

Climate Policy and Labor Market Inequality

[Early Draft]

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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 has heterogeneous employment impacts, and that complementary policies might be needed to address labor market inequalities when implementing climate policy.

1 Introduction

Climate policy and labor market inequality are two of the most debated policy issues. Yet, we know surprisingly little about the effects of climate policy on labor markets. In this paper, I study the effects of climate policy on firms and workers, using rich administrative data in a causal setting. I exploit a reform in the Swedish carbon tax which removed tax

rebates given to firms in the manufacturing sector between 2011-2018. The removal of the tax rebates increased the share of the tax manufacturing firms had to pay from 21% to 100%, substantially increasing the regulatory burden. By observing which firms that made use of tax rebate before they were phased-out, I identify which firms that were affected or not by the reform. In this framework, I study the effects of increasing climate policy stringency on two sets of outcomes. First, I study the effects on firms' environmental and economic performance, such as emissions, revenue and employment. Second, I make use of the matched employer-employee data to study heterogeneous impacts on employment for different types of workers. The second set of result provides a novel contribution to the understanding of the effects of climate policy on labor market inequality.

An increasing number of papers study various carbon pricing initiatives using firm-level data ([Vrolijk and Sato, 2023](#)). Most studies evaluate the European Union Emissions Trading System (EU ETS) or other domestic carbon taxes. The overall conclusion is that carbon pricing indeed reduces carbon emissions or emission intensity ([Ahmadi et al., 2022](#); [Jaraitė and Maria, 2016](#); [Leroutier, 2022](#); [Martin et al., 2014](#); [Martinsson et al., 2024](#)). Another general finding is the lack of significant negative effects of carbon pricing on firms' economic performance or employment ([Colmer et al., 2024](#); [Dechezleprêtre et al., 2023](#); [Marin et al., 2018](#)). However, most papers estimates the impacts on firms' aggregate employment without allowing for heterogeneous effects for different types of workers, with few exceptions. [Yamazaki \(2017, 2019\)](#) analyzes the implementation of a revenue-neutral carbon tax in British Columbia, Canada. Using other Canadian provinces as control groups, the author finds indications of a sectoral reallocation of production workers (wage-paid) towards cleaner sectors. Studying the same reform, [Yip \(2018\)](#) uses individual data from labor force surveys. The study finds that the carbon tax significantly increased unemployment, with stronger effects for low-educated males. However, without access to firm-level data, the author cannot distinguish between changes in firms' labor demand and changes in composition of the local economy.

I contribute to previous research by estimating the effects of increasing climate policy stringency using linked employer-employee data, which has not been used in the literature before. This allows me to investigate heterogeneous impacts across worker types, as well as discussing within-firm mechanisms behind observed effects. First, I find that the increase in

the effective carbon tax rate had a negative effect on firms' emissions. However, the policy also had a negative effect on firms' economic performance, as measured by revenue and employment. The negative employment impacts are concentrated among emission-intensive firms, which are more likely to face a higher cost burden by a higher tax. Importantly, I find that employment impacts are heterogeneous across workers. The reduction in employment is driven by workers without a high school degree, whose negative effect, in turn, is largely driven by males and workers between 40-64 years old. I find indications suggesting that the reduction in employment is achieved by a reduced hiring rate, rather than increased separation. The following section describes the relevant features of the Swedish carbon tax and the implementation of the form.

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 CO₂ 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 CO₂ between 2004-2018.¹ 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

¹The average exchange rate over the period was 9.39 SEK/EUR.

manufacturing process for uses *other than motorized vehicles*.² 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.³ 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.⁴ For firms regulated by the EU ETS, the carbon tax was completely removed in 2011 to avoid double carbon pricing (Ryner, 2022).

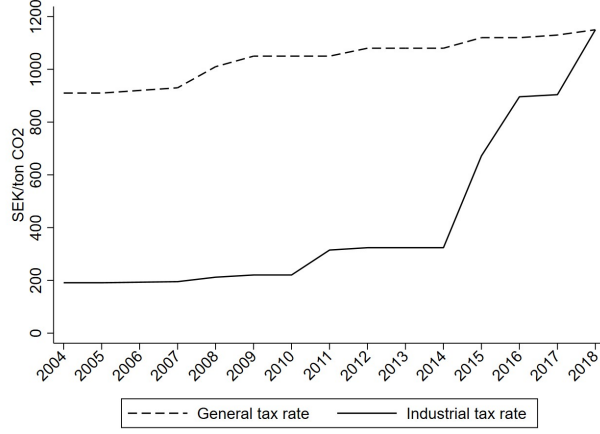
The manufacturing rebates were gradually reduced from 2011, and completely removed in 2018. The phase-out of the rebates was communicated in two steps, which are shown in Figure 1b and 1c. In 2009, the government released a new plan to achieve its medium-term climate targets. The plan included an increase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

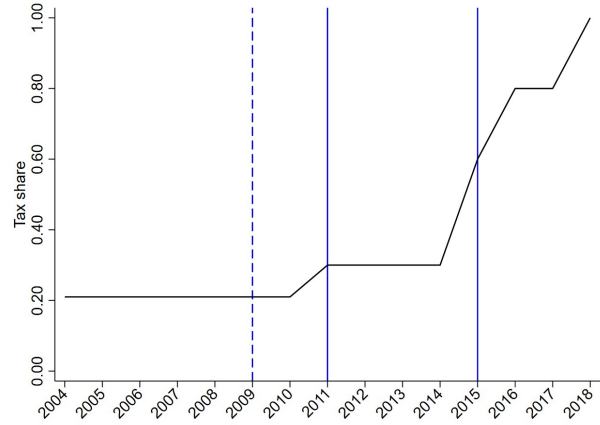
²Examples of industrial motorized vehicles are excavators and wheel loaders. This condition also excludes fuels used for transportation of goods on roads.

³The 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.

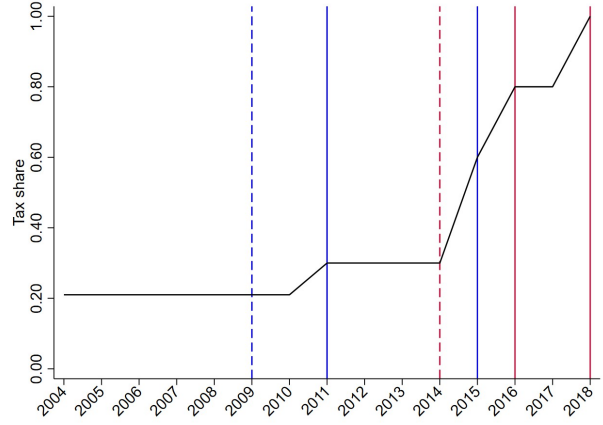
⁴Additional rebates have been directed to specific sectors. Fuels used in the production of energy products and in some metallurgical and mineralogical processes are completely exempt from carbon taxation, as was fuels used for special vehicles in manufacturing in the mining industry until 2020. In addition, tax payments were capped at 0.8% of sales until 2015.



(a) General and industrial CO₂ tax rates



(b) 2009 announcement



(c) 2014 announcement

Figure 1: Timing of policy implementation. Figure 1b shows the announcement of the new climate plan in 2009 (dashed line) with its subsequent increases in industry's carbon tax shares in 2011 and 2015. Figure 1c includes the announcement of the updated climate plan in 2014 (dashed line) with its increased tax share in 2016 and the complete phase-out of rebates in 2018.

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, 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 data 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.⁵ 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

⁵The dataset currently misses observations for 2013.

this issue by setting all negative tax rates to zero, as these firms are likely to have had some tax rebate these years.⁶

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 2008.⁷ This unbalanced panel is used to evaluate the sensitivity of the result to compositional changes (i.e. firm exit), and to conduct heterogeneity analysis across industries with higher statistical power. 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 constitutes 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 are characterized by a heavily skewed distribution, with a strong selection of energy and emission intensive firms falling under EU ETS. However, this is not the case for the number of employed workers, where the analyzed firms make up a substantial share of manufacturing employment.

⁶Since 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.

⁷Observations in 2007 are used to construct control variables related to exposure to the Great Recession in the sensitivity analysis.

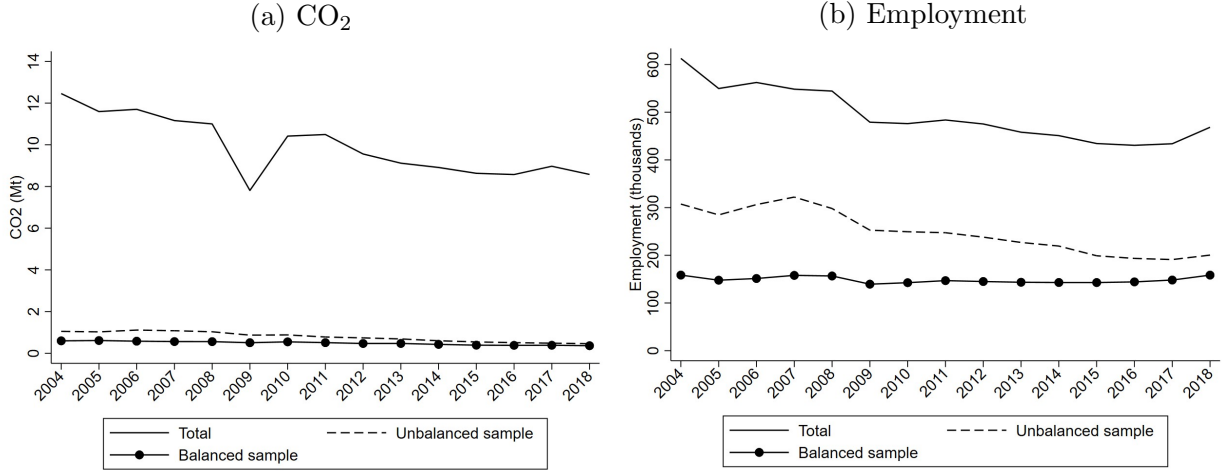


Figure 2: Aggregate emissions and employment over time

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. Treated firms (i.e. those with a carbon tax rebate in 2008) are on average larger in terms of employment, revenue and capital (fixed assets). The difference is, however, most pronounced when comparing CO₂. 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 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 dataset also covers a repeated cross-section of 298,213 unique individuals employed at the firms at some point in the sample period. The typical worker is a high school educated male, and there are small differences in worker composition between treated and control firms in the main (balanced)

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 |
| CO ₂ emissions (ton) | 320.55 | 102.47 | 543.29 | 136.64 | 711.96 |
| Fossil energy share | 0.30 | 0.22 | 0.39 | 0.18 | 0.35 |
| Workers | 298,213 | 105,218 | 192,995 | 49,848 | 106,885 |
| No high school | 0.19 | 0.19 | 0.19 | 0.18 | 0.20 |
| High school | 0.56 | 0.61 | 0.54 | 0.60 | 0.57 |
| Above high school | 0.25 | 0.20 | 0.27 | 0.22 | 0.23 |
| STEM | 0.17 | 0.14 | 0.19 | 0.15 | 0.16 |
| Female | 0.24 | 0.22 | 0.24 | 0.24 | 0.23 |
| Age | 42.82 | 42.87 | 41.80 | 41.89 | 41.74 |

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

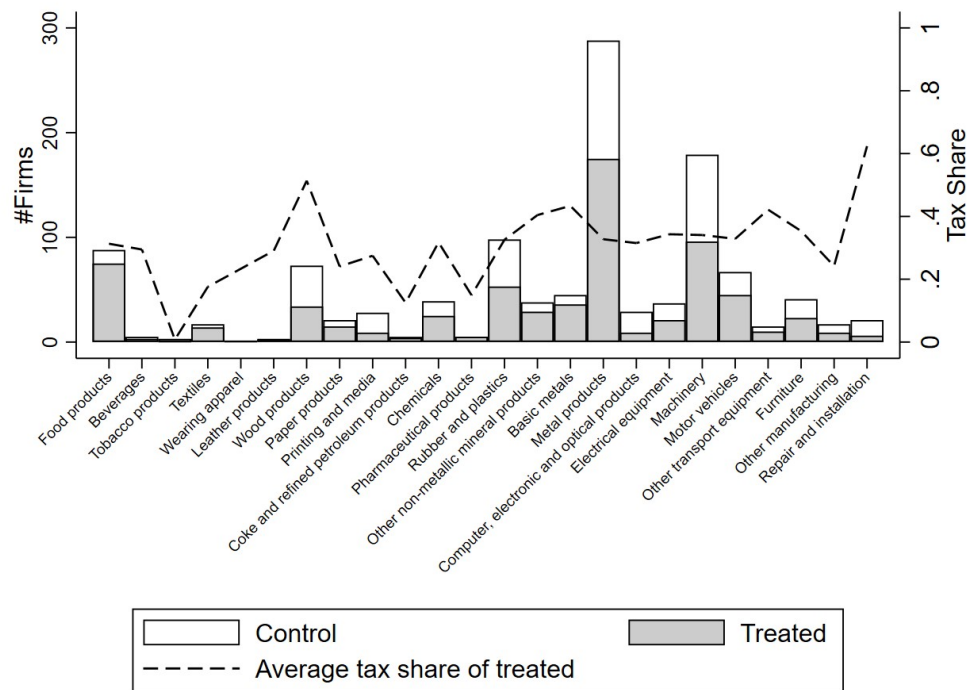


Figure 3: Treatment by industry

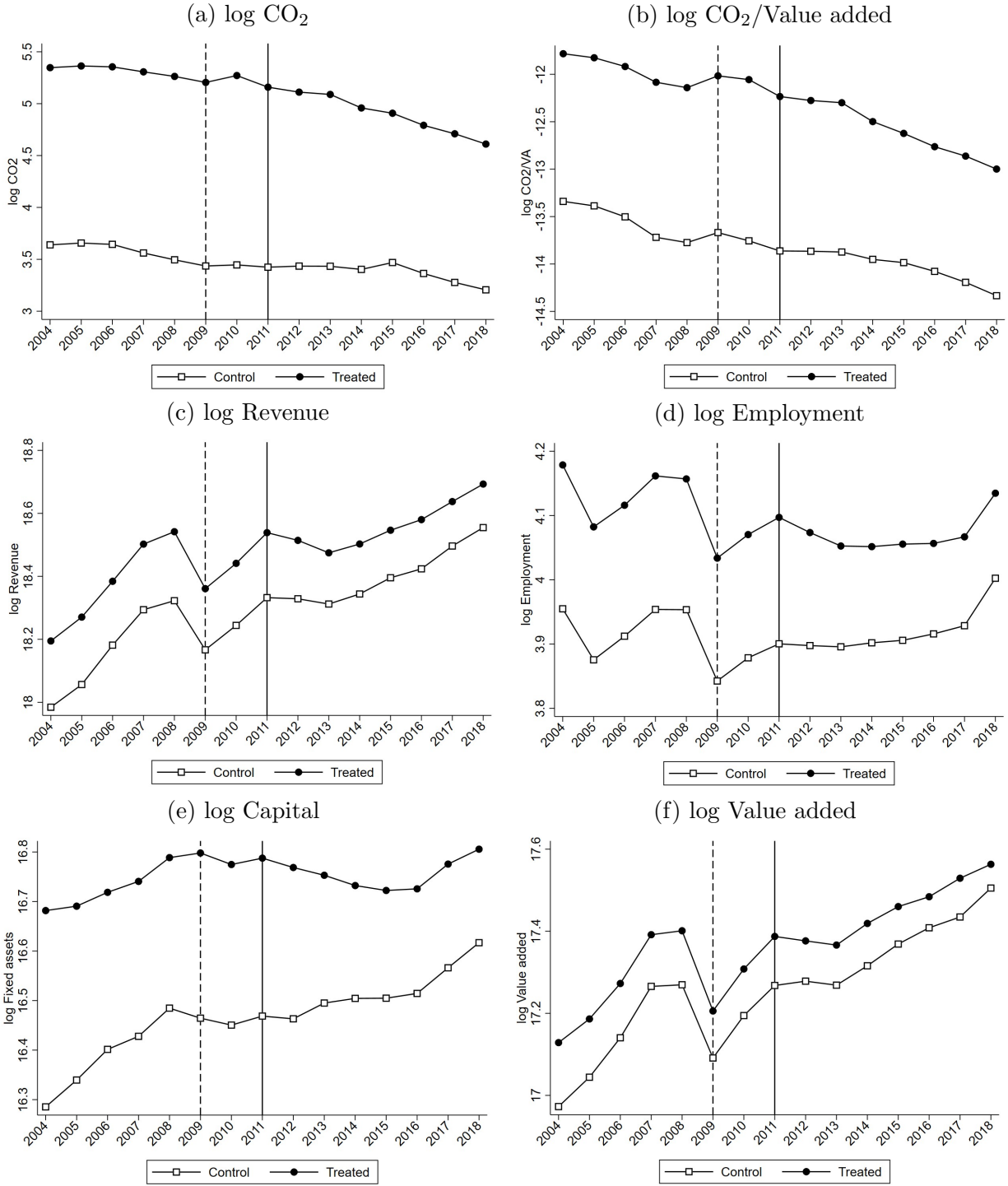


Figure 4: Trends in average outcomes

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

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

where 2008 is the omitted year of reference.⁸ 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 which estimates the following two-period equation

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

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

⁸This means that treatment effects will be compared to differences in the year before the observed impacts of the Great Recession (see [Figure 4](#)).

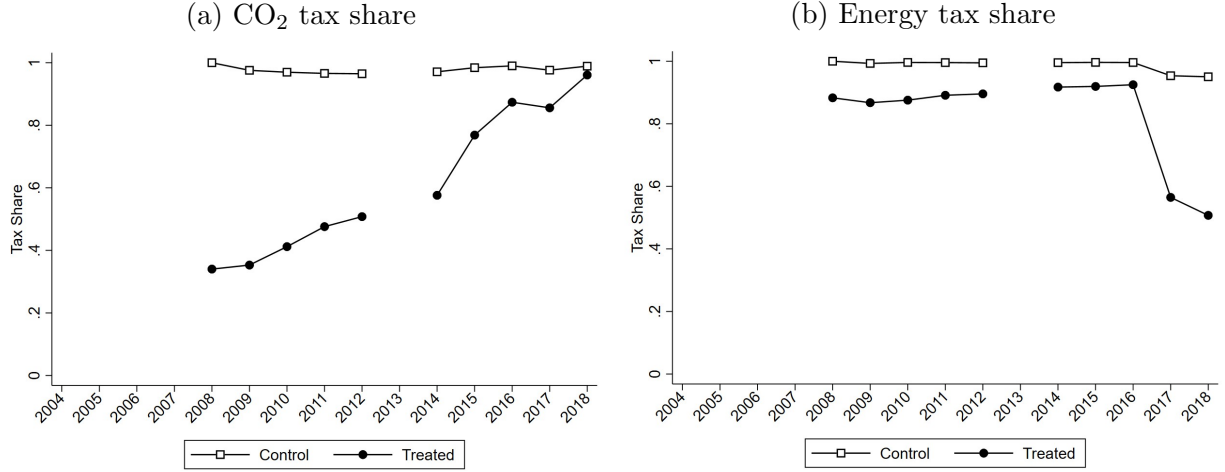


Figure 5: Firm-level fuel tax shares

to control for industry shocks. 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 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 CO₂ 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.

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 and 6b show large, significant reductions in treated firms' total emissions and emission intensity. Starting in 2011, firms reduce emissions continuously until 2015, after which the negative effects stabilize around -40%, without signs of recovery. The reform also seems to have caused a negative impact on firm performance, although not in the proportion of the emission reductions. Both revenue and employment falls after the implementation of the reform, with significantly estimated point estimates around -7%. Similar patterns are observed for capital and value added, although differential trends before the reform and large confidence intervals precludes conclusions regarding the effects on these outcomes. Treated firms are seemingly more affected by the Great Recession, as seen by the downward jump in 2009 in revenue and employment. However, this decline is fully recovered during the announcement period, and has an opposite direction of the estimated treatment effect, suggesting that differential recovery paths after the crisis are not likely to drive the result. Further analysis on the sensitivity to this shock is carried out below.

5.2 Sensitivity of the main result

Business cycle exposure This section provides more evidence on the robustness of the main result to different specifications, starting with controlling for observables related to exposure to business cycles. The announcement of the reform coincides with the negative impacts of the Great Recession in 2009. 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 $\geq 50\%$, respectively. L'_j is also a vector of two indicator variables, which equal 1 for firms whose number of employees

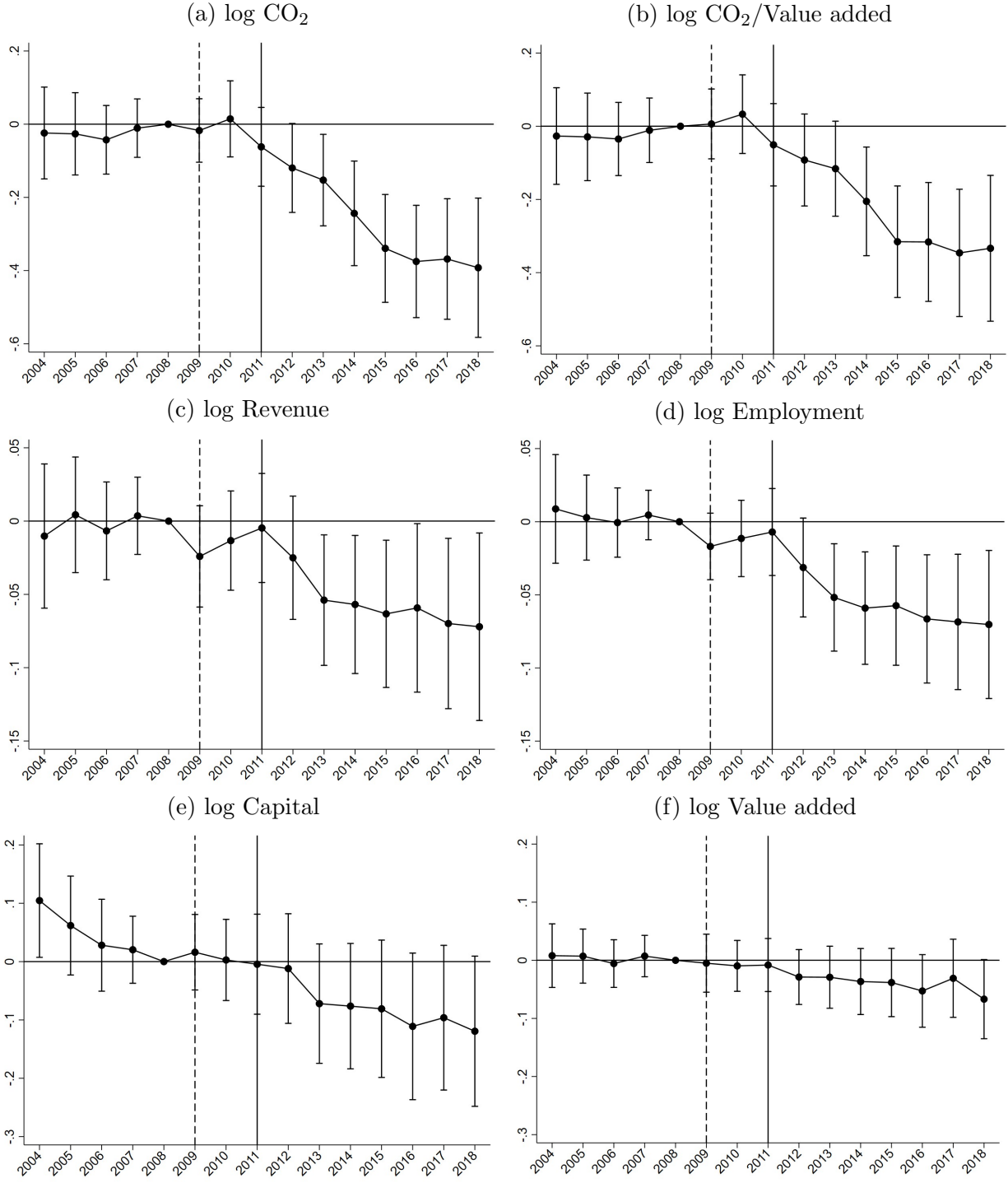


Figure 6: Event study results using a balanced panel of firms. Regressions includes industry-by-year fixed effects, and standard errors are clustered by firm. Capped spikes show 95% confidence intervals.

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 year fixed effects (or $Post_t$ in the long difference specification), 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 impacts relating to firm size.

The result is presented in Table 2. Odd columns show the baseline result of the long difference model, which repeats the observed result of Figure 6. Even columns controls for business cycles proxies. Adding these control variables have minimal impact on the estimated effects, suggesting that biases from overlapping changes in economic conditions are not a large concern for the empirical research design.

| | 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*** (0.098) | -0.396*** (0.098) | -0.347*** (0.103) | -0.343*** (0.103) | -0.080** (0.032) | -0.082** (0.032) | -0.070*** (0.025) | -0.065** (0.025) | -0.114* (0.066) | -0.129** (0.065) | -0.067* (0.034) | -0.065* (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 using a balanced panel of firms. Standard errors in parenthesis are clustered by firm. *** p<0.01, ** p<0.05, * p<0.1

Firm turnover I also investigate the sensitivity of the baseline result to sample restriction. In Figure 7 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. Nevertheless, the estimation suggests that firm exit does not play a role in previous findings, and that the validity of the baseline result is not limited to the subset of surviving firms over the entire time period.

5.3 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. 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. 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 modes of compliance than labor intensive firms (e.g. varying energy-labor substitution possibilities), leading to heterogeneous responses in outcomes. Table 3 shows the result along these dimensions, where the new variables are interacted with the previous treatment term. Column (2) reports the effects on total emissions. The point estimates suggest that both low and high emission intensive firms reduce emissions, with high emission intensive firms responding more strongly. However, the statistical precision is too low to confidently make this claim. Point estimates in column (4), which shows result for emission intensity, has a similar pattern as total emissions. Interestingly, the negative employment effects from previous estimations seem to be concentrated among emission-intensive firms. This is in line with emission-intensive firms facing higher cost increases due to the reform. Capital intensive firms have in general less negative impacts,

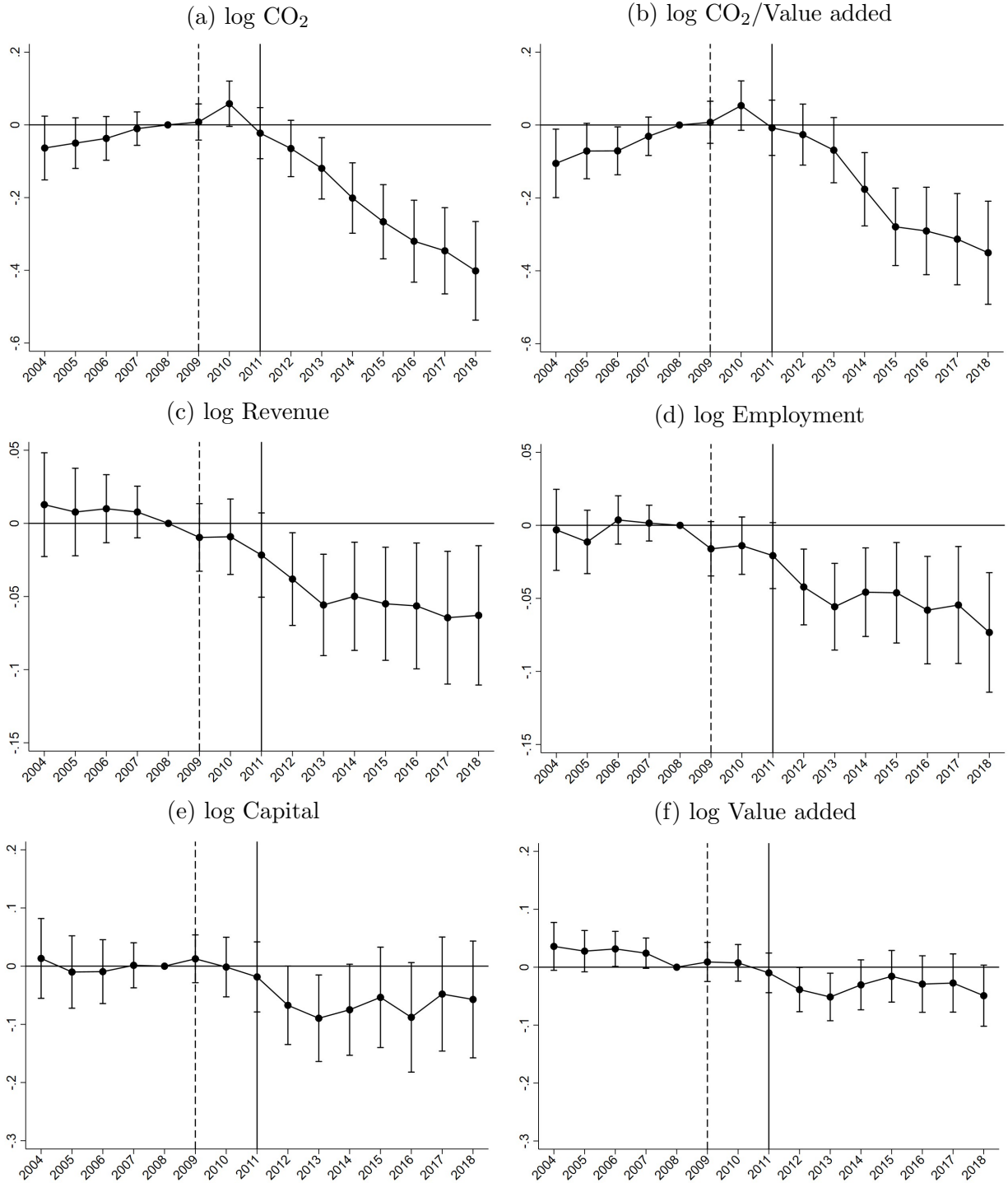


Figure 7: Event study results using an unbalanced sample of firms. Regressions includes industry-by-year fixed effects, and standard errors are clustered by firm. Capped spikes show 95% confidence intervals.

although statistical precision for this interaction term is again low.

| | log CO ₂ | | log CO ₂ /Value added | | 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*** (0.099) | -0.336** (0.168) | -0.343*** (0.103) | -0.368** (0.179) | -0.082** (0.032) | -0.040 (0.050) | -0.065** (0.025) | 0.009 (0.042) | -0.129** (0.065) | -0.145 (0.112) | -0.065* (0.034) | -0.013 (0.058) |
| $D \times Post \times CO_2 \text{ int.}$ | | -0.252 (0.179) | | -0.098 (0.182) | | -0.085 (0.061) | | -0.134** (0.053) | | 0.042 (0.146) | | -0.110 (0.069) |
| $D \times Post \times Capital \text{ int.}$ | | 0.205 (0.179) | | 0.285 (0.187) | | 0.016 (0.063) | | 0.020 (0.049) | | 0.077 (0.127) | | -0.018 (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 |
| $CO_2 \text{ int.}_j \times Post_t$ | | ✓ | | ✓ | | ✓ | | ✓ | | ✓ | | ✓ |
| $Capital \text{ int.}_j \times Post_t$ | | ✓ | | ✓ | | ✓ | | ✓ | | ✓ | | ✓ |

Table 3: Long difference results using a balanced panel of firms. $CO_2 \text{ int.}_j$ is an indicator equal to one for firms with a CO₂ intensity (defined as ton CO₂/value added) above the 2-digit industry median (balanced sample) in 2007. $Capital \text{ int.}_j$ 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

Worker heterogeneity In this part, I explore heterogeneous impacts of the employment effects for different types of workers. Table 4 presents the effects on employment for different educational categories, which are ‘No high school’, ‘High school’, and ‘Above high school’.⁹ 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 to the extent 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 the relatively compressed wage distribution, few workers in Sweden that earns less than 60% of the national median income are full-time workers. I therefore focus on workers with annual incomes above this threshold, and run the same regressions.¹⁰ The result shows a slightly smaller point estimate of -0.139, which is still significant at 1%. Part-time workers are hence unlikely to be driving the negative impacts on low-educated workers.

Next, I present further results on the heterogeneity with respect to gender and age. In Table 5, 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.

Finally, I 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 8, which shows the effects of the policy change on hiring and separation rates for each educational category at the firm. The negative

⁹The workers are categorized by their highest obtained degree.

¹⁰As median incomes are not published for 2008 by the statistical authorities, I use the mean income, which over-excludes workers.

| | log Employment | | | |
|---|----------------------|-------------------|-------------------|-------------------|
| | No high school | High school | Above high school | |
| | | | All | STEM |
| | (1) | (2) | (3) | (4) |
| <i>Panel A: All workers</i> | | | | |
| $D \times Post$ | -0.165*** (0.041) | -0.024 (0.027) | -0.050 (0.039) | -0.045 (0.041) |
| Observations | 2,264 | 2,326 | 2,134 | 1,934 |
| <i>Panel B: Full-time workers[†]</i> | | | | |
| $D \times Post$ | -0.139*** (0.040) | -0.032 (0.027) | -0.041 (0.039) | -0.053 (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 4: Long difference results on employment 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) |
| <i>Panel A: No high school</i> | | | | | | |
| $D \times Post$ | -0.172*** (0.042) | -0.071 (0.060) | -0.063 (0.077) | 0.013 (0.073) | -0.146** (0.065) | -0.127*** (0.045) |
| Observations | 2,216 | 1,380 | 1,344 | 1,150 | 1,358 | 2,032 |
| <i>Panel B: High school</i> | | | | | | |
| $D \times Post$ | -0.027 (0.028) | -0.040 (0.043) | -0.085 (0.057) | -0.012 (0.052) | -0.010 (0.046) | -0.041 (0.041) |
| Observations | 2,324 | 2,062 | 2,036 | 2,104 | 2,232 | 2,242 |
| <i>Panel C: Above high school</i> | | | | | | |
| $D \times Post$ | -0.036 (0.041) | -0.040 (0.053) | -0.080 (0.074) | 0.047 (0.061) | -0.085 (0.057) | -0.047 (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 5: Long difference results on employment using a balanced panel of firms. Standard errors in parenthesis are clustered by firm. *** p<0.01, ** p<0.05, * p<0.1

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 very large.

6 Discussion and Conclusion

In this paper, I provide new empirical evidence on the causal impacts of climate policy on firms and workers. By exploiting a reform which increased the effective carbon tax rate for a subset of manufacturing firms in Sweden, I find that increasing climate policy substantially reduced firms' emissions. However, the policy also had a negative effect on firms' economic performance, as measured by revenue and employment. Negative employment impacts are concentrated among emission-intensive firms, in line with expectations. Importantly, I find that employment impacts are heterogeneous across workers. The entire reduction in employment is driven by workers without a high school degree, whose negative effect, in turn, is largely driven by males and workers between 40-64 years old. I find indications suggesting that the reduction in employment is achieved by a reduced hiring rate, rather than increased separation.

The result is in contrast with the majority of previous research on the economic impacts of climate policy, which tend to find small or zero effects on firm performance. This difference might be explained by different levels of stringency in the policies studies, as well as different economic conditions characterizing the regulated firms. A large portion of previous studies focuses on the EU ETS, which regulates large manufacturing firms through emissions trading. The different results could therefore reflect the fact that estimated coefficients represent average treatment effects on the treated, where different papers evaluate different subset of firms. In addition, the regulatory design of emissions trading could result in positive financial gains for some firms during the studied time period in some papers, which would result in different output effects compared to a carbon tax.

The novel result on the heterogeneous impacts across workers contributes to previous literature, by exploiting the linkage between firms and workers in the Swedish administrative data. The fact that negative impacts are concentrated among low-educated workers

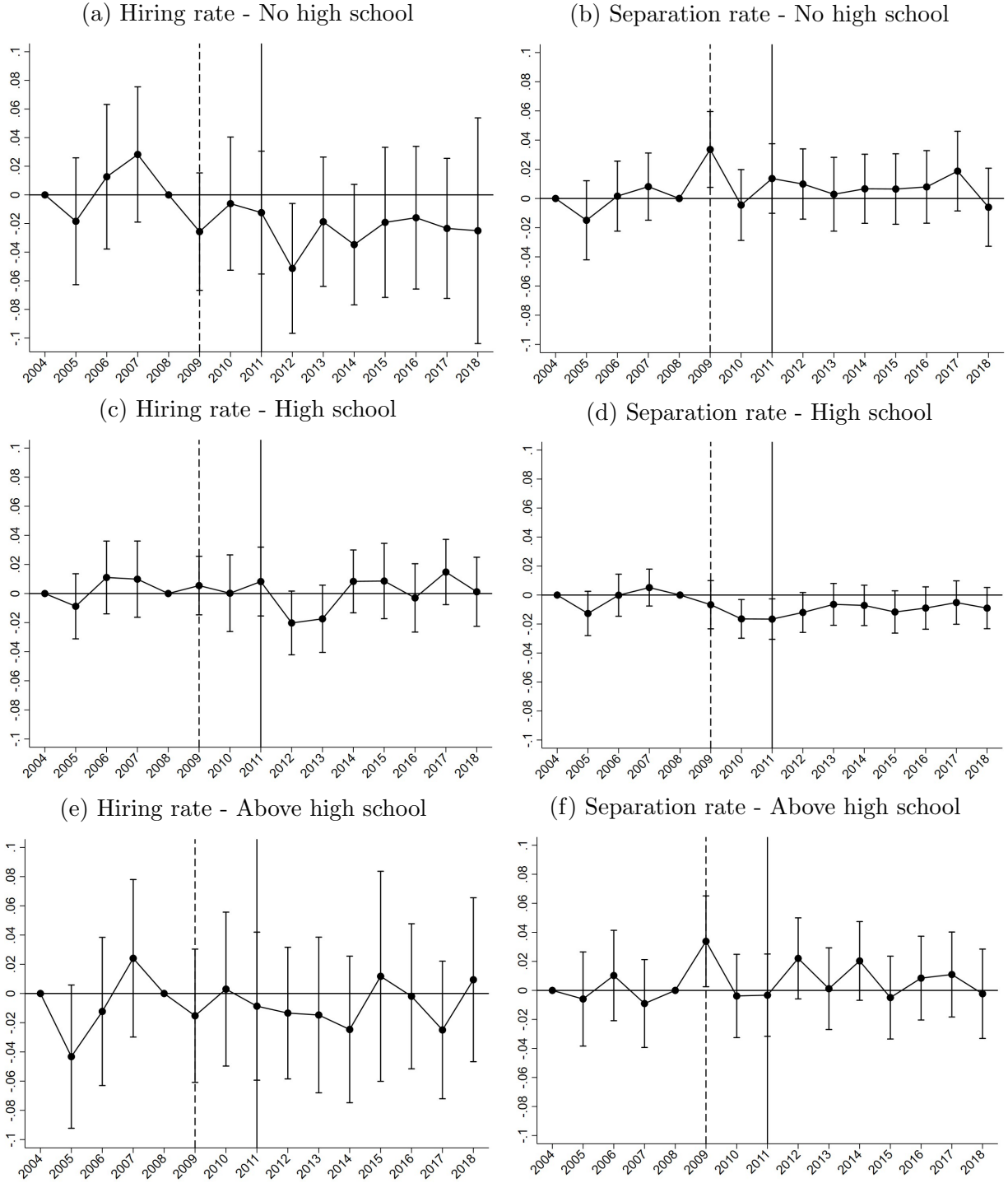


Figure 8: Event study results on labor turnover using a balanced panel of firms. Regressions includes industry-by-year fixed effects, and standard errors are clustered by firm. Capped spikes show 95% confidence intervals.

and cost-exposed emission-intensive firms, suggests that the mechanism operates through a negative output effect, without any sign of substitution effects. While the current analysis in this paper cannot differentiate between changes due to contraction of physical output and labor-replacing productivity changes, it is indicative of the former that revenue is similarly negatively affected.

Lastly, the conclusions of this paper have a strong connection to policy. Not only do the outcomes of the analysis show that carbon taxation can indeed be an effective tool for climate change mitigation. The results also emphasize the need for complementary policies in order to address labor market inequalities, which might be reinforced by cost-increasing climate policy. The fact that the results show negative employment impacts for workers without a high school degree is concerning given that the unemployment rate is high for this group. Future research should investigate the optimal strategy to mitigate these concerns.

7 Appendix

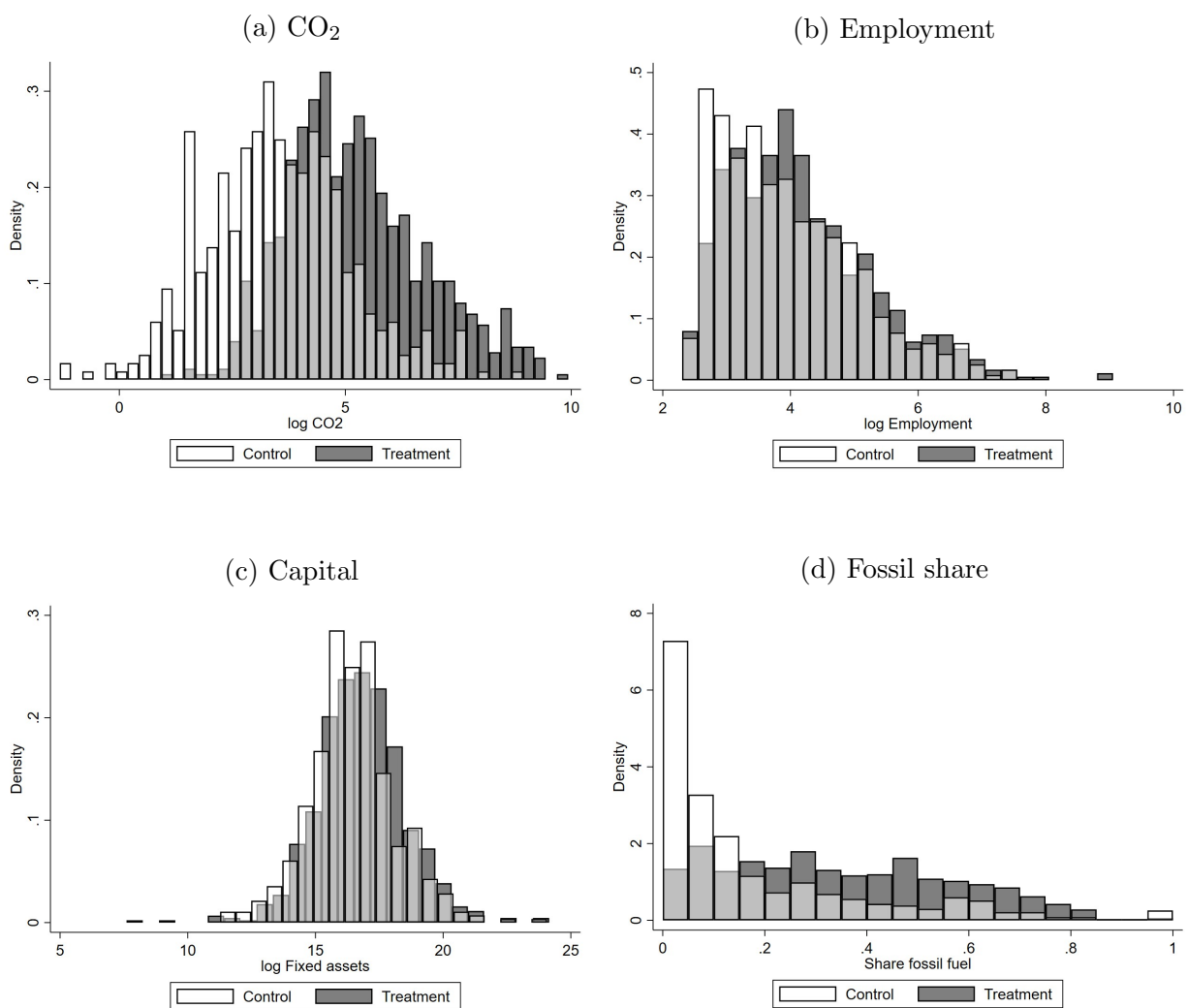


Figure 9: 2008 distribution by treatment status

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