Climate Policy and Labor Market Inequality

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Abstract

This paper investigates the effects of carbon taxation on labor market inequality in Sweden. 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. Further results suggest that this effect is driven by a reduction in the hiring rate of this group. 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.

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1 Introduction

More countries are adopting climate policy strategies to comply with their national targets as part of the Paris Agreement. Carbon taxation remains the most prominent climate policy instrument (World Bank, 2024). However, fear of negative economic impacts and increasing labor market inequality has challenged the political support for climate policy (Vona, 2019). Despite the urgency of the climate crisis, there is still a lack of empirical evidence on the distributional impacts of climate policy and its effects on labor market inequality (Gray et al., 2023; Vrolijk and Sato, 2023).

This paper studies the effects of climate policy on firm performance and labor market inequality, combining changes in the Swedish carbon tax with administrative data on firms and workers. The tax, which is imposed on fossil fuels used for heating and engine operation, is one of the highest in the world (World Bank, 2024). I exploit a reform in the policy to isolate exogeneous variation in firm-level effective carbon tax rates. The reform increased the stringency of the carbon tax for manufacturing firms by gradually removing the possibility to apply for tax refunds over the years 2011-2018. I develop an empirical framework based on firms' differential exposure to the reform, which stems from variation in pre-reform uptake of carbon tax refunds.

I find that the increase in effective carbon tax rates led to a substantial reduction in firms' total emissions as well as emission *intensity*, by up to 40% over the time period. However, I also observe significantly negative effects on firm performance. Heterogeneity analyses reveal that, while emission reductions are found in firms with low and high emission intensity, negative effects on revenue, value added and employment are concentrated in emission-intensive firms. Further, I find that the negative effect on employment is skill-biased against low-educated workers, as it is entirely driven by a reduction in the number of workers without a high school degree. Within this group, I find stronger effects for older workers and males. There are no, however, any significant effects on workers' income, regardless of educational attainment. I do not observe differential trends in outcomes before the policy change, indicating that the estimated effects are indeed causal. The results are also robust to controlling for a number time-varying shocks at the firm-level, mitigating concerns of biases from differential exposure to business cycles correlated with treatment.

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 either rely on regional treatment variation without possibilities to identify firm-level mechanisms (Yamazaki, 2017, 2019; Yip, 2018), or evaluate environmental regulations that are outside the scope of climate policy (Walker, 2013).[1]

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 occupation-level 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

[1] Walker (2013) uses linked worker-firm data to evaluate the effects on workers' earnings of the 1990 US Clean Air Act Amendments (CAAA). While being able to investigate heterogeneous effects on different types of workers, the 1990 CAAA does not constitute a market-based climate policy, making the conclusions in Walker (2013) less generalizable to carbon pricing. Some papers also study employment effects of investments in renewable energy, which is a potential outcome of climate policy (Fabra et al., 2024; Gilbert et al., 2024).

[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.

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 relevant features 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 provides a discussion of the results, 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, nd). The tax is measured in Swedish Krona (SEK) per volume of fuel and varies across fuels based on their carbon content, such 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 relies on sufficient cost pass-through from the energy sector to incentivize emission reductions in the overall economy.

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-

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

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).

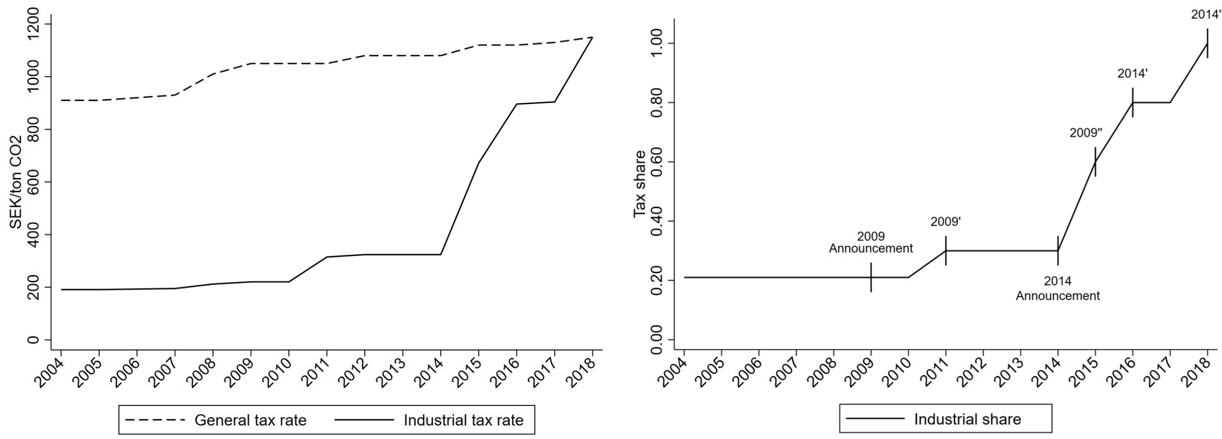
The manufacturing rebates were gradually reduced from 2011, and completely removed in 2018. The phase-out of the rebates was communicated in two steps, which are shown in Figure 1b. In 2009, the government released a new plan to achieve its medium-term climate targets. The plan included an increase in the share of the carbon 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

[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.



(a) General and industrial CO_2 tax rates

(b) Policy announcements

Figure 1: Timing of policy implementation. Figure 1a shows 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.

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 individuals in working age (16-64) by the organizational number of a worker's employer in November in a given year, which provides information on worker characteristics such as age, gender, educational history and income.

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.[7] 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.[8]

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

[7] The dataset currently misses observations for 2013.

[8] 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.

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.[9] This unbalanced panel is used to evaluate the sensitivity of the result to compositional changes (i.e. firm exit). Note that the restriction allows firms to enter the sample any year between 2004-2007, as long as they are observed in 2007-2008. A comparison of the balanced sample and the unbalanced one will also be informative regarding the external validity of the result.

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 *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 restrictions result in a dataset of 3,222 unique firms, of which 1,163 belongs to the balanced sample. Table 1 presents summary statistics for a selection of variables for the control and treatment group, respectively, for the two samples. The main outcomes analyzed in this paper are firms' CO_2 emissions, CO_2 intensity (defined as ton CO_2 over value added), revenue, employment, capital (fixed assets), and value added. All monetary variables are measured in million Swedish Krona (mSEK). Treated firms (i.e. those with a carbon tax rebate in 2008) are on average larger in terms of employment, revenue, capital (fixed assets), and value added. The difference is, however, most pronounced when comparing CO_2. Treated firms use more fossil fuels in relation to their total energy consumption, and causes substantially more emissions. To some degree, these differences might reflect the higher incentives for emission-intensive firms to apply for tax refunds, and more knowledge about refund application possibilities among larger firms. Still, Figure 9

[9]Observations in 2007 are used to construct control variables related to exposure to the Great Recession in the sensitivity analysis.

(a) CO_2 (b) Employment

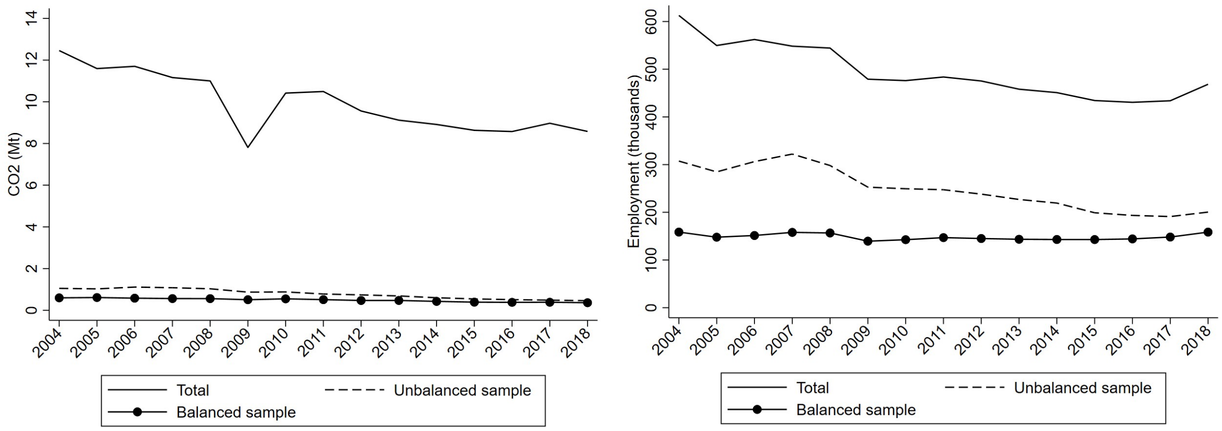


Figure 2: Aggregate manufacturing emissions and employment. Aggregates are calculated from firms in the Energy Use in Manufacturing survey (ISEN).

in the Appendix shows that there is considerable overlap in outcomes between the control and treatment group when log-transformed. Figure 3 shows the distribution of treatment across industries. Reassuringly, there is within-industry variation in treatment, as both treated and control firms are found in the majority of manufacturing industries. The firms are linked to a repeated cross-section of individuals employed at some point in the sample period, which covered 298,213 unique individuals in 2008. The typical worker is a high-school-educated male, and while workers in treated firms are slightly more educated, there are small differences in worker composition in terms of gender and age between treated and control firms in the main (balanced) sample.

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 Great Recession 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 extra caution when

Table 1: Descriptive Statistics

	Mean 2008				
	Unbalanced			Balanced	
	All	Control	Treated	Control	Treated
	(1)	(2)	(3)	(4)	(5)
Firms	3,222	1,628	1,594	464	699
Employment	92.64	64.71	121.15	107.43	152.91
Revenue (mSEK)	239.07	141.66	338.57	255.75	410.68
Fixed assets (mSEK)	85.73	56.93	115.14	128.83	159.91
Value added (mSEK)	66.06	43.17	89.44	85.16	116.81
Exporter	0.15	0.13	0.17	0.23	0.24
CO_2 emissions (ton)	320.55	102.47	543.29	136.64	711.96
CO_2 intensity (ton/mSEK)	9.36	2.64	16.24	3.02	12.48
Fossil energy share	0.30	0.22	0.39	0.18	0.35
Workers	298,213	105,218	192,995	49,848	106,885
Income (SEK)	291,722.5	294,400.1	288,989.4	302,520.6	293,306.0
No high school	0.23	0.22	0.25	0.20	0.24
High school	0.62	0.63	0.61	0.63	0.61
Above high school	0.15	0.15	0.14	0.17	0.15
STEM	0.09	0.09	0.08	0.11	0.09
Female	0.22	0.21	0.22	0.21	0.23
Age 16-29	0.20	0.19	0.20	0.18	0.19
Age 30-39	0.22	0.23	0.22	0.24	0.23
Age 40-49	0.27	0.27	0.27	0.28	0.27
Age 50-64	0.31	0.31	0.31	0.30	0.31

Note: 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. Exporter shows the share of firms with non-zero sales to abroad in 2008. 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. STEM shows the average share of employed workers with a higher education (above high school) in Science, Technology, Engineering and Mathematics.

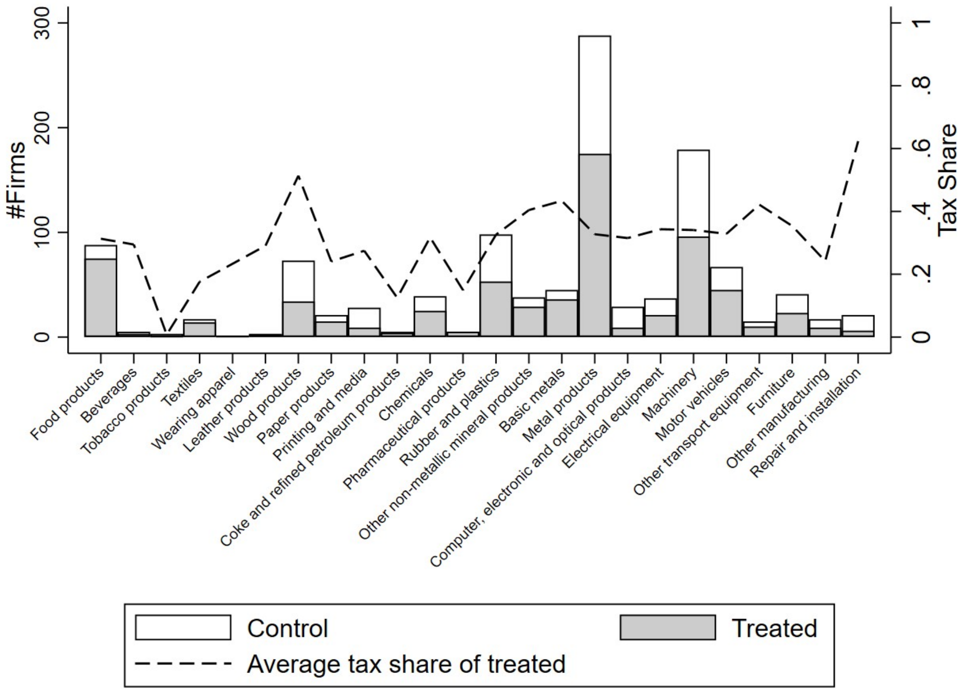


Figure 3: Treatment by industry as defined by the pre-reform uptake of CO_2 tax refunds. Dashed line represents the average tax share among treated firms within the industry.

analyzing this outcome. A general sensitivity analysis with respect to influence from the Great Recession is carried out in the empirical section.

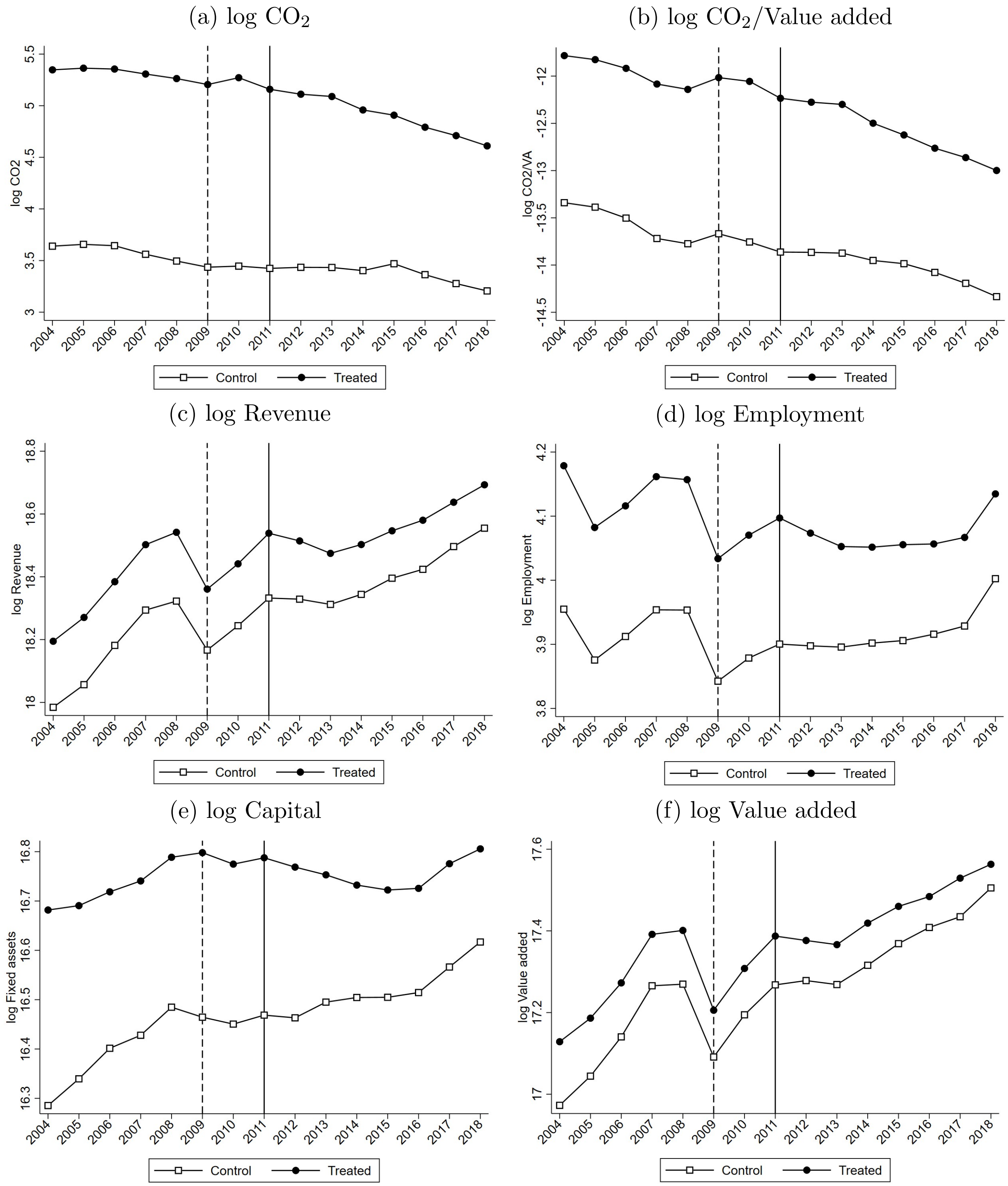


Figure 4: Trends in average outcomes

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

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

where 2008 is the omitted year of reference.[10] 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

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

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

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

to control for industry shocks. β represents the long-difference estimate of the complete phase-out of the tax rebates.

I also add control variables to test the sensitivity of the estimations to potentially confounding financial shocks, such as the Great Recession, 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 effect, 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.

Both approaches rely on the assumption that pre-announcement tax rebate status D_j is exogenous to unobserved, within-industry changes 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 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.

(a) CO_2 tax share (b) Energy tax share

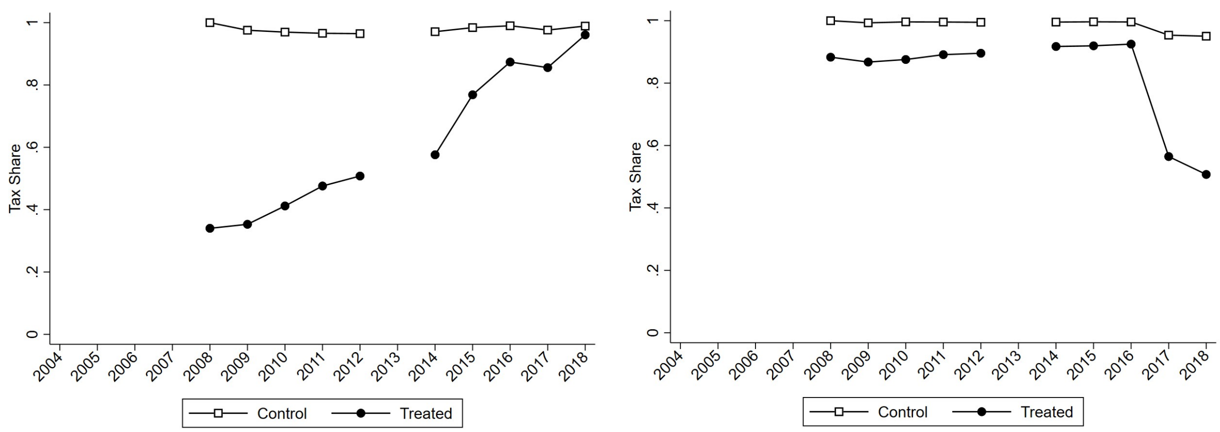


Figure 5: Firm-level fuel tax shares. CO_2 tax shares are calculated by combining each fuel's gross CO_2 tax rate with firms' fossil fuel consumption and CO_2 tax refunds. Energy tax shares are calculated by combining each fuel's gross *energy* tax rate (which is a separate tax imposed on fuels) with 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.

5 Result

5.1 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, by up to 40%. 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*.

However, I also find a significantly negative effect on revenue and employment. The negative effects on economic performance are smaller than for emissions (around -7%), and remain significant over the time period. A similar pattern is observed for value added, although without statistical significance. The differential pre-trend and large confidence intervals for capital preclude conclusions regarding the effects on this outcome.

The differential effect on revenue and employment for treated firms during the announcement period is likely caused by differential impacts of the financial crisis. The drastic fall in 2009 suggests that manufacturing firms with a carbon tax rebate in 2008 share characteristics that made them more susceptible to the economic downturn. However, this decline is fully recovered before the implementation of the reform, and has a trend in opposite direction of the estimated treatment effect. This suggests that differential recovery paths after the crisis are not likely to drive the result. To investigate this issue further, I test the robustness of previous results by controlling for business cycle exposure, as explained in Section 4. Table 2 presents the baseline result from estimating Eq. (2), as well as results from the amended regression, which interacts the year indicator with indicators for export share, employment size, and capital size. Odd columns show the baseline result, which mimics the observed dynamic result of Figure 6, in which the higher carbon tax rate was found to decrease emissions and negatively affect economic performance. Even columns control for business cycle exposure proxies. Estimated coefficients are robust to adding control variables, and remain statistically significant, suggesting that correlated changes in economic conditions is not a concern.

I also investigate the sensitivity of the baseline result to sample restriction. In Figure 11 in the Appendix, I re-estimate the event-study model using the unbalanced sample. As before, the result shows significant reductions in emissions and emission intensity, with similar magnitudes as with the balanced sample. Likewise, I find a negative effect of the reform on revenue and employment. Regarding emissions, I observe differential pre-trends, which is likely due to the firms which are now entering the sample between 2004-2007. The estimations suggest that compositional effects are not important for the mechanisms behind the main results, and that the validity of the baseline result is not limited to the subset of surviving firms over the sample period.

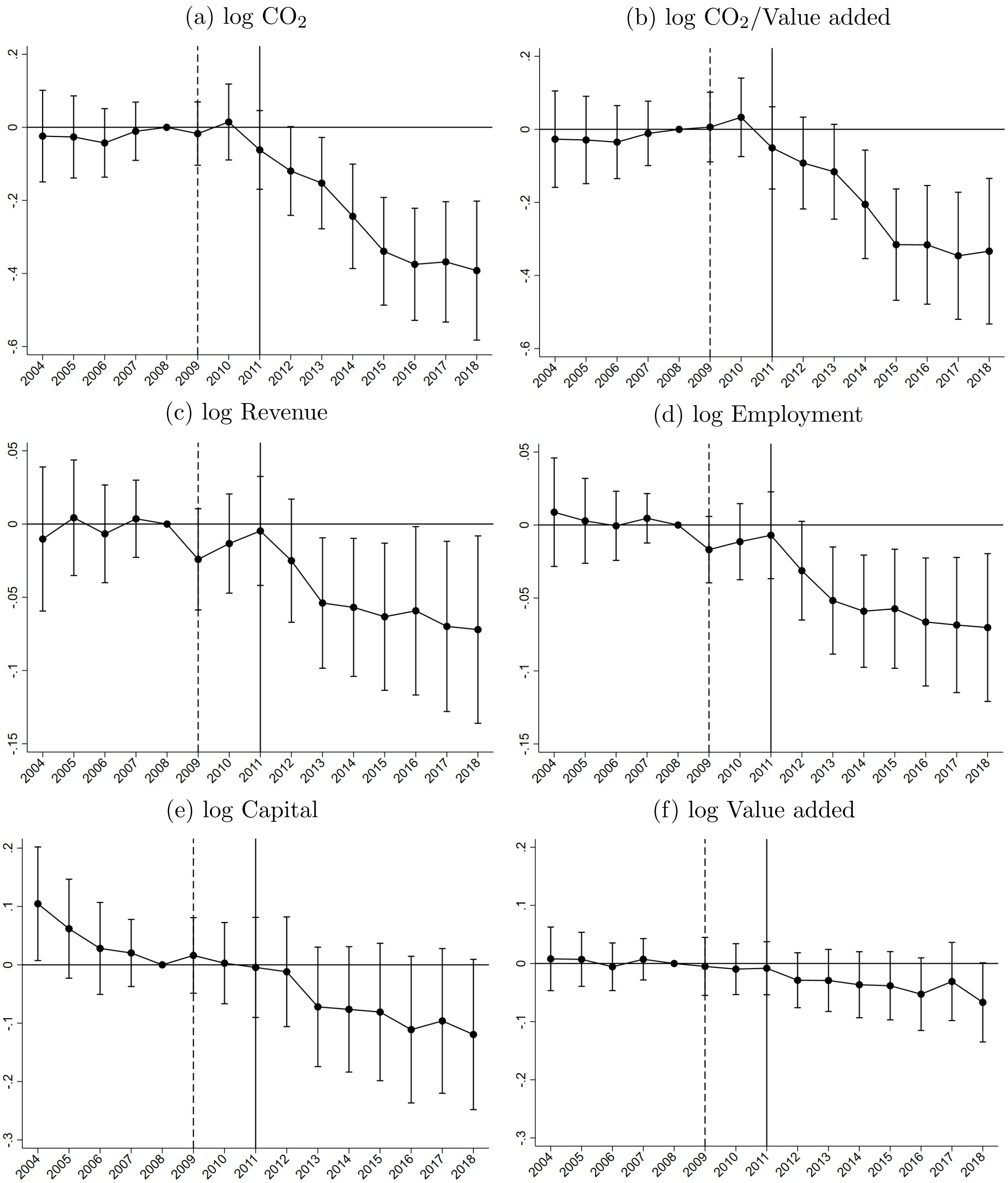


Figure 6: Event study results from estimating Eq. (1) on a balanced panel of firms. Regressions includes industry-by-year fixed effects, and standard errors are clustered by firm. Capped spikes show 95% confidence intervals.

	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)
$D \times Post$	-0.403***	-0.396***	-0.347***	-0.343***	-0.080**	-0.082**	-0.070***	-0.065**	-0.114*	-0.129**	-0.067*	-0.065*
	(0.098)	(0.098)	(0.103)	(0.103)	(0.032)	(0.032)	(0.025)	(0.025)	(0.066)	(0.065)	(0.034)	(0.034)
Observations	1,990	1,990	1,974	1,974	2,320	2,320	2,326	2,326	2,304	2,304	2,306	2,306
η_j	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
$Post_t$	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
$\Gamma_I \times Post_t$	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
$EX'_j \times Post_t$		✓		✓		✓		✓		✓		✓
$L'_j \times Post_t$		✓		✓		✓		✓		✓		✓
$K_j \times Post_t$		✓		✓		✓		✓		✓		✓

Table 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.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

5.2 Heterogeneity Analysis

Firm heterogeneity This section explores heterogeneity in the previous result, starting with 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 $\mathrm{CO_2}$ 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, and the business cycle fixed effects, to control for correlations between emission and capital intensities and firm size. I also control for an interaction with the year fixed effect for each intensity indicator, to isolate the variation in treatment.[11]

Column (2) reports the effects on total emissions. The point estimates suggest that both low and high emission-intensive firms reduce their emissions, with high emission-intensive firms responding more strongly (although not significantly). The effect on emission intensity (column (4)), has a similar pattern as total emissions. Interestingly, the negative effects on revenue and employment seem to be concentrated among emission-intensive firms (although only statistically different for employment). This may reflect that emission-intensive firms are more economically vulnerable to changes in carbon tax rates. Capital-intensive firms demonstrate a weaker effect on emissions than labor-intensive firms, possibly reflecting different technological abatement possibilities, although statistical precision for this interaction term is low. Table 4 summarizes the result of Table 3 by calculating the estimated net treatment effects for the four subgroups.

[11]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.

	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)
$D \times Post$	-0.396***	-0.336**	-0.343***	-0.368**	-0.082**	-0.040	-0.065**	0.009	-0.129**	-0.145	-0.065*	-0.013
	(0.099)	(0.168)	(0.103)	(0.179)	(0.032)	(0.050)	(0.025)	(0.042)	(0.065)	(0.112)	(0.034)	(0.058)
$D \times Post \times CO_2\ int.$		-0.252		-0.098		-0.085		-0.134**		0.042		-0.110
		(0.179)		(0.182)		(0.061)		(0.053)		(0.146)		(0.069)
$D \times Post \times Capital\ int.$		0.205		0.285		0.016		0.020		0.077		-0.018
		(0.179)		(0.187)		(0.063)		(0.049)		(0.127)		(0.067)
Observations	1,990	1,990	1,974	1,974	2,320	2,320	2,326	2,326	2,304	2,304	2,306	2,306
$CO_2\ int._j \times Post_t$		✓		✓		✓		✓		✓		✓
$Capital\ int._j \times Post_t$		✓		✓		✓		✓		✓		✓

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, industry-year FE, and the business cycle exposure controls in Table 2. 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	-0.336**	-0.368**	-0.040	0.009	-0.145	-0.013
	(0.168)	(0.179)	(0.050)	(0.042)	(0.112)	(0.058)
Low emission & Capital int	-0.130	-0.083	-0.023	0.029	-0.068	-0.031
	(0.164)	(0.171)	(0.057)	(0.042)	(0.097)	(0.055)
High emission & Labor int	-0.588***	-0.466***	-0.125**	-0.125**	-0.102	-0.124**
	(0.145)	(0.151)	(0.053)	(0.050)	(0.142)	(0.062)
High emission & Capital int	-0.382**	-0.181	-0.109*	-0.104**	-0.025	-0.142**
	(0.163)	(0.162)	(0.059)	(0.048)	(0.132)	(0.065)

Table 4: Linear combinations of estimated treatment effects from Table 3. Standard errors in parentheses are calculated using the Delta method. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Worker heterogeneity This part explores heterogeneity in employment effects for different types of workers. Table 5 presents the effects on employment for different educational categories, which are 'No high school', 'High school', and 'Above high school'.[12] The latter category is divided further into STEM (Science, Technology, Engineering and Mathematics) and non-STEM degrees. Panel A shows the result for all workers in the sample. I find that the negative effect on average employment from previous sections is entirely driven by a reduction in the number of workers without a high school degree, with a significant point estimate of -0.165. Workers in the other educational groups are not significantly affected. I neither observe any difference among highly educated workers between STEM and non-STEM degrees.

To investigate the extent to which the negative effect on low-education workers is driven by part-time workers, I make further sample restrictions in Panel B. As I do not observe worked hours, I use the notion that, due to a relatively compressed wage distribution, few

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

workers in Sweden that earn less than 60% of the national median income are full-time workers (Swedish National Mediation Office, 2020). I therefore focus on workers with annual incomes above this threshold, and run the same regressions.[13] The result shows a slightly smaller point estimate of -0.139, which is still significant at 1%. Part-time workers are hence unlikely to explain the negative impacts on low-educated workers.

Next, I present further results on the heterogeneity with respect to gender and age. In Table 6, I disaggregate the result on employment for males and females, and four different age brackets, within each educational category. The result again shows that workers with a high school degree or above are unaffected by the policy. The disaggregated heterogeneity analysis reveals that the negative employment effects for low-educated workers are largely driven by a reduction in the number of male workers, and workers between 40-64 years old.

I also investigate the margins of adjustments behind the negative effects on employment, by looking separately at changes in hiring and separation rates. The result from the event-study estimation is presented in Figure 7, which shows the effects of the policy change on hiring and separation rates for each educational category at the firm. 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. However, the statistical uncertainty in this analysis is large.

Finally, I study the effects of the carbon tax reform on workers' income. Figure 8 presents the results from estimating Eq. (1) on average annual income, aggregated to each firm-year. Figure 8a shows precisely estimated null effects, before and after the policy change. The confidence intervals are larger when running separate regressions for each educational category, although point estimates are centered around zero for each group, as shown by Figure 8b-8d. I also estimate the effect separately on the average income of new hires in each, 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 12 in the Appendix. While the point estimates indicates a negative trend in income of new hires at the announcement of the reform, they are insignificant and coinciding with the financial crisis, and therefore likely to be unrelated to the implementation of the reform.

[13]As median incomes are not published for 2008 by the statistical authorities, I use the mean income, which over-excludes workers to some extent.

	log Employment			
			Above high school	
	No high school	High school	All	STEM
	(1)	(2)	(3)	(4)
		Panel A: All workers		
$D \times Post$	-0.165***	-0.024	-0.050	-0.045
	(0.041)	(0.027)	(0.039)	(0.041)
Observations	2,264	2,326	2,134	1,934
		Panel B: Full-time workers†		
$D \times Post$	-0.139***	-0.032	-0.041	-0.053
	(0.040)	(0.027)	(0.039)	(0.042)
Observations	2,176	2,324	2,078	1,866
η_j	✓	✓	✓	✓
$Post_t$	✓	✓	✓	✓
$\Gamma_I \times Post_t$	✓	✓	✓	✓
$EX'_j \times Post_t$	✓	✓	✓	✓
$L'_j \times Post_t$	✓	✓	✓	✓
$K_j \times Post_t$	✓	✓	✓	✓

Table 5: Long difference results on employment from estimating Eq. (2) by education using a balanced panel of firms. 'STEM' includes workers with a higher education (above high school) in Science, Technology, Engineering and Mathematics. Standard errors in parenthesis are clustered by firm. *** p<0.01, ** p<0.05, * p<0.1.

†Excluding workers with incomes below 60% of mean income.

	log Employment					
			Age			
	Male	Female	16 - 29	30 - 39	40 - 49	50 - 64
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: No high school			
$D \times Post$	-0.172***	-0.071	-0.063	0.013	-0.146**	-0.127***
	(0.042)	(0.060)	(0.077)	(0.073)	(0.065)	(0.045)
Observations	2,216	1,380	1,344	1,150	1,358	2,032
			Panel B: High school			
$D \times Post$	-0.027	-0.040	-0.085	-0.012	-0.010	-0.041
	(0.028)	(0.043)	(0.057)	(0.052)	(0.046)	(0.041)
Observations	2,324	2,062	2,036	2,104	2,232	2,242
			Panel C: Above high school			
$D \times Post$	-0.036	-0.040	-0.080	0.047	-0.085	-0.047
	(0.041)	(0.053)	(0.074)	(0.061)	(0.057)	(0.053)
Observations	2,062	1,490	1,034	1,510	1,524	1,576
η_j	✓	✓	✓	✓	✓	✓
$Post_t$	✓	✓	✓	✓	✓	✓
$\Gamma_I \times Post_t$	✓	✓	✓	✓	✓	✓
$EX'_j \times Post_t$	✓	✓	✓	✓	✓	✓
$L'_j \times Post_t$	✓	✓	✓	✓	✓	✓
$K_j \times Post_t$	✓	✓	✓	✓	✓	✓

Table 6: 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

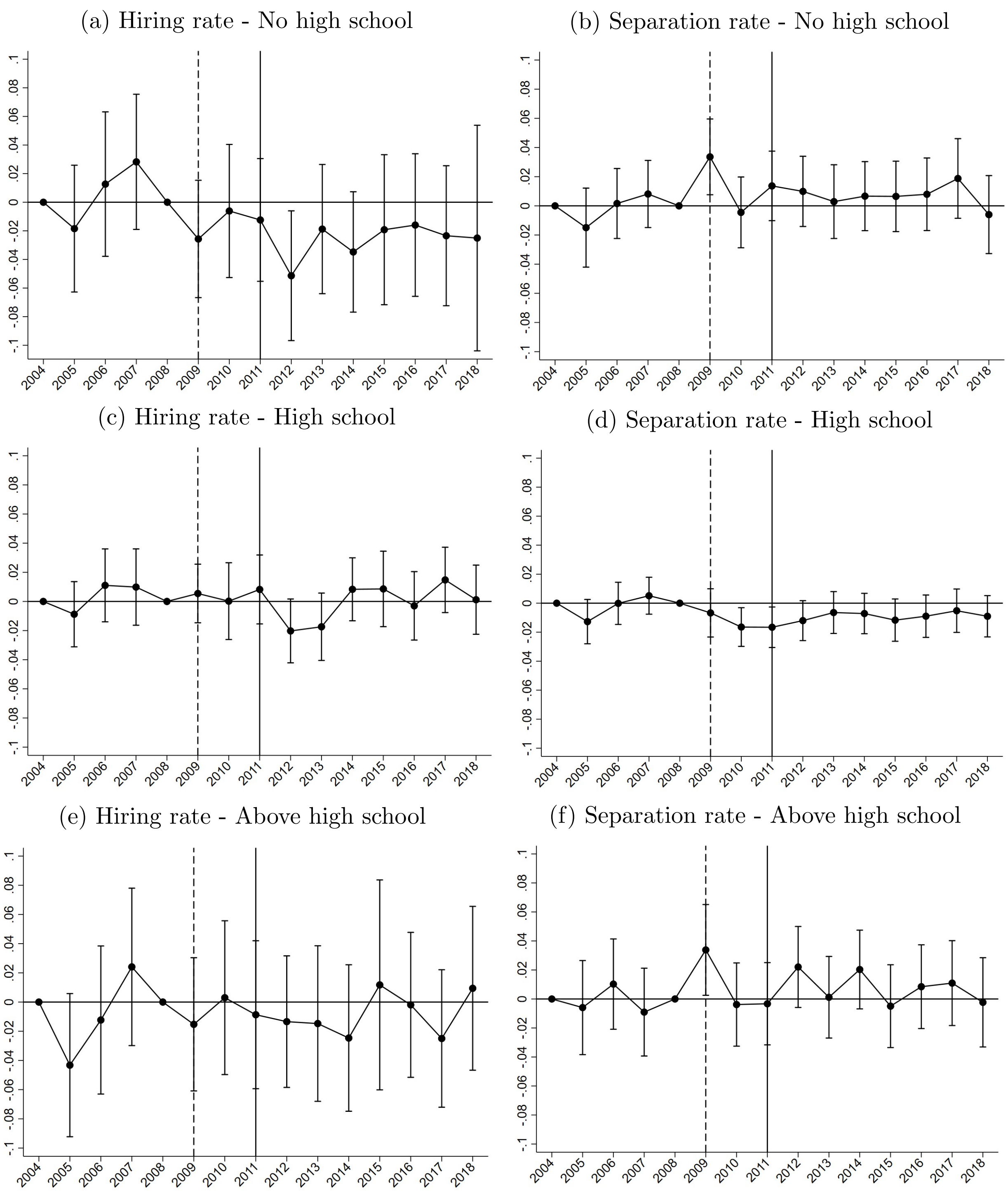


Figure 7: Event study results on labor turnover from estimating Eq. (1) using a balanced panel of firms. Regressions includes industry-by-year fixed effects, and standard errors are clustered by firm. Capped spikes show 95% confidence intervals.

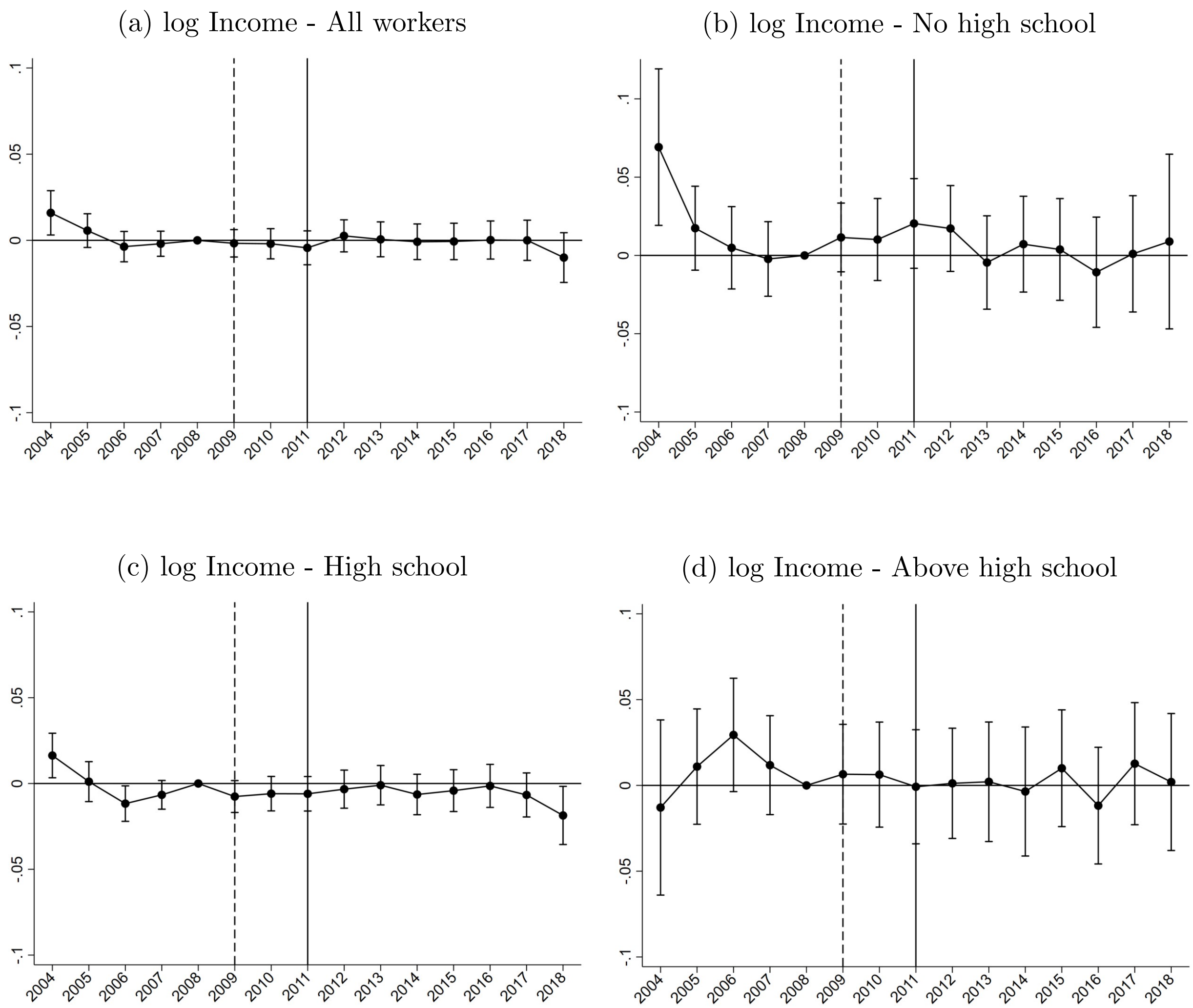


Figure 8: Event study results on workers' (within firm) average annual income from estimating Eq. (1) using a balanced panel of firms. Regressions includes industry-by-year fixed effects and time-varying controls for business cycle exposure, and standard errors are clustered by firm. Capped spikes show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

6 Discussion and Conclusion

In this paper, I provide new empirical evidence on the impacts of climate policy on firm performance and labor market inequality, 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 revenue, value added, and employment among emission-intensive firms. While the negative impacts on firms' economic performance 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 88 euros per ton CO_2 between 2010 and 2018. 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 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.

7 Appendix

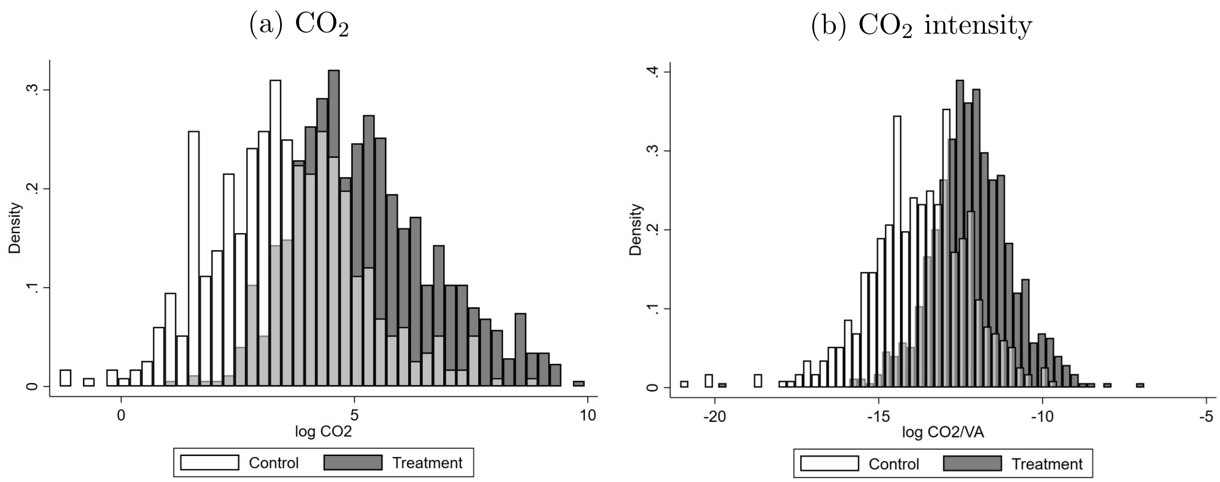


Figure 9: 2008 distribution by treatment status

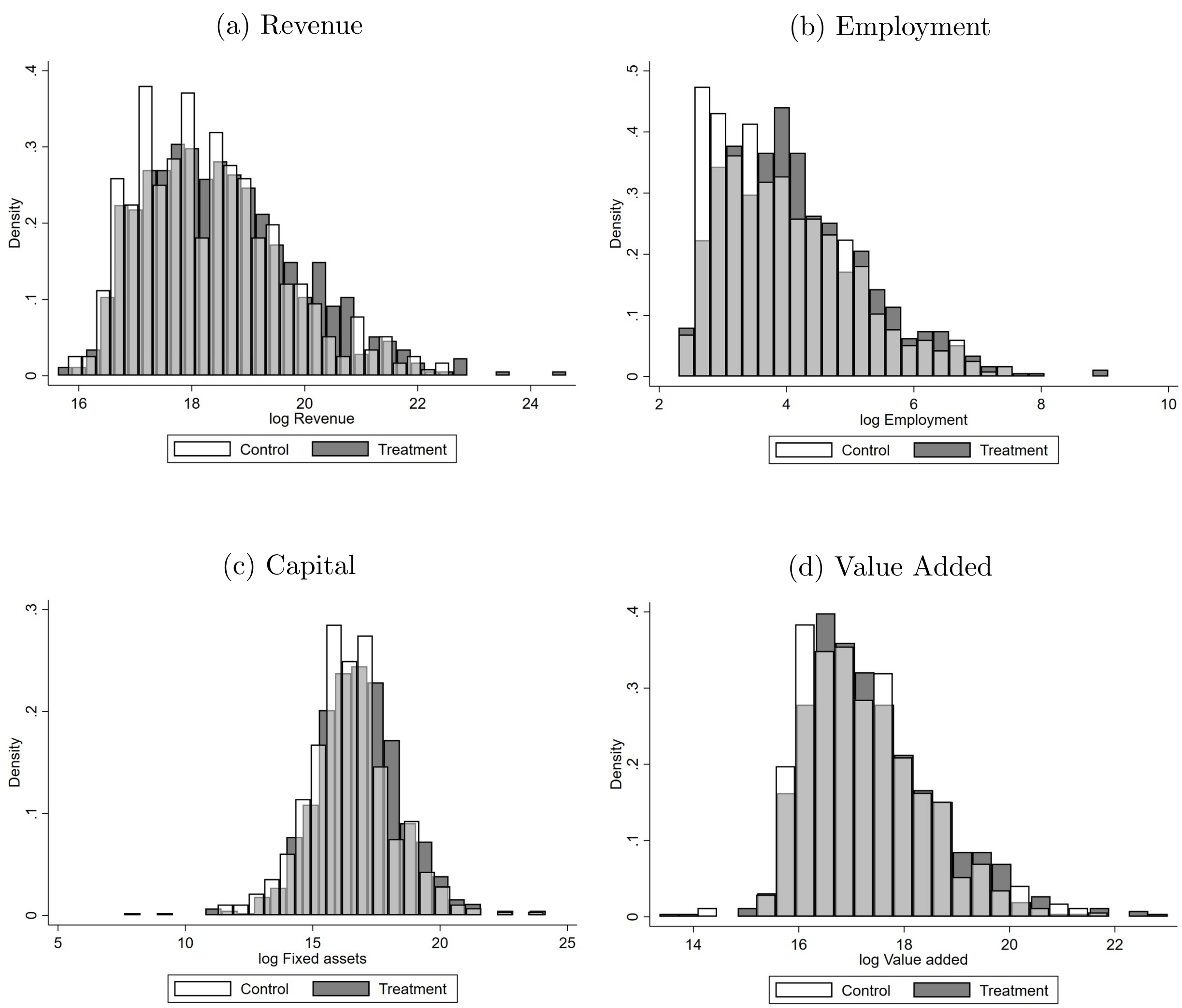


Figure 9 (continued). 2008 distribution by treatment status

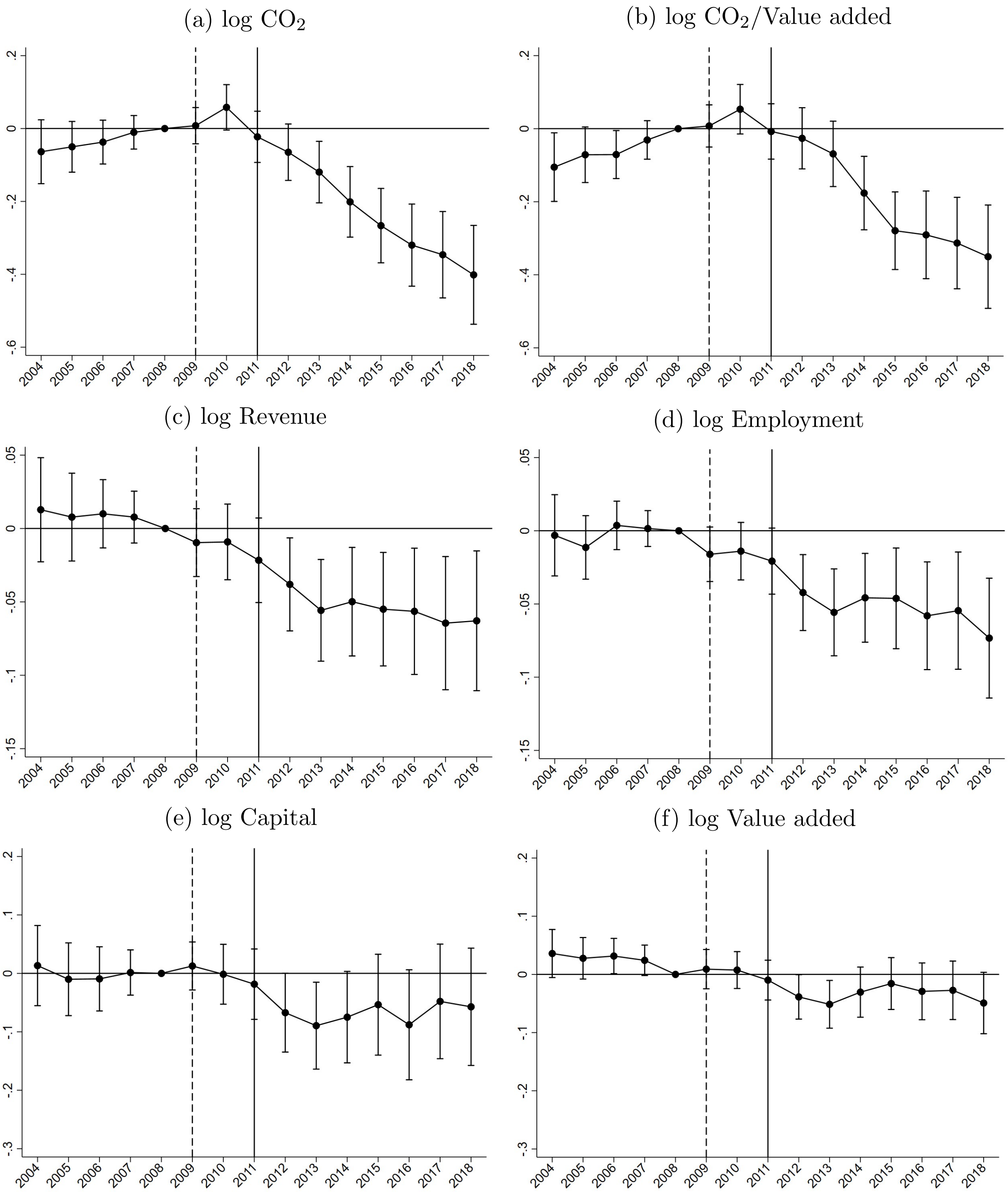


Figure 11: Event study results from estimating Eq. (1) on an unbalanced sample of firms. Regressions includes industry-by-year fixed effects, and standard errors are clustered by firm. Capped spikes show 95% confidence intervals.

Figure 12: Event study results on newly hired workers' (within firm) average annual income from estimating Eq. (1) using 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.

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