous literature, however, I also find significantly negative effects on economic outcomes. Treated firms experience significant reductions in both revenue and employment compared to control firms. The estimated average effects correspond to -5.7% and -6.7%, respectively, although the effect on revenue is less precisely estimated. I do not find any significant effects for capital or value added.

Figure 8 shows the dynamic effects from the event-study model. As seen in Figure 8a, the fall in emissions among treated firms starts in 2011, which is the first year of higher net tax rates, and becomes significant in 2012. The negative effect on emissions increases further in 2014, when the second part of the tax rebate phase-out was announced, and is stabilized at a large, negative effect at the end of the time period. The reduction in revenue and employment is less drastic, and is characterized by a smoother adjustment to lower levels. Reassuringly, I do not find significant differential trends in outcomes before the reform except for capital, which was already observed in the raw data. The parallel trends in outcomes is in line with the assumptions outlined in Section 4, and strengthen the interpretation of the estimated effects as causal effects of carbon taxation.

5.3 Heterogeneous effects on firms and workers

Firm heterogeneity This part explores heterogeneity in the average effects along different characteristics. I focus on two dimensions of reform exposure, which are treatment intensity, as defined in Section 4, and emission intensity. Emission-intensive firms might differ in the response to carbon taxation, for several reasons. While the substitution effect is determined by the shape of the individual firm's marginal abatement cost curve, which is unobserved, emission-intensive firms are more financially exposed to a higher carbon tax. Hence, emission-intensive firms are more likely to experience a negative output effect due to increasing cost of production.

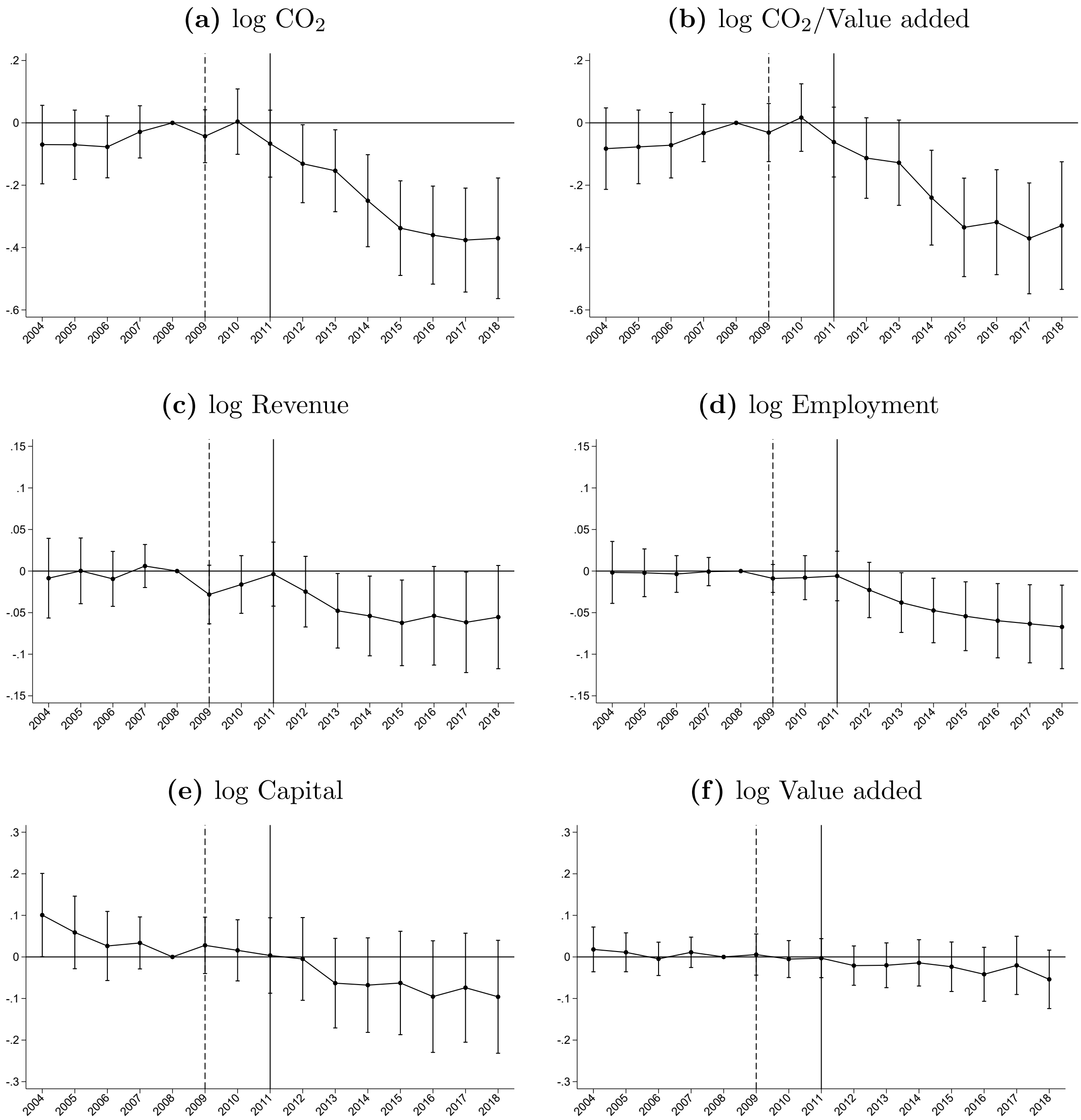


Figure 8: Event study results from estimating Eq. (1) on a balanced panel of firms. Regressions include industry-by-year fixed effects, and standard errors are clustered by firm. Capped spikes show 95% confidence intervals.

Starting with investigating heterogeneous responses with respect to different intensities of treatment, I estimate Eq. (3) for three outcomes with significant effects in the previous subsection, namely emissions, revenue and employment. The result is presented in Figure A.2. The estimated effects on emissions are significantly negative for almost all levels of treatment intensities, indicating that the relative emission reductions due to the reform are not functions of the size by which tax rates increased at the intensive margin.

The heterogeneous effects on revenue and employment exhibit a different pattern, as the negative effects are driven by firms with above average treatment intensity ($d > 3$). For these firms, estimated treatment effects are approximately -11%. This result is consistent with a higher cost exposure for these firms, leading to stronger negative output effects. The constant effects on emissions for low and high treatment groups suggest, again, a strong substitution effect towards cleaner production, since low treatment firms reduce their emissions without reducing output. While it is not possible to completely rule out substitution effects driving the reduction in employment, observing the same effect for revenue (which is strongly connected to output) and employment (an input), points to a negative output effect driving the effects on labor demand.

For the remainder of the analysis I will use the fact that the heterogeneity in treatment effects with respect to treatment intensity is well captured by dividing treated firms into two groups. Hence, in the following I define *treatment intensive* as a firm with treatment above average, i.e. with $d > 3$.

In order to investigate treatment effect heterogeneity along emission intensity, I construct the variable $CO_2\ int._j$ which equals 1 for firms with a CO_2 intensity (in terms of value added) above the 2-digit industry median in 2007. Columns (1)-(3) in Table 3 contain the effects on emissions, revenue and employment from estimating Eq. (2) for different subsamples. Panel A repeats the result for the full sample of firms, while Panel B restricts the sample to emission-intensive firms. Emission-intensive firms have slightly larger estimated effects on emissions, and substantially larger negative effects on revenue and employment of -10.2% and -12.4%, respectively. Panel C repeats the results for the subsample of treatment-intensive firms, again showing larger effects on revenue and employment (but slightly lower effects on emissions) compared to the full sample of firms. Panel D combines the two dimensions of heterogeneity and restricts the sample to emission-intensive control firms, and emissions- *and* treatment-intensive treated firms. The result shows that combining the two types of exposure to the reform leads to a negative interaction, increasing the negative impacts on revenue and employment even further (-13.7% and -14.4%, respectively). Table A.3 shows the same analysis by interaction terms with the treatment variable instead of sample restrictions, in which the effects on revenue and employment are significantly different for both treatment-intensive and

	log CO_2	log Revenue	log Employment			
			All	No high school	High school	Above high school
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: All firms			
D × *Post*	-0.361***	-0.059*	-0.070***	-0.137***	-0.037	-0.047
	(0.100)	(0.032)	(0.026)	(0.042)	(0.028)	(0.041)
Observations	1,840	2,148	2,154	2,070	2,154	1,972
			Panel B: Emission-intensive firms			
D × *Post*	-0.421***	-0.107**	-0.133***	-0.224***	-0.073	-0.171**
	(0.133)	(0.048)	(0.043)	(0.076)	(0.050)	(0.080)
Observations	932	1,054	1,060	1,018	1,060	952
			Panel C: Treatment-intensive firms			
D × *Post*	-0.256**	-0.110***	-0.109***	-0.146***	-0.067*	-0.108**
	(0.125)	(0.039)	(0.033)	(0.053)	(0.035)	(0.050)
Observations	1,272	1,524	1,530	1,470	1,530	1,398
			Panel D: Emission- & treatment-intensive firms			
D × *Post*	-0.365**	-0.147**	-0.156***	-0.225***	-0.071	-0.243***
	(0.160)	(0.057)	(0.052)	(0.085)	(0.057)	(0.091)
Observations	576	664	670	648	670	592

Table 3: Long difference results on employment from estimating Eq. (2) using a balanced panel of firms. Panel A includes the full sample, and Panel B only includes emission-intensive firms, as defined in the text. Panel C includes the full set of control firms, but only treated firms with a treatment intensity above average ($d > 3$). Panel D includes emission-intensive control firms and emission- *and* treatment-intensive treated firms. Standard errors are clustered by firm. *** p<0.01, ** p<0.05, * p<0.1

emission-intensive firms, and that the effects for firms with low treatment and emission intensity are close to zero.

Worker heterogeneity This part explores heterogeneity in labor demand effects for different types of workers. Table 4 presents heterogeneous effects on employment with respect to educational attainment, gender, and age. Starting with education, column (1) contains treatment effects for three different categories, which are 'No high school', 'High school', and 'Above high school'.[17] I find that the average effect on firms' total number of workers from previous sections masks heterogeneous impacts along this dimension. Column (1) in Panel A shows that treated firms experience a reduction in the number of workers without a high school degree by 12.2% after the reform, with smaller and insignificant effects for workers in the higher education categories. This effect exists both

[17]The workers are categorized by their highest obtained degree.

for men and women, where the average effect on low-education workers is similar to the effect on men, reflecting their larger manufacturing employment share in the sample. Columns (4)-(7) present a further break-down of the estimated treatment effects for separate age groups within educational attainment. In Panel A, the effect is significant for workers between 40-49 and 50-64 years old. For the other panels, estimates are smaller and insignificant, suggesting that the reform mainly had a negative effect on labor demand for workers between 40-64 years old without a high school degree.

Next, I combine the heterogeneity analysis with respect to workers' education and the previous analysis with respect to firms' emission and treatment intensity. Columns (4)-(6) in Table 3 show the effect of carbon taxation on employment for the three educational categories for each firm subsample. Panel A again repeats the result for the full sample of firms, where the negative effect on employment is entirely driven by workers without a high school degree. For emission-intensive firms in Panel B, the effect is larger for low-educated workers (-20.1%), but also significantly negative for workers with an academic degree above high school (-15.7%). The pattern is similar for treatment-intensive firms in Panel C, with slightly lower magnitudes. For the most exposed firms in Panel D, who are both emission- and treatment-intensive, the effect on low-educated and high-educated workers is practically the same (-20.1% and -21.6%). Hence, the results point to a skill-biased effect of carbon taxation, which in itself is heterogeneous across different types of firms.

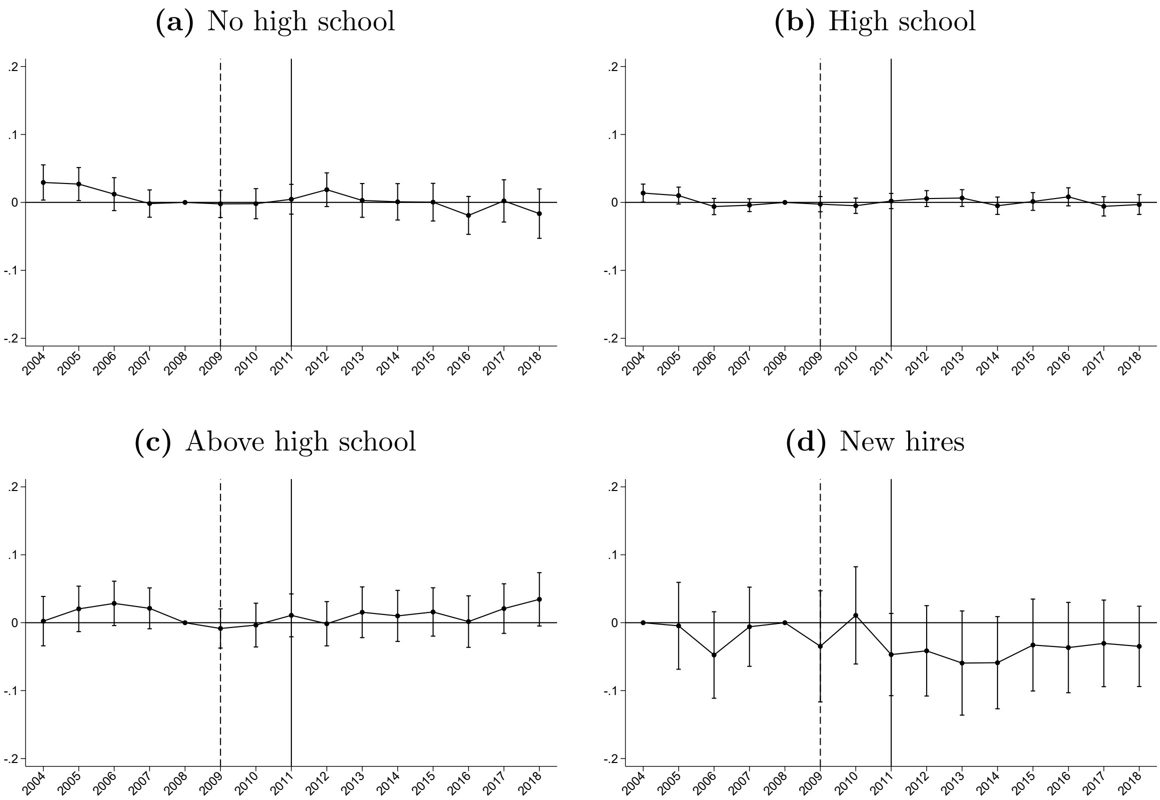
Completing the analysis on labor demand, I study the effects of the carbon tax reform on workers' income. Figure 9 presents the results from estimating Eq. (1) on annual income from workers' primary earnings, averaged to each firm-year. Figure 9a to 9c show that the reform did not have large effects on income for incumbent workers in any of the educational groups. I also estimate the effect separately on the average income of new hires, as this is a group whose wages are likely to be adjusted more quickly after a shock, compared to incumbent workers (Marinescu et al., 2021). The result is presented in Figure 9d. While the point estimates indicate a negative trend in income of new hires at the implementation of the reform, they are statistically insignificant.

5.4 Mechanisms and margins of adjustment

Labor turnover and firm exit In order to understand the margins of adjustments behind the negative effects on employment, I disentangle the changes in employment by firms' hiring and separation rates. The result from the event-study estimation is presented in Figure 10a and 10b. The point estimates are negative for hiring rates after 2011 and close to zero for separation rates. This suggests that firms are adjusting their labor force by reducing their hiring rate instead of increasing separation. Figure A.4

	log Employment						
				Age			
	All	Male	Female	16 - 29	30 - 39	40 - 49	50 - 64
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: No high school						
$D \times Post$	-0.137***	-0.134***	-0.122**	-0.051	0.057	-0.141**	-0.146***
	(0.042)	(0.043)	(0.061)	(0.084)	(0.080)	(0.071)	(0.048)
Observations	2,070	2,018	1,180	984	1,024	1,240	1,866
	Panel B: High school						
$D \times Post$	-0.037	-0.034	-0.050	-0.068	-0.019	-0.047	-0.065
	(0.028)	(0.029)	(0.045)	(0.059)	(0.057)	(0.048)	(0.045)
Observations	2,154	2,152	1,886	1,844	1,912	2,066	2,074
	Panel C: Above high school						
$D \times Post$	-0.047	-0.068	0.029	-0.134*	0.094	-0.055	-0.035
	(0.041)	(0.043)	(0.056)	(0.077)	(0.069)	(0.059)	(0.055)
Observations	1,972	1,896	1,342	956	1,350	1,388	1,458

Table 4: Long difference results on employment from estimating Eq. (2) using a balanced panel of firms. Standard errors in parenthesis are clustered by firm. *** p<0.01, ** p<0.05, * p<0.1



(a) No high school

(b) High school

(c) Above high school

(d) New hires

Figure 9: Event study results on workers' log average annual income from estimating Eq. (1) using a balanced panel of firms. Regressions include industry-by-year fixed effects, and standard errors are clustered by firm. Capped spikes show 95% confidence intervals.

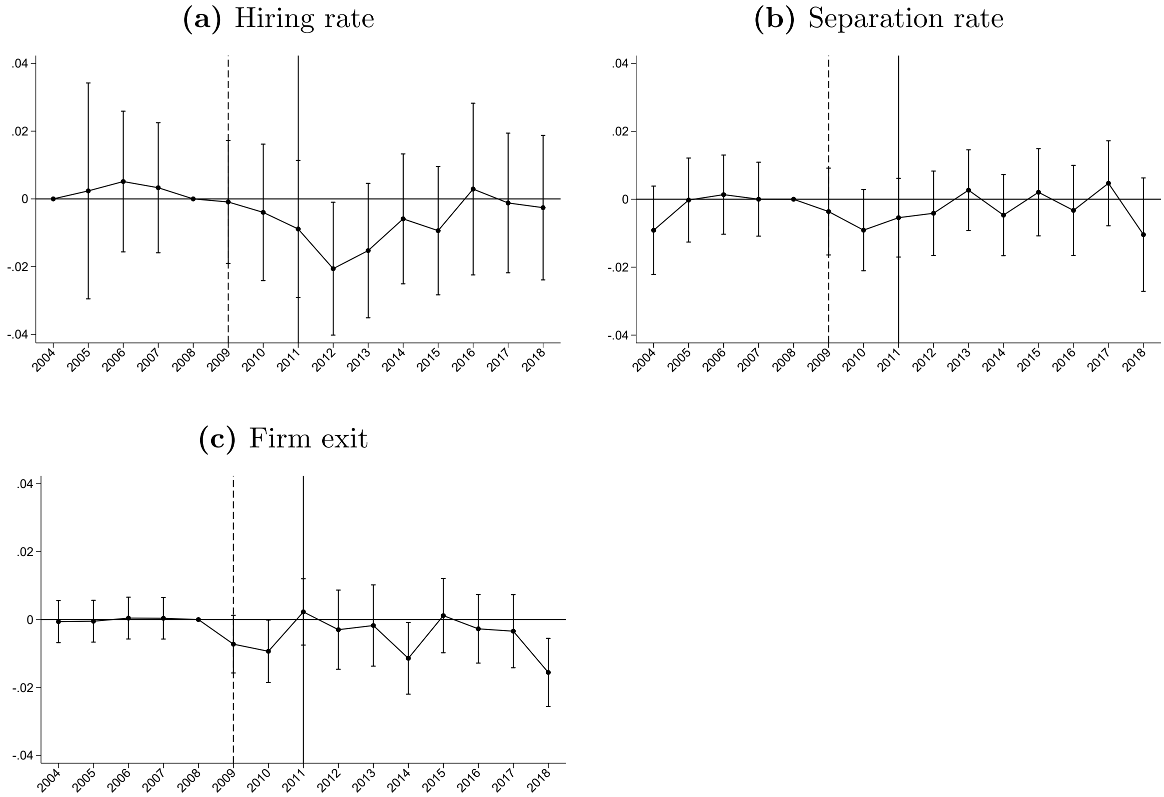


Figure 10: Event study results on labor turnover and firm exit from estimating Eq. (1). Figure 10a and 10b use a balanced sample of firms, while Figure 10c is estimated using an unbalanced sample. Regressions include industry-by-year fixed effects, and standard errors are clustered by firm. Capped spikes show 95% confidence intervals.

shows a similar pattern for low-educated workers and workers between 40-64 years old, although the statistical uncertainty is large.

Figure 10c shows the effects on firm exit, by estimating the event study specification using the unbalanced sample and a binary outcome variable.[18] The reform does not seem to have had a large impact on firms exiting the market, which implies low compositional effects due to the reform.

Fuel-switching Figure 11 explores the role of fuel-switching as a mechanism behind the estimated emission reductions. I categorize fuels as fossil fuels if they have a positive emission factor, excluding biogenic carbon emissions. The remaining fuels are categorized as biofuels, except for electricity.[19] All fuels are measured in MWh. The results in Figure 11a and 11b show clear evidence of firms substituting fossil fuels for biofuels as a response to the carbon tax increase. Consumption of the two categories changes by a similar magnitude (-29.5% and 33.8%, respectively), with opposite signs. Figure 11c

[18]The outcome variable equals 1 when the current year is the last year that the firm is observed in the data.

[19]Electricity production in Sweden is close to completely fossil-free (Energimyndigheten, 2025).

shows suggestive evidence of a lower electricity consumption. While not targeted by the carbon tax reform, this could be explained by a general energy efficiency optimization or lower production. Figure 11d shows the resulting effect on total energy use, which is significantly reduced by 8.4% at the end of the period. However, parts of this could be explained by a differential pre-trend. The effect on energy *intensity*, defined as total energy divided by value added, is small and negative after the reform but imprecisely estimated. Figure 11f relates to the initial descriptive results, and shows the estimated effects on (indirect) carbon tax payments, as a share of total costs. In line with the previous results in this paper, treated firms were not able to fully escape the additional economic burden induced by the removal of the carbon tax rebates, which increases approximately by 0.1 percentage points. The gray line represents the estimated effects on the same outcome, but keeping emissions fixed at 2008 values when calculating the cost shares. The difference between the two lines shows that the economic burden would have been clearly higher without the emission reductions.

5.5 Semi-elasticities of carbon taxation

In order to better understand the magnitude of the main results in this paper, I present the coefficients as semi-elasticities, corresponding to the relative effect of a 1 EUR/ton CO_2 increase. Relying on similar assumptions for identification as in the binary Difference-in-Differences model of Eq (2), I predict firms' average change in carbon tax rates (γ) using the previous treatment definition D_j, conditional on industry fixed effects. The predicted tax rate change is used in a second stage to estimate the relative effect on emissions and employment (ϕ), in units of the average tax rate increase. In the two-period model, I estimate the following instrumental variable regression:

$$\Delta \mathrm{CO_2TAX}_j = \Gamma_I + \gamma D_j + \Delta v_j \tag{4}$$

$$\Delta \log Y_j = \Gamma_I + \phi \widehat{\Delta \mathrm{CO_2TAX}}_j + \Delta \varepsilon_j \tag{5}$$

where $\Delta \log Y_j$ is the 2008-2018 change in $\log Y_j$ for firm j, Γ_I are industry fixed effects, and $\Delta \mathrm{CO_2TAX}_j$ is the 2008-2018 change in firm j's effective carbon tax rate. An event-study version of the first-stage estimates is presented in Figure A.3, showing that the increase in effective carbon tax rates is precisely estimated over time. The second-stage result is presented in Table 5. Panel A contains the result for the main sample of firms, where the first stage coefficient $\widehat{\gamma}$ shows that treated firms on average experienced an increase of their effective carbon tax rate by around 66 EUR/ton CO_2, compared to

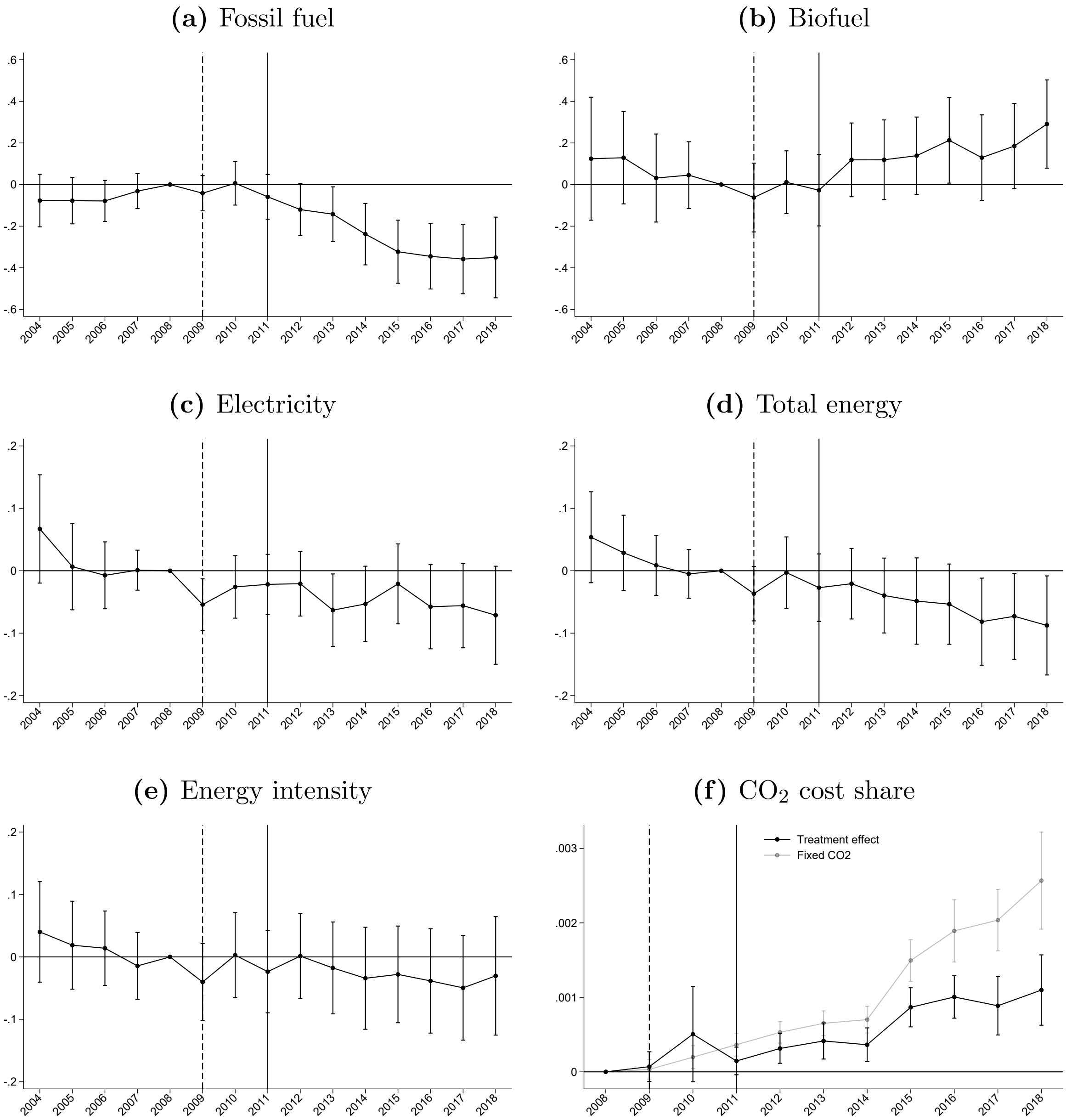


Figure 11: Event study results on the log of firms' energy consumption in MWh from estimating Eq. (1) using a balanced panel of firms. Energy intensity is measured in MWh/mSEK value added. Figure 11f shows the effect on indirect carbon tax payments as a share of total costs, where the gray line keeps emissions fixed at 2008 values. Regressions include industry-by-year fixed effects, and standard errors are clustered by firm. Capped spikes show 95% confidence intervals.

the control group. An estimated $\widehat{\phi}$ of -0.0058 suggests that emissions fall by 0.58% for each EUR/ton increase in the carbon tax. The corresponding semi-elasticity of total employment is -0.10% and significant, and employment of workers without a high school degree is estimated to fall by 0.20% for each EUR/ton. Panel B restricts the sample to firms with an emission intensity above the industry median in 2007. The predicted $\widehat{\gamma}$'s indicate that emission-intensive firms were exposed to a slightly higher tax rate shock, with a similar semi-elasticity of emissions of -0.56%. However, since the majority of treated firms are emission-intensive (67%), their treatment effects are likely estimated more precisely, and constitute a larger share of the average effect. Employment impacts are generally twice as large per EUR/ton increase for these firms compared to the full sample. The semi-elasticities for total employment and the low-educated group is -0.18% and -0.31%, respectively.

Panel C restricts the analysis to treatment-intensive firms ($\widehat{\gamma} = 91$), which indicates a (between firms) marginally decreasing effect of carbon taxation on emissions, with a lower estimated semi-elasticity of -0.32. Panel D shows estimated elasticities for emission- and treatment-intensive firms, who experienced an average tax increase by 96 EUR/ton. The semi-elasticity of -0.34 again shows a decreasing effect of carbon taxation on emissions for emission-intensive firms, as the effect per euro is lower for higher levels of tax increases. The effects on employment show smaller signs of nonlinearities, with semi-elasticities that are similar or only slightly lower.[20]

6 Robustness Checks

In this section, I explore the robustness of the results to various sample restrictions and additional controls. One potential confounder could be a differential exposure for treated and control firms to economic shocks during the reform period. For example, treated firms are larger and has a larger probability of being an exporter, and could therefore be more exposed to various business cycle fluctuations.

In addition to the industry-year fixed effects already included in the baseline regressions, I construct variables related to three dimensions of firms' exposure to business cycles, namely export share of sales (EX_j'), employment size (L_j'), and capital size (K_j). EX_j' is a vector of two indicator variables, which equal to 1 if the firm's exports as a share of total sales in 2007 is in the range $(0\%, 50\%)$ or $>= 50\%$, respectively. L_j' is also a vector of two indicator variables, which equal 1 for firms whose number of employees in 2007 is in the range $(49, 250)$ or $>= 250$, respectively. K_j is an indicator variable equal to 1 if the firm's fixed assets in 2007 exceed the 2-digit industry median for that

[20]The emission coefficient (and standard error) for low-emission firms is -0.0021 (0.0026) with $\widehat{\gamma} \approx 64$, and -0.0088 (0.0027) for low-treatment firms, with $\widehat{\gamma} \approx 44$.

	Δ log CO_2	Δ log Employment			
		All	No high school	High school	Above high school
	(1)	(2)	(3)	(4)	(5)
			Panel A: All firms		
$\widehat{\Delta CO_2 TAX}$	-0.0058***	-0.0010***	-0.0020***	-0.0005	-0.0008
	(0.0016)	(0.0004)	(0.0006)	(0.0004)	(0.0006)
$\widehat{\gamma}$	64.89	66.07	66.73	66.07	65.26
F-stat	766.80	1,080.79	1,045.16	1,080.79	948.33
Observations	920.00	1,077.00	1,035.00	1,077.00	986.00
			Panel B: Emission-intensive firms		
$\widehat{\Delta CO_2 TAX}$	-0.0056***	-0.0018***	-0.0031***	-0.0009	-0.0023*
	(0.0019)	(0.0006)	(0.0011)	(0.0007)	(0.0012)
$\widehat{\gamma}$	68.91	69.84	70.85	69.84	67.18
F-stat	185.13	236.64	225.38	236.64	199.96
Observations	466.00	530.00	509.00	530.00	476.00
			Panel C: Treatment-intensive firms		
$\widehat{\Delta CO_2 TAX}$	-0.0032**	-0.0012***	-0.0015***	-0.0007*	-0.0011**
	(0.0014)	(0.0004)	(0.0006)	(0.0004)	(0.0005)
$\widehat{\gamma}$	91.00	90.29	90.59	90.29	90.33
F-stat	2,409.93	3,439.44	3,132.68	3,439.44	3,072.21
Observations	636.00	765.00	735.00	765.00	699.00
			Panel D: Emission- & treatment-intensive firms		
$\widehat{\Delta CO_2 TAX}$	-0.0034**	-0.0016***	-0.0023***	-0.0008	-0.0024**
	(0.0016)	(0.0005)	(0.0009)	(0.0006)	(0.0010)
$\widehat{\gamma}$	96.93	95.79	96.42	95.79	95.10
F-stat	371.85	453.99	418.04	453.99	402.78
Observations	288.00	335.00	324.00	335.00	296.00

Table 5: Semi-elasticities with respect to a 1 EUR/ton CO_2 increase from estimating Eq. (5) for different subsamples. Panel A includes the full sample, and Panel B only includes emission-intensive firms, as defined in the text. Panel C includes the full set of control firms, but only treated firms with a treatment intensity above average ($d > 3$). Panel D includes emission-intensive control firms and emission- *and* treatment-intensive treated firms.

year. These variables are interacted with the year fixed effects (or *Post*), to allow for separate, non-parametric time trends along these dimensions. This will, for example, capture different exposures to exchange rate fluctuations for exporting versus non-exporting firms, or different trends for small and large firms, within 2-digit industries. Figure A.5 presents the event study result with the additional controls. The main results are robust to controlling for these trends, as the estimated treatment effects across years are very similar for all outcomes. The point estimates are similarly unaffected when estimating the long difference model, which is shown in Table A.2.

Figure A.7 presents the same sensitivity check for employment, disaggregated by education. The results are again unaffected by controlling for business cycle exposure for all subgroups. The heterogeneous event-study estimates also show that there are no clear differential trends prior to the reform, confirming the previously observed parallel trends for overall employment.

Since the geographical distribution might differ for treated and control firms, there is a potential risk of confounding effects from region-specific shocks. Figure A.8 presents the result from Eq. (1) including region-by-year fixed effects, to accommodate yearly shocks that differentially affect regions with a higher concentration of treated firms. The result is very similar to the baseline result. If anything, point estimates are slightly larger, and the treatment effects on revenue are estimated with larger precision.

The baseline results are mostly well replicated using the unbalanced sample. The result is presented in Figure A.6. However, there is a differential pre-trend for most outcomes, which is likely driven partly by compositional changes. For example, since treated firms are on average larger, and more likely to survive (and therefore enter the sample earlier), it is expected that there are heterogeneous compositional changes between treated and control firms. Nevertheless, the result shows similar significant reductions in emissions and emission intensity in the reform period as with the balanced sample. Impacts on revenue and employment are similar in the reform period, although estimated with less precision, and the effect on revenue disappears in 2018. In general, however, the sensitivity analysis in this section produces similar results as in the main part of the paper.

7 Discussion and Conclusion

In this paper, I provide new empirical evidence on the impacts of carbon taxation on firm performance and labor demand, using a quasi-experimental framework and rich administrative datasets. I find that higher stringent carbon taxation, induced by the removal of carbon tax refunds, significantly reduced emissions among Swedish manufacturing firms. I also find negative effects on revenue and employment, which are especially concentrated among emission-intensive firms. While a part of the emission reductions could be due to a contraction of output, the extended analysis reveals that a large share can be attributed to a switch to biofuels. On average, the estimated semi-elasticity with respect to a 1 EUR/ton tax increase equals -0.58 for emissions, and -0.10 for overall employment. In most cases, there is a stronger negative effect on employment for low-educated workers. This suggest a skill-biased effect of carbon taxation that might exacerbate labor market inequalities.

While the negative impacts on firms' labor demand are consistent with a higher marginal cost of production due to a higher net-of-tax price of fuels (e.g. through higher fossil fuel prices or a switch to more expensive biofuels), they are in contrast to previous studies on carbon pricing and fuel taxation (Colmer et al., 2024; Dechezleprêtre et al., 2023; Gerster & Lamp, 2024; Marin et al., 2018; Martin et al., 2014). There are several potential explanations behind this result. First, the reform studied in this paper led to a considerable increase in effective tax rates, by approximately 66 euros per ton CO_2 for the average treated firm. This increase is significantly larger than the permit price fluctuations within the EU ETS, which is the most studied policy in the literature, which varied between 0 and 30 euros between 2005 and 2015 (the implementation year and the latest year included in related research, respectively) (Dechezleprêtre et al., 2023). Second, the initial phases of the EU ETS were characterized by free allocation of emission allowances (instead of auctioning), leading to potentially large windfall profits among over-allocated firms (Ellerman et al., 2016), thus mitigating negative output effects. Third, the selection of firms into different forms of regulation raises the point of heterogeneous treatment effects across firm characteristics. The industrial firms covered by the EU ETS are substantially larger and more energy-intensive then the firms regulated by the Swedish carbon tax. These firms may have different financial and technological constraints, and therefore respond differently to carbon pricing.

When subject to a carbon tax, firms are expected to reduce emissions until the marginal cost of reducing emissions further (the marginal abatement cost) equates the level of the carbon tax. Finding the same negative semi-elasticity of emissions for low versus high emission firms would be consistent with a slope of the firms' marginal abatement cost curves that is independent of emission-intensity. This, however, does not preclude the total costs of higher carbon taxes to differ, which is seen by the larger semi-elasticities for revenue and employment for the (cost-exposed) emission-intensive firms. The lower semi-elasticity for treatment-intensive firms, i.e. firms that experienced a larger tax increase from a lower level, has several potential explanations. One reason could be technological factors limiting the possibility to reduce emissions without large investments, where the same relative emission reductions are simply divided by a larger increase in tax rates. Colmer et al. (2024) builds a theoretical model with a fixed cost of investment to rationalize finding positive effects on economic outcomes together with significant emission reductions. The negative economic impacts and the limited response by treatment-intensive firms in this paper would be consistent with that mechanism. However, causally identifying the abatement curve along different levels of tax increases requires strong assumptions of homogeneous treatment effects (Callaway et al., 2024). Another likely explanation is a selection into the level of pre-reform tax rebates, leading to systematically heterogeneous treatment effects for firms experiencing a smaller versus larger tax increase. This would suggest that firms with a low pre-reform net tax rate have steeper marginal abatement cost curves. This, in turn, implies that the foregone emission-reductions due to the subsidized tax rates were probably relatively small, compared to a different allocation of tax refunds going towards firms that could more easily reduce emissions. Still, the policy to remove the rebates was also effective for treatment-intensive firms, with a significant semi-elasticity of -0.32.

The skill-biased effect on employment against low-educated workers is in line with the (scant) previous literature analyzing heterogeneous climate policy impacts (Yamazaki, 2017, 2019; Yip, 2018), and lends support to the notion that the impacts of the green transition share similarities with those of general technological change (Marin & Vona, 2019). Previous research has linked automation and technology upgrading with increasing inequality between high- and low-skill workers (Akerman et al., 2015; Autor, 2019; Graetz & Michaels, 2018). It is, however, important to note the potentially different mechanisms behind the skill-bias in, for example, automation and carbon taxation. For example, the former is characterized by market-driven, productivity-enhancing task displacement, and a potentially negative substitution effect between new technology and low-skill workers (Acemoglu & Restrepo, 2022). The effect of the green transition on labor market inequality is less clear. While the result in this paper does not indicate a substitution effect on employment, there is probably heterogeneity depending on the size of investments in cleaner production technologies.

Nevertheless, the lower demand for workers without a high school degree, whose unemployment rates are exceptionally high (Statistics Sweden, 2024), highlights the importance of re-skilling the workforce to mitigate undesired distributional impacts (European Commission, n.d.). An important feature of the impacts on employment, however, is the suggestive evidence pointing towards a reduction in hiring as the main channel. This implies that the impact on the labor market outcomes for individuals that were not hired by specific firms due to the carbon tax reform depends on their hireability by other firms. Hence, to fully understand the transitional costs of the green transition, one must take into account the impacts on individuals' career trajectories. These costs will depend on the extent to which individuals are (re)allocated to new sectors in which their skills might be less compatible (Walker, 2013), and their ability to move to expanding, green firms

(Curtis et al., 2024; Weber, 2020). Interestingly, the similarly negative effect on the demand for high-educated workers in the most cost-exposed firms suggests that the larger average effect on low-educated workers is due to the fact that they are negatively affected in a broader range of firms subject to carbon taxation. Understanding the mechanisms behind these patterns is an interesting area for future research.

The findings in this paper are directly connected to ongoing policy developments in other settings. The estimated impacts are not only informative for the increasing number of countries that are adopting carbon taxation (World Bank, 2024), but also for the firms that will be covered by the EU's second carbon market (ETS2). The ETS2, which will be launched in 2027, will cover smaller industrial firms by upstream regulation, thus incentivizing emission reductions by the cost pass-through from energy retailers to fuel prices (European Commission, 2024). This EU-wide policy will therefore impact firms that are more similar to the sample in this paper, and share key features in regulatory design with the Swedish carbon tax. The estimated semi-elasticity of CO_2 emissions with respect to carbon taxation (-0.58%/EUR) could help predict the emission reductions from such a policy.

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A Appendix

A.1 Sample construction

Fuel tax rates and emissions I collect gross carbon and energy tax rates for each fuel over time from the regulatory text in SFS 1994:1776. Since changes in the tax rates can be implemented in any day in a given year, I calculate average annual tax rates for each fuel weighted by the number of months the tax rate was in place. I categorize fuels in to groups that can be matched with the (categorized) fuels in ISEN. I calculate annual emissions by combining firms' fuel consumption with fuel-specific emission factors from the Swedish Environmental Protection Agency (SEPA) (Swedish Environmental Protection Agency, 2021). I broadly define biofuels as all fuels with an emission factor equal to zero, excluding electricity and biogenic CO_2 emissions.

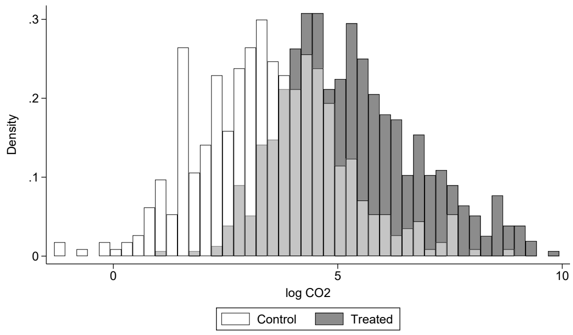
I obtain gross tax payments by multiplying firms' fuel consumption with the corresponding gross tax rate. I calculate net tax payments by subtracting the observed carbon and energy tax refunds from the gross tax repayments. I divide the net tax payments by total energy (in MWh) to obtain the net energy tax rates for each firm in a given year. To get net carbon tax rates, I multiply the official carbon tax rate with the net carbon tax payments as a share of gross carbon tax payments. There are several potential sources of measurement errors in the steps to obtain net tax rates. Some cases result in negative tax rates (around 2% of the observations). However, negative rates are not necessarily errors, due to the possibility to apply for accumulated tax refunds for the previous three years. As a rule, I remove firms with net carbon or energy tax rates that *ever* went below -100%. This removes 84 firms with negative outliers that are more likely to be measured with error. I also remove 1 additional firm with a very low energy consumption and a negative energy tax rate of below -99,000 SEK/MWh. Last, I set net tax *shares* to 1 whenever both gross and net tax *payments* are 0 (i.e. firms without taxable emissions or energy consumption who do not receive tax refunds are considered to pay 100% of the general rate), and to 0 whenever firms receive tax refunds while their gross payments are 0.

Industry classification The Swedish industry classification SNI, which is a version of NACE, changed definition in 2007. I use a correspondence key from Statistics Sweden to bridge the transition from previous (2002) definition, and assign firms to industries based on the 2007 definition for all years at 5 digits. When there are multiple matches from 2007 for a 2002-digit, I simply choose the first in the list. This has minimal impact on the analysis which only makes use of 2-digit industry codes.

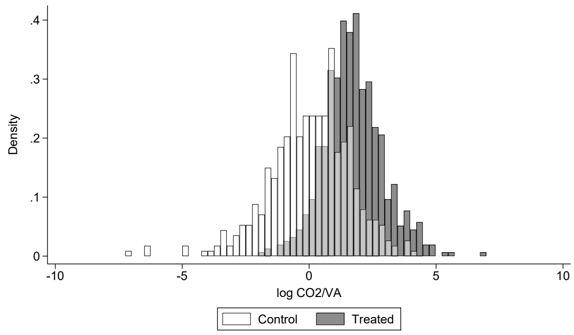
	Mean	SD	Min	Max	Pre-reform means		Difference
					Treated	Control	
	(1)	(2)	(3)	(4)	(5)	(6)	(5) - (6)
#Firms	3,026.00	0.00	3,026.00	3,026.00	1,429.00	1,597.00	-168.00
Treatment	0.47	0.50	0.00	1.00	1.00	0.00	1.00
Value added (mSEK)	63.70	539.28	-4,908.66	30,630.22	82.98	38.67	44.31***
Revenue (mSEK)	232.55	2,326.24	-0.18	120,555.02	305.05	121.51	183.53***
Fixed assets (mSEK)	75.10	934.14	-5.31	46,315.63	86.74	35.42	51.32***
Exporter	0.74	0.44	0.00	1.00	0.78	0.71	0.07***
CO_2 emissions (ton)	298.79	1,048.97	0.00	60,456.02	597.27	111.63	485.64***
CO_2 intensity (ton/mSEK)	7.65	257.40	-28,441.16	19,847.65	20.03	3.20	16.82*
Fossil energy share	0.27	0.26	0.00	1.00	0.41	0.22	0.19***
Average income	298,898.45	69,900.42	44,800.00	1,656,682.76	256,583.46	260,495.08	-3,911.62***
Employment	84.15	384.18	1.00	19,113.00	112.39	62.55	49.85***
Employment: No high school	0.21	0.13	0.00	1.00	0.25	0.22	0.03***
Employment: High school	0.63	0.15	0.00	1.00	0.61	0.63	-0.02***
Employment: Above high school	0.16	0.13	0.00	1.00	0.13	0.15	-0.01***
Employment: STEM	0.10	0.10	0.00	1.00	0.08	0.09	-0.01***
Employment: Female	0.21	0.17	0.00	1.00	0.21	0.20	0.01***
Employment: Age 16-29	0.18	0.12	0.00	1.00	0.19	0.19	0.00
Employment: Age 30-39	0.22	0.11	0.00	1.00	0.24	0.25	-0.01***
Employment: Age 40-49	0.27	0.12	0.00	1.00	0.26	0.26	-0.00
Employment: Age 50-64	0.34	0.16	0.00	1.00	0.31	0.30	0.01*
Observations	42,096						

Table A.1: Descriptive statistics for the unbalanced sample. Monetary variables are measured in million Swedish Krona (mSEK), except income, which measures workers' annual income in SEK. The average exchange rate over the period was 9.39 SEK/EUR. CO_2 intensity is measured as ton CO_2 divided by value added (in mSEK). Fossil energy share shows the firms' share of fossil fuels out of total energy consumption. Employment disaggregations represent shares of total employment at a firm in a given year. STEM shows the average share of employed workers with a higher education (above high school) in Science, Technology, Engineering and Mathematics. Column '(5) - (6)' shows differences in means in pre-reform years (2004-2008) between the treatment and control group. *** p<0.01, ** p<0.05, * p<0.1

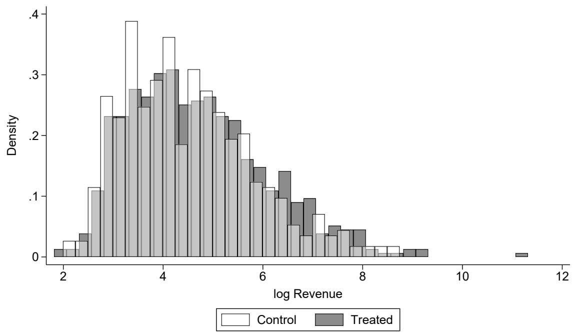
(a) CO_2



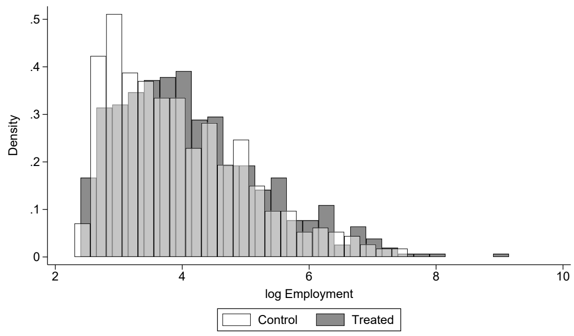
(b) CO_2 intensity



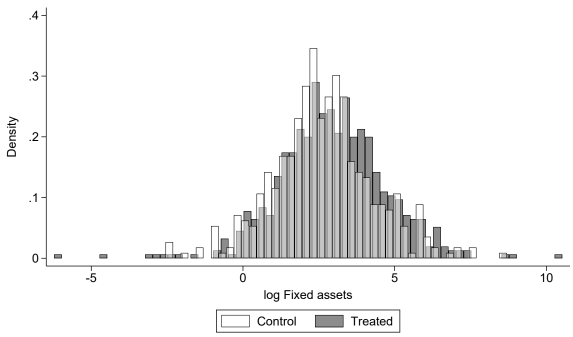
(c) Revenue



(d) Employment



(e) Capital



(f) Value Added

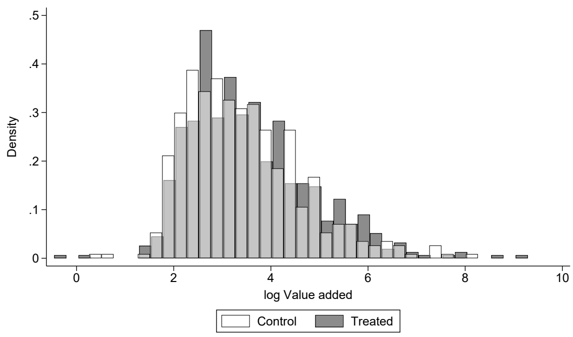


Figure A.1: 2008 distribution of outcomes (log-transformed) by binary treatment status.

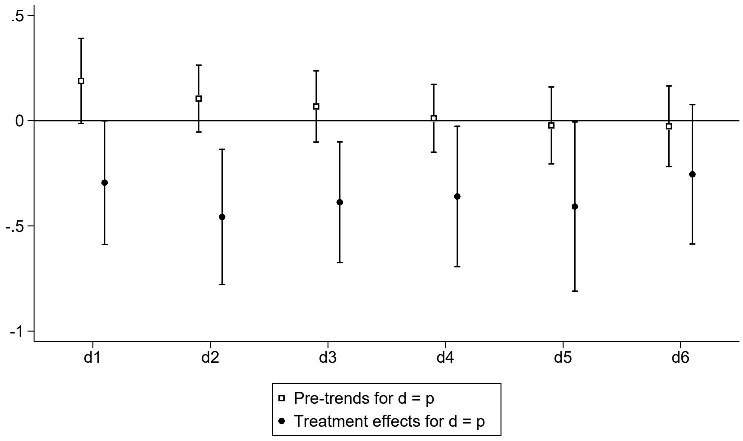
	log CO_2		log CO_2/VA		log Revenue		log Employment		log Capital		log VA	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$D \times Post$	-0.361***	-0.359***	-0.323***	-0.321***	-0.059*	-0.061*	-0.070***	-0.065**	-0.094	-0.100	-0.053	-0.051
	(0.100)	(0.100)	(0.107)	(0.107)	(0.032)	(0.032)	(0.026)	(0.026)	(0.069)	(0.069)	(0.036)	(0.036)
$EX' \times Post$		✓		✓		✓		✓		✓		✓
$L' \times Post$		✓		✓		✓		✓		✓		✓
$K \times Post$		✓		✓		✓		✓		✓		✓
Observations	1,840	1,840	1,824	1,824	2,148	2,148	2,154	2,154	2,138	2,138	2,134	2,134

Table A.2: Long difference results from estimating Eq (2) on a balanced panel of firms. Columns with odd numbers present baseline results from Eq (2), while columns with even numbers control for business cycle exposure, as explained in Section 6. Standard errors in parenthesis are clustered by firm. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

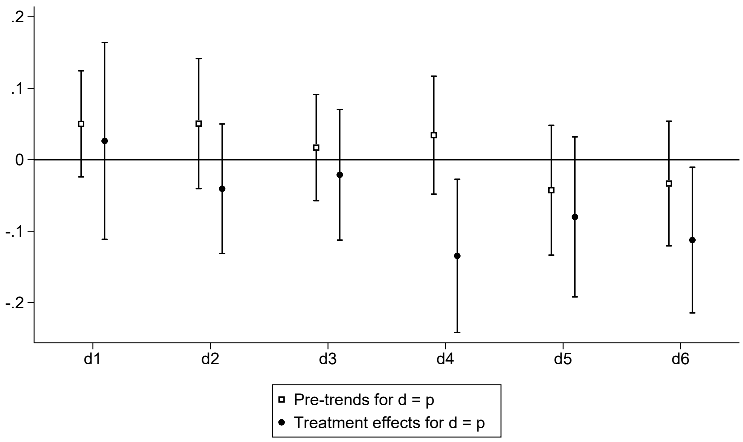
	log CO_2		log Revenue		log Employment	
	(1)	(2)	(3)	(4)	(5)	(6)
$D \times Post$	-0.205	-0.381***	0.002	-0.013	0.002	-0.031
	(0.148)	(0.113)	(0.042)	(0.037)	(0.035)	(0.028)
$D \times Post \times CO_2\ int.$	-0.219		-0.102		-0.140**	
	(0.188)		(0.063)		(0.055)	
$D \times Post \times Treatment\ int.$		0.042		-0.097**		-0.081**
		(0.119)		(0.040)		(0.032)
$CO_2\ int. \times Post$	✓		✓		✓	
Observations	1,840	1,840	2,148	2,148	2,154	2,154

Table A.3: Long difference results from estimating Eq (2) with interaction terms on a balanced panel of firms. Standard errors in parenthesis are clustered by firm. *** p<0.01, ** p<0.05, * p<0.1

(a) log CO_2



(b) log Revenue



(c) log Employment

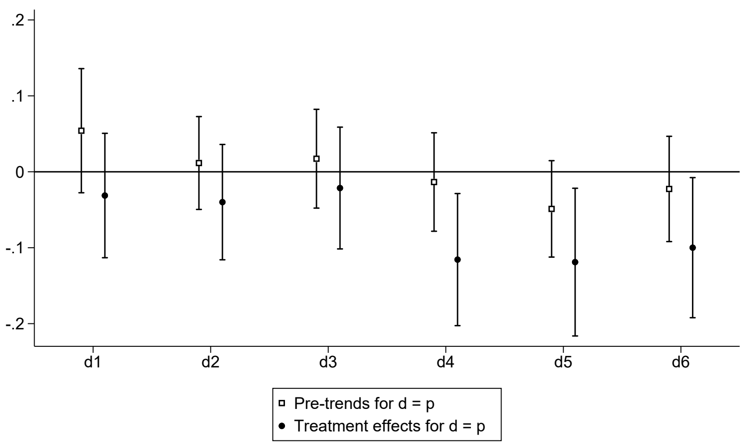


Figure A.2: Each subfigure shows the result along treatment intensity d for group p from two separate regressions, corresponding to a placebo test for pre-reform years (2004-2008) and the long-difference regression of Eq (3). Regressions include industry-by-year fixed effects, and standard errors are clustered by firm. Capped spikes show 95% confidence intervals.

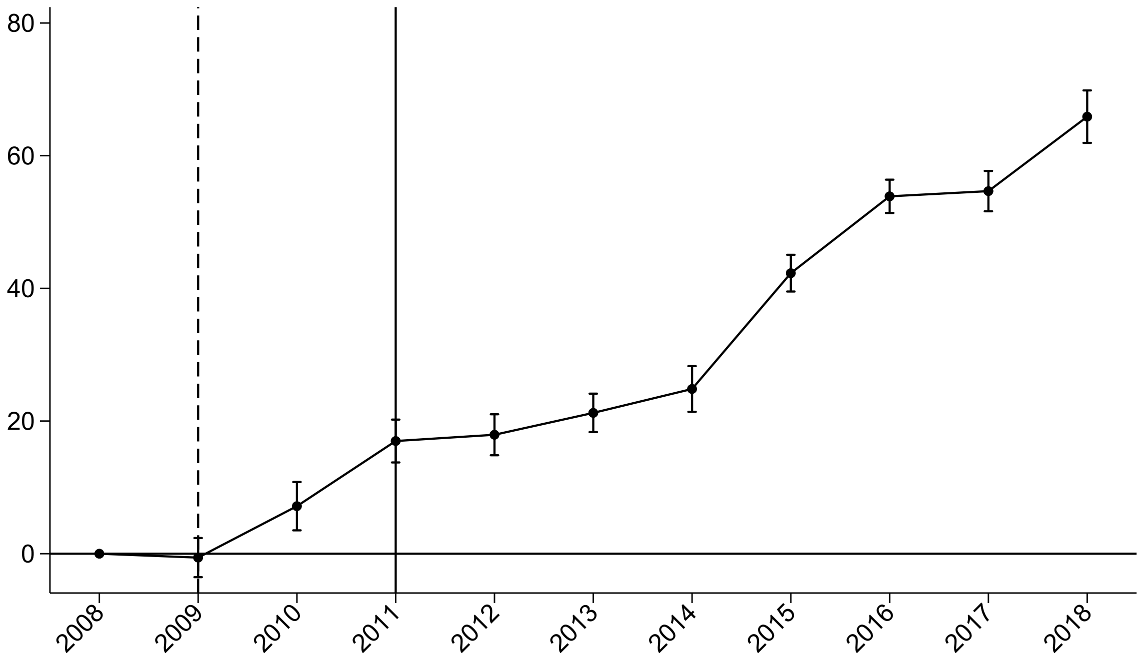
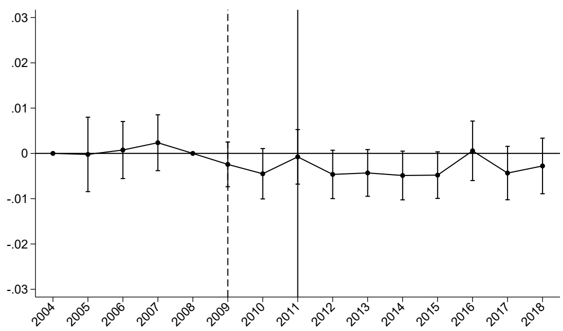
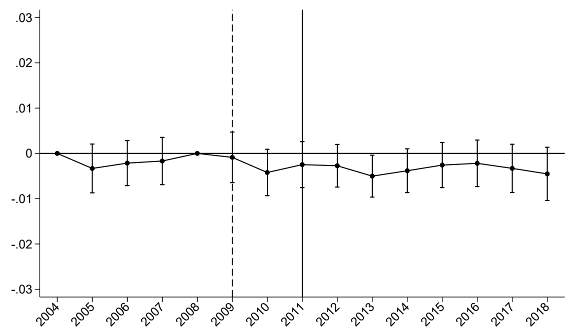


Figure A.3: First stage results on fuel tax rates (in euros) from estimating Eq. (1) on a balanced panel of firms. Regressions include industry-by-year fixed effects, and standard errors are clustered by firm. Capped spikes show 95% confidence intervals.

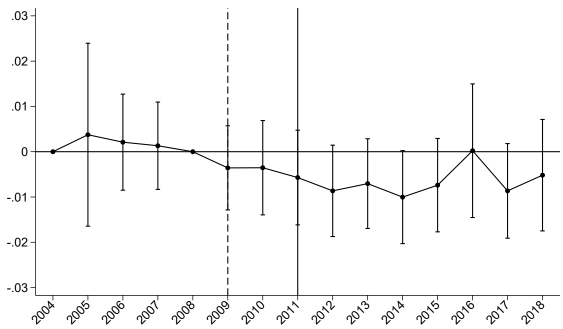
(a) Hiring rate: No high school



(b) Separation rate: No high school



(c) Hiring rate: Age 40 - 64



(d) Separation rate: Age 40 - 64

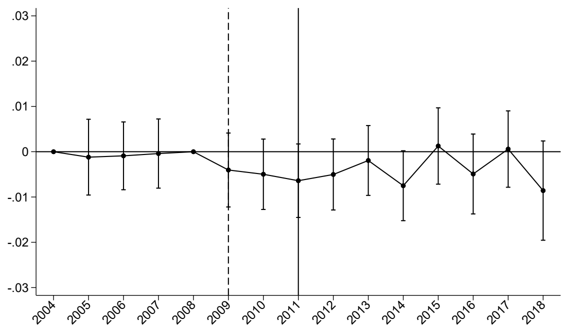


Figure A.4: Event study results from estimating Eq. (1) on a balanced panel of firms. Regressions include industry-by-year fixed effects, and standard errors are clustered by firm. Capped spikes show 95% confidence intervals.

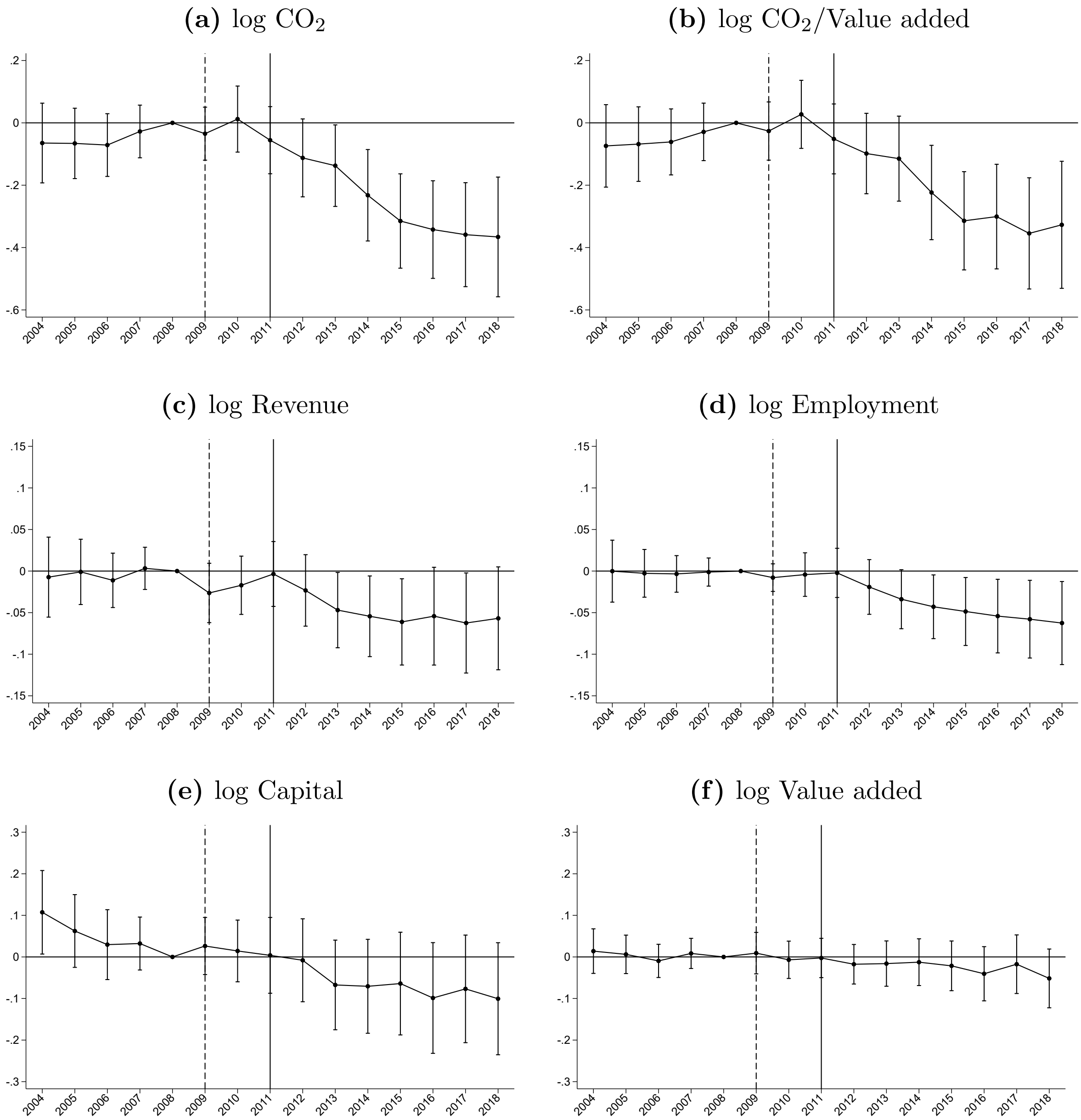
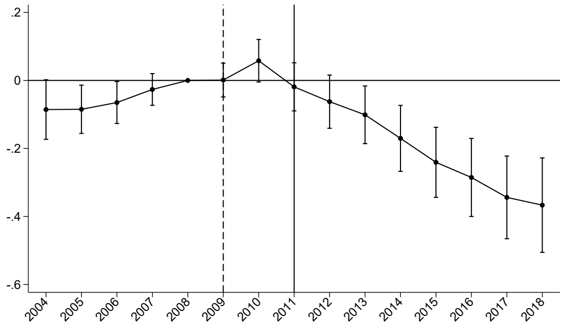
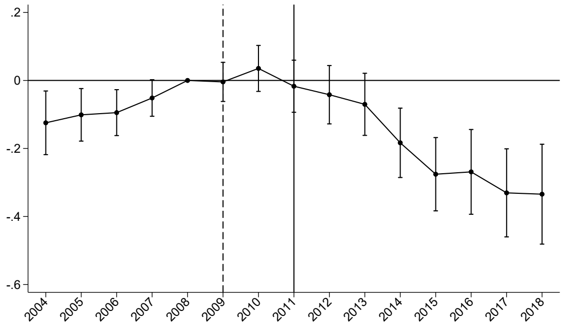


Figure A.5: Event study results from estimating Eq. (1) on a balanced panel of firms. Regressions include industry-by-year fixed effects and time-varying controls for business cycle exposure, and standard errors are clustered by firm. Capped spikes show 95% confidence intervals.

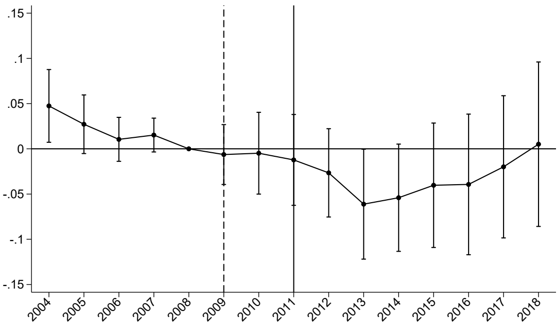
(a) log CO_2



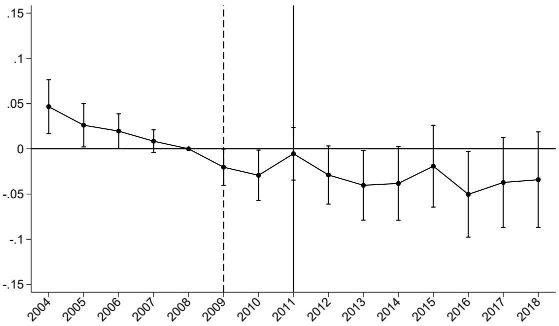
(b) log CO_2/Value added



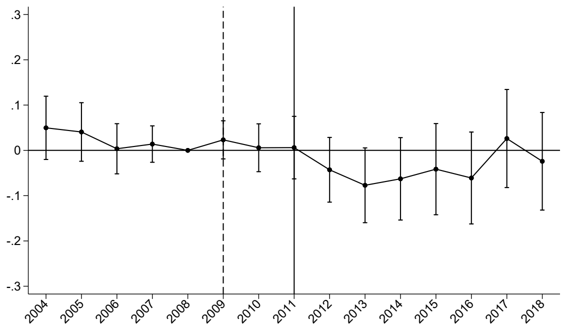
(c) log Revenue



(d) log Employment



(e) log Capital



(f) log Value added

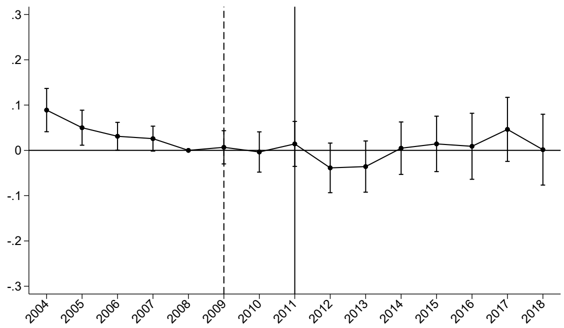
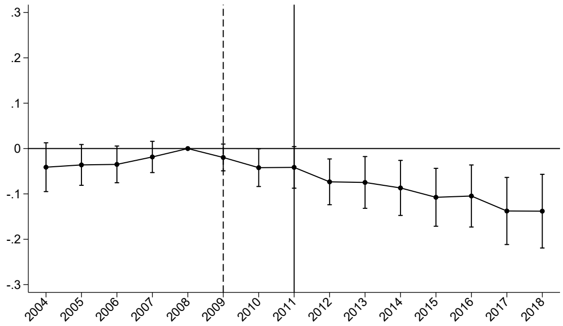
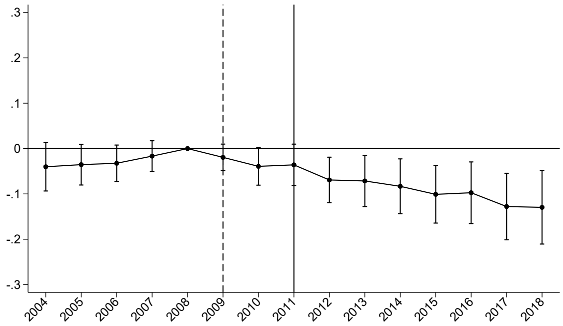


Figure A.6: Event study results from estimating Eq. (1) on the unbalanced sample. Regressions include industry-by-year fixed effects, and standard errors are clustered by firm. Capped spikes show 95% confidence intervals.

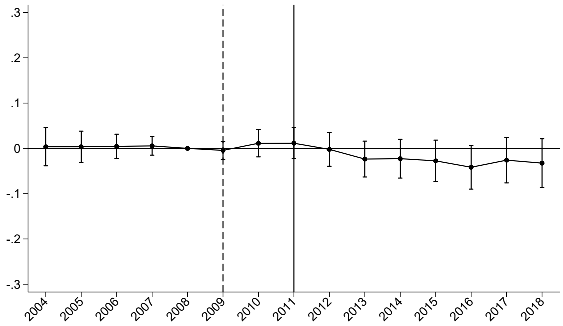
(a) No high school



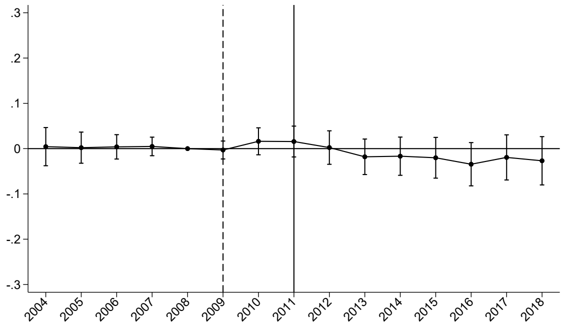
(b) No high school



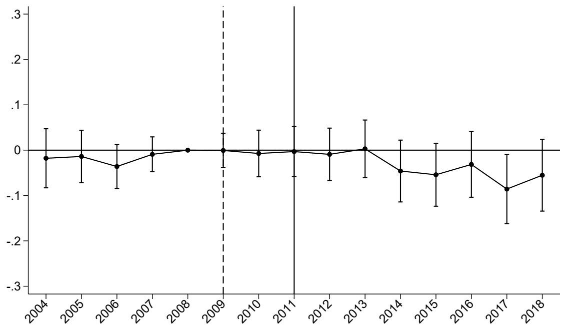
(c) High school



(d) High school



(e) Above high school



(f) Above high school

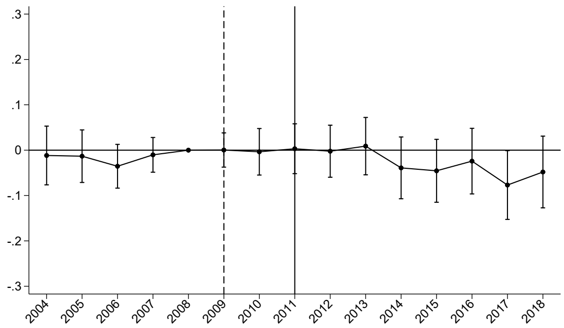


Figure A.7: Event study results on employment by educational attainment from estimating Eq. (1) using a balanced panel of firms. All regressions include industry-by-year fixed effects, and standard errors are clustered by firm. Figure A.7b, A.7d, and A.7f additionally control for time-varying business-cycle exposure. Capped spikes show 95% confidence intervals.

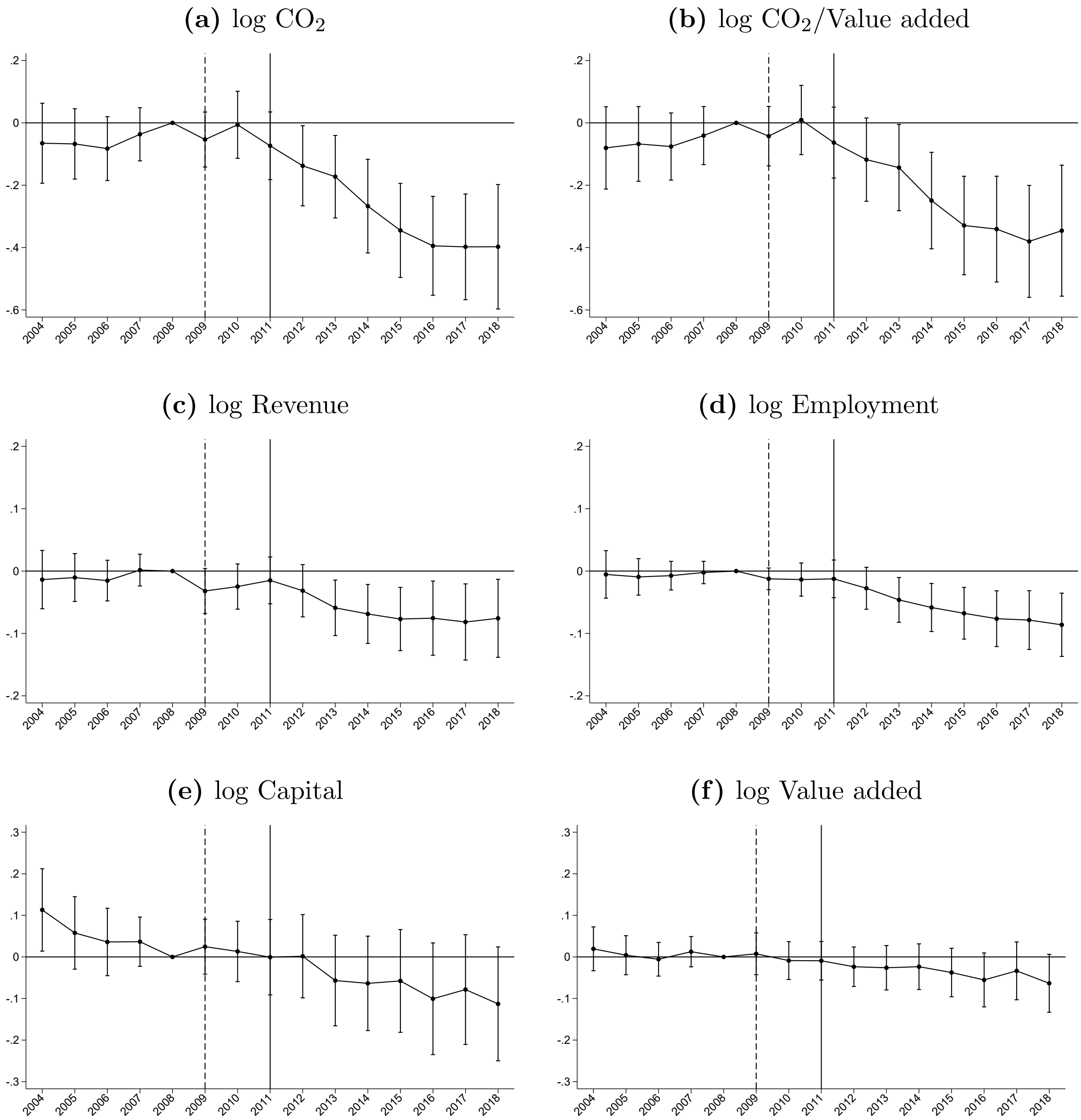


Figure A.8: Event study results from estimating Eq. (1) on a balanced panel of firms. Regressions include industry-by-year fixed effects and region-by-year fixed effects, and standard errors are clustered by firm. Capped spikes show 95% confidence intervals.