Carbon Taxation, Firm Performance, and Labor Demand

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Abstract

This paper investigates the environmental and economic effects of carbon taxation, and their implications for labor demand. Using matched employer-employee data from the Swedish registers for the years 2004-2018, I estimate the effects of a reform that increased the stringency of the tax for a subset of firms in the manufacturing sector. Using a difference-in-difference framework, I find that the reform significantly reduced emissions among treated firms. However, it also reduced the employment of workers without a high school degree. In addition, I find that negative employment impacts are concentrated among emission-intensive firms, which face the largest cost increases when carbon tax rates rise. The results show that carbon taxation, while effective at reducing emissions, may have strongly heterogeneous employment impacts, and that complementary policies might be needed to address labor market inequalities when implementing climate policy.

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

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1 Introduction

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). Carbon pricing corrects the (relative) prices of fuels to account for the negative externality stemming from carbon emissions, and incentivizes abatement while maintaining flexibility in the behavioral responses of firms and households. However, despite its appealing properties, carbon pricing advocates have faced considerable political resistance due to its perceived impacts on jobs and inequality (Vona, 2019). For example, concerns over negative effects on competitiveness in energy-intensive industries have led to a widespread use of directed exemptions in the area of energy taxation in general, and carbon taxation in particular (Ekins and Speck, 1999). While differentiated (unilateral) carbon tax rates can be socially efficient to account for distortions such as carbon leakage, whereby emissions 'move' to unregulated sectors and jurisdictions (Fowlie et al., 2016; Fowlie, 2009), there is a prominent risk of over-compensating firms exposed to carbon pricing (Martin et al., 2014). This might lead to insufficient abatement incentives to reach climate targets and distorts the allocation of production and workers (Gerster and Lamp, 2024). Learning about the environmental and economic impacts of carbon taxation is therefore paramount to understand the consequences of various regulatory designs in terms of foregone emission reductions and job-loss mitigation, as well as the distributional impacts of the green transition.

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 (SCC) (Moore et al., 2024).[1] Combined with rich employer-employee data and a quasi-experimental setting, Sweden therefore provides a rare possibility to study the effectiveness of carbon taxation and its heterogeneous impacts on different types of workers (Vrolijk and Sato, 2023).

Before 2011, Swedish manufacturing firms could apply for carbon tax refunds for fuel consumption that met certain criteria, resulting in an effective carbon tax corresponding to 21% of the nominal rate for eligible firms. Between 2011-2018, the possibility to apply for refunds was gradually phased-out, leading to a higher carbon tax stringency for affected firms. I develop an empirical framework to isolate exogenous variation in firm-level carbon tax rates, by combining the phase-out of refunds over time with the differential uptake of refunds before the reform was announced.

[1] Moore et al. (2024) provides an average of \$132/ton CO_2 in a meta-analysis of the literature on 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.

In difference-in-difference regressions, I find that the increase in effective carbon tax rates led to a substantial reduction in firms' total emissions as well as emission *intensities*, by more than 30% over the time period. The decrease in emissions corresponds to an estimated semi-elasticity of -0.64% with respect to a 1 EUR/ton CO_2 tax increase. Further analysis reveals that substitution from fossil fuels towards biofuels is the main mechanism behind this result, with a significantly negative effect on total energy use.

I also observe significantly negative effects on labor demand, with stronger effects for workers without a high school degree. In turn, the negative impacts within this group are driven by workers above 40 years old. I do not estimate any significant effects on workers' income regardless of educational attainment, and the result indicates that the margin of adjustment behind the negative employment impacts is a reduction in firms' hiring rate. This suggests that incumbent workers at the treated firms were not primarily affected by the higher carbon tax.

The estimated semi-elasticity of emissions is the same for firms with high and low emission intensity. However, the negative employment effects are generally twice as large among emission-intensive firms, where the semi-elasticity of low-educated employment is -0.32% per EUR/ton CO_2 (compared to -0.17% for all firms). These results are robust to adding time-varying controls of firm size and exporter status and different sample restrictions.

The main contribution of this paper is that it provides the first empirical evidence on heterogeneous effects of carbon pricing on employment using linked employer-employee data. The firm-level data allows me to exploit a rare quasi-experimental setting to analyze the causal effects of a carbon tax, which has been a challenge in previous research on climate policy in general (Vrolijk and Sato, 2023). The link to individual registers is crucial for being able to study heterogeneous effects on labor demand and inequality. The emerging literature in this field has, to a large extent, been restricted to estimating firm-level effects of carbon pricing policies. Prior studies have found no impact on average employment, despite reductions in emissions (Colmer et al., 2024; Dechezleprêtre et al., 2023; Marin et al., 2018; Martin et al., 2014). The existing studies analyzing heterogeneous employment impacts using individual-level data either rely on regional treatment variation without possibilities to identify firm-level mechanisms (Yamazaki, 2017, 2019; Yip, 2018).

A second strand makes use of pre-existing definitions of 'green' tasks and occupations, predominantly based on measures available in the US Bureau of Labor Statistics' O*NET database (Apostel and Barslund, 2024). This approach has been used to study, for example, the prevalence of green jobs and skills in the economy (Bowen et al., 2018; Curtis and Marinescu, 2022; Popp et al., 2021; Saussay et al., 2022; Vona et al., 2019), the characteristics of skills associated with green tasks (Vona et al., 2018), and occupationlevel impacts on employment of innovation (Elliott et al., 2024) and environmental policy (Popp et al., 2021; Vona et al., 2018).[2] I contribute to this strand by studying within-firm employment changes for different workers, thus providing evidence of distributional impacts of climate policy on firms' labor demand without relying on green definitions.

In addition, this paper complements previous research on the effectiveness of carbon pricing (Andersson, 2019; Brännlund et al., 2014; Jaraite et al., 2014; Jaraitė and Maria, 2016; Leroutier, 2022; Martinsson et al., 2024), by developing a novel empirical methodology using an unexploited register on excise tax refunds to observe firm-level treatment variation. Last, I contribute to the broader literature on the economic impacts of environmental regulation (Berman and Bui, 2001; Greenstone, 2002; Morgenstern et al., 2002; Walker, 2011).

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 provides a discussion of the results, and, finally, 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, nd). The tax is measured in Swedish Krona (SEK) per volume of fuel and varies across fuels based on their carbon content, so that the SEK/ton CO_2 tax rate is constant. To facilitate administration, the regulation has adopted a *tax suspension regime* in which, in principle, upstream firms that import, produce or sell energy products are tax liable and must register as taxpayers. The tax is levied when a fuel is sold by a registered taxpayer to a consumer. Between registered taxpayers, however, taxation is suspended (Hammar and Åkerfeldt, 2011). Most industrial firms are not registered taxpayers, but are instead affected by the carbon tax through higher prices on fossil fuels as energy retailers pass on their tax payments to consumers. The regulatory design thus incentivizes downstream emission reductions if there is sufficient pass-through of carbon pricing from the energy sector.

[2] A related strand of literature exploits variation in energy prices as a proxy for climate policy stringency. Within this framework, Marin and Vona (2019, 2021) find that climate policy is potentially skill-biased at the occupational level, leading to a higher demand for technicians and a lower demand for manual workers. However, as noted by Andersson (2019) and Brännlund et al. (2014), firms and households respond differently to variations in tax levels than prices, potentially through different expectations of future changes, leading to stronger taxation effects.

(a) CO_2 tax rates (b) Policy announcements

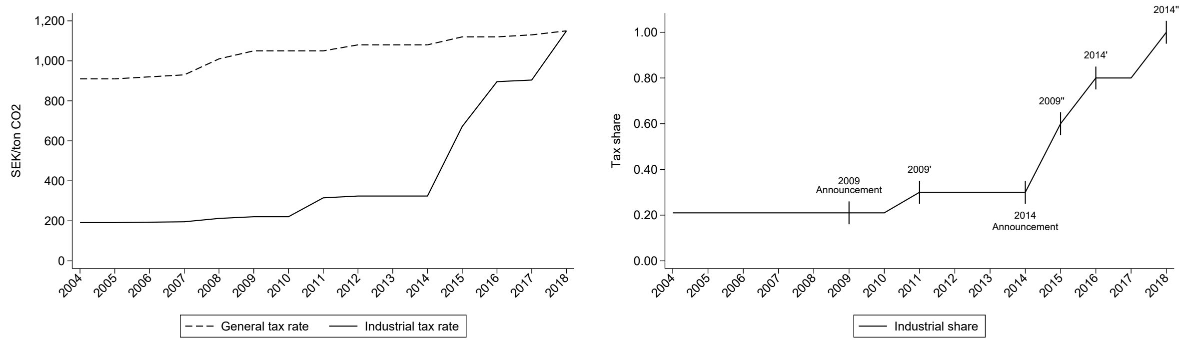


Figure 1: Timing of policy implementation. Figure 1a shows the evolution of the Swedish carbon tax with and without industrial rebates, in SEK/ton CO_2. Figure 1b shows the timing of announcements of the two policy changes (in 2009 and 2014), and their corresponding increases in industrial tax shares. The average exchange rate over the period was 9.39 SEK/EUR.

Figure 1a plots the carbon tax in SEK per ton CO_2 between 2004-2018.[3] However, for the industrial sector, the regulation has featured generous rebates over time. Between 2004-2010, the government offered a 79% refund of the tax paid on industrial fuel consumption fulfilling certain criteria. First, tax refunds were only granted for fuel consumed in the manufacturing process for uses *other than motorized vehicles*.[4] Second, the manufacturing process in which fuel has been used must be the *main activity* of the firm. This implies that refunds were largely given to firms for heating in the manufacturing process.[5] Firms received the tax refund through application up to three years after fuel purchase, assuming a 100% pass-through of the tax to fuel prices. The resulting net tax rate is represented by the solid line in Figure 1a.[6] For firms regulated by the EU ETS, the carbon tax was completely removed in 2011 to avoid double carbon pricing (Ryner, 2022).

The manufacturing rebates were gradually reduced from 2011, and completely removed in 2018. The phase-out of the rebates was communicated in two steps, which are shown in Figure 1b. 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

[3] The average exchange rate over the period was 9.39 SEK/EUR.

[4] Examples of industrial motorized vehicles are excavators and wheel loaders. This condition also excludes fuels used for transportation of goods on roads.

[5] The rebate was also given to utilities delivering heating to manufacturing firms for this purpose, such that total tax burden along the supply chain was independent of whether heating was delivered or generated on-site by the firm.

[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, as was fuels used for special vehicles in manufacturing in the mining industry until 2020. In addition, tax payments were capped at 0.8% of sales until 2015.

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, 2009). After elections in 2014, the new government already presented an updated climate plan which mandated further emission reductions domestically (Government Bill 2014/15:1, 2014). The new plan featured an increase in the industrial tax share in 2016, and a complete phase-out of the rebates in 2018.

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 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 climate policy 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 the Swedish Environmental Protection Agency (2023), I obtain firm-level annual emissions. The dataset is further 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.[7] 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.

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

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.[8] 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. I approach this issue by setting all negative tax rates to zero, as these firms are likely to have had some tax rebate these years.[9]

Sample restriction As outlined in the section below, treatment is defined as having 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. 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). 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.[10] 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 2 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

[8]The dataset misses observations for 2013.

[9]Since the empirical framework of the paper is based on a binary definition of treatment (having a tax rebate or not), the exact level will not be important for the result.

[10]Observations in 2007 are used to construct control variables related to exposure to the financial crisis in the sensitivity analysis.

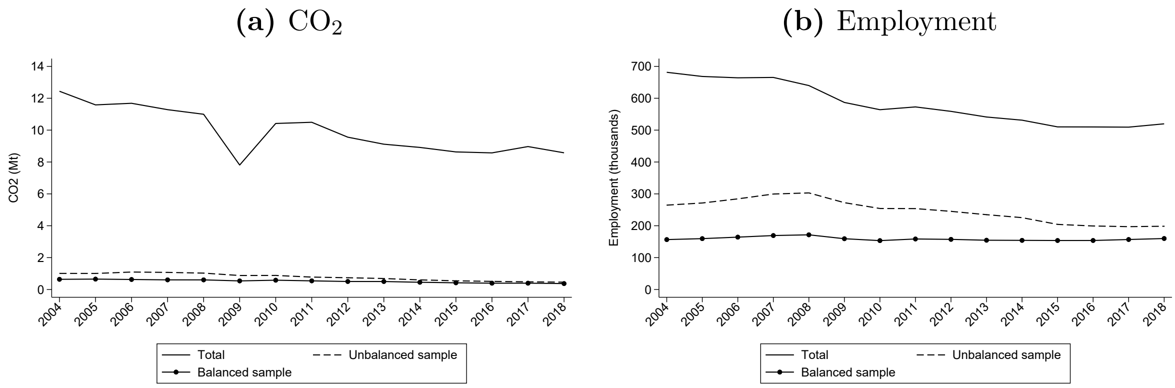


Figure 2: Aggregate manufacturing emissions and employment. Aggregates are calculated from firms in the Energy Use in Manufacturing survey (ISEN).

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 1,464 unique firms (3,211 in the unbalanced sample), of which 58% are treated. Table 1 presents summary statistics for the main sample over 2004-2018, and Table 2 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. Treated firms use more fossil fuels in relation to their total energy consumption, and emit significantly more CO_2. Workers at treated firms significantly less, and are less likely to have obtained a high school degree. Figure 3 shows the distribution of treatment across manufacturing industries, confirming that treated firms are not clustered in specific sectors. Figure A.1 in the Appendix shows that there is considerable overlap in outcomes between the control and treatment group when log-transformed.

Figure 4 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 seems to have a steeper increase in the leading years, which warrants

	Mean	SD	Min	Max
#Firms	1,464.00	0.00	1,464.00	1,464.00
Treatment	0.58	0.49	0.00	1.00
Value added (mSEK)	87.97	412.90	-311.14	15,423.59
Revenue (mSEK)	300.17	1,685.78	-0.18	78,894.10
Fixed assets (mSEK)	100.44	926.65	-0.01	33,733.99
Exporter	0.81	0.39	0.00	1.00
CO_2 emissions (ton)	374.35	1,138.22	0.00	26,554.27
CO_2 intensity (ton/mSEK)	9.85	244.39	-15,324.40	19,847.65
Fossil energy share	0.27	0.25	0.00	1.00
Average income	305,395.82	62,542.30	47,000.00	1,120,550.00
Employment	109.27	330.10	1.00	9,368.00
Employment: No high school	0.21	0.12	0.00	1.00
Employment: High school	0.63	0.13	0.00	1.00
Employment: Above high school	0.17	0.12	0.00	1.00
Employment: STEM	0.10	0.10	0.00	1.00
Employment: Female	0.21	0.16	0.00	1.00
Employment: Age 16-29	0.17	0.11	0.00	1.00
Employment: Age 30-39	0.21	0.10	0.00	1.00
Employment: Age 40-49	0.27	0.10	0.00	1.00
Employment: Age 50-64	0.34	0.14	0.00	1.00

Table 1: 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.

	Pre-reform means		
	Treated	Control	Difference
	(1)	(2)	(1) - (2)
Value added (mSEK)	92.33	62.57	29.76***
Revenue (mSEK)	329.25	185.12	144.13***
Fixed assets (mSEK)	101.29	48.73	52.56***
Exporter	0.83	0.79	0.04***
CO_2 emissions (ton)	642.03	133.48	508.54***
CO_2 intensity (ton/mSEK)	29.13	4.33	24.80**
Fossil energy share	0.39	0.22	0.17***
Average income	258,642.10	266,368.56	-7,726.46***
Employment	130.09	87.78	42.31***
Employment: No high school	0.25	0.22	0.03***
Employment: High school	0.61	0.62	-0.02***
Employment: Above high school	0.14	0.15	-0.02***
Employment: STEM	0.08	0.10	-0.01***
Employment: Female	0.22	0.20	0.02***
Employment: Age 16-29	0.19	0.18	0.01**
Employment: Age 30-39	0.24	0.25	-0.01***
Employment: Age 40-49	0.26	0.26	-0.00
Employment: Age 50-64	0.31	0.30	0.00

Table 2: Differences in means in pre-reform years (2004-2008) between the treatment and control group. See Table 1 for details. *** p<0.01, ** p<0.05, * p<0.1

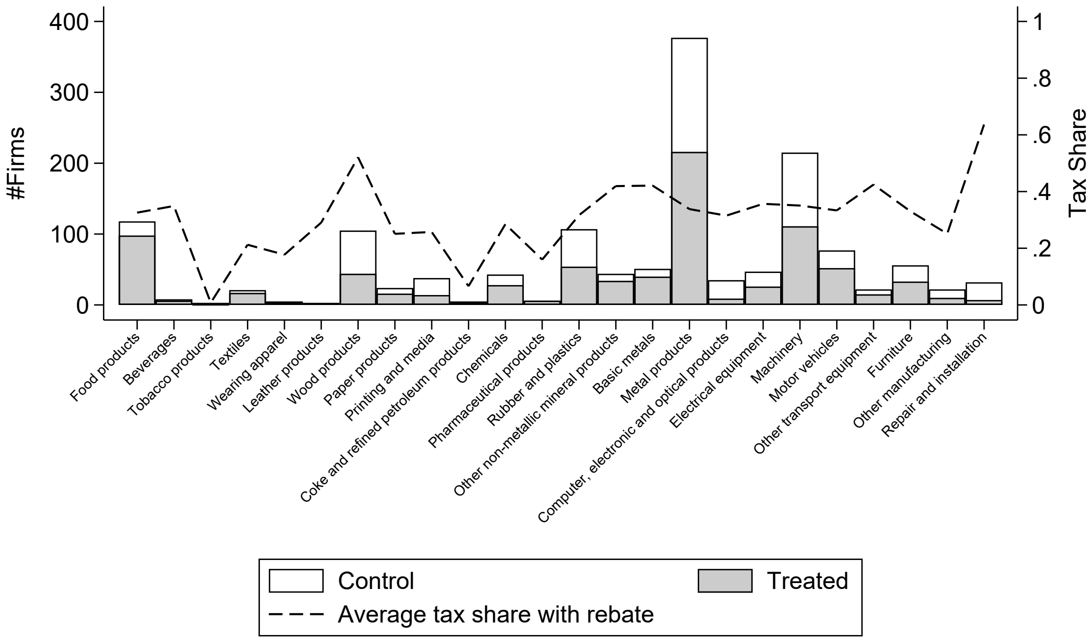


Figure 3: Treatment by industry as defined by the pre-reform uptake of CO_2 tax refunds. Dashed line represents the average tax share among treated firms within the industry.

extra caution when analyzing this outcome. A general sensitivity analysis with respect to influence from the financial crisis is carried out in the empirical section.

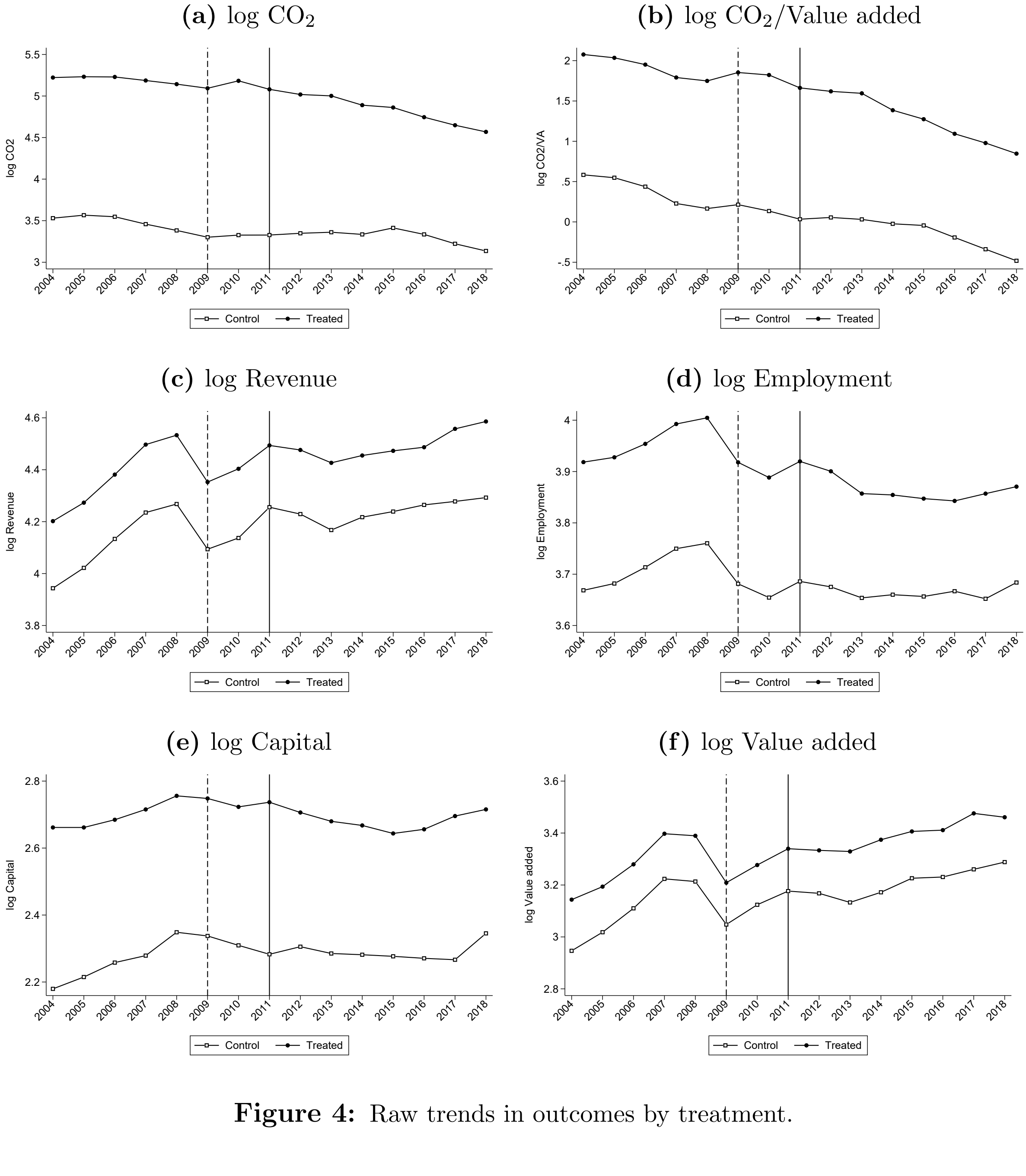


Figure 4: Raw trends in outcomes by treatment.

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 decrease the tax rebates for industrial 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.

The analysis is based on two empirical models. 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.[11] 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. Treatment adoption occurs simultaneously for all firms, and the binary definition of treatment overcomes potential issues related to negative weights and heterogeneous treatment effects discussed in the recent econometrics literature (Callaway et al., 2024). ϵ_{jt} is an error term allowed to correlate over time within firms.

The second empirical model is a long difference approach that estimates the following two-period equation

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

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 represents the long-difference estimate of the complete phase-out of the tax rebates.

Both approaches rely on the assumption that pre-announcement tax rebate status D_j is exogenous to unobserved, within-industry trends in outcomes ϵ_{jt}. Figure 5 provides information of a potential source of bias, namely concurring changes in other fuel policies. Using the actual data, Figure 5a shows that average calculated CO_2 tax shares follow the pattern of the reform for treated firms, with average shares close to 1 for control

[11] This means that treatment effects will be compared to differences in the year before the observed impacts of the financial crisis (see Figure 4).

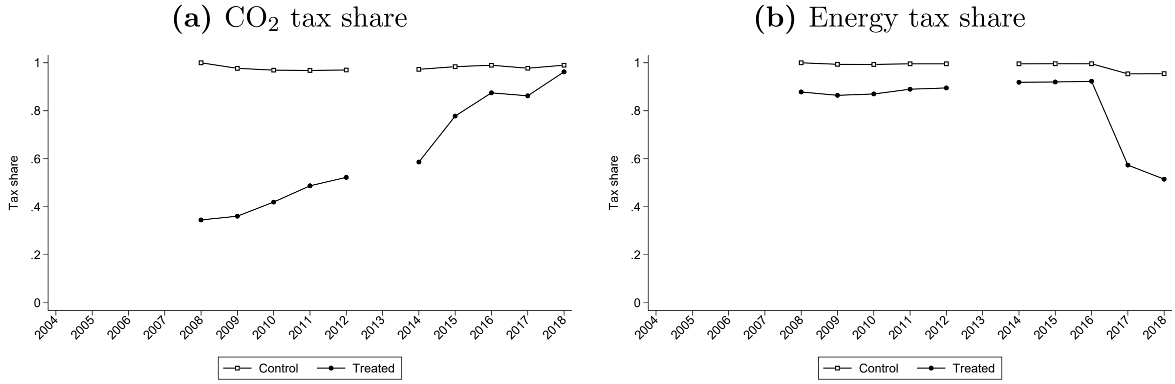


Figure 5: Firm-level fuel tax shares. CO_2 tax shares are calculated by combining each fuel's gross CO_2 tax rate with firms' fossil fuel consumption and CO_2 tax refunds. Energy tax shares are calculated by combining each fuel's gross *energy* tax rate (which is a separate tax imposed on fuels) with firms' energy consumption and energy tax refunds. Treatment is based on CO_2 tax refund uptake in 2008 for both figures. Data for 2013 is missing.

firms, validating the treatment assignment procedure. Figure 5b shows the respective average *energy* tax share, which is also imposed on fuels. The figure shows no evidence of diverging trends between the groups for most of the time period. However, treated firms' energy tax share falls in 2017 and 2018, which could influence outcomes in these years. Event-study estimates makes it possible to compare treatment effects across years, and therefore to assess the importance of these changes. If the assumption holds, the empirical model will identify the average treatment effect on the treated of increases in the stringency of climate policy.

5 Result

Main result Figure 6 presents the estimated effects of increasing climate policy stringency on firms' environmental and economic performance. Reassuringly, I do not find significant differential trends in outcomes before the reform except for capital, which was already observed in the raw data. Figure 6a reveals large and significant negative effects on emissions, corresponding to 33% in 2018. The fall in emissions among treated firms starts in 2011, which is the year tax rebates started to be phased-out, and levels out in 2016. The dynamics of the effect suggest that the change in energy taxation in 2017 (see Figure 5) is not a driving factor. Figure 6b also shows that the reduction in emissions is, to a large extent, not driven by a reduction in output, since there is a similar significant improvement in emission *intensity*. I also find a significantly negative effect on employment. The coefficients are smaller than for emissions (around -5%), and remain significant until 2016. A similar pattern (and magnitude) is observed for revenue, although without statistical significance. I find no significant effects for capital or value added.

Firm heterogeneity This part explores heterogeneity in the previous result along firm characteristics. In order to investigate the extent to which estimated treatment effects vary across firms, I focus on two dimensions, which are emission intensity and firms' capital-labor ratio. Emission-intensive firms face higher incentives to reduce emissions as effective tax rates rise, due to the higher costs of compliance. It is also possible that capital-intensive firms have different technologies than labor-intensive firms (e.g. varying energy-labor substitution possibilities), leading to heterogeneous responses in outcomes.

First, 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. Second, I construct the variable $Capital\ int._j$ which equals to 1 for firms with a capital intensity, measured as the ratio of fixed assets to employees, above the 2-digit industry median in 2007. Table 3 shows the result along these dimensions, where the new variables are interacted with the previous treatment term. All regressions include firm and industry-year fixed effects. I also control for an interaction with the year fixed effect for each intensity indicator, to isolate the variation in treatment.[12]

Column (2) reports the effects on total emissions. The point estimates suggest that both high- and low-emission firms reduce their emissions, with emission-intensive firms responding more strongly (although not significantly). The remaining outcomes do not show any significant patterns in treatment effects along the emission and capital intensity heterogeneity. Table A.3 summarizes the linear combinations by calculating the estimated net treatment effects for the four subgroups. It highlights that, while not significantly different from each other, there are significant emission reductions in all subgroups except low-emission capital-intensive firms, and that emission *intensity* reductions are driven by labor-intensive firms.

[12]The identifying variation in treatment in these estimations corresponds to variation in separate regressions for each (out of four) subcategory combinations of emission intensity and capital intensity.

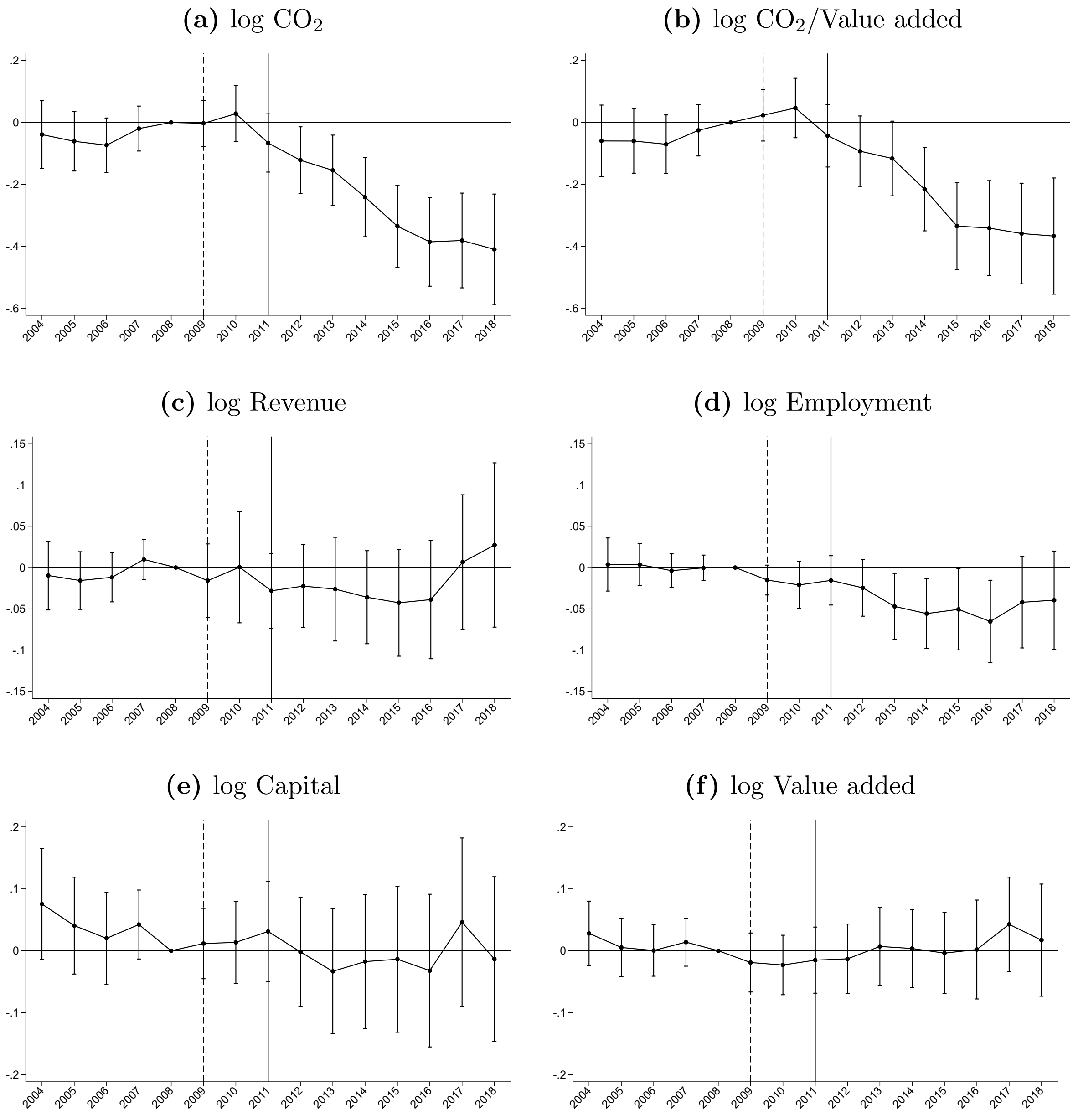


Figure 6: Event study results from estimating Eq. (1) on a balanced panel of firms. Regressions includes industry-by-year fixed effects, and standard errors are clustered by firm. Capped spikes show 95% confidence intervals.

	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)
$D \times Post$	-0.407***	-0.380**	-0.357***	-0.416**	0.019	0.097	-0.042	0.009	-0.021	0.036	0.023	0.084
	(0.095)	(0.170)	(0.100)	(0.179)	(0.051)	(0.072)	(0.031)	(0.049)	(0.068)	(0.114)	(0.045)	(0.066)
$D \times Post \times CO_2\ int.$		-0.177		0.017		0.008		-0.056		0.049		0.010
		(0.181)		(0.189)		(0.124)		(0.070)		(0.144)		(0.110)
$D \times Post \times Capital\ int.$		0.212		0.265		-0.011		0.024		0.018		-0.027
		(0.174)		(0.182)		(0.101)		(0.060)		(0.132)		(0.091)
$CO_2\ int. \times Post$		✓		✓		✓		✓		✓		✓
$Capital\ int. \times Post$		✓		✓		✓		✓		✓		✓
Observations	2,122	2,122	2,104	2,104	2,894	2,894	2,850	2,850	2,864	2,864	2,858	2,858

Table 3: Long difference results from estimating an extension of Eq. (2) on a balanced panel of firms. $CO_2\ int._j$ is an indicator equal to one for firms with a CO_2 intensity (defined as ton CO_2/value added) above the 2-digit industry median (balanced sample) in 2007. $Capital\ int._j$ is an indicator equal to one for firms with a capital ratio (fixed assets/employees) above the 2-digit industry median (balanced sample) in 2007. All regressions include firm FE and industry-year FE. Standard errors in parenthesis are clustered by firm. *** p<0.01, ** p<0.05, * p<0.1

Worker heterogeneity This part explores heterogeneity in employment 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'.[13] I find that the modest effect on average employment 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 10% after the reform, with smaller and insignificant effects for workers in the higher education categories. This effect exists both 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. The effect is also significantly negative for women with a high school degree (-9%). Columns (4)-(7) estimate the treatment effects for separate age groups. In Panel A, the result is significant for workers between 40-49 and 50-64 years old, 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 repeat the heterogeneity analysis with respect to firms' emission and capital intensities from Table 3 for employment across educational categories. The result is presented in Table 5, which summarizes the resulting linear combinations of firm subgroups. The result shows that the negative effects on the number of low-education workers is concentrated among emission-intensive firms, regardless of capital-intensity. This is in line with emission-intensive firms being more financially exposed to increases in carbon tax rates. The impacts in column (4) also indicate the employment is decreasing in the group of workers with a degree above high school, although statistical precision is low.

Labor turnover and firm exit In order to understand the margins of adjustments behind the negative effects on employment, I disentangle the changes in firms' average hiring and separation rates. The result from the event-study estimation is presented in Figure 7a and 7b. The negative point estimates for hiring rates after 2011 and close-to-zero estimates for separation rates for workers without a high school degree suggests that firms are adjusting their labor force by reducing their hiring rate instead of layoffs. However, the statistical uncertainty in this analysis is large. Figure 7c analyze the effects on firm exit, by estimating the event study specification using the unbalanced sample and a binary outcome variable indicating whether the current year is the last year that the firm is observed in the data. The reform does not seem to have had an impact on firms exiting the market.

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

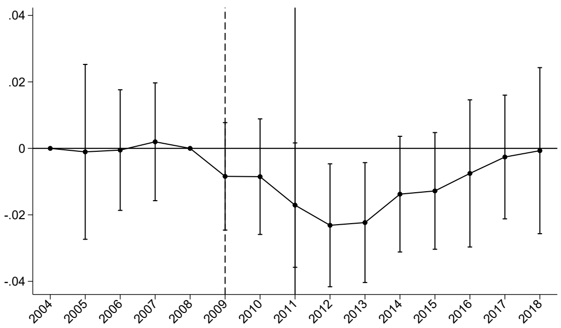
	log Employment						
				Age			
	All	Male	Female	16 - 29	30 - 39	40 - 49	50 - 64
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: No high school						
$D \times Post$	-0.103***	-0.102***	-0.155***	-0.068	0.009	-0.186***	-0.123***
	(0.038)	(0.039)	(0.055)	(0.076)	(0.071)	(0.065)	(0.043)
Observations	2,660	2,574	1,458	1,208	1,250	1,522	2,354
	Panel B: High school						
$D \times Post$	-0.007	0.005	-0.092**	-0.080	-0.046	-0.046	-0.020
	(0.031)	(0.032)	(0.040)	(0.053)	(0.051)	(0.044)	(0.042)
Observations	2,846	2,842	2,404	2,344	2,426	2,664	2,696
	Panel C: Above high school						
$D \times Post$	-0.049	-0.059	-0.018	-0.111	0.035	-0.071	-0.071
	(0.038)	(0.040)	(0.050)	(0.072)	(0.062)	(0.055)	(0.050)
Observations	2,532	2,382	1,682	1,136	1,646	1,678	1,784

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

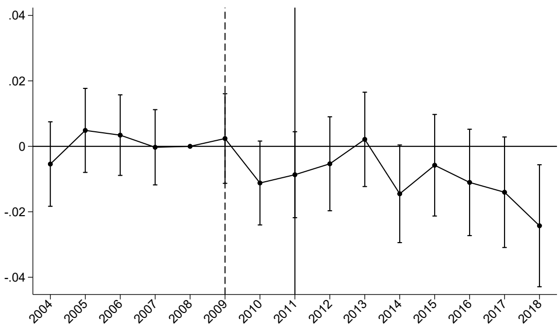
	log Employment			
	All	No high school	High school	Above high school
	(1)	(2)	(3)	(4)
Low emission & Labor int.	0.009	0.042	0.023	-0.015
	(0.049)	(0.065)	(0.052)	(0.063)
Low emission & Capital int.	0.033	-0.029	0.043	0.046
	(0.045)	(0.056)	(0.047)	(0.057)
High emission & Labor int.	-0.046	-0.170**	0.004	-0.150*
	(0.060)	(0.074)	(0.065)	(0.082)
High emission & Capital int.	-0.023	-0.242***	0.024	-0.089
	(0.079)	(0.076)	(0.078)	(0.087)

Table 5: Linear combinations of estimated treatment effects from the specification presented in Table 3. Standard errors in parentheses are calculated using the Delta method. *** p<0.01, ** p<0.05, * p<0.1

(a) Hiring rate



(b) Separation rate



(c) Firm exit

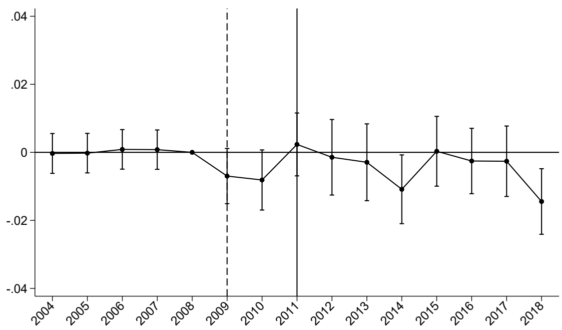


Figure 7: Event study results on labor turnover and firm exit from estimating Eq. (1). Figure 7a and 7b use a balanced sample of firms, while Figure 7c is estimated using an unbalanced sample. Regressions includes industry-by-year fixed effects, and standard errors are clustered by firm. Capped spikes show 95% confidence intervals.

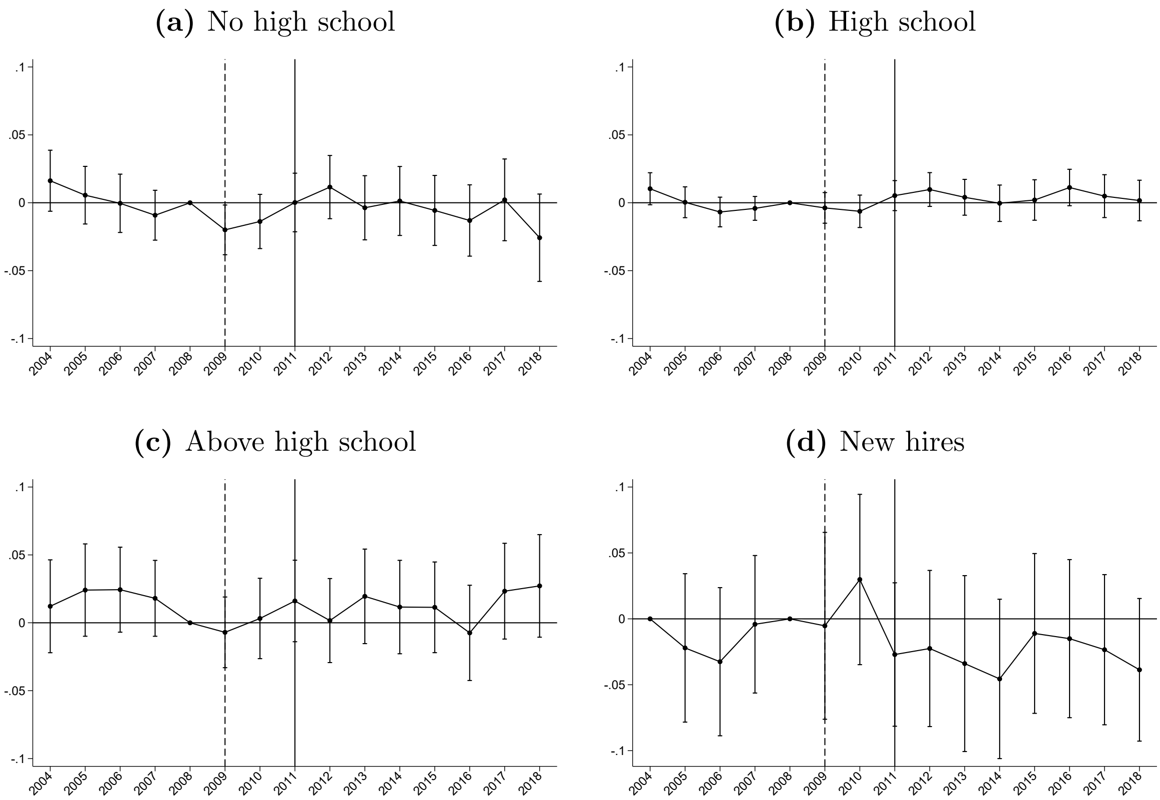


Figure 8: Event study results on workers' log average annual income from estimating Eq. (1) using a balanced panel of firms. Regressions includes industry-by-year fixed effects, and standard errors are clustered by firm. Capped spikes show 95% confidence intervals.

Worker income Finally, I study the effects of the carbon tax reform on workers' income. Figure 8 presents the results from estimating Eq. (1) on annual income from workers' primary earnings, averaged to each firm-year. Figure 8a to 8c show that the reform did not have large effects on income for 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 8d. While the point estimates indicates a negative trend in income of new hires at the implementation of the reform, they are statistically insignificant.

Fuel-switching Figure 9 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 carbon tax rate. The remaining fuels are categorized as biofuels, except for electricity. All fuels are measured in MWh. The results in Figure 9a and 9b 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 (-32% and 36%, respectively), with opposite signs. Figure 9c shows suggestive evidence of a lower electricity consumption, which could be explained by a general energy efficiency optimization or lower production. Figure 9d shows the resulting effect on total energy use, which is significantly reduced by 10% at the end of the period.

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-difference 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:

$$\Delta \text{CO}_2\text{TAX}_j = \Gamma_I + \gamma D_j + \Delta v_j \tag{3}$$

$$\Delta \log Y_j = \Gamma_I + \phi \widehat{\Delta \text{CO}_2\text{TAX}}_j + \Delta \varepsilon_j \tag{4}$$

where $\Delta \log Y_j$ is the 2018-2008 change in $\log Y_j$ for firm j, Γ_I are industry fixed effects, and $\Delta \text{CO}_2\text{TAX}_j$ is the 2018-2008 change in firm j's effective carbon tax rate. The

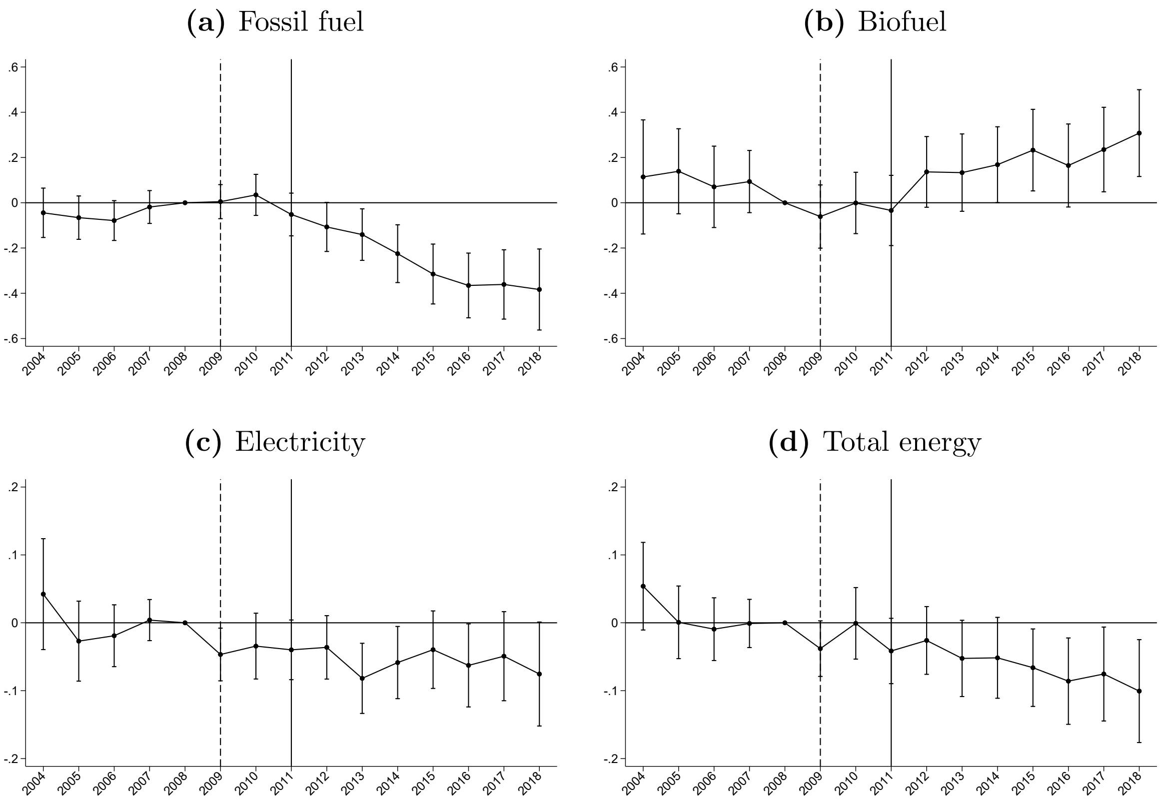


Figure 9: Event study results on the log of firms' energy consumption in MWh from estimating Eq. (1) using a balanced panel of firms. Regressions includes industry-by-year fixed effects, and standard errors are clustered by firm. Capped spikes show 95% confidence intervals.

	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			
$\widehat{\Delta CO_2 TAX}$	-0.0064***	-0.0010***	-0.0017***	-0.0006	-0.0008
	(0.0014)	(0.0004)	(0.0006)	(0.0004)	(0.0006)
$\widehat{\gamma}$	66.12	67.66	68.22	67.66	67.41
F-stat	1,527.76	2,145.57	2,089.70	2,145.57	1,882.37
Observations	1,061.00	1,258.00	1,204.00	1,258.00	1,144.00
		Panel B: Emission-intensive firms			
$\widehat{\Delta CO_2 TAX}$	-0.0060***	-0.0018***	-0.0032***	-0.0011	-0.0024**
	(0.0020)	(0.0006)	(0.0011)	(0.0007)	(0.0012)
$\widehat{\gamma}$	67.53	68.23	69.14	68.23	66.74
F-stat	449.46	599.48	566.95	599.48	496.16
Observations	523.00	602.00	573.00	602.00	539.00

Table 6: Semi-elasticities with respect to a 1 EUR/ton CO_2 increase from estimating Eq. (4)

result is presented in Table 6. 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 rates by around 67 EUR/ton CO_2, compared to the control group. An estimated $\widehat{\phi}$ of -0.0064 suggests that emissions fall by 0.64% 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 0.17% for each EUR/ton. Panel B restricts the sample to firms with an emission intensity above the industry median in 2007, with the same definition as in Table 3. The predicted $\widehat{\gamma}$'s indicate that emission-intensive firms were exposed to the same tax rate shock, with a similar semi-elasticity of emissions of -0.60%. 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 -0.32%, respectively. In line with the suggestive evidence in Table 5, emission-intensive firms also significantly reduce the number of high-educated workers.

6 Robustness Checks

In this section, I explore the robustness of the results to various sample restrictions and additional controls. First, I add control variables to test the sensitivity of the estimations to potentially confounding economic shocks, such as the financial crisis, and business cycles in general. 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 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.2 presents the event study result from estimating Eq. (1) with year indicators interacted with the set of business cycle 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.4 presents the event-study estimates for employment for each educational group to investigate pre-trends, to lend credibility to the heterogeneity analysis along this dimension. Subfigures to the left in the figure represent estimates from the baseline regression, and do not point to differential employment trends in any of the groups before the reform. Subfigures to the right add the business cycle exposure variables defined above, and results are again robust to these controls.

The baseline results are also well replicated using the unbalanced sample. The result is presented in Figure A.3 and shows significant reductions in emissions and emission intensity, with similar magnitudes as the balanced sample.[14] I again find a negative effect of the reform on revenue (now significant) and employment. The estimations suggest that compositional effects are not important for the mechanisms behind the main results, and

[14]The differential pre-trends in Figure A.3 are likely partly driven by the selection on firm survival. Firms are entering the sample between 2004-2007, thus changing the composition of the treatment and control group compared to 2008. Since treated firms are larger, and larger firms are more likely to survive (and therefore to enter the sample earlier), the firms entering are to a larger extent changing the composition of untreated firms. The untreated firms that entered in 2004 are more likely larger than those entering in 2007, which causes a differential (negative) trend.

that the validity of the baseline result is not limited to the subset of surviving firms over the sample period.

7 Discussion and Conclusion

In this paper, I provide new empirical evidence on the impacts of climate policy on firm performance and labor demand, using a novel methodology and rich administrative datasets. I find that more stringent climate policy, induced by the removal of carbon tax refunds, significantly reduced emissions among Swedish manufacturing firms. I also find negative effects on employment among emission-intensive firms. 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 (Colmer et al., 2024; Dechezleprêtre et al., 2023; 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 67 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.

The last point is important for the external validity of the results in this paper. 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, nda). 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 (-0.64%/EUR) could help predict the emission reduction gains from such a policy.

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 and 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 and Michaels, 2018). It is, however, important to note the potentially different mechanisms behind the skill-bias in, for example, automation and carbon taxation, where the former is characterized by market-driven, productivity-enhancing (at the firm-level) task displacement and a potentially negative substitution effect between new technology and low-skill workers (Acemoglu and Restrepo, 2022). It is possible that the estimated skill-bias in this paper is channeled by a combination of a negative output effect and ambiguous substitution effects, depending on the induced behavior among regulated firms and their possibilities to adopt new technologies. The existence of a dominating output effect would suggest different relative impacts on high- versus low-educated workers under, for example, green subsidies (Popp et al., 2021).

The lower labor 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, ndb). However, to fully understand the transitional costs of the green transition, one must be able to observe the impacts on individuals' career trajectories. These costs will depend on the extent to which individuals are reallocated to new sectors in which their skills are less compatible (Walker, 2013), and their ability to move to expanding, green firms (Curtis et al., 2024; Weber, 2020). These issues warrant further research.

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A Appendix

	Mean	SD	Min	Max
#Firms	3,211.00	0.00	3,211.00	3,211.00
Treatment	0.50	0.50	0.00	1.00
Value added (mSEK)	67.29	542.48	-4,908.66	30,630.22
Revenue (mSEK)	242.25	2,283.21	-0.18	120,555.02
Fixed assets (mSEK)	82.06	945.91	-5.31	46,315.63
Exporter	0.74	0.44	0.00	1.00
CO_2 emissions (ton)	309.93	1,091.54	0.00	54,779.79
CO_2 intensity (ton/mSEK)	7.61	249.52	-28,441.16	19,847.65
Fossil energy share	0.27	0.26	0.00	1.00
Average income	299,162.01	69,793.56	44,800.00	1,656,682.76
Employment	88.99	406.64	1.00	19,113.00
Employment: No high school	0.21	0.13	0.00	1.00
Employment: High school	0.63	0.15	0.00	1.00
Employment: Above high school	0.16	0.13	0.00	1.00
Employment: STEM	0.10	0.10	0.00	1.00
Employment: Female	0.21	0.17	0.00	1.00
Employment: Age 16-29	0.18	0.12	0.00	1.00
Employment: Age 30-39	0.22	0.11	0.00	1.00
Employment: Age 40-49	0.27	0.11	0.00	1.00
Employment: Age 50-64	0.34	0.16	0.00	1.00

Table A.1: 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.

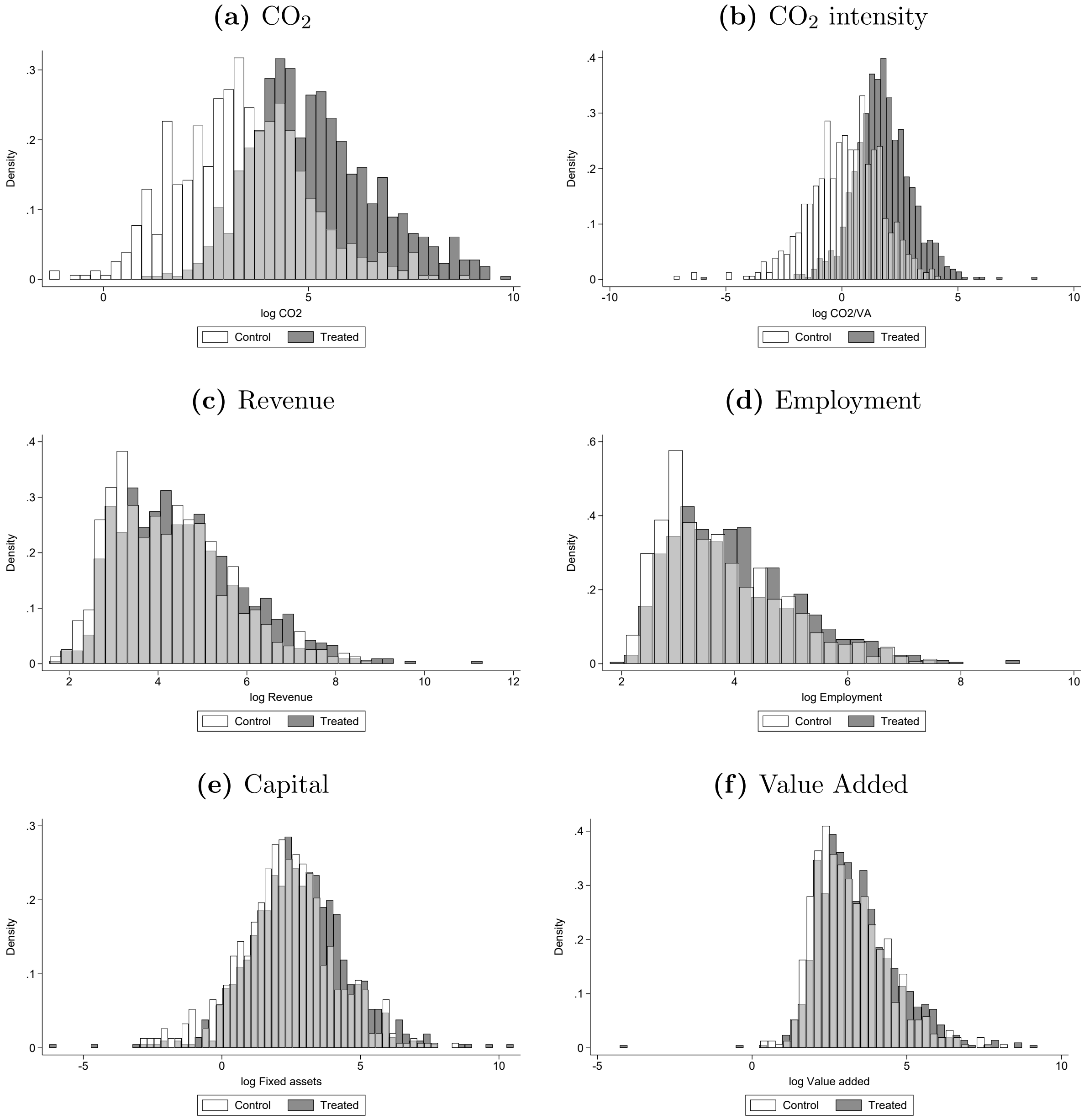


Figure A.1: 2008 distribution by treatment status

	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)
$D \times Post$	-0.407***	-0.401***	-0.357***	-0.354***	0.019	0.005	-0.042	-0.041	-0.021	-0.030	0.023	0.017
	(0.095)	(0.095)	(0.100)	(0.100)	(0.051)	(0.050)	(0.031)	(0.031)	(0.068)	(0.067)	(0.045)	(0.045)
$EX' \times Post$		✓		✓		✓		✓		✓		✓
$L' \times Post$		✓		✓		✓		✓		✓		✓
$K \times Post$		✓		✓		✓		✓		✓		✓
Observations	2,122	2,122	2,104	2,104	2,894	2,894	2,850	2,850	2,864	2,864	2,858	2,858

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 4. Standard errors in parenthesis are clustered by firm. *** p<0.01, ** p<0.05, * p<0.1

	log CO_2	log CO_2/VA	log Revenue	log Employment	log Capital	log VA
	(1)	(2)	(3)	(4)	(5)	(6)
Low emission & Labor int.	-0.380**	-0.416**	0.097	0.009	0.036	0.084
	(0.170)	(0.179)	(0.072)	(0.049)	(0.114)	(0.066)
Low emission & Capital int.	-0.167	-0.151	0.086	0.033	0.054	0.057
	(0.151)	(0.158)	(0.069)	(0.045)	(0.100)	(0.065)
High emission & Labor int.	-0.556***	-0.400**	0.105	-0.046	0.084	0.094
	(0.158)	(0.166)	(0.117)	(0.060)	(0.136)	(0.107)
High emission & Capital int.	-0.344**	-0.135	0.093	-0.023	0.102	0.067
	(0.159)	(0.166)	(0.135)	(0.079)	(0.137)	(0.113)

Table A.3: Linear combinations of estimated treatment effects from Table 3. Standard errors in parentheses are calculated using the Delta method. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

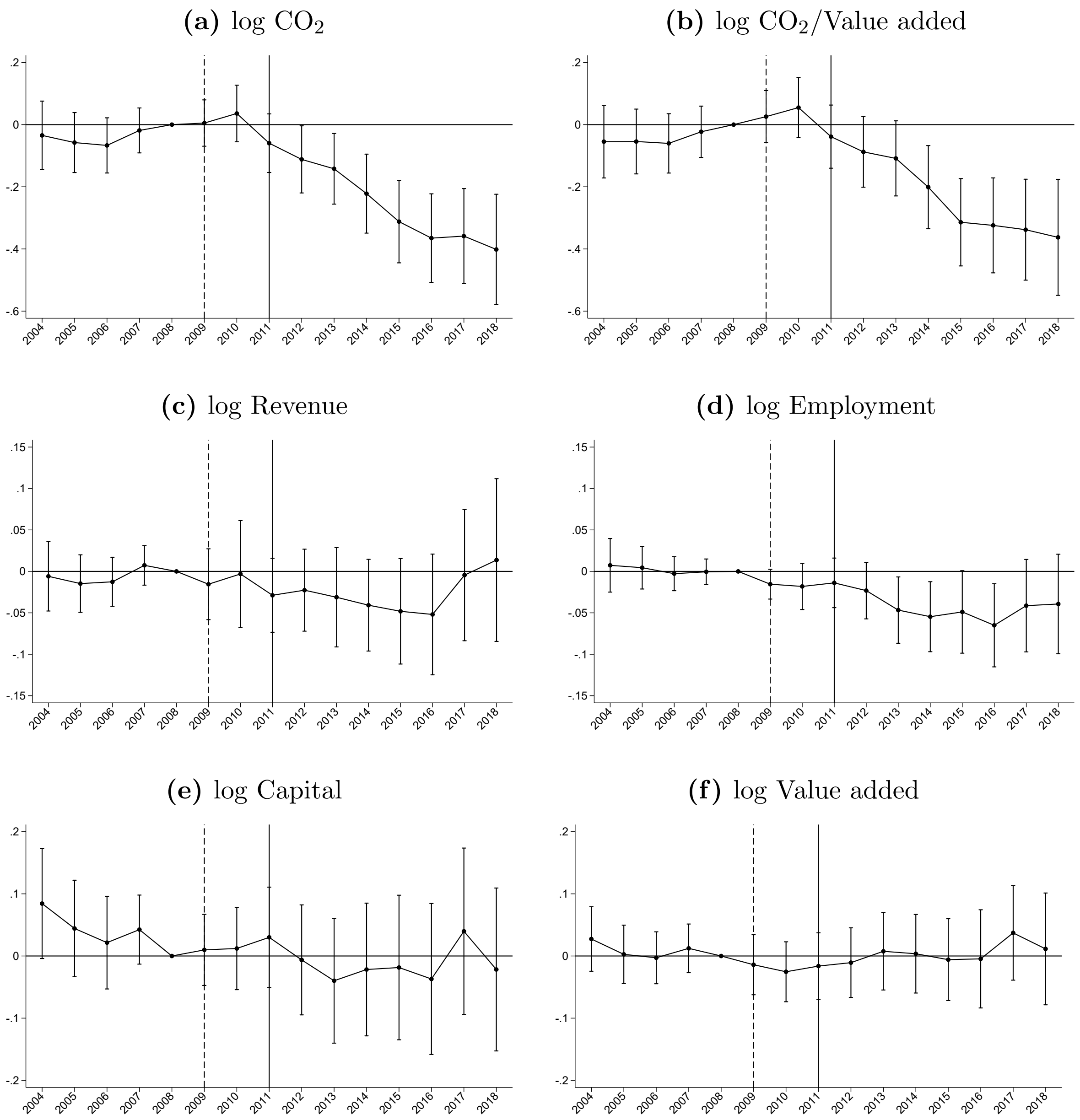


Figure A.2: Event study results from estimating Eq. (1) on a balanced panel of firms. Regressions includes industry-by-year fixed effects and time-varying controls for business cycle exposure, and standard errors are clustered by firm. Capped spikes show 95% confidence intervals.

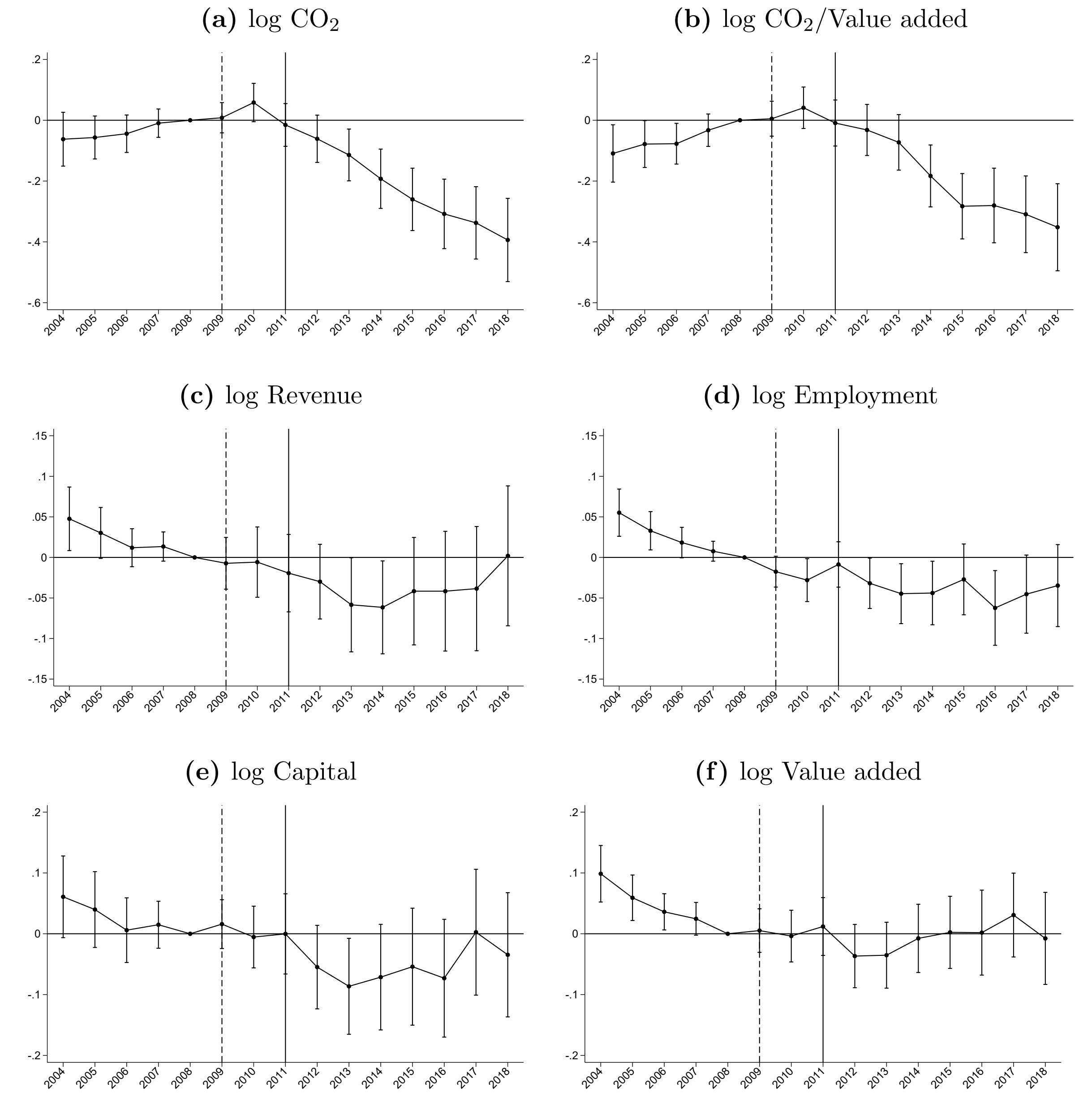
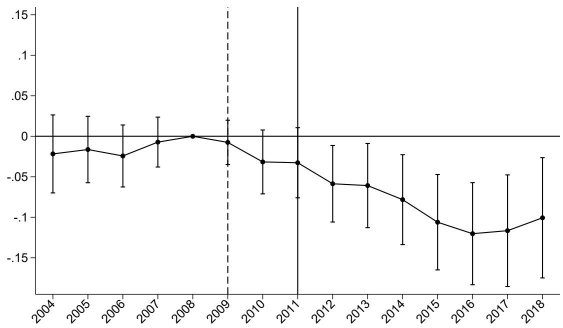
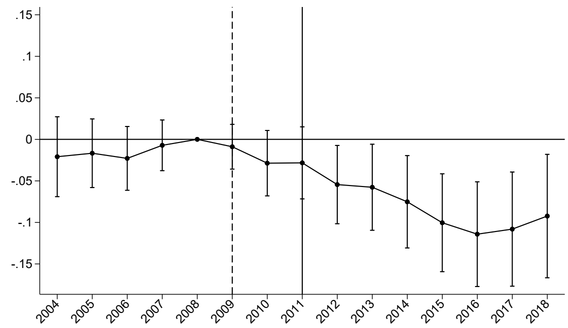


Figure A.3: Event study results from estimating Eq. (1) on the unbalanced sample. Regressions includes industry-by-year fixed effects, and standard errors are clustered by firm. Capped spikes show 95% confidence intervals.

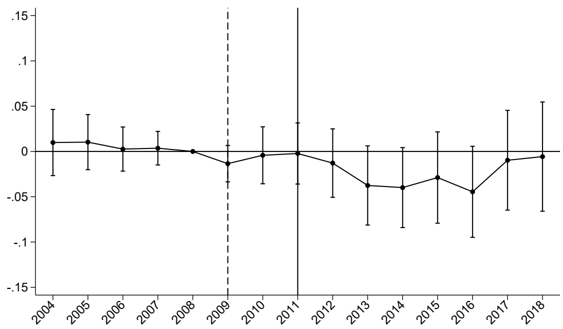
(a) No high school



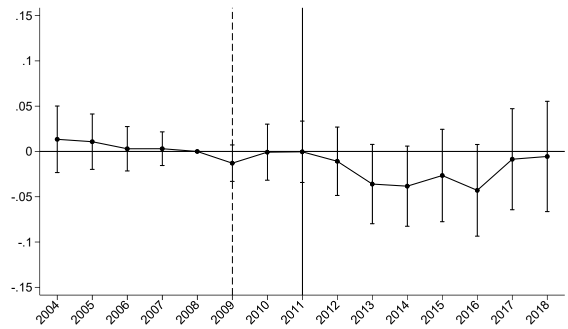
(b) No high school



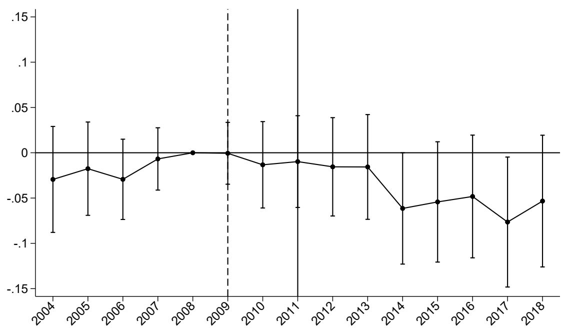
(c) High school



(d) High school



(e) Above high school



(f) Above high school

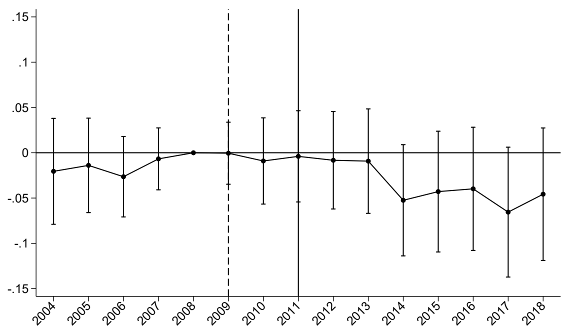


Figure A.4: Event study results on employment by educational attainment from estimating Eq. (1) using a balanced panel of firms. All regressions includes industry-by-year fixed effects, and standard errors are clustered by firm. Figure A.4b, A.4d, and A.4f additionally control for time-varying business-cycle exposure. Capped spikes show 95% confidence intervals.